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RISK AND RATIONALITY: UNCOVERING HETEROGENEITY IN PROBABILITY DISTORTION

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It has long been recognized that there is considerable heterogeneity in individual risk taking behavior, but little is known about the distribution of risk taking types. We present a parsimonious characterization of risk taking behavior by estimating a finite mixture model for three different experimental data sets, two Swiss and one Chinese, over a large number of real gains and losses. We find two major types of individuals: In all three data sets, the choices of roughly 80% of the subjects exhibit significant deviations from linear probability weighting of varying strength, consistent with prospect theory. Twenty percent of the subjects weight probabilities near linearly and behave essentially as expected value maximizers. Moreover, individuals are cleanly assigned to one type with probabilities close to unity. The reliability and robustness of our classification suggest using a mix of preference theories in applied economic modeling.

KEYWORDS: Individual risk taking behavior, latent heterogeneity, finite mixture models, prospect theory.

1. INTRODUCTION

RISK IS A UBIQUITOUS FEATURE of social and economic life. Many of our everyday choices, and often the most important ones, such as what trade to learn and where to live, involve risky consequences. While it has long been recognized that individuals differ in their risk taking attitudes, comparatively little is known about the distribution of risk preferences in the population.[2] Since preferences are one of the ultimate drivers of behavior, knowledge of the composition of risk attitudes is paramount to predicting economic behavior. Economic models often allow for heterogeneity, but this heterogeneity is usually defined by the boundaries of the standard model of preferences, expected utility theory (EUT). The empirical evidence, however, reveals that heterogeneity in risk taking behavior is of a substantive kind, that is, some people evaluate risky prospects consistently with EUT, whereas other people depart substantially from expected utility maximization (Hey and Orme (1994)). Moreover, it seems to be the case that rational decision makers, revealing EUT preferences, constitute only a minority of the population (Lattimore, Baker, and Witte (1992)).

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[2]Exceptions include Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner (2005), Eckel, Johnson, and Montmarquette (2005), Harrison, Lau, Rutström, and Sullivan (2005), and Harrison, Lau, and Rutström (2007).

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To improve descriptive performance, a plethora of alternative theories have been developed. Unfortunately, no single best fitting model has been identified so far (Harless and Camerer (1994), Starmer (2000)) and, depending on the individual, one or the other model fits better. This finding poses a serious problem for applied economics. What the modeler needs is a *parsimonious* representation of risk preferences that is empirically well grounded and robust, and not a host of different functionals. Providing such a parsimonious characterization of heterogeneity in risk taking behavior is the objective of this paper.

Our method is based on a literature on classifying individuals which has been recently adopted by the social sciences. On the basis of statistical classification procedures, such as finite mixture models, investigators have tried to discover which decision rules people actually apply when playing games or dealing with complex decision situations (El-Gamal and Grether (1995), Stahl and Wilson (1995), Houser, Keane, and McCabe (2004), Houser and Winter (2004)). The finite mixture approach does not require fitting a model for each individual, which is—given the usual quality of choice data—frequently impossible and often not desirable in the first place. Instead, our method reveals latent heterogeneity by estimating the proportions of distinct behavioral types in the population and assigning each individual to one endogenously defined behavioral type, characterized by a unique set of parameter values.

We apply such a finite mixture model to choice data from three different experiments, two of which were conducted in Zurich, Switzerland. The third experiment took place in Beijing, People's Republic of China. We analyze 448 subjects' decisions over real monetary gains and losses, which comprise a total of nearly 18,000 observations. All three experiments were designed in a similar manner and served to elicit certainty equivalents for binary lotteries. Using a flexible sign-dependent functional as the basic behavioral model, we show the following main results.

First, the estimation procedure renders a robust classification of risk taking behavior across all three data sets. Moreover, the proportions of these distinct types in their respective populations are very similar.

Second, almost all the experimental subjects are unambiguously assigned to one distinct type. Measuring the quality of classification by the normalized entropy criterion (Celeux and Soromenho (1996)), ambiguity of assignments turns out to be extremely low. Thus, we observe hardly any mixed types, that is, individuals with a high probability (of say 0.4) of being one type *and* a high probability (of say 0.6) of being another type. This clean segregation suggests that the classification procedure is able to capture the distinctive characteristics of each behavioral type.

Third, without restricting parameter values a priori, we find that in all three data sets, the minority type, which constitutes about 20% of the population, weights probabilities and values monetary outcomes near linearly. Consequently, this group of individuals can essentially be characterized as expected value maximizers. This result is particularly interesting in the light of Rabin's (2000) calibration theorem, which shows that expected utility maximizers should be approximately risk neutral for small stakes, which typically are encountered in laboratory experiments, if behavior under high stakes is to remain within a plausible range of risk aversion. Therefore, we label subjects belonging to this group of nearly risk neutral people as EUT types. Moreover, the EUT group remains robust to increasing the number of types in the mixture.

Fourth, the majority of individuals, labeled cumulative prospect theory (CPT) types, are characterized by significant departures from linear probability weighting, consistent with prospect theory. As three-group classifications show, this group's behavior can be characterized as a mixture of two different types: In all three data sets a proportion of approximately 30% of the subjects display pronounced departures from linear probability weighting, whereas the relative majority of 50% differ less radically from linear probability weighting.

Finally, within the class of CPT types, we find major differences between Swiss and Chinese behavior. Sensitivity to changes in probabilities is generally lower for the Chinese subjects than for the Swiss. While the majority CPT groups' probability weighting curves do not differ dramatically between countries, the minority groups display diametrically opposed patterns of probability weighting. In particular, the minority Chinese CPT group weights probabilities extremely favorably, rendering them risk seeking over a considerable range of probabilities. The minority Swiss CPT group, however, is characterized by the opposite behavior. Thus, our analysis provides a deeper understanding for the finding that, on average, the Chinese tend to be more risk seeking than westerners (Kachelmeier and Shehata (1992)).

Our results show that the classification procedure successfully uncovers latent heterogeneity in the population. If there is heterogeneity of a substantive kind, as the data suggest, basing predictions on a single preference theory is inappropriate and may lead to biased results (Wilcox (2006)). EUT preferences should be taken into account alongside prospect theory preferences, even if rational EUT individuals constitute only a minority in the population. As the literature on the role of bounded rationality under strategic complementarity and substitutability shows, the mix of rational and irrational actors may be decisive for aggregate outcomes (Haltiwanger and Waldman (1985, 1989), Fehr and Tyran (2005), Camerer and Fehr (2006), Fehr and Tyran (2008)). Depending on the nature of strategic interdependence, the behavior of even a minority of players may drive the aggregate outcome. Therefore, the mix of types in the population is a crucial variable in predicting market outcomes. Since the finite mixture model provides a robust and reliable classification of individuals, the resulting estimates of group sizes and group-specific parameters may serve as valuable inputs for applied economics.

The finite mixture method has been used by others in the context of modeling risk taking. However, to the best of our knowledge, there is no previous study showing a nearly identical classification of risk preference types for three independent data sets. Additionally, our analysis breaks new ground by showing that EUT types emerge endogenously and by extending classification to three groups. Related work by Harrison and colleagues (Andersen, Harrison, and Rutström (2006), Harrison, Humphrey, and Verschoor (2010), Harrison and Rutström (2009)) applies finite mixture models as well, but differs from our approach. Their estimation procedure is based on the a priori assumption that *choices*, irrespective of by whom they were taken, are either EUT consistent or CPT consistent, that is, it sorts choices by a predefined decision model. In contrast, we aim to classify *individuals* by endogenously defined type. Therefore, if there is a group of people whose behavior can best be described by EUT, they should get identified by the classification procedure. Furthermore, in certain decision situations, choices of EUT individuals and CPT individuals do not differ substantially from one another and, therefore, both decision models fit equally well. Consequently, depending on the data available, classification by EUT- and CPT-consistent *decisions* may differ markedly from classification by *decision makers' types*.

A recent study by Conte, Hey, and Moffatt (2010) is also dedicated to finite mixture modeling of risk taking behavior. Their results for British subjects corroborate our conclusions: Even though their work differs from ours in set of lotteries, elicitation method, and estimation procedure, and restricts one behavioral type to be EUT a priori, they also find that in the domain of gains, 80% of the individuals exhibit nonlinear probability weighting, whereas 20% are assigned to EUT.

The paper is structured as follows. Section 2 describes the experimental design and procedures of the three experiments. The functional specification of the behavioral model and the finite mixture model are discussed in Section 3. Section 4 presents descriptive statistics of the data and the results of the classification procedure. Section 5 concludes.

2. EXPERIMENTAL DESIGN

In the following section we describe the experimental setup and procedures. The experiments took place in Zurich in 2003 and 2006 as well as in Beijing in 2005. In Zurich, all subjects were recruited from the subject pool of the Institute for Empirical Research in Economics, which consists of students of all fields of the University of Zurich and the Swiss Federal Institute of Technology Zurich. In Beijing, subjects were recruited by flier distributed at the campuses of Peking University and Tsinhua University. Since all three experiments are based on the same design principles, we will present the prototype experiment Zurich 2003 in detail and describe the extent to which the other two experiments deviate. The main distinguishing features of the different experiments are summarized in Table I.

We elicited certainty equivalents for a large number of two-outcome lotteries. One-half of the lotteries were framed as choices between risky and certain

TABLE I

DIFFERENCES IN EXPERIMENTAL DESIGN

	Zurich 03	Zurich 06	Beijing 05
Number of			
Subjects	179	118	151
Lotteries	50	40	28
Observations	8906	4669	4225
Procedure	Computerized	Computerized	Paper and pencil
Framing	Abstract and contextual	Contextual	Abstract and contextual

gains ("gain domain"); the other half were presented as choices between risky and certain losses ("loss domain").[3] For each decision in the loss domain, subjects were endowed with a specific monetary amount, which served to cover potential losses and equalized expected payoffs of corresponding gain and loss lotteries. In the Zurich 2003 and the Beijing experiments, 50% of the subjects were confronted with decisions framed in the standard gamble format. The other 50% of the subjects had to make choices framed in contextual terms, that is, gains were represented as risky or sure investment gains, and losses were represented as repair costs and insurance premiums, respectively. The Zurich 2006 experiment was based on contextually framed lotteries only. In Zurich, outcomes x_1 and x_2 ranged from 0 to 150 Swiss francs.[4] The payoffs in the Beijing 2005 experiment were commensurate with the compensation in Zurich and varied between 4 and 55 Chinese yuan.[5] Expected payoffs per subject amounted to approximately 31 Swiss francs and 20 Chinese yuan, respectively, which was considerably more than a local student assistant's hourly compensation, plus a show up fee of 10 Swiss francs and 20 Chinese yuan, thus generating salient incentives. Probabilities p of the lotteries' higher gain or loss x_1 varied from 5% to 95%. The gain lotteries for Zurich 2003 are presented in Table II. The other two experiments essentially included a subset of these. The lotteries appeared in random order on a computer screen[6] in the Swiss experiments and on paper in Beijing.

In the computerized experiments, the screen displayed a decision sheet containing the specifics of the lottery under consideration and a list of 20 equally spaced certain outcomes, ranging from the lottery's maximum payoff to the

[3]There were no mixed lotteries involving both gains and losses.

[4]At the time of the experiments, 1 Swiss franc equaled about 0.76 and 0.84 U.S. dollars, respectively.

[5]At the time of the experiment, 1 Chinese yuan equaled about 0.12 U.S. dollars.

[6]The experiment was programmed and conducted with the software z-Tree (Fischbacher (2007)).

TABLE II

GAIN LOTTERIES (x_1, p; x_2), ZURICH 2003[a]

p	x_1	x_2	p	x_1	x_2	p	x_1	x_2
0.05	20	0	0.25	50	20	0.75	50	20
0.05	40	10	0.50	10	0	0.90	10	0
0.05	50	20	0.50	20	10	0.90	20	10
0.05	150	50	0.50	40	10	0.90	50	0
0.10	10	0	0.50	50	0	0.95	20	0
0.10	20	10	0.50	50	20	0.95	40	10
0.10	50	0	0.50	150	0	0.95	50	20
0.25	20	0	0.75	20	0			
0.25	40	10	0.75	40	10			

[a]Outcomes x_1 and x_2 are denominated in Swiss francs.

lottery's minimum payoff, as shown in Figure 1.[7] The subjects had to indicate in each row of the decision sheet whether they preferred the lottery or the certain payoff. The lottery's certainty equivalent was calculated as the arithmetic mean of the smallest certain amount the subject preferred to the lottery and the subsequent certain amount on the list, when the subject had, for the first time, reported preference for the lottery. For example, if the subject had decided as indicated by the small circles in Figure 1, her certainty equivalent would amount to 13.5 Swiss francs.

Before subjects were permitted to start working on the real decisions, they had to correctly calculate the payoffs for two hypothetical choices. In the computerized experiments, there were two trial rounds to familiarize the subjects with the procedure. At the end of the experiment, one row number of one decision sheet was randomly selected for each subject, and the subject's choice in that row determined her payment. Subjects were paid in private afterward. The subjects could work at their own speed; the vast majority of them needed less than an hour to complete the experimental tasks as well as a socio-economic questionnaire.

3. ECONOMETRIC MODEL

This section discusses the specification of the finite mixture model, which allows controlling for latent heterogeneity in risk taking behavior in a parsimonious way. For the purpose of classifying subjects according to risk taking type, we need to specify three ingredients of the mixture model: the basic theory of decision under risk, the functional form of the decision model, and the specification of the error term.

[7]The format of the decision sheet for the Beijing experiment was identical to the Zurich one.

Decision situation:
22

	Option A	**Your Choice:**				**Option B Guaranteed payoff amounting to:**
1		**A**		**o**	**B**	20
2		**A**		**o**	**B**	19
3		**A**		**o**	**B**	18
4		**A**		**o**	**B**	17
5		**A**		**o**	**B**	16
6		**A**		**o**	**B**	15
7	A profit of CHF 20 with	**A**		**o**	**B**	14
8		**A**	**o**		**B**	13
9	probability 75%	**A**	**o**		**B**	12
10		**A**	**o**		**B**	11
11	and a profit of CHF 0 with	**A**	**o**		**B**	10
12		**A**	**o**		**B**	9
13	probability 25%	**A**	**o**		**B**	8
14		**A**	**o**		**B**	7
15		**A**	**o**		**B**	6
16		**A**	**o**		**B**	5
17		**A**	**o**		**B**	4
18		**A**	**o**		**B**	3
19		**A**	**o**		**B**	2
20		**A**	**o**		**B**	1

OK

FIGURE 1.—Design of the decision sheet.

The underlying theory of decision under risk should be able to accommodate a wide range of different behaviors. Sign- and rank-dependent models capture reference dependence and nonlinear probability weighting. Therefore, a flexible approach in the spirit of cumulative prospect theory (CPT) lends itself to describing risk taking behavior. Moreover, CPT nests EUT as special case.[8] If there is a group of people whose behavior can best be described by EUT, these individuals should be identified by the finite mixture estimation as a unique group exhibiting the predicted behavior.

Suppose that there are C different types of individuals in the population. According to CPT, an individual belonging to a certain group $c \in \{1, \dots, C\}$ values any binary lottery $\mathcal{G}_g = (x_{1g}, p_g; x_{2g}),\ g \in \{1, \dots, G\}$, where $|x_{1g}| > |x_{2g}|$, by

$$v(\mathcal{G}_g) = v(x_{1g})w(p_g) + v(x_{2g})(1 - w(p_g)).$$

[8]The bulk of previous research has been conducted under the tacit assumption that utility is defined over lottery outcomes rather than lottery outcomes integrated with total wealth. In Section 4.8.1, we extend the model to accommodate the possibility of integration.

The function $v(x)$ describes how monetary outcomes x are valued, whereas the function $w(p)$ assigns a subjective weight to every outcome probability p. The lottery's certainty equivalent $\hat{\text{ce}}_g$ can then be written as

$$\hat{\text{ce}}_g = v^{-1}\big[v(x_{1g})w(p_g) + v(x_{2g})(1 - w(p_g))\big].$$

To make CPT operational, we have to assume specific functional forms for the value function $v(x)$ and the probability weighting function $w(p)$. A natural candidate for $v(x)$ is a sign-dependent power function

$$v(x) = \begin{cases} x^\alpha, & \text{if } x \geq 0, \\ -(-x)^\beta, & \text{otherwise,} \end{cases}$$

which can be conveniently interpreted and has turned out to be the best compromise between parsimony and goodness of fit in the context of prospect theory (Stott (2006)). Our specification of the value function seems to lack a prominent feature of prospect theory, loss aversion, capturing that "[...] most people find symmetric bets of the form $(x, 0.5; -x, 0.5)$ distinctly unattractive" (Kahneman and Tversky (1979, p. 279)). In this interpretation, loss aversion measures a decision maker's *attitude toward mixed lotteries*, encompassing both gains and losses.[9] Our lottery design does not contain any mixed lotteries, however. When there are only single-domain lotteries and loss aversion is introduced into our model in the conventional way, that is, by assuming $v(x) = -\lambda(-x)^\beta$ for $x < 0$ and $\lambda > 0$ (Tversky and Kahneman (1992)), the parameter of loss aversion λ is not identifiable: λ cancels out in the definition of the certainty equivalent ce of a loss lottery $(x_1, p; x_2)$ with $x_1 < x_2 \leq 0$, as $\lambda(-\text{ce})^\beta = \lambda(-x_1)^\beta w(p) + \lambda(-x_2)^\beta(1 - w(p))$ holds for *any* value of λ. Consequently, when there are no mixed lotteries available, estimating such a parameter is neither feasible nor meaningful.

Obviously, this argument rests on the assumption that subjects' reference point with respect to which gains and losses are defined is equal to zero. However, subjects might encode positive payments as gains only if they exceed a certain threshold, which would turn some of the objectively given gain lotteries into mixed ones, containing both subjective gains and losses. While in principle this is possible, estimating this reference point is questionable when there are no mixed lotteries from the onset, which would provide valuable additional information for locating the reference point reliably. To complicate matters, near linear value functions, as is predominantly the case for our data, pose severe

[9]Köbberling and Wakker (2005, p. 125) viewed loss aversion as a component of risk attitudes which is logically independent from basic utility: "Prospects [...] will exhibit considerably less risk aversion if [...] they are nonmixed than if [...] they are mixed. [...] [T]he difference in risk aversion between them is due to loss aversion."

identification problems.[10] For these reasons, we stick to common practice and assume a zero reference point.

Turning to the second component of the model, a variety of functional forms for modeling probability weights $w(p)$ have been proposed in the literature (Quiggin (1982), Tversky and Kahneman (1992), Prelec (1998)). We use the two-parameter specification suggested by Goldstein and Einhorn (1987) and Lattimore, Baker, and Witte (1992):

$$w(p) = \frac{\delta p^{\gamma}}{\delta p^{\gamma} + (1-p)^{\gamma}}, \quad \delta \geq 0, \gamma \geq 0.$$

We favor this specification because it has proven to account well for individual heterogeneity (Wu, Zhang, and Gonzalez (2004)) and the parameters are nicely interpretable. The parameter $\gamma < 1$ largely governs the slope of the curve and measures sensitivity toward changes in probability. The smaller the value of γ is, the more strongly the probability weighting function departs from linear weighting.[11] The parameter δ largely governs curve elevation and measures the relative degree of optimism. The larger is the value of δ for gains, the more elevated is the curve, the higher is the weight placed on every probability, and, consequently, the more optimistically the prospect is valued, ceteris paribus. For losses, the opposite holds. Linear weighting is characterized by $\gamma = \delta = 1$. In a sign-dependent model, the parameters may take on different values for gains and for losses.

We now turn to the third step of model specification. In the course of the experiments, we measured risk taking behavior of individual $i \in \{1, \dots, N\}$ by her certainty equivalents ce_{ig} for a set of different lotteries. Since CPT explains *deterministic* choice, we have to add an error term ε_{ig} so as to estimate the parameters of the model based on the elicited certainty equivalents. The observed certainty equivalent ce_{ig} can then be written as $\mathrm{ce}_{ig} = \hat{\mathrm{ce}}_g + \varepsilon_{ig}$. There may be different sources of error, such as carelessness, hurrying, or inattentiveness, that result in accidentally wrong answers (Hey and Orme (1994)). The central limit theorem supports our assumption that the errors are normally distributed and simply add white noise.

Furthermore, we allow for three different sources of heteroskedasticity in the error variance. First, for each lottery, subjects had to consider 20 certain outcomes, which are equally spaced throughout the lottery's range $|x_{1g} - x_{2g}|$. Since the observed certainty equivalent ce_{ig} is calculated as the arithmetic mean of the smallest certain amount preferred to the lottery and the subsequent amount on the list, the error is proportional to the lottery range.[12]

[10]Previous attempts to estimate model parameters simultaneously with the reference point are extremely scarce and suggest that the reference point is of negligible magnitude (Harrison, List, and Towe (2007)); their experimental design included mixed lotteries, however.

[11]If linear probability weighting is accepted as a standard of rationality, $\gamma < 1$ can be interpreted as an index of departure from rationality (Tversky and Wakker (1995)).

[12]See Wilcox (2010) for a similar approach in the context of discrete choice under risk.

Second, as the subjects may be heterogeneous with respect to their previous knowledge, their attention span, and their ability to find the correct certainty equivalent, we expect the error variance to differ by individual. Third, lotteries in the gain domain may be evaluated differently from those in the loss domain. Therefore, we allow for domain-specific variance in the error term. This yields the form $\sigma_{ig} = \xi_i|x_{1g} - x_{2g}|$ for the standard deviation of the error distribution, where ξ_i denotes an individual domain-specific parameter. Note that the model allows us to test for both individual-specific and domain-specific heteroskedasticity either by imposing the restriction $\xi_i = \xi$ or by forcing all the ξ_i to be equal in both decision domains. Both types of restrictions are rejected by their corresponding likelihood ratio tests in all three samples with p-values close to zero. Therefore, we control for all three types of heteroskedasticity in the estimation procedure.

Having discussed all the necessary ingredients, we now turn to the specification of the finite mixture model. The basic idea of the mixture model is assigning an individual's risk taking choices to one of C types of behavior, each characterized by a distinct vector of parameters $\theta_c = (\alpha_c, \beta_c, \gamma_c', \delta_c')'$.[13] When estimating the model parameters, the number of types C is held fixed. The optimum number of classes is determined by estimating mixture models with varying C and applying some suitable test to decide among them (see Section 4.2). We denote the proportions of the C different types in the population by π_c. Given our assumptions on the distribution of the error term, the density of type c for the ith individual can be expressed as

$$f(\mathrm{ce}_i, \mathcal{G}; \theta_c, \xi_i) = \prod_{g=1}^{G} \frac{1}{\sigma_{ig}} \phi\left(\frac{\mathrm{ce}_{ig} - \hat{\mathrm{ce}}_g}{\sigma_{ig}}\right),$$

where ϕ denotes the density of the standard normal distribution. Since we do not know a priori to which group a certain individual belongs, the proportions π_c are interpreted as probabilities of group membership. Therefore, each individual density of type c has to be weighted by its respective mixing proportion π_c, which, of course, is unknown and has to be estimated as well. Summing over all C components yields the individual's contribution to the model's likelihood L. The log likelihood of the finite mixture model is then given by

$$\ln L(\Psi; \mathrm{ce}, \mathcal{G}) = \sum_{i=1}^{N} \ln \sum_{c=1}^{C} \pi_c f(\mathrm{ce}_i, \mathcal{G}; \theta_c, \xi_i),$$

where the vector $\Psi = (\theta_1', \ldots, \theta_C', \pi_1, \ldots, \pi_{C-1}, \xi_1, \ldots, \xi_N)'$ summarizes all the parameters of the model.

[13]The vectors γ_c and δ_c contain the domain-specific parameters for the slope and the elevation of the probability weighting functions.

The parameters are estimated by the iterative expectation maximization (EM) algorithm (Dempster, Laird, and Rubin (1977)),[14] which provides an additional feature: In each iteration, the algorithm calculates by Bayesian updating an individual's posterior probability τ_{ic} of belonging to group c. The final posterior probabilities represent a particularly valuable result of the estimation procedure. Not only do we obtain the probabilities of individual group membership, but we also have a method of judging the quality of classification at our disposal. If all the τ_{ic} are either close to 0 or 1, all the individuals are unambiguously assigned to one specific group. The τ_{ic} can be used to calculate a suitable measure of entropy, such as the normalized entropy criterion (Celeux and Soromenho (1996)), to gauge the extent of ambiguous assignments. If classification has been successful, that is, if genuinely distinct types have been identified, we should observe a low measure of entropy.

4. RESULTS

In this section we present descriptive statistics of the raw data and the results of the finite mixture estimations.

4.1. *Descriptive Statistics*

At the level of observed data, risk taking behavior can be conveniently summarized by relative risk premia $\text{RRP} = (\text{ev} - \text{ce})/|\text{ev}|$, where ev denotes the expected value of a lottery's payoff and ce stands for its certainty equivalent. $\text{RRP} > 0$ indicates risk aversion, $\text{RRP} < 0$ risk seeking, and $\text{RRP} = 0$ risk neutrality. In the context of EUT, risk preferences are captured solely by the curvature of the utility function, which in turn determines the sign of relative risk premia. Hence, the sign of RRP should be independent of p, the probability of the more extreme lottery outcome. In Figures 2–4, median risk premia sorted by p show a systematic relationship between RRP and p, however: In all three data sets subjects' choices display a fourfold pattern, that is, they are risk averse for low-probability losses and high-probability gains, and they are risk seeking for low-probability gains and high-probability losses. Therefore, at a first glance, average behavior is adequately described by a model such as CPT rather than EUT. As the following sections show, the median RRPs gloss over an important feature of the data as there is substantial latent heterogeneity in risk taking behavior.

[14]Various problems may be encountered when maximizing the likelihood function of a finite mixture model and, therefore, a customized estimation procedure was used that can adequately deal with these problems. Details of the estimation procedure, written in the R environment (R Development Core Team (2006)), are discussed in the Supplemental Material (Bruhin, Fehr-Duda, and Epper (2010)) available online.

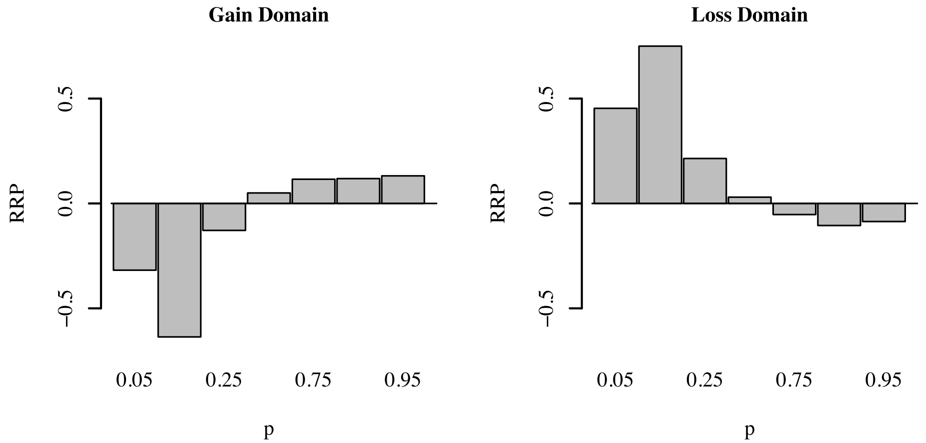

FIGURE 2.—Median relative risk premia, Zurich 2003.

4.2. *Model Selection*

So far we have not addressed the issues of whether a finite mixture model is actually to be preferred over a single-component model in the first place, and of what the number of groups C in the mixture model, often termed *model size*, should be. To deal with these questions, the researcher needs a criterion for assessing the correct number of mixture components. The literature on model selection in the context of mixture models is quite controversial, however, and

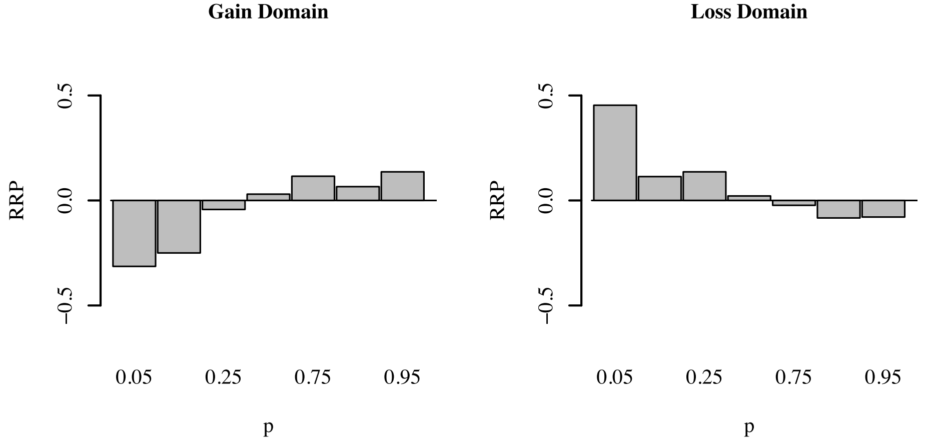

FIGURE 3.—Median relative risk premia, Zurich 2006.

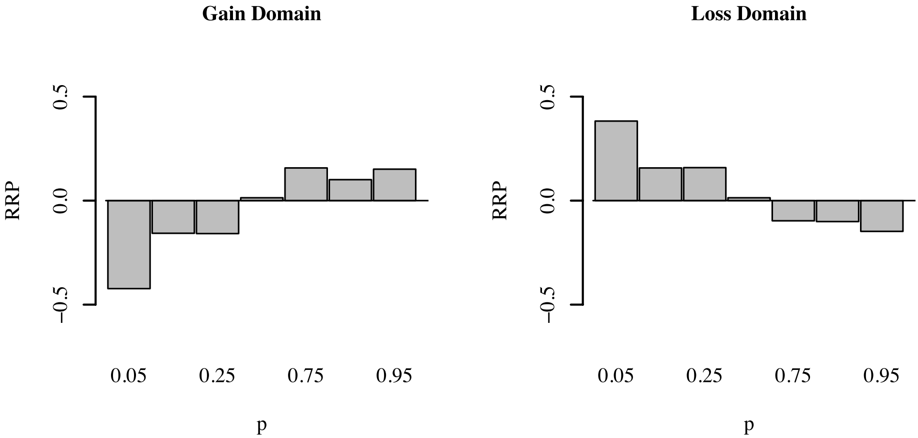

FIGURE 4.—Median relative risk premia, Beijing 2005.

there is no best solution.[15] For this reason, rather than relying on a single measure, we examine several criteria with differing characteristics to get a handle on the problem of model selection.

Obviously, the classical information criteria, the *Akaike information criterion* (AIC) and the *Bayesian information criterion* (BIC), are a natural starting point for our analysis. Unfortunately, the AIC is order inconsistent, that is, the probability that it is minimized at the true model size does not approach unity with increasing sample size, and it tends to overfit models (Atkinson (1981), Geweke and Meese (1981), Celeux and Soromenho (1996)). The BIC, on the other hand, has been proved to be consistent under suitable regularity conditions, but may suffer from over- or underestimating the number of mixture components (Biernacki, Celeux, and Govaert (2000)).

Aside from these problems, both classical criteria share the principle of trading off model parsimony against goodness of fit, but do not directly measure the ability of the mixture to provide well separated and nonoverlapping components, which, ultimately, is the objective of estimating mixture models. Therefore, Celeux and Soromenho (1996) proposed the *normalized entropy criterion* (NEC), which is based on the posterior probability of group membership τ_{ic}. Biernacki, Celeux, and Govaert (1999) argued that the NEC criterion appears to be less sensitive than AIC and BIC. However, the NEC focuses solely on the quality of classification and does not take model fit into account.

Ideally, what the researcher would like to have at her disposal is a criterion that delivers an assessment of both model fit, making allowance for parsimony,

[15]"The problem of identifying the number of classes is one of the issues in mixture modeling with the least satisfactory treatment" (Wedel (2002, p. 364)). For example, a standard likelihood ratio test is not appropriate here (Cameron and Trivedi (2005, p. 624)).

and the quality of classification. Biernacki, Celeux, and Govaert (2000) therefore suggested modifying the BIC criterion by factoring in a penalty for mean entropy. When the mixture components are well separated, mean entropy is close to zero and its weight in their proposed *integrated completed likelihood criterion* (ICL) is negligible. In the one-component case, there is no entropy by definition, and therefore ICL coincides with BIC. While there is no theoretical justification for this approach, simulations seem to show a superior performance compared to other heuristic criteria, such as NEC (Biernacki, Celeux, and Govaert (2000)), as well as compared to AIC and BIC (McLachlan and Peel (2000)).

As different criteria may come up with conflicting results concerning the correct number of mixture components, model selection is a difficult problem. One way to deal with this issue is to use one's central research question as a guideline.[16] Our concern here is twofold: First, given the vast heterogeneity in individual risk taking behavior, it is doubtful whether a single-component model is adequate. Therefore, the crucial question is whether $C > 1$ should be preferred to $C = 1$.[17] Second, considering the heated dispute about the "right" model of choice under risk, another objective of our study is to identify relative group sizes of EUT and non-EUT types. Bearing these objectives in mind, we calculated values for four different criteria, AIC, BIC, NEC, and ICL, and three different model sizes, $C \in \{1, 2, 3\}$, which are presented in Table III. According to these criteria, the model size which minimizes the respective criterion value should be preferred.

TABLE III

MODEL SELECTION CRITERIA

	AIC	BIC	NEC	ICL
Zurich 03				
$C = 1$	−38,398	−35,815	n.a.	−35,815
$C = 2$	−39,629	−36,997	**0.0099**	−36,991
$C = 3$	**−40,504**	**−37,822**	0.0131	**−37,807**
Zurich 06				
$C = 1$	−20,858	−19,297	n.a.	−19,297
$C = 2$	−22,173	−20,568	**0.0041**	−20,566
$C = 3$	**−22,622**	**−20,971**	0.0049	**−20,968**
Beijing 05				
$C = 1$	−18,485	−16,529	n.a.	−16,529
$C = 2$	−19,585	−17,585	**0.0061**	−17,582
$C = 3$	**−19,965**	**−17,920**	0.0114	**−17,912**

[16]Cameron and Trivedi (2005, p. 622) argued in this context: "Therefore, it is very helpful in empirical application if the components have a natural interpretation."

[17]Parameter estimates for $C = 1$ are presented in the Supplemental Material.

As AIC, BIC, and therefore also ICL, are highest at $C = 1$ for all three data sets, $C > 1$ is clearly favored over $C = 1$. As the NEC criterion is not defined for $C = 1$, Biernacki, Celeux, and Govaert (1999) argued in favor of a multicomponent model if there is a $C > 1$ with $\mathrm{NEC}(C) \leq 1$, which is the case here. We therefore conclude that a finite mixture model is superior to a single-component model, given the unanimous recommendation by all four criteria.

With regard to the choice between $C = 2$ and $C = 3$, the three-group classifications seem to be favored by all criteria but NEC. Given the minimum level of NEC at $C = 2$, a two-group classification is preferable if the central issue is a parsimonious representation of risk taking types rather than best model fit. As entropy is generally extremely low for both the two-group and three-group classifications, both model sizes seem quite sensible, however. Before we infer from these results that we should choose $C = 3$, we take a closer look at the difference between the two-group and the three-group classifications.[18] What is of special interest here is whether one group remains fairly stable and the other group gets subdivided into two new ones when model size is increased, or whether the individuals get reshuffled to three new types. If the latter were the case, a two-group specification would clearly be misleading. To answer this question, we examine relative group sizes and transition patterns of individuals' type assignment.

Table IV displays the estimated relative group sizes of the behavioral types for model sizes $C = 2$ and $C = 3$. As the percentages reveal, all the Type I groups remain stable with respect to relative group size. Moreover, with a few exceptions, Type I individuals remain Type I when model size is increased: Only a total of 2% of the individuals move into or out of Type I when an additional component is introduced into the finite mixture model.[19] Increasing model size results in a decomposition of the original Type II groups into two subtypes, Type IIa and Type IIb, as there is still considerable heterogeneity within these groups. Thus, from the point of view of identifying Type I individuals, the

[18]Since there is quite some heterogeneity within the majority group, it is to be expected that even finer segmentations improve model fit. However, when we extend the number of groups beyond three, multimodality of the log likelihood function becomes a severe problem as, depending on the randomly drawn start values, even a stochastic extension of the EM algorithm tends to converge to local maxima. For "poorly drawn" start values, the estimation algorithm diverges, with one group getting smaller in each iteration, which might indicate that the likelihood is unbounded (McLachlan and Peel (2000, p. 54)). Therefore, estimating larger models may ask too much of our data. See also the discussion of overparametrization in Cameron and Trivedi (2005, p. 625)). Nevertheless, in the case of Zurich 06 we were able to estimate four- and five-group models: In both cases, the relative size of the minority group declines only slightly. This finding supports our conjecture that heterogeneity is particularly pronounced within the majority group, whereas the minority group is fairly homogeneous and robust to model size. Since we are not able to present results for all three data sets, we do not discuss these findings here.

[19]Across all three data sets, only two individuals are newly assigned to Type I and seven individuals leave Type I when C is increased from 2 to 3.

TABLE IV

RELATIVE GROUP SIZES

	Type I	Type II/IIa	Type IIb
Zurich 03			
$C = 2$	17.1%	82.9%	
$C = 3$	16.7%	27.3%	56.0%
Zurich 06			
$C = 2$	22.3%	77.7%	
$C = 3$	22.0%	29.8%	48.2%
Beijing 05			
$C = 2$	20.3%	79.7%	
$C = 3$	19.9%	29.3%	50.8%

two-group classifications are informative by themselves and provide the most parsimonious classification, whereas three groups render a more detailed description of Type II individuals. To keep interpretation of graphs manageable, we will present results for $C = 2$ when contrasting Type I with Type II, and for $C = 3$ when discussing subtypes of Type II behavior.[20]

4.3. *Clean and Robust Segregation of Behavioral Types*

To be of value to applied economics, a classification of risk taking behavior should meet two conditions. First, it should be clean, that is, all the individuals should be clearly associated with one specific risk taking type. Second, the classification should be robust across different experiments based on the same design principles. Regarding the first condition, entropy criteria, based on the posterior probabilities of group membership, can be used to evaluate the quality of classification. One such measure is the *normalized entropy criterion* introduced in the previous section. If all the individuals can be clearly assigned to one of the different behavioral groups, the posterior probabilities of group membership τ_{ic} are close to 0 or 1, and NEC ≈ 0. A τ_{ic} distinctly different from 0 or 1 indicates that the individual is classified as a "mixed" type belonging to group c with probability τ_{ic} and to the other group(s) with probability $1 - \tau_{ic}$. As Table III shows, NEC always lies in the vicinity of 0, irrespective of model size C being $1, 2$, or 3, that is, there are hardly any mixed types with ambiguous group affiliation.

The high quality of classification can also be inferred directly from the distributions of the individuals' posterior probabilities of group membership. In Figure 5, based on $C = 2$, τ_{EUT} denotes the posterior probability of belonging

[20]The interested reader is referred to Bruhin, Fehr-Duda, and Epper (2007) for an extensive discussion of $C = 2$.

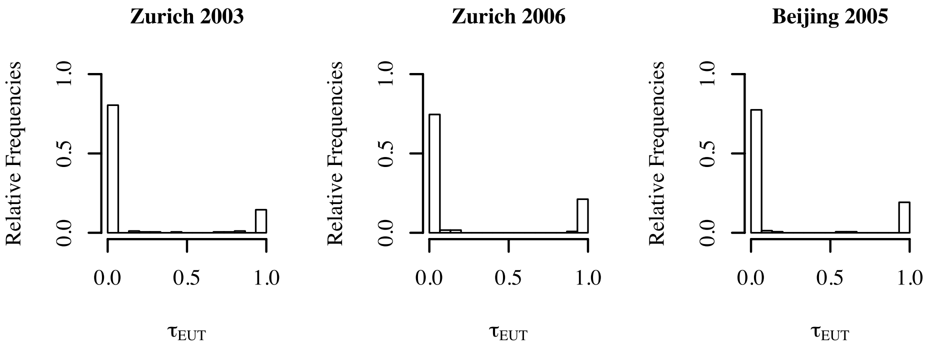

FIGURE 5.—Distribution of posterior probability of assignment to EUT, τ_{EUT} ($C = 2$).

to the first type, which can indeed be characterized, as we will demonstrate below, as expected utility maximizers.[21] As the distributions of τ_{EUT} show, the individuals' posterior probabilities of behaving consistently with EUT are either close to 1 or close to 0 for practically all the individuals in all three data sets, indicating an extremely clean segregation of subjects to types. Our result is quite remarkable as it substantiates that there are distinct types in the population—be they Swiss or Chinese—and it also shows that the underlying behavioral model provides a sound basis of discriminating between them.

With respect to the second criterion, robustness of classification, Figure 5 illustrates the probably most striking result of our study, namely similar distributions of types across all three data sets. In all three histograms of Figure 5, there are about four times as many individuals with τ_{EUT} close to 0, compared to individuals with τ_{EUT} close to 1. This finding is mirrored by the estimates of the relative group sizes, displayed in Table IV, which show a stable proportion of Type I of about 20%, irrespective of model size C. Moreover, it can be shown that the hypothesis of the same distribution of types prevailing in all three data sets cannot be rejected. Similarly, when model size is increased to $C = 3$, relative group sizes turn out to be of equal magnitudes in all three data sets and are statistically indistinguishable from one another. Therefore, classification is not only unambiguous, but also results in roughly equal mixing proportions, demonstrating that classification is robust across experiments.

This finding leads us to the next question. Do the respective types identified in each data set also exhibit similar patterns of behavior? This question will be addressed in the following sections, dedicated to the characterization of the endogenously defined types of behavior.

[21]As group membership is stable, histograms of τ_{EUT} for $C = 3$ are qualitatively the same.

4.4. *Characterization of the Minority Type*

Irrespective of model size, the first type of individuals encompasses about 20% of the subjects in all three data sets, thus constituting the minority type. To characterize risk taking behavior, we examine the parameter estimates of the value functions and probability weighting functions. Table V displays, for $C = 2$, the type-specific parameter estimates of the finite mixture model and their standard errors, obtained by the bootstrap method with 4000 replications (Efron and Tibshirani (1993)).[22] When model size is increased to three groups, parameter estimates, presented in Tables VI–IX, remain unchanged for the minority type, as group membership does not change substantially. Therefore, from the point of view of identifying this type of individuals, model size is not

TABLE V

CLASSIFICATION OF BEHAVIOR ($C = 2$)[a]

	EUT Types				CPT Types			
Parameters	ZH 03	ZH 06	BJ 05	Pooled	ZH 03	ZH 06	BJ 05	Pooled
π	0.171	0.223	0.203	0.193	0.829	0.777	0.797	0.807
	(0.026)	(0.025)	(0.020)	(0.013)				
Gains								
α	0.978	0.988	1.083	0.981	1.054	0.901	0.389	0.941
	(0.014)	(0.018)	(0.102)	(0.011)	(0.021)	(0.026)	(0.107)	(0.013)
γ	0.954	0.945	0.911	0.943	0.415	0.425	0.245	0.377
	(0.022)	(0.020)	(0.033)	(0.021)	(0.015)	(0.015)	(0.014)	(0.009)
δ	0.910	0.909	0.889	0.911	0.845	0.862	1.315	0.926
	(0.015)	(0.019)	(0.052)	(0.012)	(0.022)	(0.028)	(0.074)	(0.013)
Losses								
β	1.007	1.013	1.023	1.015	1.107	1.122	1.144	1.139
	(0.018)	(0.023)	(0.084)	(0.013)	(0.028)	(0.047)	(0.107)	(0.019)
γ	0.871	0.953	0.949	0.950	0.417	0.451	0.309	0.397
	(0.043)	(0.020)	(0.040)	(0.023)	(0.017)	(0.014)	(0.013)	(0.009)
δ	0.967	1.049	1.066	1.072	1.025	1.059	0.937	0.991
	(0.062)	(0.033)	(0.065)	(0.026)	(0.028)	(0.044)	(0.053)	(0.016)
$\ln L$	20,185	11,336	10,108	41,385				
Parameters	371	249	315	909				
Individuals	179	118	151	448				
Observations	8906	4669	4225	17,800				

[a]Standard errors (in parentheses) are based on the bootstrap method with 4000 replications. Parameters include additional estimates for ξ_i for domain- and individual-specific error variances. ZH stands for Zurich; BJ stands for Beijing.

[22]"[U]nless the sample size is very large, the standard errors found by an information-based approach may be too unstable to be recommended" (McLachlan and Peel (2000, p. 68)).

TABLE VI

CLASSIFICATION OF BEHAVIOR WITH $C = 3$, ZURICH 2003[a]

	Gains				Losses		
	EUT	CPT-I	CPT-II		EUT	CPT-I	CPT-II
π	0.167	0.273	0.560				
	(0.016)	(0.015)	(0.022)				
α	0.954	1.007	1.075	β	1.006	1.237	1.091
	(0.013)	(0.016)	(0.015)		(0.020)	(0.044)	(0.015)
γ	0.944	0.302	0.467	γ	0.885	0.304	0.459
	(0.041)	(0.031)	(0.013)		(0.042)	(0.029)	(0.015)
δ	0.930	0.622	0.944	δ	1.024	1.371	0.897
	(0.020)	(0.023)	(0.017)		(0.043)	(0.075)	(0.016)
$\ln L$			20,630				
Parameters			378				
Individuals			179				
Observations			8906				

[a]Standard errors (in parentheses) are based on the bootstrap method with 4000 replications. Parameters include estimates of ξ_i for domain- and individual-specific error variances.

a crucial issue and the two-group classifications nicely contrast the distinctive characteristics of the minority type with the majority type.

Concerning probability weighting, Table V displays almost identical parameter estimates across all three data sets as well as the pooled data. Without any restrictions imposed on the parameters, we find that the minority types' probability weighting functions are roughly linear, as the parameter estimates for both γ and δ are close to 1. Since the probability weights are a nonlinear combination of these parameters, inference needs to be based on γ and δ jointly. Therefore, we constructed the 95%-confidence bands for the probability weighting curves by the bootstrap method. Figures 6, 7, and 8 contain the graphs of the type-specific probability weighting functions for each decision domain. The gray solid lines correspond to the estimated curves for the first type, referred to as EUT type, and the gray dashed lines delimit their respective confidence bands. For both gains and losses, the confidence bands for the first type in fact include the diagonal over a wide range of probabilities, demonstrating high congruence with linear probability weighting. Where the confidence bands do not include the diagonal, the curves still lie extremely close to linear weighting. In sum, in all three data sets, we find the first behavioral type to exhibit near linear probability weighting.

With respect to the valuation of monetary outcomes, the second element of the decision model, the estimated parameters α and β also display a high degree of conformity. As can be inferred from the standard errors in Table V, the 95%-confidence intervals of each single curvature estimate contain unity,

TABLE VII

CLASSIFICATION OF BEHAVIOR WITH $C = 3$, ZURICH 2006[a]

	Gains				Losses		
	EUT	CPT-I	CPT-II		EUT	CPT-I	CPT-II
π	0.220	0.298	0.482				
	(0.020)	(0.025)	(0.030)				
α	0.990	0.884	0.908	β	1.012	1.100	1.141
	(0.024)	(0.042)	(0.031)		(0.029)	(0.083)	(0.049)
γ	0.946	0.362	0.465	γ	0.952	0.393	0.491
	(0.084)	(0.081)	(0.022)		(0.081)	(0.078)	(0.023)
δ	0.905	0.658	1.012	δ	1.054	1.460	0.878
	(0.042)	(0.054)	(0.043)		(0.074)	(0.122)	(0.054)
$\ln L$			11,567				
Parameters			256				
Individuals			118				
Observations			4669				

[a]Standard errors (in parentheses) are based on the bootstrap method with 4000 replications. Parameters include estimates of ξ_i for domain- and individual-specific error variances.

TABLE VIII

CLASSIFICATION OF BEHAVIOR WITH $C = 3$, BEIJING 2005[a]

	Gains				Losses		
	EUT	CPT-I	CPT-II		EUT	CPT-I	CPT-II
π	0.199	0.293	0.508				
	(0.017)	(0.026)	(0.027)				
α	1.083	0.032	0.489	β	1.023	1.348	1.111
	(0.098)	(0.155)	(0.113)		(0.070)	(0.149)	(0.102)
γ	0.911	0.244	0.254	γ	0.948	0.263	0.332
	(0.051)	(0.049)	(0.023)		(0.053)	(0.046)	(0.019)
δ	0.889	2.194	1.085	δ	1.062	0.600	1.106
	(0.094)	(0.241)	(0.113)		(0.057)	(0.093)	(0.075)
$\ln L$			10,304				
Parameters			322				
Individuals			151				
Observations			4225				

[a]Standard errors (in parentheses) are based on the bootstrap method with 4000 replications. Parameters include estimates of ξ_i for domain- and individual-specific error variances. Estimates for CPT-I α statistically not distinguishable from logarithmic utility.

TABLE IX

CLASSIFICATION OF BEHAVIOR WITH $C = 3$, POOLED[a]

	Gains				Losses		
	EUT	CPT-I	CPT-II		EUT	CPT-I	CPT-II
π	0.198 (0.010)	0.316 (0.011)	0.486 (0.013)				
α	0.960 (0.009)	0.901 (0.009)	0.957 (0.010)	β	1.019 (0.008)	1.250 (0.010)	1.139 (0.009)
γ	0.915 (0.032)	0.309 (0.015)	0.451 (0.010)	γ	0.935 (0.027)	0.339 (0.013)	0.444 (0.011)
δ	0.935 (0.009)	0.726 (0.012)	1.063 (0.010)	δ	1.055 (0.013)	1.230 (0.013)	0.878 (0.011)
$\ln L$			42,105				
Parameters			916				
Individuals			448				
Observations			17,800				

[a]Standard errors (in parentheses) are based on the bootstrap method with 4000 replications. Parameters include estimates of ξ_i for domain- and individual-specific error variances.

implying that the hypothesis of linear value functions cannot be rejected. Together with near linear probability weighting, this result justifies regarding the first type of individuals as largely consistent with expected value maximization, and therefore EUT.

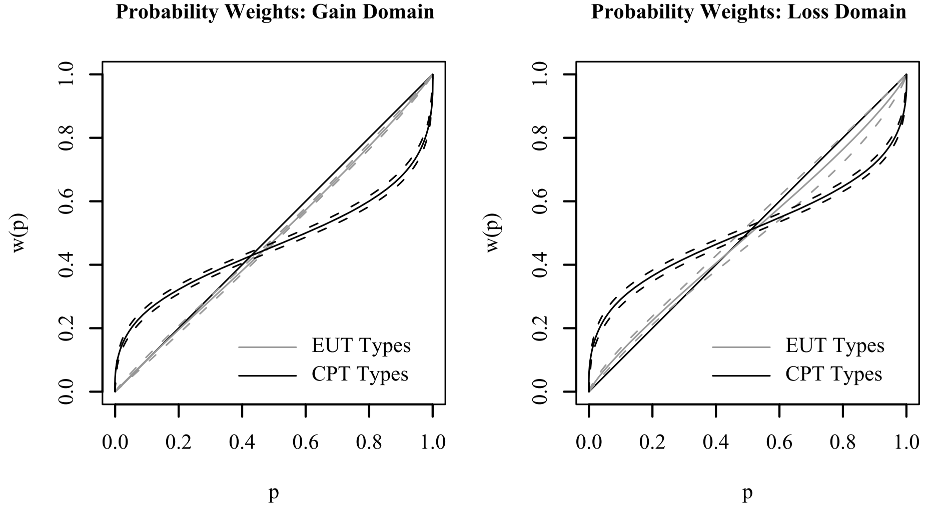

FIGURE 6.—Type-specific probability weighting functions, Zurich 2003.

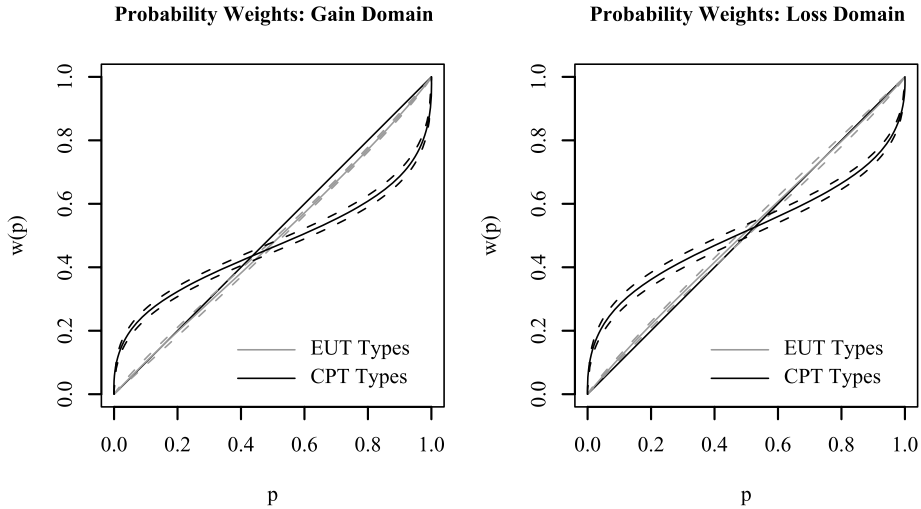

FIGURE 7.—Type-specific probability weighting functions, Zurich 2006.

4.5. *Characterization of the Majority Types*

As the discussion on model selection revealed, model size makes a difference when characterizing the majority types. Due to the stability of the minority EUT groups in all three data sets, the behavior of the majority groups can be described by a mixture of two different subtypes. As the majority groups exhibit

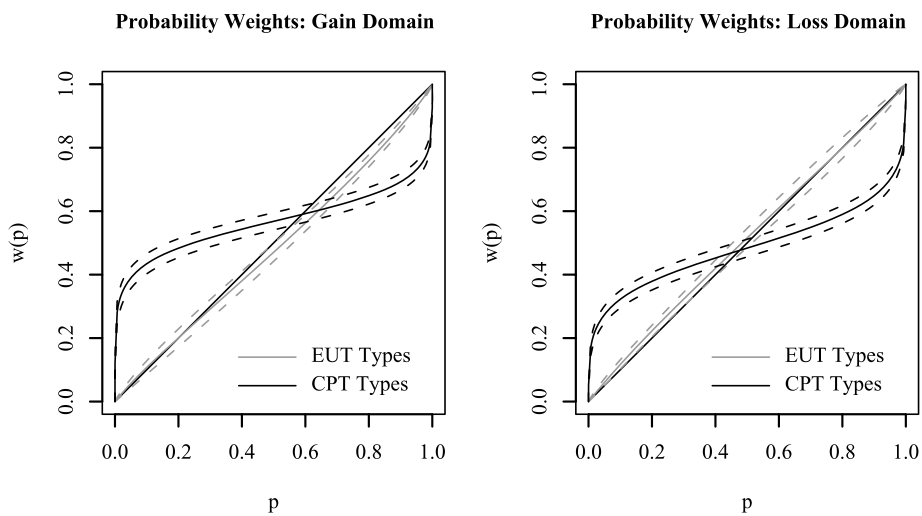

FIGURE 8.—Type-specific probability weighting functions, Beijing 2005.

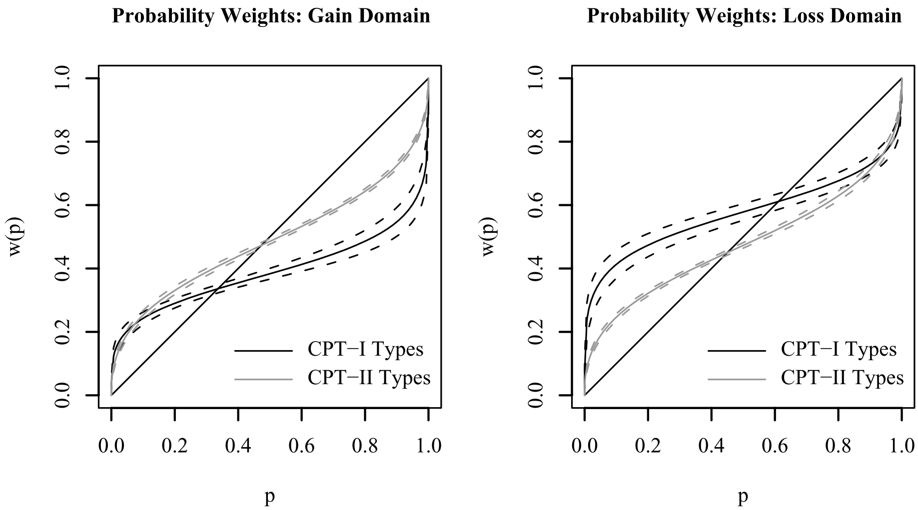

FIGURE 9.—Probability weights CPT-I versus CPT-II, Zurich 2003.

inverted S-shaped probability weighting curves, apparent in Figures 6, 7, and 8, we label them CPT types and label their corresponding subtypes CPT-I and CPT-II.

CPT-I and CPT-II groups are characterized by specific varieties of nonlinear probability weighting as Figures 9–11 show. The difference between CPT-I

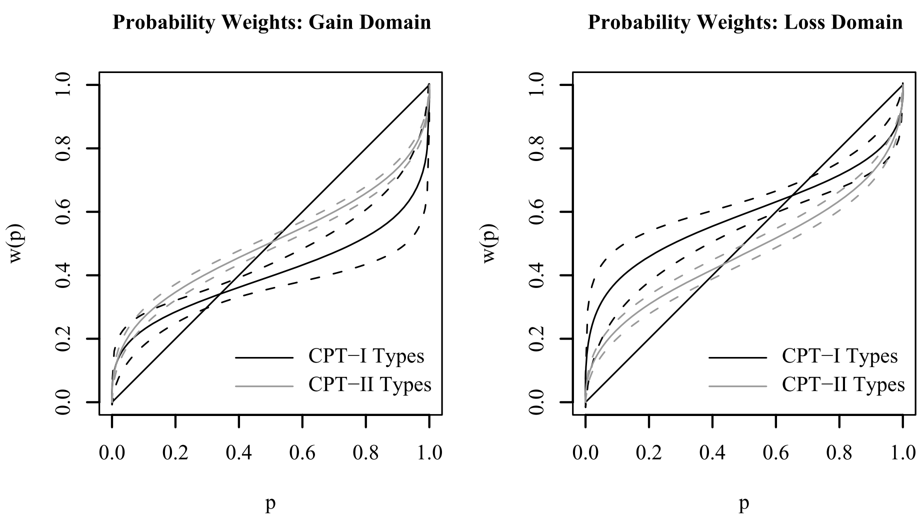

FIGURE 10.—Probability weights CPT-I versus CPT-II, Zurich 2006.

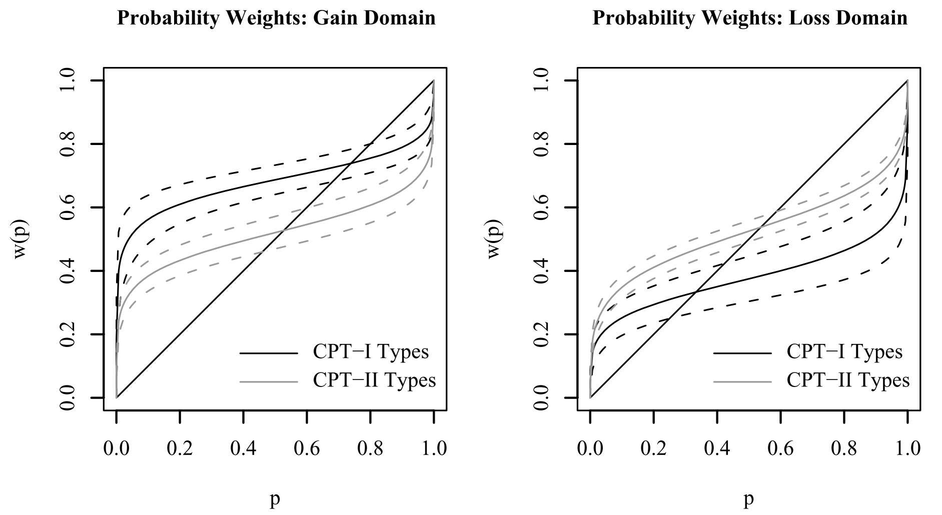

FIGURE 11.—Probability weights CPT-I versus CPT-II, Beijing 2005.

and CPT-II types manifests itself predominantly in relative strength of optimism: the elevation of the probability weighting curves, measured by δ,[23] differs substantially between CPT-I and CPT-II. CPT-II individuals, who constitute the relative majority of approximately 50% in all three data sets, exhibit moderately S-shaped probability weighting curves with δ in the vicinity of 1. The remaining 30% of the individuals, however, are characterized by differing patterns of behavior. Swiss CPT-I individuals are systematically less optimistic than Swiss CPT-II types, whereas the Chinese CPT-I group encompasses highly optimistic individuals, overweighting gain probabilities and underweighting loss probabilities over a wide range of probabilities. This specific feature of Chinese CPT-I types might explain the prevalence of high risk tolerance in the Chinese population, documented by previous research (Kachelmeier and Shehata (1992)).

The three-group classifications constitute a valuable piece of information when more disaggregate estimates of risk taking behavior are called for. When the focus of research lies on a parsimonious characterization of risk taking types, juxtaposing rational decision makers, not prone to probability distortions, with nonrational ones, two-group classifications are sufficiently informative due to the stability of EUT group membership.

[23]Parameter estimates are presented in Tables VI–IX.

4.6. *Observed Behavior by Type*

So far we have characterized the different behavioral types by their estimated parameter values. The obvious question that arises is whether the discriminatory power of the classification procedure can also be traced at the behavioral level. After assigning the subjects to one of the types, EUT, CPT-I, or CPT-II, based on their posterior probability of group membership τ_{ic}, the observed relative risk premia can be broken down by type as depicted in Figure 12, aggregated for the pooled data set. As can be seen, median RRP of the EUT types are close to 0, reflecting near risk neutral behavior in accordance with expected value maximization.

When we trace the behavior of the CPT-I and CPT-II types at the level of observed RRP in Figure 12, we find a fourfold pattern of risk attitudes, with distinctive departures from risk neutrality. Consistent with the characterizations before, CPT-I types exhibit more pronounced deviations. These findings document that individuals' type assignment is highly congruent with observed behavioral differences.

Obviously, the type-specific median relative risk premia do not differ greatly at $p = 0.5$. In decision situations when the more extreme reward materializes with a 50% chance, the typical CPT individual will not over- or underweight its probability significantly, and therefore her behavior will often not be distinguishable from a typical EUT type's behavior. This consideration can be illustrated by means of Figure 13, which displays the departures of average CPT behavior, aggregated over both subtypes CPT-I and CPT-II, from EUT, measured by the type-specific differences in median normalized certainty equivalents. Each circle in Figure 13 corresponds to one specific lottery played in any of the three experiments, encompassing a total of 59 gain and 59 loss lotteries, ordered by the probability of the more extreme lottery outcome. At a gain probability of 25%, for instance, CPT lottery evaluations, on average, exceed EUT by up to 30% of their corresponding expected values. The dashed lines in the graphs represent the case when median CPT behavior does not differ from median EUT behavior. Positive values in the graphs indicate that, on average, CPT types are relatively more risk seeking than EUT types. The opposite holds for negative values. As the graphs show, zero differences occur solely at the 0.5 probability level, where, in some cases, average CPT behavior is totally indistinguishable from EUT behavior. The bulk of type-specific differences in lottery evaluations lie in the range of about ±20% of expected values, but there are also a few observations with up to ±300% of expected value, where the more extreme outcomes materialize with a low probability. In these cases, CPT types tend to overreact pronouncedly to stated probabilities. To provide an overall measure, we conducted two-sided Mann–Whitney tests which indicate significant differences (at the 5% level) in the type-specific distributions of the certainty equivalents for 75% of the lotteries.

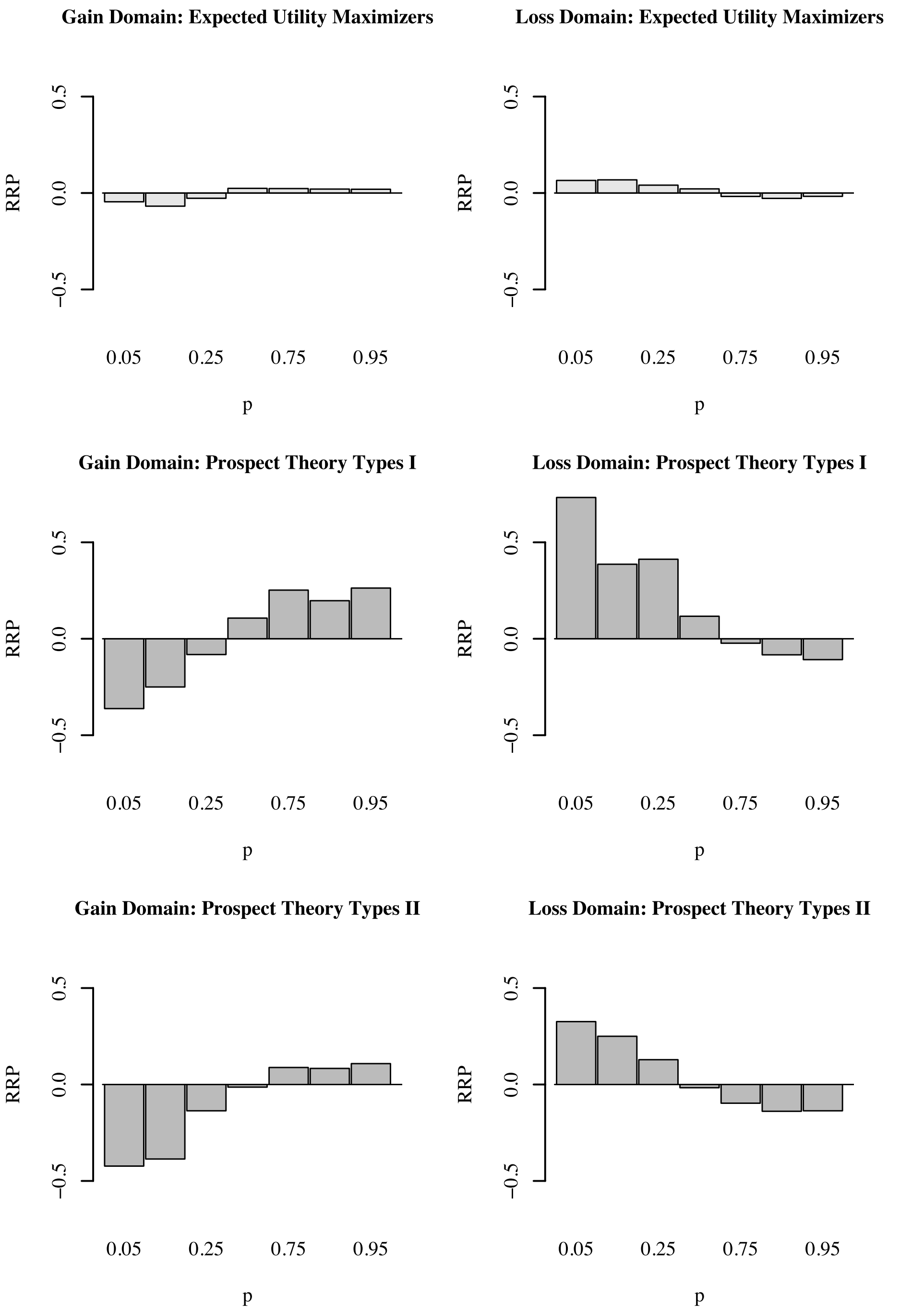

FIGURE 12.—Median relative risk premia by type, pooled.

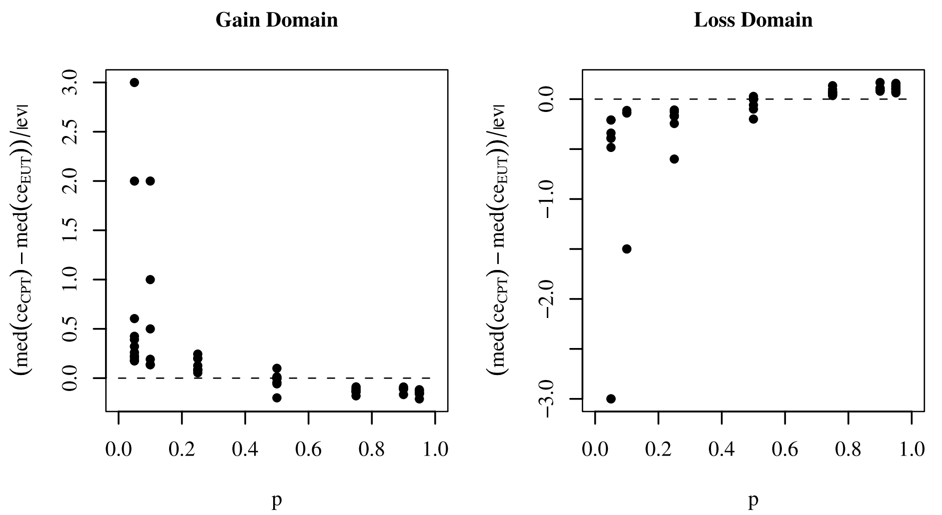

FIGURE 13.—Differences in median normalized certainty equivalents, pooled.

4.7. *Demographics and Group Membership*

The finite mixture model is a powerful tool to uncover latent heterogeneity in behavior. Given our clean and robust classification of types, an interesting question is whether we can characterize the composition of the different groups by demographic variables. In particular, can we explain who the EUT types are? To answer this question, we conducted two kinds of analyses. First, we estimated a single-component model with demographic variables as covariates. This procedure uncovers systematic behavioral differences among groups defined by observable socio-economic characteristics. We included the following variables: a gender dummy *female*, the number of semesters enrolled at university *semester*, and a binary variable *highincome* for incomes above a certain threshold. Summary statistics for these variables are included in the Supplemental Material. It turns out that the only variable that consistently affects behavioral parameters across experiments is *female*[24]: Being female is associated with a substantially lower value of γ, the slope of the probability weighting function. This finding implies that women tend to be less sensitive to changes in probability than men, in line with the evidence in Fehr-Duda, de Gennaro, and Schubert (2006).[25]

[24]Note that the percentage of females is approximately 50% in all three data sets. Parameter estimates for the single-component model are available in the Supplemental Material.

[25]In our experience, in student subject pools we generally do not find socio-economic characteristics, other than gender, that are systematically correlated with the curvature of the probability weighting function. Factors other than demographics may be more important here, but this question is still underresearched.

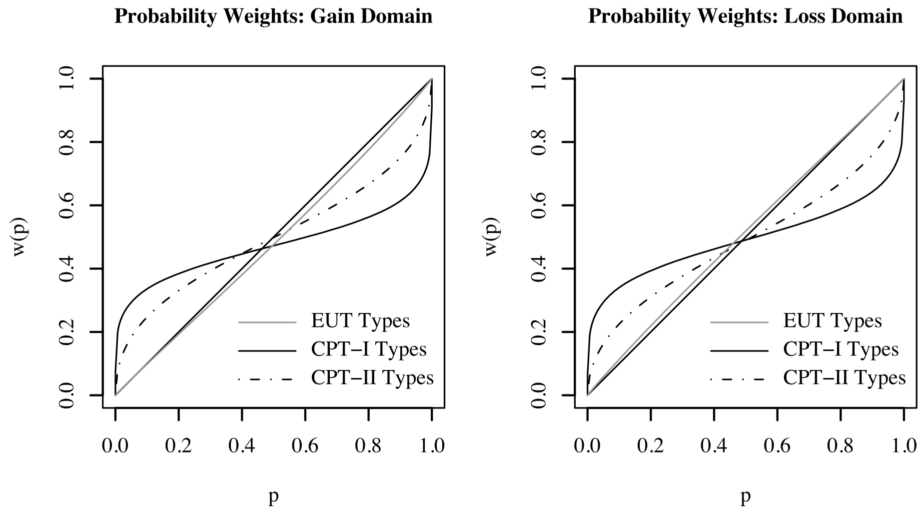

FIGURE 14.—Type-specific probability weighting functions: men.

Second, given that only gender systematically influences parameter values, we estimated the finite mixture model separately for men and women. In the following text we limit discussion to the results for the pooled model with $C = 3$. The gender-specific probability weighting functions classified by types are presented in Figures 14 and 15.[26] Whereas the distributions of types are quite similar, the probability weights display a striking gender difference. The men's groups differ essentially by their degree of rationality, characterized by the magnitude of the slope parameter γ. As in the overall data, the EUT group's probability weights lie very close to the diagonal. The male CPT-I types deviate quite strongly from linear weighting, whereas the CPT-II types, who constitute the relative majority of 49% of the men, lie somewhere in between these two more extreme groups. The women's minority group, however, departs more strongly from linear weighting than does the men's. One may conclude from these findings that the overall EUT group is dominated by the behavior of male individuals exhibiting near rational probability weighting.

The female CPT-I and CPT-II curves differ predominantly in the size of the elevation parameter δ. Compared with its male counterpart, the female CPT-I type also exhibits quite pronounced probability distortions, albeit with a larger fraction of optimistically weighted probabilities. The largest gender difference is displayed by the CPT-II types. Women in this group are characterized by the widest region of pessimistically weighted probabilities. This group's behavior seems to have a decisive influence on women's greater average risk aversion, usually found in empirical studies. While previous research has typically

[26]Parameter estimates are available in the Supplemental Material.

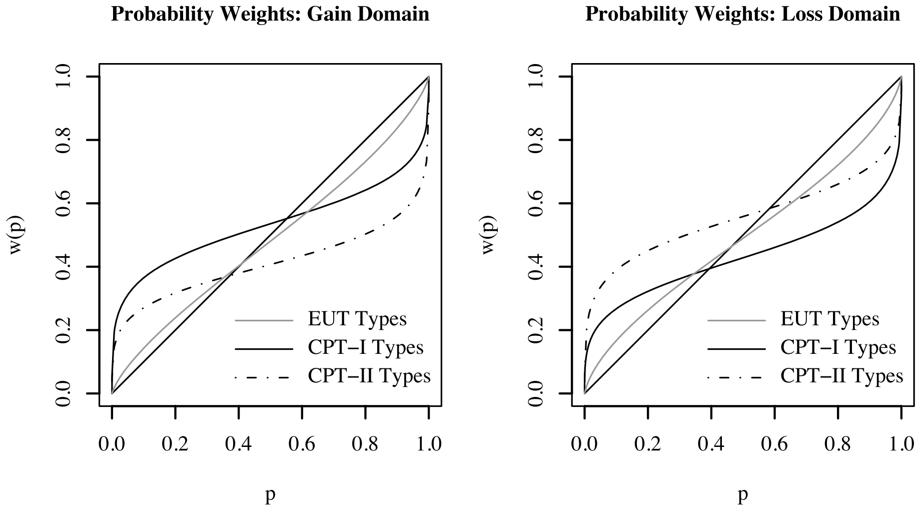

FIGURE 15.—Type-specific probability weighting functions: women.

centered on comparative risk aversion, our finite mixture estimations provide new, much more detailed, insights in gender-specific differences in risk taking behavior.

4.8. *Extensions*

4.8.1. *Robustness to Model Specification*

An additional part of our analysis concerns the robustness of classification results with respect to alternative specifications of the value function. For instance, people may not evaluate lotteries in isolation, but integrate prospective outcomes with their wealth or consumption spending. To account for the possibility that subjects integrate prospective outcomes with some background variable, we reestimated the model with the value function being defined over the sum of the prospective lottery outcome and an additional type-specific background parameter k, to be estimated, such that $v(x) = (x + k)^\alpha$ over gains and *mutatis mutandis* over losses, that is, $v(x) = (x + \omega + k)^\beta$, where ω stands for the initial endowment.[27]

[27]Estimating such an additional parameter comes at a cost, however. As Wakker (2008, p. 1338) noted, k represents an "anti-index of concavity" and therefore serves a similar function as the exponents of the value function α and β. For this reason, their respective contributions to utility curvature cannot be reliably separated unless one has observations over two distinct sets of lotteries (e.g., over low stakes and high stakes) at one's disposal (Heinemann (2008)). Moreover, k is not identifiable when functions v are near linear. Previous research suggests that under EUT, people generally do not integrate their wealth in their choices over risky lotteries (Binswanger

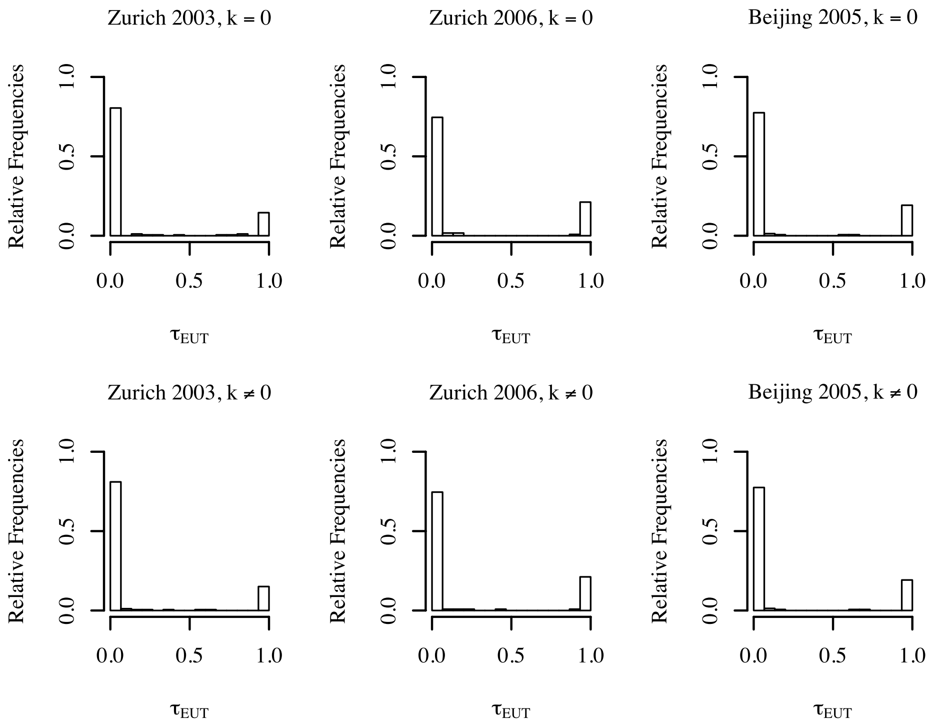

FIGURE 16.—Distribution of posterior probability of assignment to EUT τ_{EUT}.

Extending our model in such a manner yields the following insights. First, the stability of classification is not affected by the alternative model specification: For all three data sets, the distribution of the posterior probability of belonging to EUT is almost unaltered when background consumption is introduced into the model as Figure 16 shows. The stability of group assignment is also reflected in the estimated relative group sizes π_{EUT}. Table X clearly shows

TABLE X
ESTIMATED MODEL-SPECIFIC PROPORTIONS OF EUT TYPES, π_{EUT}

	Zurich 03	Zurich 06	Beijing 05
$k = 0$	0.171	0.223	0.203
k endogenous	0.163	0.227	0.203

(1981), Harrison, List, and Towe (2007), Heinemann (2008)). Since group affiliation of EUT types remains stable, we limit discussion to $C = 2$ here.

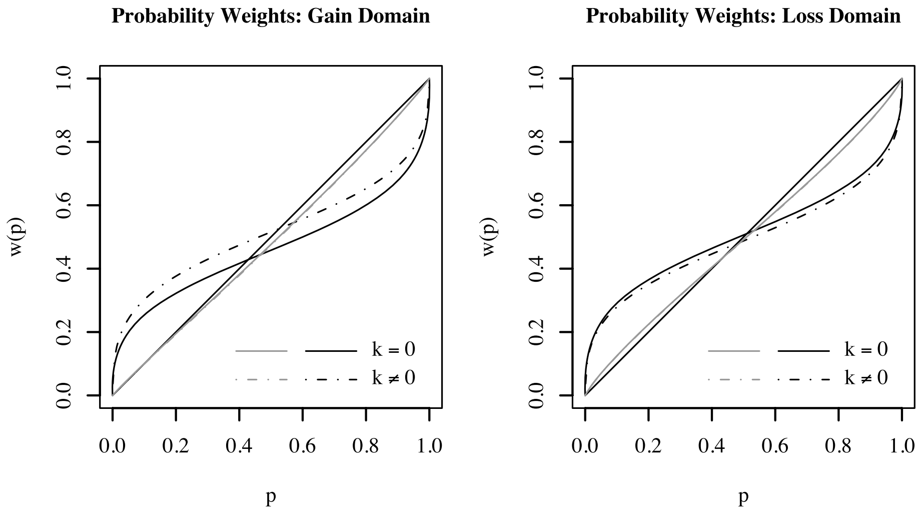

FIGURE 17.—Probability weights Zurich 2003, k endogenous.

that these values practically do not change. Moreover, not a single subject out of 448 is assigned to a different group, defined by $\tau_{ic} \geq 0.5$, after allowing for integration with background consumption. Finally, the estimated probability weighting functions for both the EUT types and the CPT types remain stable as well, as Figures 17–19, confirm. In sum, our analysis attests that the distribution of types, individuals' type affiliations, and the estimated probability weighting functions are robust to inclusion of background consumption. This robustness result represents further evidence that decision makers' tendency to weight probabilities nonlinearly is the driving force of classification.

4.8.2. *Heterogeneity in Error Variance*

The finite mixture model supplied us not only with estimates of type-specific behavioral parameters, but also with estimates of the error parameters, ξ_i—the normalized standard deviations of the error distributions. These parameters are idiosyncratic to the individual and, thus, capture some of the heterogeneity across subjects. A high error variance does not necessarily stem from random behavior, however. In an aggregate model such as ours, individual errors also reflect the degree of congruence between individual and group behavior. The question then arises of how well average behavioral group parameters describe subjects with differing degrees of departure from average behavior.[28] To investigate this matter, we classified individuals as either low- or high-variance

[28]We are grateful to an anonymous referee who called our attention to this issue.

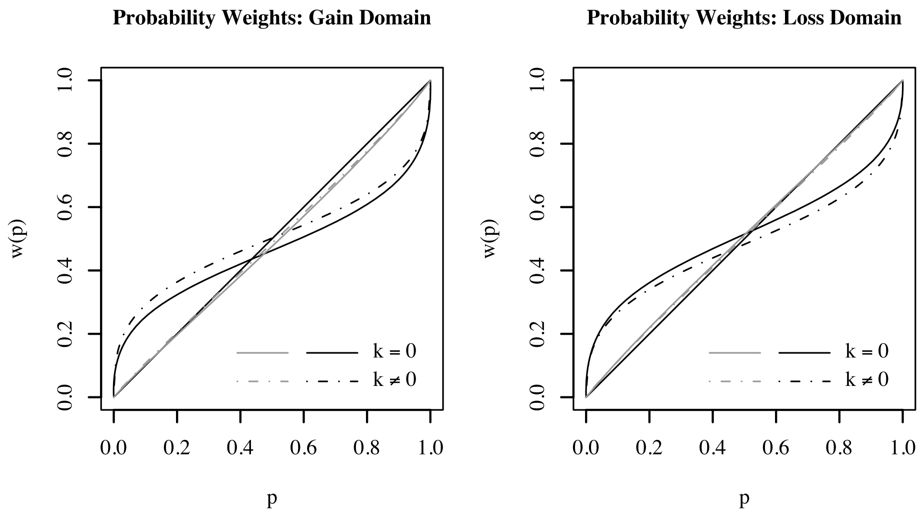

FIGURE 18.—Probability weights Zurich 2006, k endogenous.

type, depending on their estimated ξ_i being below or above the respective median value, and reestimated the behavioral parameters for each of the resulting six types (two types of variance $\times$ three types of behavior), pooled over all three data sets. The upper panel in Figure 20 displays the average probability weighting curves for the aggregate types estimated from the pooled data.

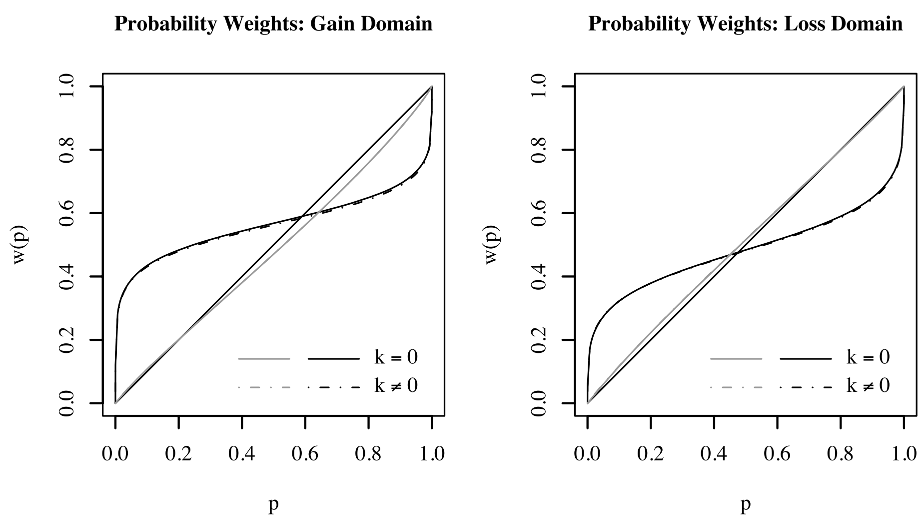

FIGURE 19.—Probability weights Beijing 2005, k endogenous.

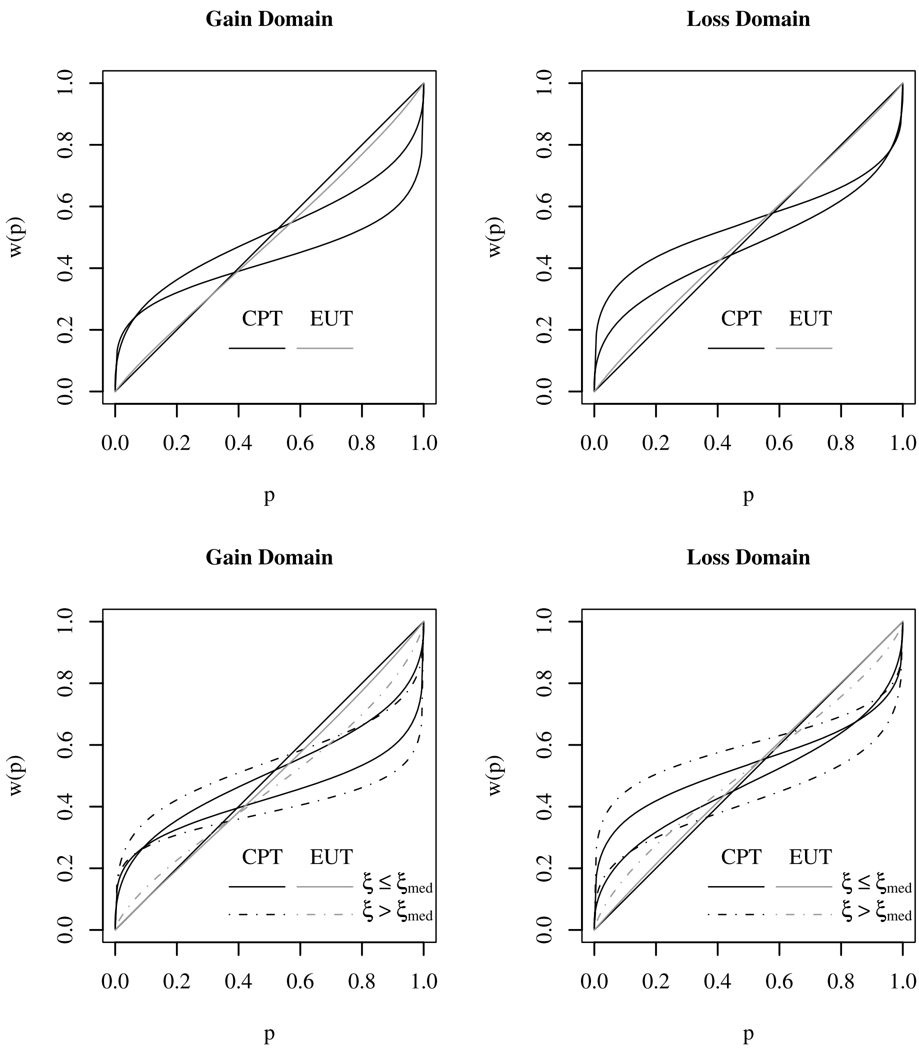

FIGURE 20.—Probability weights by error variance, pooled.

The lower panel contains the curves for the variance-specific types where the solid lines mark the low-variance people's curves and the dot-dashed lines denote the respective high-variance ones. Comparing the variance-specific curves with the overall averages reveals that low- and high-variance EUT probability weighting functions generally differ somewhat in degree of rationality, but largely remain within a comparatively narrow band around linear weighting. CPT individuals, however, exhibit a wide range of degrees of optimism. Typical high-variance individuals are either distinctly less optimistic (CPT-I) or more optimistic (CPT-II) than their low-variance colleagues.[29] Not surprisingly perhaps, decomposing behavior according to error variance widens the spectrum of emerging probability weighting types. These findings underscore that EUT types are a fairly homogeneous group, whereas CPT types display a much wider range of behaviors.

5. CONCLUDING REMARKS

We conducted three experiments based on the same design principles and applied a finite mixture model to the choice data. Our results provide novel insights: In all three data sets, the procedure renders a parsimonious characterization of risk taking behavior. Across experiments, we find an equal mix of distinct types, each characterized by a specific pattern of probability distortion. Almost every single individual is identified as one specific type, rendering segregation extremely clean. By and large, 20% of the population adhere to linear probability weighting and behave essentially as expected value maximizers, whereas majority preferences are more suitably represented by a model such as prospect theory, which can accommodate nonlinear probability weighting. In each data set, the overall CPT group is composed of a smaller group of 30% of the subjects who display substantial departures from linear probability weighting, and a relative majority of 50% who depart less radically from linear probability weighting. Moreover, classification is robust to an alternative model specification.

Whereas the distribution of types is the same in the Swiss and the Chinese data sets, there are substantial cultural differences in CPT-type behavior, the most prominent being the existence of a pronouncedly optimistic group of Chinese subjects who distort small- and medium-sized probabilities much more strongly than do the Swiss. This prevalence of Chinese optimism in lottery valuation may explain previous findings that, on average, Chinese respondents are relatively more risk seeking than westerners (Kachelmeier and Shehata (1992), Hsee and Weber (1999)). We also identify a gender difference in risk taking behavior: Women generally depart more strongly from linear probability weighting than men. This finding corroborates previous research (Fehr-Duda, de Gennaro, and Schubert (2006), Harrison and Rutström (2009)). Moreover, on average, female probability distortions vary predominantly in degree of optimism, whereas male probability distortions vary in degree of rationality.

Our findings demonstrate that the finite mixture approach is a powerful tool to identify and to characterize the distribution of risk taking types in the population. In this study, the individual is the unit of classification, that is, the *entirety* of an individual's choices governs group affiliation. As the low measures of entropy demonstrate, almost every individual got unambiguously assigned to one

[29] In the upper panel of Figure 20, comparatively more optimistic probability weighting represents CPT-II and comparatively less optimistic weighting represents CPT-I.

endogenously defined behavioral type. Previous work by Harrison and collaborators tried to accomplish a different goal: They estimated the probability that any one lottery choice, irrespective of the identity of the decision maker, was consistent with EUT or CPT, respectively, and found that "each [specification] is equally likely for these data" (Harrison and Rutström (2009, p. 144)). Clearly, in certain decision situations CPT-consistent choices are indistinguishable from EUT-consistent ones. Our findings indicate, for example, that this is the case for outcome probabilities in the neighborhood of 0.5. Since a CPT individual's choices in this region are interlinked with all her other choices, the respective observations are categorized as CPT by our method, but may be interpreted as either CPT or EUT in the choice-based approach. Therefore, classification results may differ depending on the unit of classification and the type of data available.

When we started this project, we were quite confident that we would find a considerable percentage of expected utility maximizers. What really surprised us is the robust percentage of EUT individuals, even across two so different cultures as the Swiss and Chinese. Our findings were recently corroborated by Conte, Hey, and Moffatt (2010), who found a similar distribution of behavioral types for British subjects.

Since the subject pools in all three of our experiments consisted of students, further research is needed to see whether the proportion of near rational EUT types changes significantly in a representative sample and whether the complexity of decision tasks greatly alters type assignment. If it can be ascertained that near rational actors constitute a nonnegligible proportion of the population, their behavior, depending on the nature of the strategic environment, may be decisive for aggregate outcomes. The existence of a robust share of near rational actors suggests using a mix of preference theories for modeling behavior rather than a single theory, which would yield systematically biased results. In our data, prospect theory adequately describes behavior of the majority of subjects, but the parameter estimates exhibit culture- as well as type-specific values. Researchers should take this evidence into account when constructing, estimating, and applying models of choice under risk.

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Consistency and Heterogeneity of Individual Behavior under Uncertainty

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By using graphical representations of simple portfolio choice problems, we generate a very rich dataset to study behavior under uncertainty at the level of the individual subject. We test the data for consistency with the maximization hypothesis, and we estimate preferences using a two-parameter utility function based on Faruk Gul (1991). This specification provides a good interpretation of the data at the individual level and can account for the highly heterogeneous behaviors observed in the laboratory. The parameter estimates jointly describe attitudes toward risk and allow us to characterize the distribution of risk preferences in the population. (*JEL* D11, D14, D81, G11)

We report the results of a series of experiments studying decision making under uncertainty. In our experimental design, we use an innovative graphical interface. Subjects see a graphical representation of a standard budget constraint on a computer screen. This can be interpreted either as a portfolio choice problem (the allocation of wealth between two risky assets) or a consumer decision problem (the selection of a bundle of contingent commodities subject to a standard budget constraint). Subjects use the mouse to choose a portfolio by pointing and clicking on the budget line. This intuitive and user-friendly interface allows for the quick and efficient elicitation of many decisions per subject from a wide variety of budget constraints. The result is a rich individual-level dataset that constitutes the foundation of this paper's contribution.

The richness of the dataset is immediately evident from inspecting the scatterplots corresponding to individual subjects' choices. These diagrams reveal distinctive behavioral patterns. Some individuals behave as if they were highly risk averse and always choose safe portfolios. Others behave as if they were risk neutral and maximize the expected value of payoffs. Still others combine elements of these behaviors with an apparent attempt to exploit the usual risk-return trade-off. The behavior of subjects is generally complex and we found it impossible to classify in a simple taxonomy.

Although individual behavior is quite heterogeneous, a second striking fact is the high level

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of consistency in the individual-level decisions. That is, most subjects behave as if they were maximizing a complete, transitive preference ordering over lotteries (portfolios). A well-known theorem of Sidney N. Afriat (1967) states that an individual's choices from a *finite* number of budget sets are consistent with maximization of a well-behaved utility function if and only if they satisfy the Generalized Axiom of Revealed Preference (GARP). In our experiment, individuals make a large number of choices on very different budget constraints. In particular, the shifts in income and relative prices are such that budget lines cross frequently. The variety of different choice problems faced by subjects produces data that allow for a powerful test of GARP. Subjects attain very high scores on standard measures of consistency, and most are close to the ideal of perfectly rational behavior.

The consistency of individual decisions naturally leads us to ask what kind of preferences are consistent with the observed choices. Our third discovery is that the data are well explained by a preference ordering in which the indifference curves have a kink at the 45-degree line, which corresponds to a portfolio with a certain payoff. One interpretation of this preference ordering is that it displays loss or disappointment aversion (Eddie Dekel 1986; Gul 1991). Expected utility theory (EUT) is a special case of this theory. The family of utility functions we estimate is characterized by two parameters, one of which measures loss or disappointment aversion.

To implement this approach, we have followed prior literature in using a constant relative risk aversion (CRRA) specification, assuming the power utility function commonly employed in the empirical analysis of choice under uncertainty. We have also estimated the model using a constant absolute risk aversion (CARA) specification, assuming the exponential form, and integrated the results of the CRRA and CARA specifications. For simplicity, the estimation technique, for both power and exponential utilities, is nonlinear least squares (NLLS), rather than maximum likelihood (ML). We also carry out the ML estimation, however, which is relegated to our Web appendices (http://www.e-aer.org/data/dec07/20060377_app.pdf).

The parameter estimates vary dramatically across subjects, implying that individual behavior under uncertainty is very heterogeneous. Over half of our subjects, however, have a significant degree of loss or disappointment aversion. The remainder appear to be well approximated by preferences consistent with EUT (John von Neumann and Oskar Morgenstern 1947; Leonard J. Savage 1954). Because preferences are characterized by two parameters, we cannot easily summarize attitudes toward risk by a single number. We can, however, compute a risk premium based on the difference between the expected value of a gamble and its certainty equivalent. Comparing the risk premium to a standard measure of risk aversion suggests that our estimates are within the range found by other researchers (cf. Kay-Yen Chen and Charles R. Plott 1998; Charles A. Holt and Susan K. Laury 2002; Jacob K. Goeree, Holt, and Thomas R. Palfrey 2002, 2003; Goeree and Holt 2004).

The rest of the paper is organized as follows. Section I provides a discussion of closely related literature. Section II describes the experimental design and procedures. Section III illustrates some important features of the data and establishes the consistency of the data with utility maximization. Section IV provides the econometric analysis, and Section V concludes. Experimental instructions, technical details, and individual-level data are gathered in the Web appendices.

I. Related Literature

The experimental literature on choice under uncertainty is vast and cannot be summarized here. Colin F. Camerer (1995) provides a comprehensive discussion of the experimental and theoretical work, and Chris Starmer (2000) provides a more recent review that focuses on evaluating non-EUT theories. The typical experimental design presents subjects with a number of binary choices. The objective is to test the empirical validity of particular axioms or to compare the predictive abilities of competing theories. These theories tend to be systematically disconfirmed by the data. This has motivated researchers to develop more descriptive models, and the investigation of these models has led to the discovery of new empirical regularities in the laboratory.

Typically, the criterion used to evaluate a theory is the fraction of choices it predicts correctly. A theory is "rejected" when the pattern of violations appears to be systematic. More recently, following the seminal work of John D. Hey and Chris Orme (1994) and David W. Harless and Camerer (1994), a number of papers compare models while allowing for randomness. In these studies, randomness can be interpreted as the effect of a trembling hand, calculation error, and so forth. While Harless and Camerer (1994) fit models to aggregate data, Hey and Orme (1994) use data derived from decisions over a very large menu of binary choices and estimate functional forms for individual subjects. They test EUT as a restriction on non-EUT theories and find that EUT appears to fit as well as non-EUT alternatives for almost 40 percent of their subjects, and that violations of EUT decay with repetition.

A few other studies, such as Imran S. Currim and Rakesh K. Sarin (1989), Richard L. Daniels and L. Robin Keller (1990), and Pamela K. Lattimore, Joanna R. Baker, and A. Dryden Witte (1992) have also estimated parametric utility functions for individual subjects. These studies find that many subjects obey EUT, with considerable variation in risk aversion across subjects. Our paper—both in its experimental method and theoretical apparatus—substantially extends this research program by providing new techniques and larger samples that enable more precise estimation and better predictions. Camerer (1995) emphasizes the need for such improvements in advancing the research program in this area.

The distinctive features of the present paper are the new experimental design and the application of tools from consumer demand theory to individual decision making in the laboratory. This experimental design generates data that are better suited in a number of ways to estimating risk preferences. First, the choice of a portfolio from a convex budget set provides more information about preferences than a discrete choice.[1] Second, the large amount of level data generated by this design allows us to apply statistical models to individual data rather than pooling data or assuming homogeneity across subjects. Hence, we may generate better individual-level estimates of risk aversion. Third, these decision problems are representative, both in the statistical sense and in the economic sense, rather than, as in existing methods, being designed to test a particular theory.

Choi, Fisman, Gale, and Kariv (2007) extend the revealed preference techniques used in this paper to test the rationality of individual behavior. They also illustrate how revealed preference techniques can be used to recover underlying preferences nonparametrically.

The experimental technique described in this paper can also be applied to other types of individual choice problems. For example, Fisman, Kariv, and Daniel Markovits (2007) employ a similar experimental methodology to study social preferences. While the papers share a similar experimental methodology, they address very different questions and produce very different behaviors.

II. Experimental Design and Procedures

A. *Design*

In the experimental task we study, individuals make decisions under conditions of uncertainty about the objective parameters of the environment. In our preferred interpretation, there are two *states of nature* denoted by $s = 1, 2$ and two associated *Arrow securities*, each of which promises a payoff of one unit of account in one state and nothing in the other. We consider the problem of allocating an individual's wealth between the two Arrow securities. Let x_s denote the demand for the security that pays off in state s and let p_s denote its price. We normalize the individual's wealth to 1. The budget constraint is then $p_1x_1 + p_2x_2 = 1$ and the individual can choose any portfolio $(x_1, x_2) \geq 0$ that satisfies this constraint.

An example of a budget constraint defined in this way is the straight line AB drawn in Figure 1. The axes measure the future value of a possible portfolio in each of the two states. The point C, which lies on the 45-degree line, corresponds to a portfolio with a certain payoff. By contrast, point A (point B) represents a portfolio in which all wealth is invested in the security that pays off in state 1 (state 2). A portfolio such

[1] In Graham Loomes (1991), subjects also allocate wealth within a portfolio of risky assets. The focus of his paper is on providing a test of the independence axiom, so the results are not directly comparable to those presented here. Loomes (1991) shows that most subjects made nearly rational choices but systematically violated the independence axiom, and that the observed behavior cannot be accommodated by a number of non-EUT alternatives.

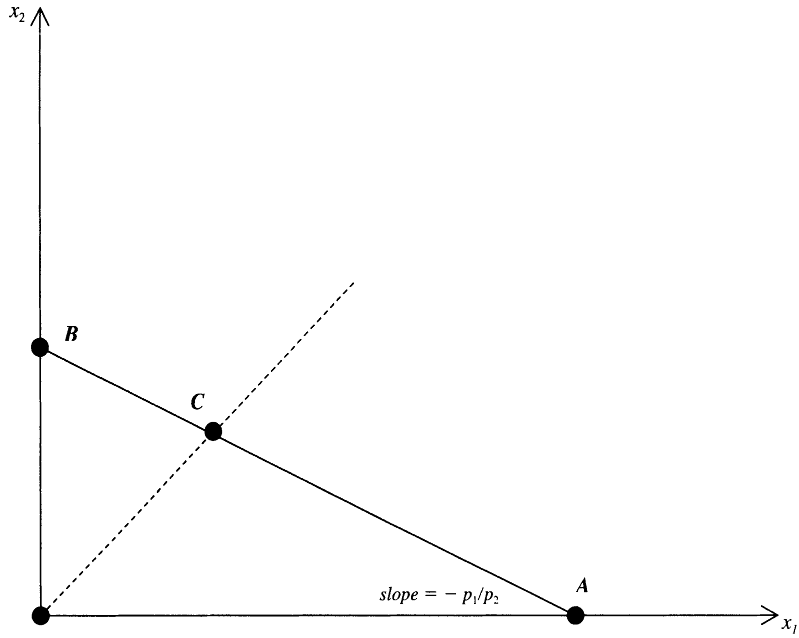

FIGURE 1. EXAMPLE OF A BUDGET CONSTRAINT WITH TWO STATES AND TWO ASSETS

as C is called a *safe* portfolio and portfolios such as A and B are called *boundary* portfolios. A portfolio that is neither a safe nor a boundary portfolio is called an *intermediate* portfolio. Notice that, given the objective probabilities of each state, positions on AB do not represent *fair* bets (portfolios with the same expected value as the safe portfolio). If π is the probability of state 1, and the slope of the budget line $-p_1/p_2$ is steeper than $-\pi/(1 - \pi)$, positions along AC have a higher payoff in state 1, a lower payoff in state 2, and a lower expected portfolio return than point C.

B. *Procedures*

The experiment was conducted at the Experimental Social Science Laboratory (X-Lab) at UC Berkeley under the X-Lab Master Human Subjects Protocol. The 93 subjects in the experiment were recruited from undergraduate classes and staff at UC Berkeley. After subjects read the instructions (reproduced in Web Appendix A), the instructions were read aloud by an experimenter. Each experimental session lasted about one and a half hours. Payoffs were calculated in terms of tokens and then converted into dollars. Each token was worth \$0.5. A \$5 participation fee and subsequent earnings, which averaged about \$19, were paid in private at the end of the session.

Each session consisted of 50 independent decision rounds. In each round, a subject was asked to allocate tokens between two accounts, labeled x and y. The x account corresponds to the x-axis and the y account corresponds to the y-axis in a two-dimensional graph. Each choice involved choosing a point on a budget line of possible token allocations. Each round started by having the computer select a budget line randomly from the set of lines that intersect at least one axis at or above the 50-token level and intersect both axes at or below the 100-token level. The budget lines selected for each subject in his decision problems were independent of each other and of the budget lines selected for other subjects in their decision problems.

The x-axis and y-axis were scaled from 0 to 100 tokens. The resolution compatibility of the budget lines was 0.2 tokens. At the beginning of each decision round, the experimental program dialog window went blank and the entire setup reappeared. The appearance and behavior of the pointer were set to the Windows mouse default and the pointer was automatically repositioned randomly on the budget line at the beginning of each round. To choose an allocation, subjects used the mouse or the arrows on the keyboard to move the pointer on the computer screen to the desired allocation. Subjects could either left-click or press the enter key to record their allocations. No subject reported difficulty understanding the procedures or using the computer interface. (The computer program dialog window is shown in the experimental instructions reproduced in Web Appendix A.)

At the end of the round, the computer randomly selected one of the accounts, x or y. Each subject received the number of tokens allocated to the account that was chosen. We studied a *symmetric* treatment (subjects ID 201–219 and 301–328), in which the two accounts were equally likely ($\pi = 1/2$), and two *asymmetric* treatments (subjects ID 401–417, 501–520, and 601–609) in which one of the accounts was selected with probability 1/3 and the other account was selected with probability 2/3 ($\pi = 1/3$ or $\pi = 2/3$). The treatment was held constant throughout a given experimental session. Subjects were not informed of the account that was actually selected at the end of each round. At the end of the experiment, the computer selected one decision round for each participant, where each round had an equal probability of being chosen, and the subject was paid the amount he had earned in that round.

III. From Data to Preferences

A. *Data Description*

We begin with an overview of some important features of the experimental data. We will focus on the symmetric treatment, where the regularities in the data are very clear, and select a small number of subjects who illustrate salient features of the data. One must remember, however, that for most subjects the data are much less regular. Figure 2 depicts, for each subject, the relationship between the log-price ratio $\ln(p_1/p_2)$ and the token share $x_1/(x_1 + x_2)$. The figures for the full set of subjects are available in Web Appendix B, which also shows the portfolio choices (x_1, x_2) as points in a scatterplot, and the relationship between the log-price ratio $\ln(p_1/p_2)$ and the budget share p_1x_1 (prices are normalized by income so that $p_1x_1 + p_2x_2 = 1$). Clearly, the distinction between token share and budget share is relevant only in the presence of price changes.

Figure 2A depicts the choices of a subject (ID 304) who always chose nearly safe portfolios $x_1 = x_2$. This behavior is consistent with infinite risk aversion. Figure 2B shows the choices of the only subject (ID 303) who, with a few exceptions, made nearly equal expenditures $p_1x_1 = p_2x_2$. This behavior is consistent with a logarithmic von Neumann–Morgenstern utility function. This is a very special case, where the regularity in the data is very clear. We also find many cases of subjects who implemented "smooth" responsiveness of portfolio allocations to prices, albeit less precisely. Among these subjects, we find considerable heterogeneity in price sensitivity. Perhaps most interestingly, no subject in the symmetric treatment allocated all the tokens to x_1 if $p_1 < p_2$ and to x_2 if $p_1 > p_2$. This is the behavior that would be implied by pure risk neutrality, for example. Nevertheless, boundary portfolios $(x_1, 0)$ and $(0, x_2)$ were used in combination with other portfolios by many subjects, as we will see below.[2]

Another interesting regularity is illustrated in Figure 2C, which depicts the decisions of a subject (ID 307) who allocated all of his tokens to x_1 (x_2) for values of $\ln(p_1/p_2)$ that give a flat (steep) budget line. This aspect of his behavior would be consistent with risk neutrality. However, for a variety of intermediate prices corresponding to $\ln(p_1/p_2)$ around zero, this subject chose nearly safe portfolios $x_1 = x_2$. This aspect of his choice behavior is consistent with infinite risk aversion. So this subject is apparently switching between behaviors that are individually consistent with EUT, but mutually inconsistent. In fact, as we will see in the econometric analysis below, this subject's preferences exhibit loss or disappointment aversion (where the safe portfolio $x_1 = x_2$ is taken to be the reference point).

There are yet more fine-grained cases where the behavior is less stark, such as the subject (ID 216) whose choices are depicted in Figure 2D.

[2] A single subject (ID 508) almost always chose $x_1 = 0$ if $p_1 > p_2$, and $x_2 = 0$ otherwise. However, he participated in the asymmetric treatment ($\pi = 2/3$), and thus his choices do not correspond to risk neutrality. Three subjects (ID 205, 218, and 320) chose a minimum level of consumption of ten tokens in each state, and allocated the residual to the less expensive security.

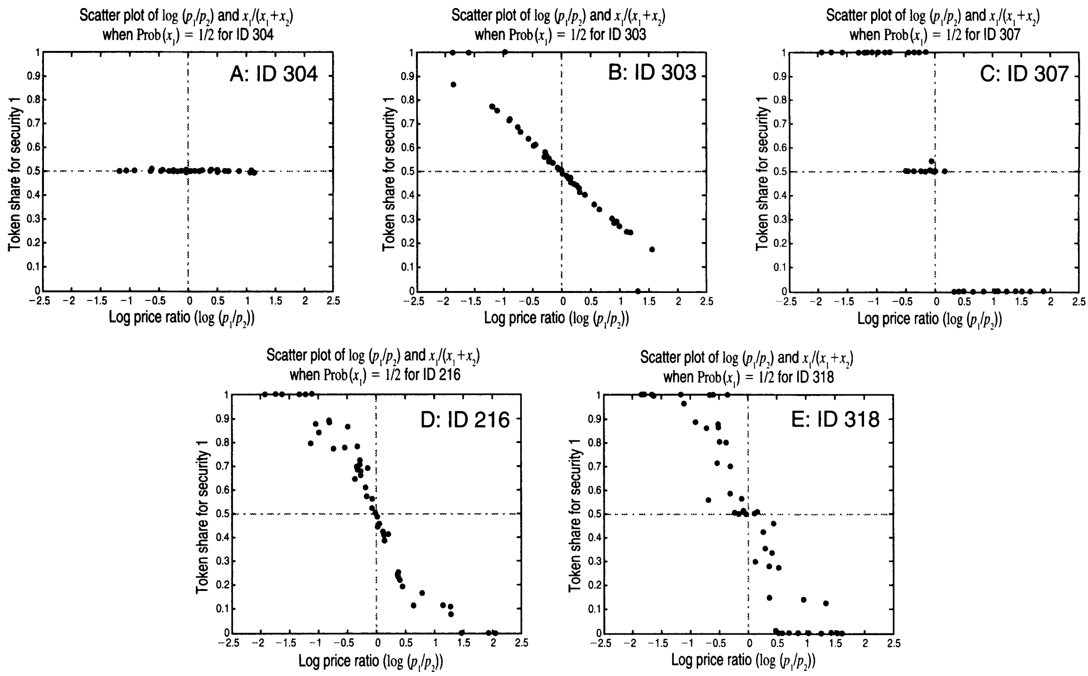

FIGURE 2. RELATIONSHIP BETWEEN THE LOG-PRICE RATIO $\ln(p_1/p_2)$ AND THE TOKEN SHARE $x_1/(x_1 + x_2)$ FOR SELECTED SUBJECTS

This subject combines intermediate portfolios for a variety of intermediate relative prices with boundary portfolios for prices that give sufficiently flat or steep budget lines. Further, the subject (ID 318) whose choices are depicted in Figure 2E combines safe, intermediate, and boundary portfolios. There is something distinctly discontinuous in the behavior of these subjects, and their choices are clearly not consistent with the standard interpretation of EUT.

These are of course special cases, where the regularities in the data are very clear. There are many subjects for whom the behavioral rule is much less clear, and there is no taxonomy that allows us to classify all subjects unambiguously. But even in cases that are harder to classify, we can pick out the safe, intermediate, and boundary portfolios described above. Overall, a review of the full dataset reveals striking regularities *within* and marked heterogeneity *across* subjects.

B. *Testing Rationality*

Before proceeding to a parametric analysis of the data, we want to check whether the observed data are consistent with any preference ordering, EU or non-EU. To answer this question, we need to make use of some results from the theory of revealed preference. A well-known result, due to Afriat (1967), tells us that a *finite* dataset generated by an individual's choices can be rationalized by a well-behaved (piecewise linear, continuous, increasing, and concave) utility function, if and only if the data satisfy GARP.[3] GARP requires that if a portfolio x is revealed preferred to x', then x' is not strictly revealed preferred to x. So, in order to show that the data are consistent with utility-maximizing behavior, we can simply check whether they satisfy GARP (simple in theory, though difficult in practice for moderately large datasets).

Since GARP offers an exact test (either the data satisfy GARP or they do not) and choice data almost always contain at least some violations, we also wish to measure the *extent* of GARP violations. We report measures of GARP violations based on an index proposed by Afriat (1972). Afriat's *critical cost efficiency index*

[3] This statement of the result follows Hal R. Varian (1982), who replaced the condition Afriat called *cyclical consistency* with GARP.

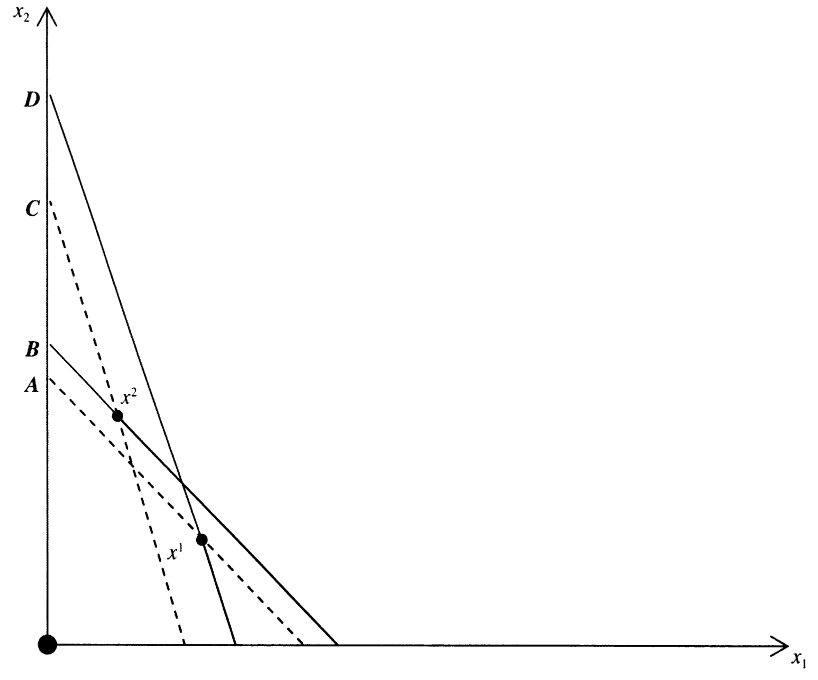

FIGURE 3. CONSTRUCTION OF THE CCEI FOR A SIMPLE VIOLATION OF GARP

(CCEI) measures the amount by which each budget constraint must be adjusted in order to remove all violations of GARP. Figure 3 illustrates one such adjustment for a simple violation of GARP involving two portfolios, x^1 and x^2.[4] It is clear that x^1 is revealed preferred to x^2, because x^2 is cheaper than x^1 at the prices at which x^1 is purchased, and x^2 is revealed preferred to x^1, since x^1 is cheaper than x^2 at the prices at which x^2 is purchased. If we shifted the budget constraint through x^2 as shown, the violation would be removed. In this case, the CCEI would equal A/B ($A/B > C/D$).

By definition, the CCEI is a number between zero and one, where a value of one indicates that the data satisfy GARP perfectly. There is no natural threshold for determining whether subjects are close enough to satisfying GARP that they can be considered utility maximizers. Varian (1991) suggests a threshold of 0.95 for the CCEI, but this is purely subjective. A more scientific approach, proposed by Stephen G. Bronars (1987), calibrates the various indices using a hypothetical subject whose choices are uniformly distributed on the budget line. We generated a random sample of 25,000 subjects and found that their scores on the Afriat CCEI indices averaged 0.60.[5] Furthermore, all 25,000 random subjects violated GARP at least once, and none had a CCEI score above Varian's 0.95 threshold. If we choose the 0.9 efficiency level as our critical value, we find that only 12 of the random subjects had CCEI scores above this threshold.

Figure 4 compares the distributions of the CCEI scores generated by the sample of 25,000 hypothetical subjects (gray) and the distributions of the scores for the actual subjects (black).[6] The horizontal axis shows the value of the index, and the vertical axis measures the percentage of subjects corresponding to each interval. The histograms clearly show that a significant majority of

[4] In fact, here we have a violation of the weak axiom of revealed preference (WARP). Note that choices that violate WARP also violate GARP, but the opposite need not hold.

[5] Each of the 25,000 random subjects makes 50 choices from randomly generated budget sets, in the same way that the human subjects do.

[6] To allow for small trembles resulting from the slight imprecision of subjects' handling of the mouse, all the results presented below allow for a narrow confidence interval of one token (for any i and $j \neq i$, if $|x^i, x^j| \leq 1$, then x^i and x^j are treated as the same portfolio). We generate virtually identical results allowing for a narrower confidence interval.

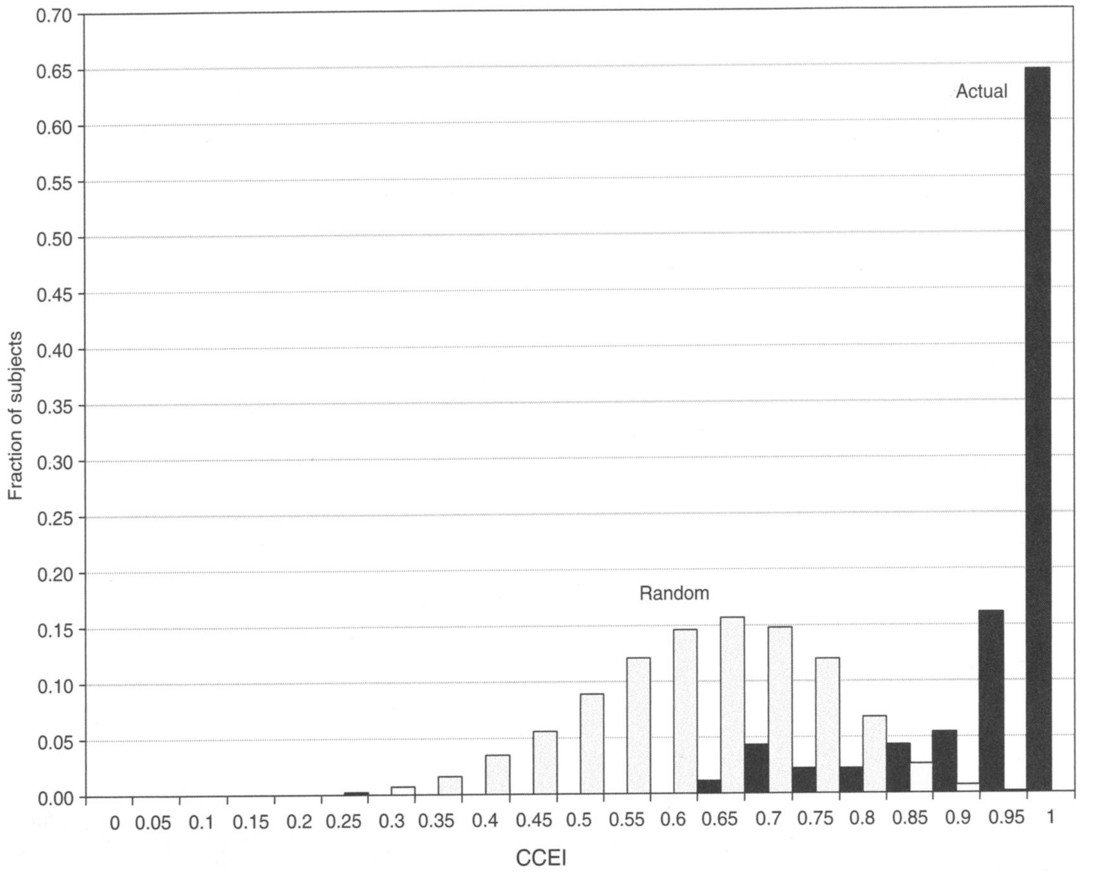

FIGURE 4. DISTRIBUTIONS OF AFRIAT'S (1972) CRITICAL COST EFFICIENCY INDEX (CCEI)

the subjects did much better than the randomly generated subjects and only a bit worse than an ideal (rational) subject. Our experiment is thus sufficiently powerful to exclude the possibility that consistency is the accidental result of random behavior. As a practical note, the consistency results presented above suggest that subjects did not have any difficulty in understanding the procedures or using the computer program.

The power of the experiment is very sensitive to the number of observations for each subject. To illustrate this point, we simulated the choices of random subjects in two experiments that used the design of this paper, except that in one, subjects made 10 choices and in the other, they made 25 choices. In each case, the simulation was based on 25,000 random subjects. In the simulated experiment with 25 choices, 4.3 percent of random subjects were perfectly consistent, 14.3 percent had CCEI scores above Varian's 0.95 threshold, and 28.9 percent had values above 0.90. In the simulated experiment with only 10 choices, the corresponding percentages were 20.2, 37.3, and 50.6. In other words, there is a very high probability that random behavior will pass the GARP test if the number of individual decisions is as low as it usually has been in earlier experiments. We refer the interested reader to Choi, Fisman, Gale, and Kariv (2007) for further details on the power of tests for consistency with GARP.

Web Appendix C lists, by subject, the number of violations of WARP and GARP, and also reports the values of three indices according to descending CCEI scores. Although it provides a summary statistic of the overall consistency of the data with GARP, the CCEI does not give any information about which of the observations are causing the most severe violations. We refer the interested reader to Web Appendix C for precise details on testing for consistency with GARP and other indices that have been proposed for this purpose by Varian (1991) and Martijn

Houtman and J. A. H. Maks (1985). The various indices are all computationally intensive for even moderately large datasets. (The computer program and details of the algorithms are available from the authors upon request.)

IV. Econometric Analysis

A. *Specification*

The near consistency of subjects' choices tells us that there exists a well-behaved utility function that rationalizes *most* of the data. Additionally, because of the nature of the data, particularly the clustering at the safe and boundary portfolios, EUT cannot provide a plausible fit for the data at the individual level. The particular patterns observed in the data lead us to consider the theory of loss/disappointment aversion proposed by Gul (1991), which implies that in the symmetric case ($\pi = 1/2$) the utility function over portfolios (x_1, x_2) takes the form

$$(1) \quad \min\{\alpha u(x_1) + u(x_2), u(x_1) + \alpha u(x_2)\},$$

where $\alpha \geq 1$ is a parameter measuring loss/disappointment aversion and $u(\cdot)$ is the utility of consumption in each state. In this interpretation, the safe portfolio $x_1 = x_2$ is taken to be the reference point. If $\alpha > 1$ there is a kink at the point where $x_1 = x_2$, and if $\alpha = 1$ we have the standard EUT representation. This formulation thus embeds EUT as a parsimonious and tractable special case and allows for the estimation of the parameter values in our empirical analysis below.

B. *Constant Relative Risk Aversion* (*CRRA*)

To implement this approach, we assume that $u(\cdot)$ takes the power form commonly employed in the analysis of choice under uncertainty,

$$(2) \qquad u(x) = \frac{x^{1-\rho}}{(1-\rho)},$$

where ρ is the Arrow-Pratt measure of relative risk aversion. The parameters in this two-parameter specification, α and ρ, jointly describe the attitudes toward risk and allow us to characterize the distribution of risk preferences in the population.

The use of the power function has one limitation, however, in that the function is not well defined for the boundary portfolios. We incorporate the boundary observations $(1/p_1, 0)$ or $(0, 1/p_2)$ into our estimation using strictly positive portfolios where the zero component is replaced by a small consumption level such that the demand ratio x_1/x_2 is either $1/\omega$ or ω, respectively. The minimum ratio is chosen to be $\omega = 10^{-3}$. The selected level did not substantially affect the estimated coefficients for any subject.

With this adjustment, maximizing the utility function subject to the budget constraint yields a nonlinear relationship between $\ln(p_1/p_2)$ and $\ln(x_1/x_2)$, which is illustrated in Figure 5 below. If the security prices are very different, then the optimum is the boundary portfolio with the larger expected payoff. If the security prices are very similar (log-price ratios are close to zero), then the optimum is the safe portfolio. In these cases, the optimal choice is insensitive to small price changes. For log-price ratios that are neither extreme nor close to zero, the optimum is an intermediate portfolio and the choice is sensitive to small changes in the risk-return trade-off.

The subject's demand will belong to one of five possible cases: (a) a corner solution in which $x_1 = \omega\bar{x}_2$ if $x_1/x_2 < \omega$; (b) an interior solution where $\omega \leq x_1/x_2 < 1$; (c) a corner solution where $x_2 = \omega\bar{x}_1$ if $1/\omega < x_1/x_2$; (d) an interior solution where $1 < x_1/x_2 \leq 1/\omega$; and (e) a solution at the kink where $x_1/x_2 = 1$.[7] The two interior solutions are characterized by first-order conditions in the form of equations; the two corner solutions and the kink are characterized by inequalities. Combining these cases, we can define an individual-level econometric specification for each subject n separately, and generate estimates of $\hat{\alpha}_n$ and $\hat{\rho}_n$ using NLLS.

The data generated by an individual's choices are $\{(\bar{x}_1^i, \bar{x}_2^i, x_1^i, x_2^i)\}_{i=1}^{50}$, where (x_1^i, x_2^i) are the coordinates of the choice made by the subject, and $(\bar{x}_1^i, \bar{x}_2^i)$ are the endpoints of the budget line (so we can calculate the relative prices $p_1^i/p_2^i = \bar{x}_2^i/\bar{x}_1^i$ for each observation i). Next, we identify the five different cases discussed above (corner solutions, interior solutions, kink). The first-order conditions at the optimal choice (x_1^{i*}, x_2^{i*}), given $(\bar{x}_1^i, \bar{x}_2^i)$, can thus be written as follows (here we

[7] Intuitively, these conditions set the ratio of demands x_1/x_2 equal to ω or $1/\omega$ when observations are near to the boundary.

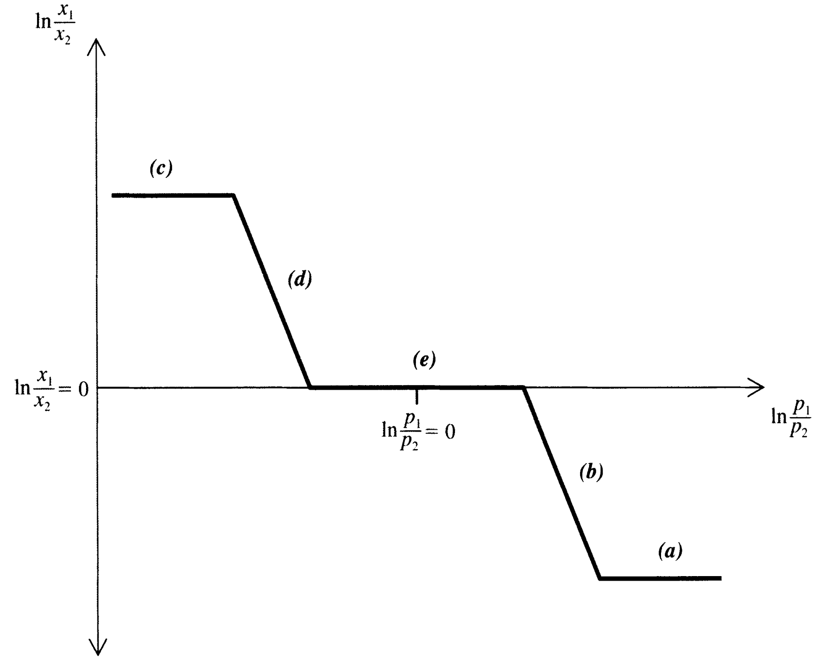

FIGURE 5. ILLUSTRATION OF THE RELATIONSHIP BETWEEN $\ln(p_1/p_2)$ AND $\ln(x_1/x_2)$

have taken logs of the first-order conditions and then replaced prices with the observed values):

$$(3)\quad \ln\left(\frac{x_1^{i*}}{x_2^{i*}}\right)=f\left[\ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right);\alpha,\rho,\omega\right]$$

$$=\begin{cases} \ln\omega & \text{if } \ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right)\geq \ln\alpha - \rho\ln\omega, \\ -\frac{1}{\rho}\left[\ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right)-\ln\alpha\right] & \text{if } \ln\alpha<\ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right)<\ln\alpha-\rho\ln\omega, \\ 0 & \text{if } -\ln\alpha\leq\ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right)\leq\ln\alpha, \\ -\frac{1}{\rho}\left[\ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right)+\ln\alpha\right] & \text{if } -\ln\alpha+\rho\ln\omega<\ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right)<-\ln\alpha, \\ -\ln\omega & \text{if } \ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right)\leq-\ln\alpha+\rho\ln\omega. \end{cases}$$

Then, for each subject n, we choose the parameters, α and ρ, to minimize

$$(4)\quad \sum_{i=1}^{50}\left[\ln\left(\frac{x_1^i}{x_2^i}\right)-f\left(\ln\left(\frac{\bar{x}_2^i}{\bar{x}_1^i}\right);\alpha,\rho,\omega\right)\right]^2.$$

Before proceeding to estimate the parameters, we omit the nine subjects with CCEI scores below 0.80 (ID 201, 211, 310, 321, 325, 328, 406, 504, and 603) as their choices are not sufficiently consistent to be considered utility-generated. We also exclude the three subjects (ID 205, 218, and 320) who almost always chose a minimum level of consumption of ten tokens in each state, and the single subject (ID 508) who almost always chose a boundary portfolio. This leaves a total of 80 subjects (86.0 percent) for whom we recover preferences by estimating the model. Finally, we note that out of the 80 subjects, 33 subjects (41.3 percent) have no boundary observations, and this increases to a total of 60 subjects (75.0 percent) if we consider subjects with fewer than five boundary observations.

Web Appendix D presents the results of the estimations $\hat{\alpha}_n$ and $\hat{\rho}_n$ for the full set of subjects. Table 1 displays summary statistics for

TABLE 1—SUMMARY STATISTICS OF INDIVIDUAL-LEVEL CRRA ESTIMATION

α	All	$\pi = 1/2$	$\pi \neq 1/2$
Mean	1.315	1.390	1.248
Std	0.493	0.584	0.388
p5	1.000	1.000	1.000
p25	1.000	1.000	1.000
p50	1.115	1.179	1.083
p75	1.445	1.477	1.297
p95	2.427	2.876	2.333
ρ	All	$\pi = 1/2$	$\pi \neq 1/2$
Mean	1.662	2.448	0.950
Std	7.437	10.736	1.206
p5	0.053	0.048	0.080
p25	0.233	0.165	0.290
p50	0.481	0.438	0.573
p75	0.880	0.794	0.990
p95	3.803	3.871	3.693

the estimation results. Of the 80 subjects listed in Web Appendix D, 56 subjects (70.0 percent) exhibit kinky preferences ($\hat{\alpha}_n > 1$). Also, a significant fraction of our subjects in both treatments have moderate levels of $\hat{\rho}_n$. However, our specification allows the kink (α) to "absorb" some of the curvature in the indifference curves (ρ). More importantly, because the model has two parameters, α and ρ, it is not obvious how to define a measure of risk aversion. In the next section, we define one particularly useful measure and discuss its properties.

Figure 6 presents, in graphical form, the data from Web Appendix D by showing a scatterplot of $\hat{\alpha}_n$ and $\hat{\rho}_n$, split by symmetric (black) and asymmetric (white) treatments. Two subjects with high values for $\hat{\rho}_n$ (ID 304 and 516) are omitted to facilitate presentation of the data. The most notable features of the distributions in Figure 6 are that both the symmetric and asymmetric subsamples exhibit considerable heterogeneity in both $\hat{\alpha}_n$ and $\hat{\rho}_n$ and that their values are not correlated ($r^2 = 0.000$).

Finally, Figure 7 shows the relationship between $\ln(p_1/p_2)$ and $\ln(\hat{x}_1/\hat{x}_2)$ for the same group of subjects (ID 304, 303, 307, 216, and 318) that we followed in the nonparametric analysis. Figure 7 also depicts the actual choices (x_1,x_2). The figures for the full set of subjects are available in Web Appendix E. An inspection of the estimation results against the observed data reveals that the fit is quite good for most subjects. It also shows, however, that the specification has difficulty dealing with the subject (ID 307) who combines safe portfolios for values of $\ln(p_1/p_2)$ close to zero with boundary portfolios for values of $\ln(p_1/p_2)$ that give steep or flat budget lines. His estimated parameters $\hat{\alpha} = 1.043$ and $\hat{\rho} = 0.076$ may be reasonable given the fact that boundary portfolios are chosen also for intermediate values of $\ln(p_1/p_2)$, but leaves the safe portfolio choices largely unexplained. For similar reasons, the estimated curve does not pick up the apparent kink in the scatterplot of the subject (ID 318) with $\hat{\alpha} = 1.056$ and $\hat{\rho} = 0.173$ who often chose safe portfolios. Clearly, no continuous relationship could replicate these patterns.

The estimation also seems sensitive to "outliers," as can be seen in the case of the subject (ID 303) with $\hat{\alpha} = 1.641$ and $\hat{\rho} = 0.284$, who is the only subject who very precisely implemented logarithmic preferences, apart from a small number of deviations. Although his behavior is very regular and consistent with standard preferences, the attempt to fit the outlying observations exaggerates the nonlinearity and leads to the insertion of a spurious kink. Apart from this subject, the individual-level relationship between $\ln(p_1/p_2)$ and $\ln(\hat{x}_1/\hat{x}_2)$ does not have a kink unless one is clearly identifiable in the data. In fact, a review of our full set of subjects shows that the estimation is more likely to ignore a kink that is evident in the data than to invent one that is not there. Perhaps most notably, the estimation fits the "switch" points, when they exist, quite well.

C. *Measuring Risk Aversion*

Since we have estimated a two-parameter utility function, risk aversion cannot be represented by a single univariate measure. To summarize the risk aversion of our subjects, we use the concept of the *risk premium*. Specifically, we propose a gamble over wealth levels which offers 50-50 odds of winning or losing some fraction $0 < h < 1$ of the individual's initial wealth ω_0. The risk premium for h is the fraction of wealth r that satisfies the certainty equivalence relationship

$$(5)\quad (1 + \alpha)u(\omega_0(1 - r)) = \alpha u(\omega_0(1 - h)) + u(\omega_0(1 + h)).$$

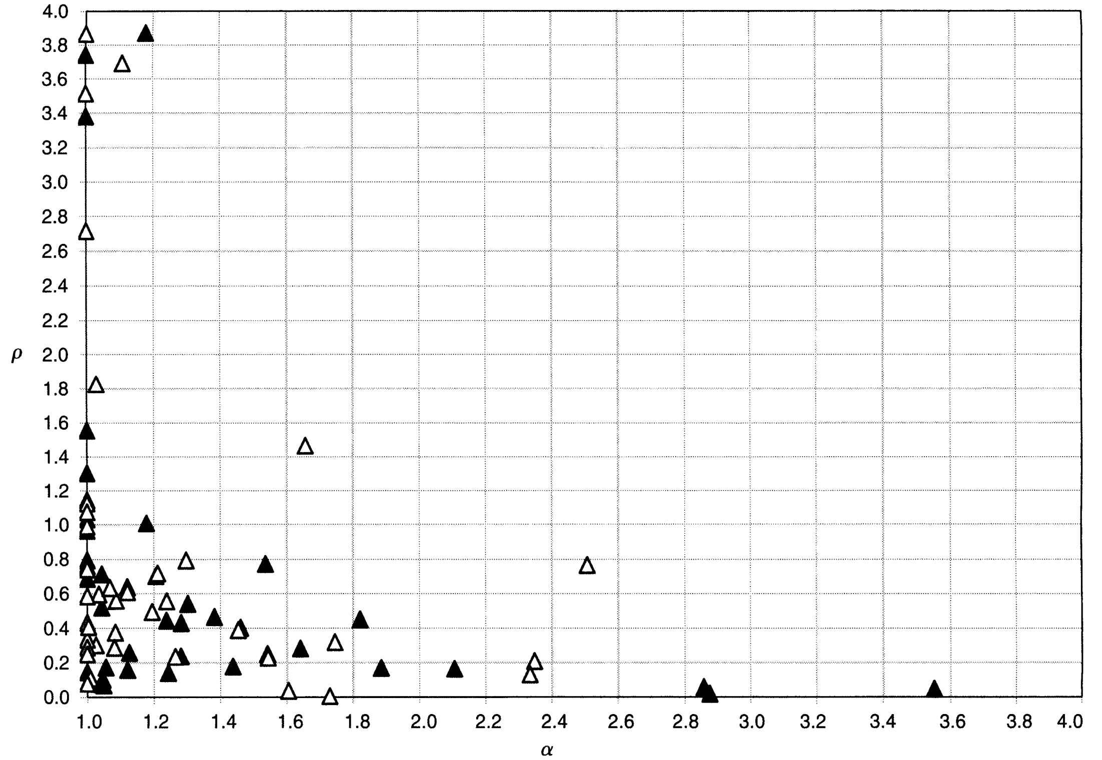

FIGURE 6. SCATTERPLOT OF THE ESTIMATED CRRA PARAMETERS $\hat{\alpha}_n$ AND $\hat{\rho}_n$

Substituting the power function yields

$$(6)\quad (1+\alpha)(1-r)^{1-\rho} = \alpha(1-h)^{1-\rho} + (1+h)^{1-\rho},$$

which is independent of the initial wealth level ω_0. This equation can be rearranged to yield

$$(7)\quad r(h) = 1 - \left[\frac{\alpha(1-h)^{1-\rho} + (1+h)^{1-\rho}}{1+\alpha}\right]^{\frac{1}{1-\rho}}.$$

To help us understand the meaning of the parameters α and ρ, Figure 8 plots the risk premium $r(h)$ for different values of α and ρ. Note that an increase in α makes the risk premium curve $r(h)$ steeper and an increase in ρ makes it more convex.

To see the role of α and ρ more clearly, we consider the second-order approximation of $r(h)$. Direct calculation yields

$$(8)\quad r(h) \approx r(0) + r'(0)h + r''(0)\frac{h^2}{2} = 0 + \frac{\alpha-1}{\alpha+1}h + \rho\frac{2\alpha}{(\alpha+1)^2}h^2,$$

which reduces to the usual case $r(h) \approx \rho\frac{h^2}{2}$ when $\alpha = 1$. The approximation clearly tells us that α has a first-order effect on the risk premium r while ρ has a second-order effect, so the standard practice of considering small gambles is inadequate. Motivated by the second-order approximation of $r(h)$, we calculate the following weighted average of ρ and α:

$$(9)\qquad r(1) \approx \frac{\alpha-1}{\alpha+1} + \rho\frac{2\alpha}{(\alpha+1)^2},$$

which is proportional to the Arrow-Pratt measure of relative risk aversion when $\alpha = 1$. We will use $r(1)$ as a summary measure of risk aversion.

Although there is no strong theoretical rationale for adopting this formula as our summary measure of risk aversion, it agrees with other measures of risk aversion. As a benchmark, we use the "low-tech" approach of estimating an individual-level power utility function directly from the data. By straightforward calculation,

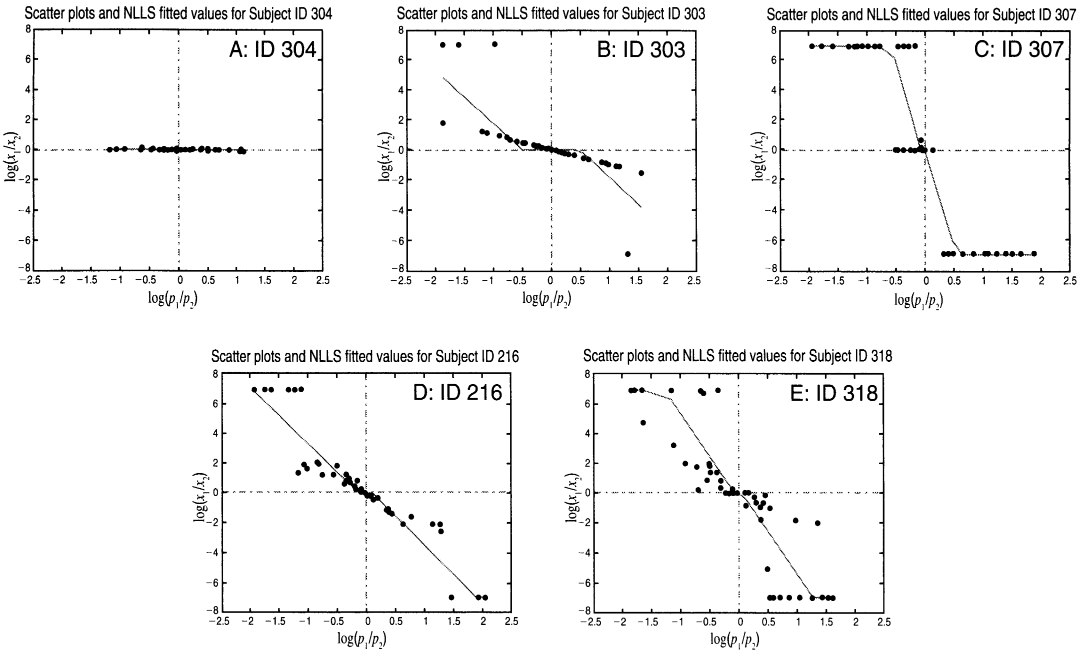

FIGURE 7. RELATIONSHIP BETWEEN $\ln(p_1/p_2)$ AND $\ln(\hat{x}_1/\hat{x}_2)$ FOR SELECTED SUBJECTS

the solution to the maximization problem (x_1^*, x_2^*) satisfies the first-order condition

$$(10) \qquad \frac{\pi}{1-\pi}\left(\frac{x_2^*}{x_1^*}\right)^{\rho} = \frac{p_1}{p_2}$$

and the budget constraint $p \cdot x^* = 1$. This generates the following individual-level econometric specification for each subject n:

$$(11) \quad \log\left(\frac{x_{2n}^i}{x_{1n}^i}\right) = \alpha_n + \beta_n \log\left(\frac{p_{1n}^i}{p_{2n}^i}\right) + \varepsilon_n^i,$$

where ε_n^i is assumed to be distributed normally with mean zero and variance σ_n^2. We generate estimates of $\hat{\alpha}_n$ and $\hat{\beta}_n$ using ordinary least squares (OLS), and use this to infer the values of the underlying parameter $\hat{\rho}_n = 1/\hat{\beta}_n$.

Before proceeding to the estimations, we again omit the nine subjects with CCEI scores below 0.80, as well the four subjects (ID 307, 311, 324, and 508) for whom the simple power formulation is not well defined. This leaves the group of 80 subjects (82.8 percent) for whom we estimated parameters. For these subjects, we discard the boundary observations, for which the power function is not well defined, using a narrow confidence interval of one token (if $x_1^i \leq 1$ or $x_2^i \leq 1$, then x^i is treated as a boundary portfolio). This results in many fewer observations for a small number of subjects.

Web Appendix F lists the estimated risk measures $\hat{r}_n$ and values of $\hat{\rho}_n$ derived from the simple OLS estimation for the full set of subjects. The last column of Appendix F reports the number of observations per subject in the OLS estimation. Table 2 displays summary statistics. Most notably, the distribution shifts to the left when calculated using the $\hat{r}_n$ estimates as compared to the distribution calculated using the OLS $\hat{\rho}_n$ estimates. The reason may be the upward bias in the OLS estimates due to the omission of boundary observations.

Figure 9 shows a scatterplot of $\hat{r}_n$ and values of $\hat{\rho}_n$, split by symmetric (black) and asymmetric (white) treatments. Subjects with high values for $\hat{\rho}_n$ (ID 203, 204, 210, 304, 314, 515, 516, and 607) are omitted to facilitate presentation of the data. Note that once more we obtain very similar distributions for the symmetric and asymmetric subsamples, and that there is a strong correlation between the estimated $\hat{r}_n$ parameters and individual-level estimates of $\hat{\rho}_n$ that come from a simple expected-utility model ($r^2 = 0.850$).

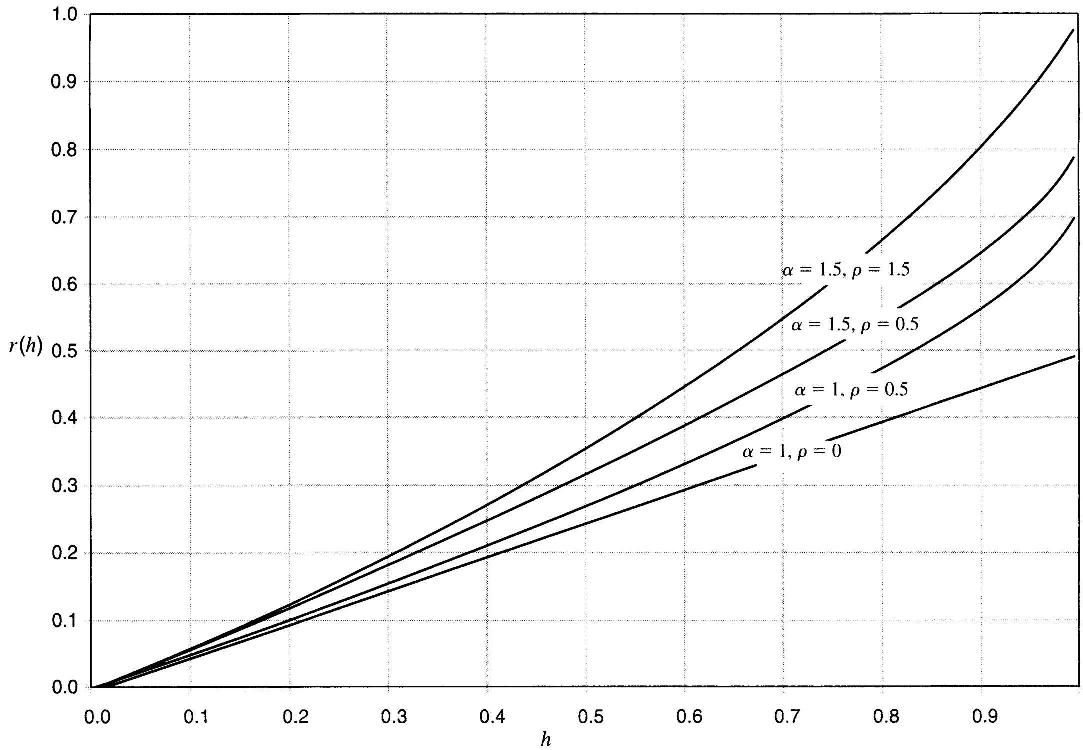

FIGURE 8. RISK PREMIUM $r(h)$ FOR DIFFERENT VALUES OF α AND ρ

Much of the existing evidence about risk preferences is based on laboratory experiments. Our individual-level measures of risk aversion are very similar to some recent estimates that come out of the simple expected-utility model. For comparison, Chan and Plott (1998) and Goeree, Holt, and Palfrey (2002) report, respectively, $\rho = 0.48$ and 0.52 for private-value auctions. Goeree, Holt, and Palfrey (2003) estimate $\rho = 0.44$ for asymmetric matching pennies games, and Goeree and Holt (2004) report $\rho = 0.45$ for a variety of one-shot games. Holt and Laury (2002) estimate individual degrees of risk aversion from ten paired lottery-choices under both low- and high-money payoffs. Most of their subjects in both treatments exhibit risk preferences in the 0.3–0.5 range.

D. *Constant Absolute Risk Aversion* (*CARA*)

While we have followed prior literature in using a CRRA specification, we are concerned that our estimates may be sensitive to this assumption. In particular, one difficulty with assuming CRRA is that behavior depends on the initial level of wealth ω_0, and since ω_0 is unobserved, the model is not completely identified. In the analysis above, we have followed the standard procedure of setting $\omega_0 = 0$. To provide a check on the robustness of these results, we have also estimated the model under the assumption of CARA. The CARA utility function has two advantages. First, it allows us to get rid of the nuisance parameter ω_0 (which bedevils most attempts to estimate power utility functions). Secondly, it easily accommodates boundary portfolios.

To implement this approach, we assume the exponential form

$$u(x) = -e^{-Ax}, \tag{12}$$

where $A \geq 0$ is the coefficient of absolute risk aversion (we assume without loss of generality that $\omega_0 = 0$). By direct calculation, the first-

TABLE 2—SUMMARY STATISTICS OF RISK MEASURES AND OLS ESTIMATION RESULTS

$r(1)$	All	$\pi = 1/2$	$\pi \neq 1/2$
Mean	0.919	1.316	0.559
Std	3.588	5.177	0.588
p5	0.066	0.059	0.125
p25	0.246	0.266	0.233
p50	0.379	0.383	0.372
p75	0.529	0.516	0.538
p95	1.914	2.005	1.894
OLS	**All**	$\pi = 1/2$	$\pi \neq 1/2$
Mean	3.168	1.401	4.888
Std	15.025	1.362	21.060
p5	0.439	0.439	0.375
p25	0.648	0.597	0.700
p50	0.904	0.826	1.011
p75	1.434	1.426	1.533
p95	5.348	5.158	5.448

TABLE 3—SUMMARY STATISTICS OF INDIVIDUAL-LEVEL CARA ESTIMATION

α	All	$\pi = 1/2$	$\pi \neq 1/2$
Mean	1.154	1.121	1.182
Std	0.488	0.332	0.595
p5	1.000	1.000	1.000
p25	1.000	1.000	1.000
p50	1.000	1.000	1.000
p75	1.083	1.066	1.110
p95	1.787	1.929	1.506
A	**All**	$\pi = 1/2$	$\pi \neq 1/2$
Mean	0.043	0.038	0.047
Std	0.052	0.042	0.059
p5	0.003	0.004	0.003
p25	0.014	0.016	0.014
p50	0.029	0.029	0.031
p75	0.046	0.038	0.050
p95	0.159	0.144	0.159

order conditions at the optimal choice (x_1^{i*}, x_2^{i*}), given $(\bar{x}_1^i, \bar{x}_2^i)$, can be written as follows:

$$(13)\quad x_2^{i*} - x_1^{i*} = f\left[\bar{x}_1^i, \bar{x}_2^i; \alpha, A\right]$$

$$= \begin{cases} \bar{x}_2^i & \text{if } \ln\left(\dfrac{\bar{x}_2^i}{\bar{x}_1^i}\right) \geq \ln\alpha + A\bar{x}_2^i, \\ \frac{1}{A}\left[\ln\left(\dfrac{\bar{x}_2^i}{\bar{x}_1^i}\right) - \ln\alpha\right] & \text{if } \ln\alpha < \ln\left(\dfrac{\bar{x}_2^i}{\bar{x}_1^i}\right) < \ln\alpha + A\bar{x}_2^i, \\ 0 & \text{if } -\ln\alpha \leq -\ln\left(\dfrac{\bar{x}_2^i}{\bar{x}_1^i}\right) \leq \ln\alpha, \\ \frac{1}{A}\left[\ln\left(\dfrac{\bar{x}_2^i}{\bar{x}_1^i}\right) + \ln\alpha\right] & \text{if } -\ln\alpha + A\bar{x}_1^i < \ln\left(\dfrac{\bar{x}_2^i}{\bar{x}_1^i}\right) < -\ln\alpha, \\ -\bar{x}_1^i & \text{if } \ln\left(\dfrac{\bar{x}_2^i}{\bar{x}_1^i}\right) \leq -\ln\alpha + A\bar{x}_1^i. \end{cases}$$

Then, for each subject n, we choose the parameters, α and A, to minimize

$$(14)\quad \sum_{i=1}^{50} \left[(x_2^i - x_1^i) - f(\bar{x}_1^i, \bar{x}_2^i; \alpha, A)\right]^2.$$

The CARA specification implies a (nonlinear) relationship between $\log(p_1/p_2)$ and $x_1 - x_2$. Since the variation in $\log(p_1/p_2)$ is quite small relative to the variation in $x_1 - x_2$, the estimated individual-level regression coefficients are bound to be small. This implies that the estimated coefficients of absolute risk aversion, $\hat{A}_n$, will be small too. The individual-level estimation results, $\hat{\alpha}_n$ and $\hat{A}_n$, are also presented in Web Appendix G. Table 3 displays summary statistics.

To make the coefficients of absolute and relative risk aversion comparable, we *multiply* the absolute risk aversion by average consumption and *divide* relative risk aversion by average consumption. As our measure of a subject's *average consumption*, we use the average demand for the security that pays off in state 1 over the 50 budgets.[8] Figure 10A shows a scatterplot of the estimates of relative risk aversion from the CRRA specification ($\hat{\rho}_n$) and estimates of absolute risk aversion from the CARA specification ($\hat{A}_n$) multiplied by average consumption (*RRA*), with the sample split by symmetric (black) and asymmetric (white) treatments. Similarly, Figure 10B shows a scatterplot of the estimates of absolute risk aversion from the CARA specification ($\hat{A}_n$) and estimates of relative risk aversion from the CRRA specification ($\hat{\rho}_n$) divided by average consumption (*ARA*) (subjects ID 304 and 516 are omitted because they have very high values of $\hat{A}_n$). In both scatterplots, we see

[8] We have also used the subject's average value of $(x_1+x_2)/2$ as an adjustment factor with very similar results.

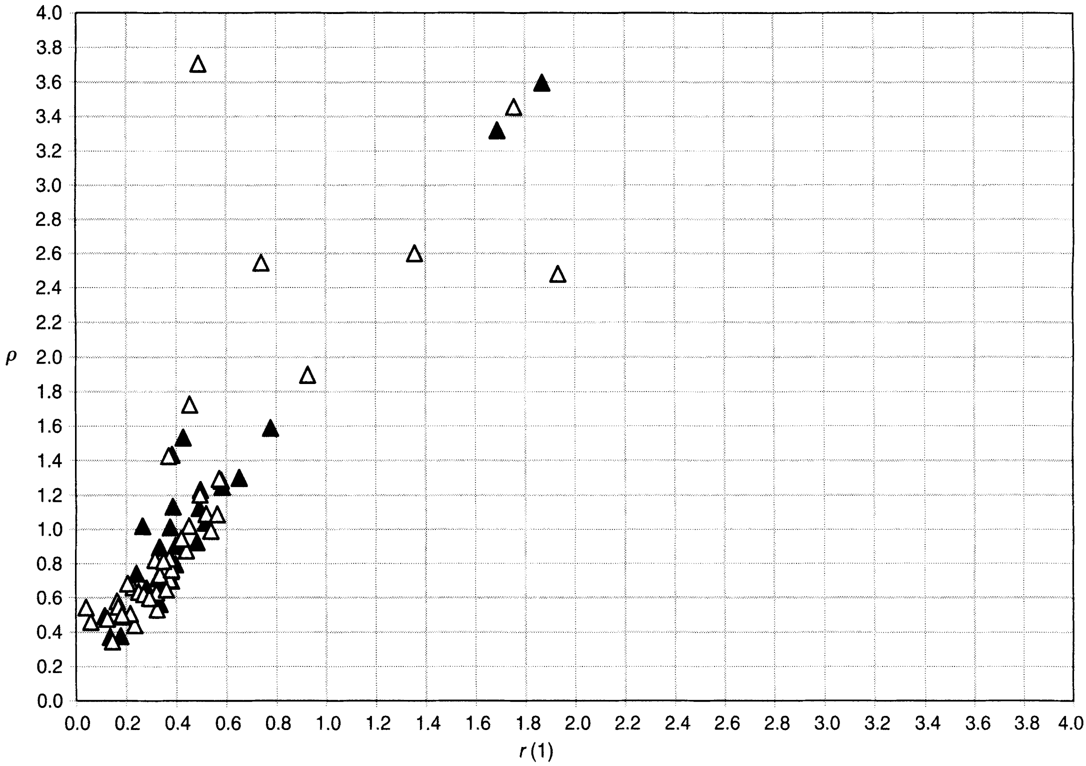

FIGURE 9. SCATTERPLOT OF THE RISK MEASURES $\hat{r}_n$ AND VALUES $\hat{\rho}_n$ DERIVED FROM THE SIMPLE OLS ESTIMATION

a strong linear relationship between the suitably scaled coefficients of risk aversion.

E. *Maximum Likelihood Estimation*

Finally, we note that we have also explored a maximum likelihood (ML) estimation of the utility function in (1). In contrast to the NLLS estimation reported above, the parameter estimates from the ML method seemed implausible in certain situations. Specifically, the values of ρ and A we obtained were much lower than those estimated by NLLS, and in fact were close to zero when we observed clustering of choices around the safe portfolio. As a result, the corresponding values of α were significantly greater than one. Although the specified error structure is consistent with the observed choices, it makes such choices very unlikely. Intuitively, with a sharp kink and very flat indifference curves away from the kink, the observed choices should be almost always either at the kink or at the boundary. The specification of the error structure we used may have been inappropriate for this purpose, which is why we adopted the NLLS method, which is consistent with a broad range of possible error structures. We refer the interested reader to Web Appendix H for precise details on the ML estimation.

V. Conclusion

We present a set of experimental results that build on a graphical computer interface that contains a couple of important innovations over previous work. The primary contribution is an experimental technique for collecting richer data on choice under uncertainty than was previously possible. Perhaps the most interesting aspect of the dataset generated by this approach is the heterogeneity of behavior. In the present paper, we have shown that this behavior can be rationalized by "kinky" preferences that are consistent with loss or disappointment aversion. The potential of this dataset to teach us about individual behavior has not been exhausted, however. One aspect of the data that invites further scrutiny is the "switching" between stylized behavior

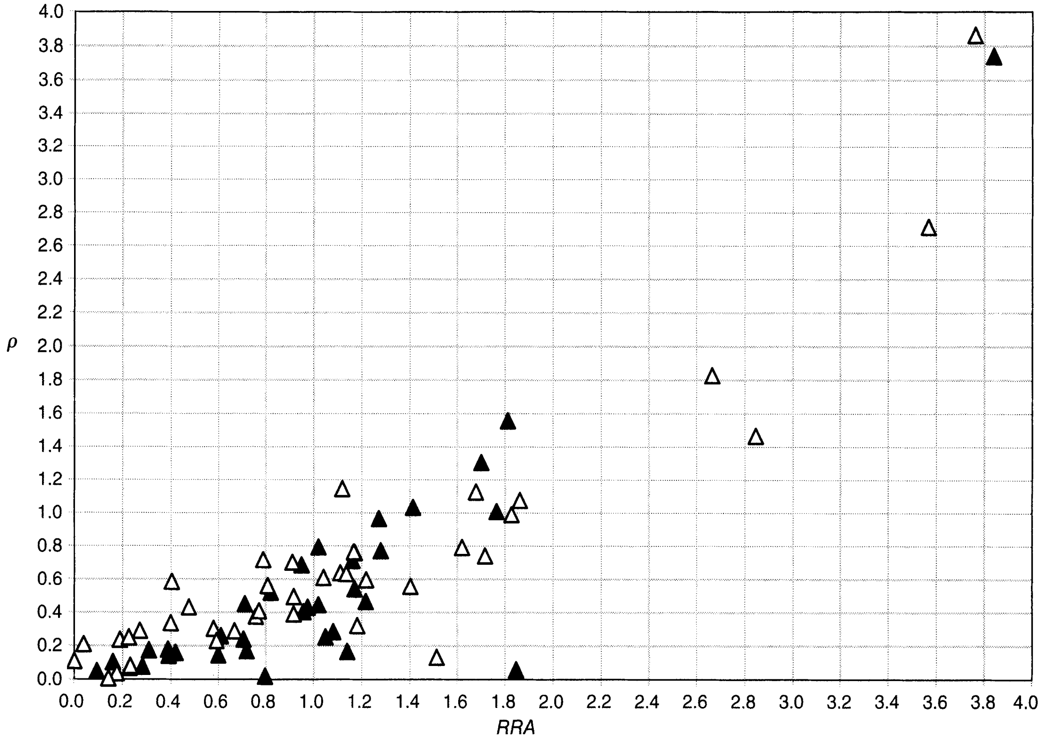

FIGURE 10A. SCATTERPLOT OF THE CRRA $\hat{\rho}_n$ ESTIMATES AND THE CARA ADJUSTED RELATIVE RISK AVERSION (*RRA*) ESTIMATES

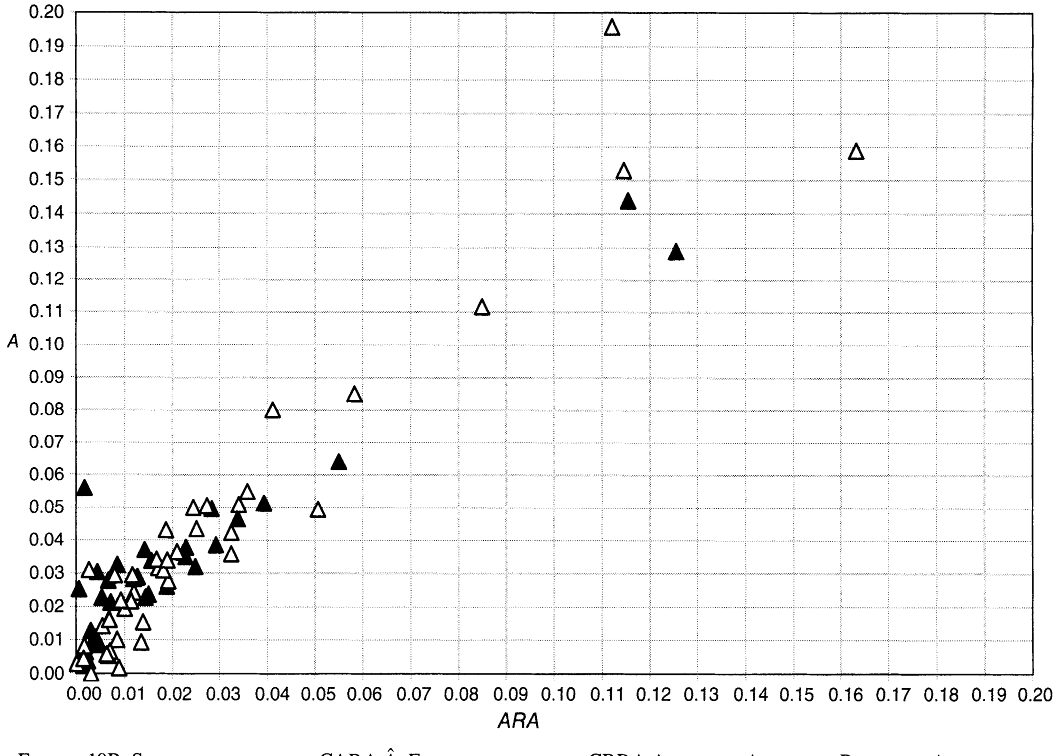

FIGURE 10B. SCATTERPLOT OF THE CARA $\hat{A}_n$ ESTIMATES AND THE CRRA ADJUSTED ABSOLUTE RELATIVE AVERSION (*ARA*) ESTIMATES patterns exhibited by some subjects. Subjects' behavior appears to be made up of a small number of stylized patterns of behavior, sometime choosing safe portfolios, sometimes choosing boundary portfolios, and sometimes choosing intermediate portfolios. We plan to explore this and other themes in future work based on extensions of the present experimental design.

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Who Is (More) Rational?

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Who Is (More) Rational?†

By Syngjoo Choi, Shachar Kariv, Wieland Müller, and Dan Silverman*

Revealed preference theory offers a criterion for decision-making quality: *if decisions are high quality then there exists a utility function the choices maximize. We conduct a large-scale experiment to test for consistency with utility maximization. Consistency scores vary markedly within and across socioeconomic groups. In particular, consistency is strongly related to wealth: A standard deviation increase in consistency is associated with 15–19 percent more household wealth. This association is quantitatively robust to conditioning on correlates of unobserved constraints, preferences, and beliefs. Consistency with utility maximization under laboratory conditions thus captures* decision-making ability *that applies across domains and influences important real-world outcomes.* (*JEL* D12, D14, D81, D83, D91, G11)

In his Foundations of Economic Analysis (1947), Paul Samuelson offered a natural criterion for decision-making quality based solely on observable behavior. Adopting Samuelson's approach, we test whether individual behavior in a choice under risk experiment is consistent with the utility maximization model. We conducted the experiment using the CentERpanel, a panel study of a large representative sample of households in the Netherlands that collects a wide range of individual sociodemographic and economic information about its members.

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The article provides three types of analysis. First, we offer a purely descriptive overview of the consistency of the experimental data with the utility maximization model. Second, we analyze the correlation between the levels of consistency and socioeconomic characteristics. In this way we address the question: "who is (more) rational?" Third, we test the validity of a causal interpretation of the relationship between our proposed measure of decision-making quality—the consistency of the experimental data with utility maximization—and wealth accumulation in the real world.

In accordance with Samuelson's approach, traditional economic analysis assumes that individual behavior can be rationalized by a utility function. In this standard view, heterogeneity in choices is attributed to heterogeneity in preferences, constraints, information, or beliefs. More recently, several strands of research consider heterogeneity in choices driven also by differences in decision-making ability. Different from traditional analysis, this literature allows that the choices that some people actually make may be different from the choices they would make if they had the skills or knowledge to make better decisions. This research thus takes the view that those with lower decision-making *ability* may make choices of lower decision-making *quality*.

The idea that people vary in their decision-making ability, and therefore make choices of different decision-making quality, has intuitive appeal and important consequences for economics. It is difficult, however, to make definitive judgments about which choices exhibit low decision-making quality, and which people have inferior decision-making ability, due to twin problems of *identification* and *measurement*. The identification problem is to distinguish differences in decision-making ability from unobserved differences in preferences, constraints, information, or beliefs. The measurement problem is to define and implement a practical, portable, quantifiable, and economically interpretable measure of decision-making quality. The two problems are conceptually distinct, but tightly linked in practice.

The identification problem emerges because it is usually unclear whether those with lower decision-making ability—as evidenced by less education, lower cognitive abilities, or less financial literacy—are making choices of lower decision-making quality. They might have different preferences over the same outcomes, or face different but unobserved incentives and constraints, or have different information, or hold different beliefs. The measurement problem emerges because only rarely are the relevant incentives so clear and the data quality so high that classifying some choices as of low decision-making quality is straightforward and uncontroversial. Typically, a measure of decision-making quality is challenging to formalize, quantify, and make practical for application in a variety of choice environments.

We offer a new approach to the challenges posed by the identification and measurement problems. Our point of departure is a proposal to measure decision-making quality by the consistency of choices with economic rationality, in the sense of a complete and transitive preference ordering. Adopting this standard for decision-making quality, we present individuals with an economic choice experiment which provides a stringent test of utility maximization. The measure thus has a well-established economic interpretation, and revealed preference theory tells us whether we have enough data to make it statistically useful. In addition, the analytical techniques and experimental platform are easily portable to a variety of choice problems. Our approach thus addresses the measurement problem.

Furthermore, an experiment like ours, to the extent possible, holds information and beliefs constant within subject and controls the relevant constraints. If decision-making ability is defined simply as the capacity to make choices of higher decision-making quality, then our approach addresses the identification problem inside the laboratory. It can distinguish individual heterogeneity in decision-making ability from unobserved differences in preferences, constraints, information, or beliefs.

Our interest in decision-making quality under laboratory conditions largely derives, however, from the possibility that it reflects decision-making ability that affects important outcomes outside the laboratory. We evaluate this possibility first by examining the correlation between decision-making quality in the experiment and socioeconomic characteristics. The goal of this analysis is not to establish causation. If, however, we find a significant correlation between decision-making quality and certain characteristics, this lends a basic level of credence to the idea that people with these characteristics tend to make different choices not just because they face different constraints or have different preferences, but also because they tend to have different levels of decision-making ability.

To evaluate a causal interpretation of the link between decision-making quality in the experiment and important economic outcomes in the real world, we examine whether our measure of decision-making quality from the experiment can independently and robustly explain real-world economic outcomes, conditional on socioeconomic characteristics including income, age, education, and occupation. If heterogeneity in decision-making ability is an important source of heterogeneity in real-world outcomes, and if decision-making quality in the experiment is a good proxy for decision-making ability, then differences in the experiment-based measure across subjects should help explain differences in their real-world outcomes.

We chose wealth as the outcome of interest because the task of explaining wealth provides a strong test of the idea that decision-making quality in the experiment reflects a more general form of decision-making ability. The test is strong because wealth is determined by countless decisions, made over time in many different settings, and involving many different trade-offs, thus increasing our chance of rejecting a relationship. We are also motivated to study wealth by prior research that documents large wealth differentials among households with similar lifetime income. The extent to which these differentials can be explained either by standard observables, such as family structure or income volatility, or by standard unobservables, such as risk tolerance or intertemporal substitution, is a subject of some debate (see Bernheim, Skinner, and Weinberg 2001; Ameriks, Caplin, and Leahy 2003; and Scholz, Seshadri, and Khitatrakun 2006 for different perspectives).

A causal interpretation of the relationship between decision-making quality in the experiment and wealth depends importantly on the estimated correlation between these two measures being quantitatively robust to conditioning on additional correlates of unobserved preferences, constraints, information, and beliefs. This assessment of robustness is not an effort to "control for everything." Instead, in the spirit of Altonji, Elder, and Taber (2005), we examine whether the estimated correlation is much affected by the inclusion of additional controls that, a priori, should be correlated with economic outcomes through their correlation with unobserved or misspecified variables. If these unobservables are indeed important sources of the observed correlation between consistency and economic outcomes, then adding the controls should have a substantial effect on the estimated correlation coefficients.

We measure the decision-making quality in the experiment by evaluating the consistency of individual choices with the Generalized Axiom of Revealed Preference (GARP). We assess how nearly individual behavior complies with GARP using standard measures of consistency that have been proposed for quantifying the extent of violations. There is considerable heterogeneity within and across sociodemographic groups. Taking advantage of the large and heterogeneous CentERpanel sample, we find that high-income and high-education subjects display greater levels of consistency than lower-income and lower-education subjects. In addition, men are more consistent than women, and young subjects tend more toward utility maximization than those who are old. The magnitudes imply that low-income subjects on average "leave on the table" as much as 3.3 percentage points more of their earnings, relative to high-income subjects, by making inconsistent choices. The corresponding numbers for low-education subjects, females, and old subjects are 2.6, 2.4, and 5.1, respectively.

We also find an economically large and statistically significant correlation between consistency in the experiment and household wealth. The point estimates indicate that, conditional on measures of current income, age, education, occupation, basic demographic characteristics, and household structure, a standard deviation increase in the consistency score of the person who is primarily responsible for household financial matters is associated with 15–19 percent more household wealth. As important, this estimated correlation is quantitatively robust to conditioning on many additional correlates of unobserved preferences, constraints, information, and beliefs. We interpret the economically large, statistically significant, and quantitatively robust relationship between decision-making quality in the experiment—the consistency of the experimental data with the utility maximization model—and household wealth as evidence of decision-making ability that applies across choice domains and affects important real-world outcomes.

We also show that alternative measures of decision-making quality from the experiment and decision-making ability from the CentERpanel survey are not substitutes for our measure of compliance with GARP. The alternatives include (i) a stronger notion of decision-making quality in our experiment that measures the extent of violations of both GARP and first-order stochastic dominance; (ii) parametric estimates of a tendency to "tremble" in an experiment by von Gaudecker, van Soest, and Wengström (2011) with an overlapping sample of CentERpanel members; and (iii) scores from Frederick's (2005) Cognitive Reflection Test that, in other samples, is well correlated with measures of cognitive ability. We find that these three alternatives either have no independent power to predict wealth, or are not well correlated with compliance with GARP in the experiment. Finally, we investigate the correlation between decision-making quality in the experiment and details of household saving allocations that influence wealth.

The rest of the article is organized as follows. Section I describes the experimental design and procedures. Section II describes decision-making quality in the experimental data. Section III contains analysis of the correlation between decision-making quality and socioeconomic characteristics. Section IV discusses the relationship between wealth differentials and decision-making ability. Section V describes the margins along which we extend the previous literature. Section VI contains some concluding remarks. The article also includes six online data and technical Appendices for the interested reader.

I. The Experiment

A. *Sample*

The experiment uses the CentERpanel, an online, weekly, and stratified survey of a sample of over 2,000 households and 5,000 individual members. The sample is designed to be representative of the Dutch-speaking population in the Netherlands. Via the Internet, the survey instrument allows researchers to implement experiments and collects a great deal of individual demographic and economic information from its respondents. The subjects in the experiment were recruited at random from the entire CentERpanel sample. The experiment was conducted online with 1,182 CentERpanel adult members. Table 1 provides summary statistics of individual characteristics. We present the data for *participants* (completed the experiment), *dropouts* (logged in but quit the experiment), and *nonparticipants* (recruited for the experiment but never logged in). In later analysis we will control for sample selection using the Heckman (1979) model.

B. *Design*

In our experiment, we present subjects with a sequence of decision problems under risk. Each decision problem was presented as a choice from a two-dimensional budget line. A choice of the allocation from the budget line represents an allocation of points between accounts x and y (corresponding to the horizontal and vertical axes). The actual payoffs of a particular choice were determined by the allocation to the x and y accounts; the subject received the points allocated to one of the accounts x or y, determined at random and equally likely. An example of a budget line defined in this way is the line AB drawn in Figure 1. The point C, which lies on the 45-degree line, corresponds to the equal allocation with certain outcome, whereas points A and B represent allocations in which all points are allocated to one of the accounts. Notice that points along AC are risky—they have a lower payoff in state x and a higher payoff in state y—but because the slope of the budget line AB is steeper than -1, they have higher expected return than point C. By contrast, points along BC have lower expected return than point C.

C. *Procedures*

The procedures described below are identical to those used by Choi et al. (2007), with the exception that the experiment described here consisted of 25, rather than 50, decision problems.[1] We also made some minor changes to accommodate the online

[1] The number of individual decisions is still higher than usual in the literature, and revealed preference analysis presented below shows the experiment provides a dataset consisting of enough individual decisions over a sufficiently wide range of budget lines to provide a powerful test of consistency.

TABLE 1—SOCIODEMOGRAPHIC INFORMATION

	Participants	Dropouts	Nonparticipants
Female	45.43	37.89	50.00
Age			
16–34	18.53	3.16	26.14
35–49	26.14	12.11	32.13
50–64	35.62	38.42	27.58
65+	19.71	46.32	14.15
Education			
Low	33.59	42.63	30.99
Medium	29.70	22.63	31.61
High	36.72	34.74	37.40
Household monthly income			
€0–2,500	22.76	40.00	22.62
€2,500–3,499	25.55	22.11	18.18
€3,500–4,999	29.19	15.79	28.51
€5,000+	22.50	22.11	30.68
Occupation			
Paid work	53.13	39.47	62.91
House work	11.59	7.89	8.78
Retired	20.90	42.63	13.95
Others	14.38	10.00	14.36
Household composition			
Partner	80.88	67.89	82.64
Number of children	0.84	0.32	1.09
Observations	1,182	190	968

Notes: Participants completed the experiment, dropouts logged in and quit the experiment, and nonparticipants were recruited for the experiment but never logged in. The low, medium, and high education levels correspond to primary or prevocational secondary education, preuniversity secondary education or senior vocational training, and vocational college or university education, respectively. We use household monthly gross income-level categories such that the proportions of participants in each category are approximately equal. The classification of levels of completed education and occupations are based on the categorization of Statistics Netherlands (Centraal Bureau voor de Statistiek).

experimental setting. Each decision problem started with the computer selecting a budget line randomly from the set of budget lines that intersect with at least one of the axes at 50 or more points, but with no intercept exceeding 100 points. The budget lines selected for each subject in different decision problems were independent of each other and of the sets selected for any of the other subjects in their decision problems. Choices were restricted to allocations on the budget constraint.[2] Choices were made using the computer mouse to move the pointer on the computer screen to the desired point and then clicking the mouse or hitting the enter key. More information and full experimental instructions, including the computer program dialog window, are available in online Appendix I.

[2]Like Choi et al. (2007), we restricted choices to allocations on the budget line so that subjects could not dispose of payoffs. In Fisman, Kariv, and Markovits (2007), each decision involved choosing a point on a graph representing a budget set that included interior allocations. Since most of their subjects had no violations of budget balancedness (those who did violate budget balancedness also had many GARP violations even among their choices that were on the budget line), we restricted choices to allocations on the budget constraint to make the computer program easier to use.

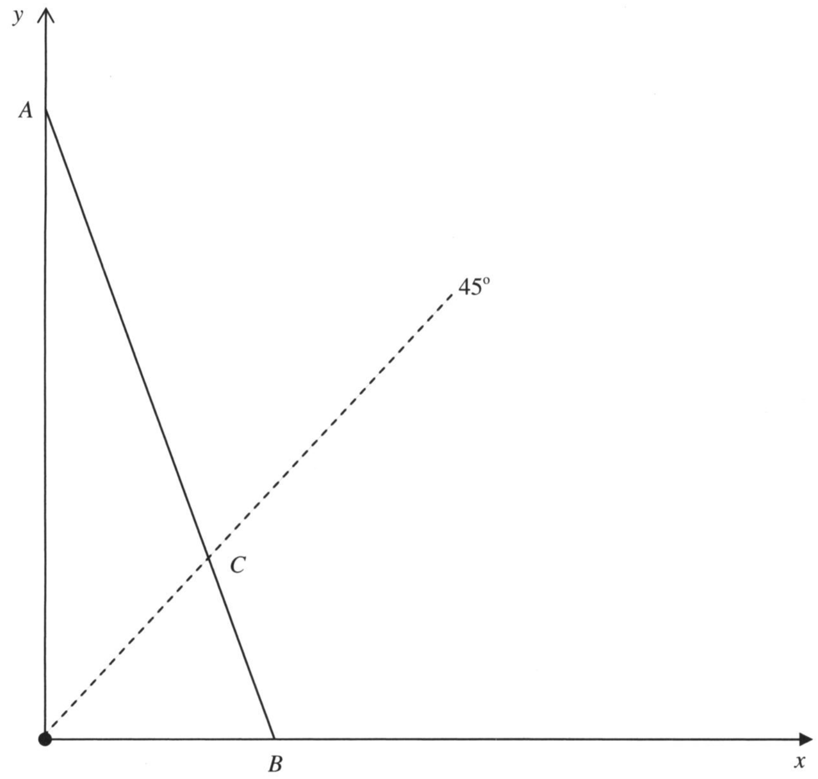

Figure 1. An Illustration of the Budget Line

During the course of the experiment, subjects were not provided with any information about the account that had been selected in each round. At the end of the experiment, the computer selected one decision round for each subject, where each round had an equal probability of being chosen, and the subject was paid the amount he had earned in that round. Payoffs were calculated in terms of points and then converted into euros. Each point was worth €0.25. Subjects received their payment from the CentERpanel reimbursement system via direct deposit into a bank account.

II. Decision-Making Quality

We propose to measure decision-making quality by the consistency of choices with economic rationality, and we described a simple economic choice experiment in which we can measure decision-making quality with a high degree of precision, and separate it from other sources of heterogeneity in choice. Specifically, we employ the GARP to test whether the finite set of observed price and quantity data that our experiment generated may be rationalized by a utility function. GARP generalizes various revealed preference tests. It requires that if allocation $\mathbf{x}^i$ is revealed preferred to $\mathbf{x}^j$, then $\mathbf{x}^j$ is not *strictly* and directly revealed preferred to $\mathbf{x}^i$;

that is, allocation $\mathbf{x}^i$ must cost at least as much as $\mathbf{x}^j$ at the prices prevailing when $\mathbf{x}^j$ is chosen.[3]

If choices are generated by a nonsatiated utility function, then the data must satisfy GARP. Conversely, the result due to Afriat (1967) tells us that if a *finite* dataset generated by an individual's choices satisfies GARP, then the data can be rationalized by a utility function. Consistency with GARP has long been a touchstone for rationality, but it demands only a complete and transitive preference ordering. It places no restrictions on the utility function and makes no assumptions about what is reasonable to maximize. Since it is possible to "pump" an indefinite amount of money out of an individual making intransitive decisions, consistency with GARP provides a crucial test of decision-making quality.

An individual's decisions may violate GARP and thus be of low decision-making quality for a number of reasons. First, violations of GARP can result from "trembles." Subjects may compute payoffs incorrectly, execute intended choices incorrectly, or err in other less obvious ways. Second, inconsistency can result from bounded rationality or cognitive biases such as "framing effects" and "mental accounting" (Kahneman and Tversky 1984). The resources needed for determining optimal choices are limited. Thus, especially in complex or unfamiliar environments, the cost of computing an optimal decision can be high. Some subjects may therefore adopt simple decision rules, and this "simplification" may cause their choices to be inconsistent.[4] Third, if the data $(\mathbf{p}^i, \mathbf{x}^i)$ satisfy GARP, then they can be rationalized by an outcome-based utility function $U(x_1, x_2)$. However, unobserved factors could enter the utility function so the "true" underlying preference ordering is represented by a utility function $U(x_1, x_2, \omega)$ parametrized by ω. If the data $(\mathbf{p}^i, \mathbf{x}^i)$ are generated by a utility function $U(x_1, x_2, \omega)$ and ω is fixed, then the data will still satisfy GARP. An example of this is the disappointment aversion model proposed by Gul (1991), where the safe allocation $x_1 = x_2$ is the reference point. If, however, ω is not fixed then subjects can exhibit "preference reversals," and the data $(\mathbf{p}^i, \mathbf{x}^i)$ might not satisfy GARP (cf. Bernheim and Rangel 2009 where ω may be time). The variable ω may also be interpreted, as in Kőszegi and Rabin (2006), as a *dynamic* reference point determined endogenously by the environment.

A. *Consistency with GARP*

Although testing conformity with GARP is conceptually straightforward, there is a difficulty: GARP provides an exact test of utility maximization—either the data satisfy GARP or they do not. We assess how nearly individual choice behavior complies with GARP by using the Afriat (1972) Critical Cost Efficiency Index (CCEI), which measures the fraction by which all budget constraints must be shifted in order

[3] Without loss of generality, assume the individual's payout is normalized to 1. The budget line is then $p_1x_1 + p_2x_2 = 1$, and the individual can choose any allocation $\mathbf{x}$ that satisfies this constraint. Let $(\mathbf{p}^i, \mathbf{x}^i)$ be the data generated by an individual's choices, where $\mathbf{p}^i$ denotes the ith observation of the price vector and $\mathbf{x}^i$ denotes the associated allocation. An allocation $\mathbf{x}^i$ is directly revealed preferred to $\mathbf{x}^j$ if $\mathbf{p}^i\mathbf{x}^i \geq \mathbf{p}^i\mathbf{x}^j$. An allocation $\mathbf{x}^i$ is revealed preferred to $\mathbf{x}^j$ if there exists a sequence of allocations $\{\mathbf{x}^k\}_{k=1}^K$ with $\mathbf{x}^1 = \mathbf{x}^i$ and $\mathbf{x}^K = \mathbf{x}^j$, such that $\mathbf{x}^k$ is directly revealed preferred to $\mathbf{x}^{k+1}$ for every $k = 1, \dots, K-1$.

[4] Consistency is also endogenous: subjects can make decisions that are consistent with GARP in a complex decision problem because they adopt simple choice rules to cope with complexity. But in that case, the "revealed" preference ordering may not be the "true" underlying preference ordering.

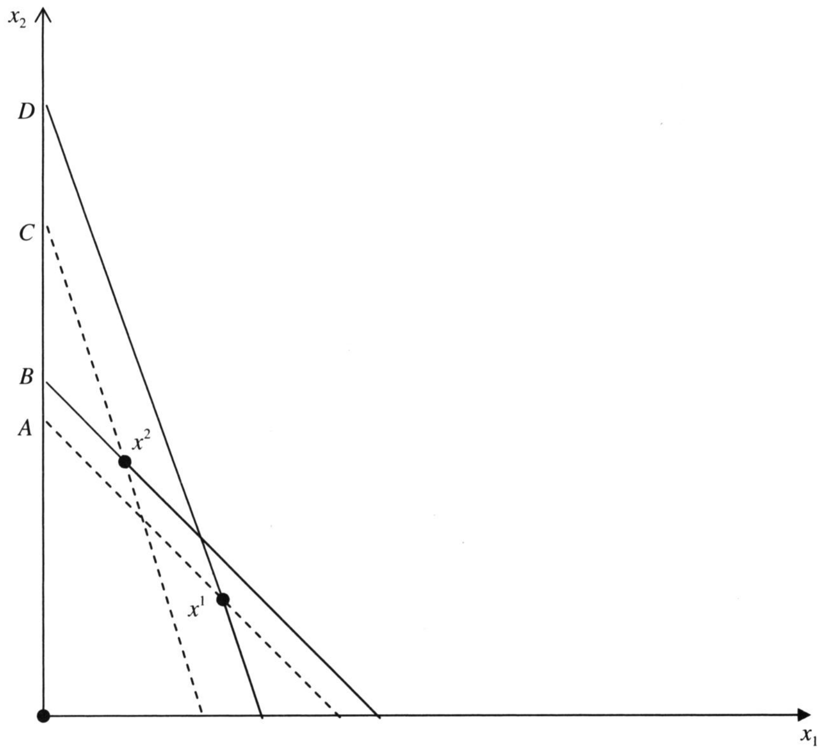

FIGURE 2. THE CONSTRUCTION OF THE CCEI FOR A SIMPLE VIOLATION OF GARP

Notes: Here we have a violation of the Weak Axiom of Revealed Preference (WARP) since x^1 is directly revealed preferred to x^2 and x^2 is directly revealed preferred to x^1. A perturbation $A/B < C/D$ of the budget line through allocation x^1 removes the violation.

to remove *all* violations of GARP. Put precisely, for any number $0 \leq e \leq 1$, define the direct revealed preference relation

$$\mathbf{x}^i R^D(e)\mathbf{x}^j \Leftrightarrow e\mathbf{p}^i \cdot \mathbf{x}^i \geq \mathbf{p}^i \cdot \mathbf{x}^j,$$

and define $R(e)$ to be the transitive closure of $R^D(e)$. Let e^* be the largest value of e such that the relation $R(e)$ satisfies GARP. The CCEI is the e^* associated with the dataset.

Figure 2 illustrates the construction of the CCEI for a simple violation of the GARP involving two allocations, $\mathbf{x}^1$ and $\mathbf{x}^2$. If we shift the budget line through $\mathbf{x}^1$ as shown $(A/B < C/D)$ the violation would be removed so the CCEI score associated with this violation is A/B. By definition, the CCEI is between zero and one—the closer the CCEI is to one, the smaller the perturbation of the budget constraints required to remove all violations and the closer the data are to satisfying GARP. The CCEI thus provides a summary statistic of the overall consistency with GARP, reflecting the minimum adjustment required to eliminate all violations of GARP associated with the dataset.

In our experiment, the CCEI scores averaged 0.881, which implies that on average budget lines needed to be reduced by about 12 percent to eliminate a subject's

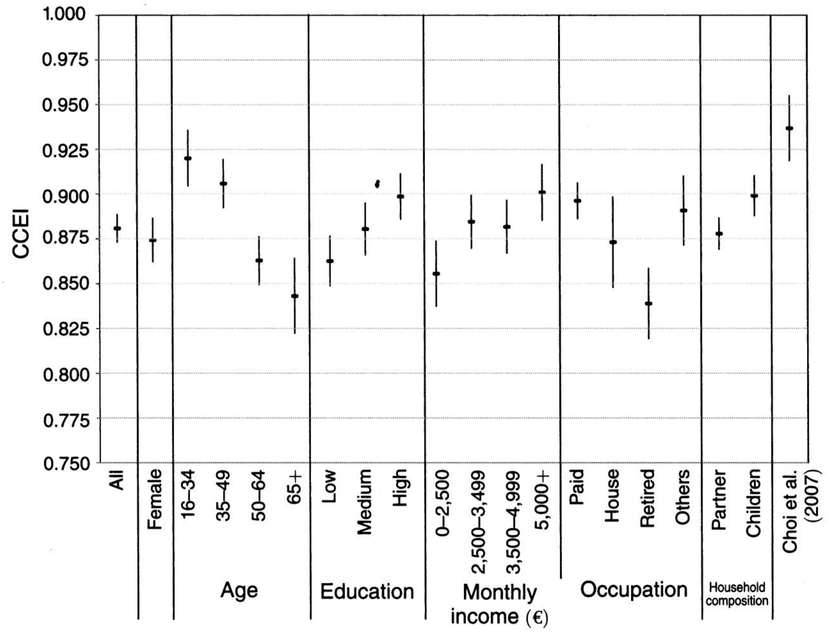

FIGURE 3. MEAN CCEI SCORES

GARP violations.[5] There is also marked heterogeneity in the CCEI scores within and across socioeconomic groups. Figure 3 summarizes the mean CCEI scores and 95 percent confidence intervals across selected socioeconomic categories. On average, high-income and high-education subjects display greater levels of consistency than lower-income and lower-education subjects. Men are more consistent than women, and young subjects tend more toward utility maximization than those who are old. The magnitudes imply that, in order to eliminate their GARP violations, low-income subjects require an average contraction of their budgets that is 3.3 percentage points larger than that of high-income subjects. The corresponding numbers for low-education subjects, females, and old subjects are 2.6, 2.4, and 5.1, respectively.[6] We will further analyze the relationship between consistency scores and socioeconomic characteristics, and address sample selection, in our regression analysis below.

A key advantage of the CCEI is its tight connection to economic theory. This connection makes the CCEI economically quantifiable and interpretable. Moreover, the same economic theory that inspires the measure also tells us when we have enough data to make it statistically useful. Thus, this theoretically grounded measure of decision-making quality helps us design and interpret the experiments in several

[5] In contrast, Choi et al. (2007) report that 60 of their 93 subjects (64.6 percent) had CCEI scores above the 0.95 threshold and that over all subjects the CCEI scores averaged 0.954 (see Figure 1). The subjects of Choi et al. (2007) were undergraduate students and staff at UC Berkeley, and the experiment was conducted in the laboratory. We note that the subjects of Choi et al. (2007) were given a menu of 50 budget sets which provides a more stringent test of GARP.

[6] To allow for small trembles resulting from the slight imprecision of subjects' handling of the mouse, our consistency results allow for a narrow confidence interval of one point (that is, for any i and $j \neq i$, if $|x^i - x^j| \leq 1$ then x^i and x^j are treated as the same portfolio).

ways. In online Appendix II, we provide more details on testing for consistency with GARP, discuss the power of the revealed preference tests, explain other indices that have been proposed for this purpose, and describe the related empirical literature on revealed preferences. In reporting our results, we focus on the CCEI. The results based on alternative indices are presented in online Appendix III. In practice, all indices yield similar conclusions.

B. *Beyond Consistency*

Stochastic Dominance.—Consistency with GARP requires consistent preferences over all possible alternatives, but any consistent preference ordering is admissible. In this way, we see consistency as a necessary, but not sufficient, condition for choices to be considered of high decision-making quality. Indeed, choices can be consistent with GARP and yet fail to be reconciled with any utility function that is normatively appealing. For example, consider an individual who always allocates all points to the same account as measured by x_1. This behavior is consistent with maximizing the utility function $U(x_1, x_2) = x_1$ and would generate a CCEI score of one.

Such preferences are, however, hard to justify in this setting because for many of the budget lines that a subject faces, always allocating all points to the same account means allocating all points to the more expensive account, a violation of monotonicity with respect to first-order stochastic dominance. Violations of stochastic dominance may reasonably be regarded as errors, regardless of risk attitudes—that is, as a failure to recognize that some allocations yield payoff distributions with unambiguously lower returns. Stochastic dominance is, thus, a compelling criterion for decision-making quality and is generally accepted in decision theory (Quiggin 1990, and Wakker 1993).

To provide a unified measure of the extent of GARP violations *and* violations of stochastic dominance (for a given subject), we combine the actual data from the experiment and the *mirror image* of these data obtained by reversing the prices and the associated allocation for each observation. That is, we assume that if (x_1, x_2) is chosen subject to the budget constraint $p_1x_1 + p_2x_2 = 1$, then (x_2, x_1) would have been chosen subject to the mirror-image budget constraint $p_2x_1 + p_1x_2 = 1$. We then compute the CCEI for this combined dataset and compare that number to the CCEI for the actual data. The CCEI score for the combined data consisting of 50 observations can be no bigger than the CCEI score for the actual data.[7]

For example, recall the preferences represented by $U(x_1, x_2) = x_1$, where the individual always allocates all points to x_1. Such choices are perfectly consistent with GARP, but they generate severe violations when combined with their mirror images. Indeed, as Figure 4 illustrates, any decision to allocate *fewer* points to the cheaper account will generate a violation of GARP involving the mirror image of this decision. Hence, the CCEI score for the combined data provides a summary statistic of the overall consistency with GARP *and* first-order stochastic dominance.

[7] Put precisely, the data generated by an individual's choices are $\{(p_1^i, p_2^i, x_1^i, x_2^i)\}_{i=1}^{25}$, and the mirror-image data are $\{(p_2^i, p_1^i, x_2^i, x_1^i)\}_{i=1}^{25}$.

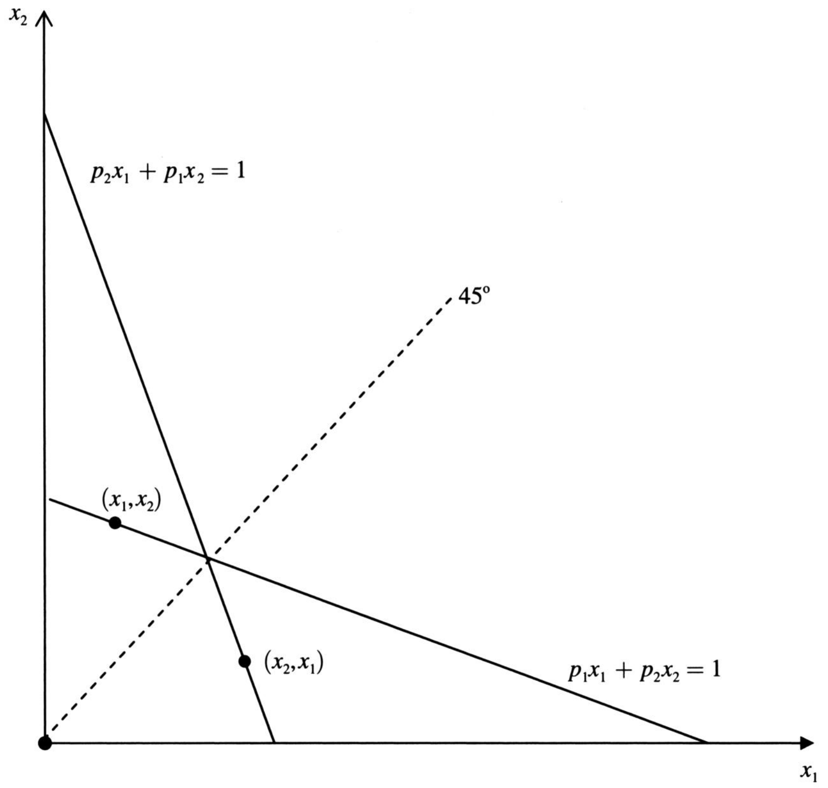

FIGURE 4. A VIOLATION OF GARP INVOLVING THE MIRROR-IMAGE BUDGET LINE

Notes: An individual choosing allocation (x_1, x_2) subject to budget constraint $p_1x_1 + p_2x_2 = 1$ violates first-order stochastic dominance. This decision generates a violation of the Weak Axiom of Revealed Preference involving the mirror-image decision of choosing (x_1, x_2) subject to budget constraint $p_2x_1 + p_1x_2 = 1$.

On average, the CCEI score is 0.733 for the combined data compared to 0.881 for the actual data.[8] In our econometric analysis below, we use both the CCEI scores for the actual dataset and for the combined dataset. In online Appendix IV, we assess how closely individual choice behavior complies *only* with first-order stochastic dominance using an alternative measure based on expected payoff calculations. Replacing the CCEI score for the combined data with this measure of stochastic dominance in the econometric analysis below yields similar conclusions.

Risk Attitudes.—Our experimental task delivers measures of both decision-making quality and (risk) preferences from a single realm of decision-making. We summarize an individual's attitudes toward risk with a single statistic: the fraction of total points he allocated to the cheaper account. We choose this measure because in each problem that a subject faces, each account is equally likely to be chosen, and the budget line is drawn from a symmetric distribution. Thus, the only behavior consistent with infinite risk aversion is always allocating the points equally between

[8] Of the 1,182 subjects in the experiment, only 29 subjects (2.5 percent) almost always allocated all points to one of the accounts by choosing the same endpoint of the budget line.

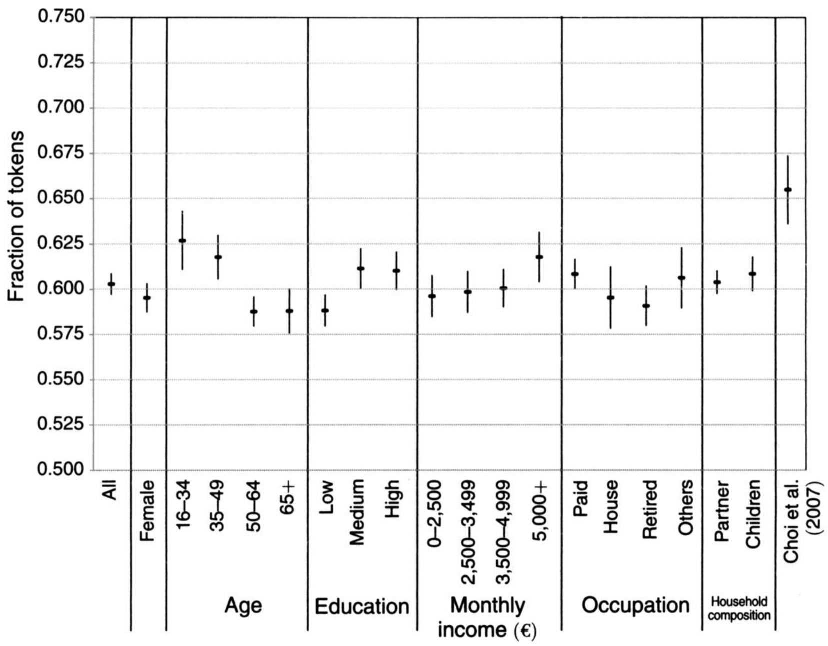

Figure 5. The Average Fraction of Tokens Allocated to the Cheaper Asset

the two accounts. Conversely, always allocating all points to the cheaper account is the behavior that would be implied by risk neutrality. More generally, subjects who are less averse to risk will allocate a larger fraction of points to the cheaper account. Like the revealed preference tests, an advantage of this measure is that it is nonparametric. It measures attitudes toward risk without making assumptions about the parametric form of the underlying utility function.[9] Figure 5 displays the mean fraction of points allocated to the cheaper account and 95 percent confidence intervals across the socioeconomic categories. We note that there is considerable heterogeneity in risk attitudes across categories, which is characteristic of all these data, and that risk attitudes and CCEI scores are effectively uncorrelated ($\rho = 0.113$).[10,11]

III. Decision-Making Quality and Socioeconomics

We next perform an analysis of the correlation between decision-making quality—the consistency of the experimental data with GARP—and socioeconomic

[9] In parametric estimation, beyond the scope of this article, we find that the choice data of many subjects are well explained by a preference ordering in which the indifference curves have a kink at the 45-degree line, corresponding to an allocation with a certain payoff. One interpretation of this preference ordering is the disappointment aversion model proposed by Gul (1991). This finding corroborates the results in Choi et al. (2007) with undergraduate students.

[10] As Figure 5 shows, our individual-level measures of risk aversion are higher than the measures reported in Choi et al. (2007), but they are within the range of estimates from recent studies (see Choi et al. 2007 for a discussion of these studies).

[11] The seminal paper by Holt and Laury (2002) also reports substantial low-stakes risk aversion in the lab, as does Andersen et al. (2006) in the field. Risk aversion over moderate stakes contradicts the validity of Expected Utility over wealth (Rabin 2000, and Rabin and Thaler 2001). We emphasize that GARP does not imply the Savage (1954) axioms on which Expected Utility is based, and Expected Utility need not be assumed to investigate the decision-making quality of choice under uncertainty.

characteristics. Table 2 below presents the results of the econometric analysis. In column 1, we present estimates with the CCEI scores for the actual dataset using ordinary least squares.[12] The results show significant correlations. We obtain statistically significant coefficients in nearly all socioeconomic categories, ranging in absolute values from about 0.025 to just over 0.050. Most notably, female, low-education, low-income, and older subjects on average "waste" as much as 2.4, 2.6, 3.3, and 5.1 percentage points more of their earnings, respectively, by making inconsistent choices. In column 2 we repeat the estimation reported in column 1 using the CCEI scores for the combined dataset. The two scores are highly correlated ($\rho = 0.645$) and, unsurprisingly, the results are qualitatively similar.

The preceding analysis is based on the nonrandomly selected subsample of participants. The lack of observations on panel members who chose not to participate or did not complete the experiment creates a missing data problem. We evaluate sample selection bias in our econometric analysis using the Heckman (1979) method. Our exclusion restriction involves the number of completed CentERpanel questionnaires out of the total invitations to participate in the three months prior to our experiment. This variable enters the participation equation but, we assume, is conditionally uncorrelated with the CCEI (see Bellemare, Kröger, and van Soest 2008). To economize on space, the estimation results are reported in online Appendix V. The estimated parameters from the OLS and the sample selection estimations are virtually identical. We interpret these results to indicate that self-selection is not importantly driving the results.

IV. Wealth Differentials and Decision-Making Ability

The preceding analysis investigates the correlation between the consistency scores and the sociodemographic characteristics of subjects. This analysis makes no effort to evaluate a causal interpretation of these relationships. Nevertheless, the higher CCEI scores among high-income, high-education, and younger subjects suggest that these groups may have better economic outcomes, not only because they face fewer constraints or have more normative preferences, but also because they tend to have superior decision-making ability.

We next evaluate a causal interpretation of the correlation between the CCEI scores and important economic outcomes in the real world. If heterogeneity in decision-making ability is an important source of heterogeneity in economic outcomes, and if decision-making quality in the experiment as measured by the CCEI is a good proxy for decision-making ability, then differences in the CCEI scores across subjects should independently and robustly explain differences in their real-world outcomes.

We focus on household wealth as the real-world economic outcome of interest. As we argued, the investigation of wealth offers a strong test of the idea that decision-making quality in the experiment captures decision-making ability. Wealth is also of special interest because of a debate about the sources of its dispersion. Bernheim,

[12]To test for a potential misspecification, we used the Ramsey (1969) RESET test by adding the squared and cubed fitted values of the regression equation as additional regressors and found no evidence of misspecification (p-value $= 0.3098$).

Table 2—The Correlation between CCEI Scores and Subjects' Individual Characteristics (OLS)

	(1)	(2)
Constant	0.887***	0.735***
	(0.022)	(0.037)
Female	−0.024***	−0.011
	(0.009)	(0.015)
Age		
35–49	−0.016	−0.007
	(0.011)	(0.020)
50–64	−0.052***	−0.077***
	(0.011)	(0.020)
65+	−0.051**	−0.081**
	(0.020)	(0.032)
Education		
Medium	0.009	0.021
	(0.011)	(0.017)
High	0.026**	0.060***
	(0.011)	(0.018)
Income		
€2,500–3,499	0.026**	0.026
	(0.012)	(0.019)
€3,500–4,999	0.020	0.006
	(0.013)	(0.020)
€5,000+	0.033**	0.017
	(0.014)	(0.022)
Occupation		
Paid work	0.028	0.03
	(0.018)	(0.026)
House work	0.047**	0.039
	(0.021)	(0.030)
Others	0.037*	0.035
	(0.019)	(0.030)
Household composition		
Partner	−0.026**	−0.023
	(0.011)	(0.018)
Number of children	0.001	0.001
	(0.004)	(0.007)
R^2	0.068	0.058
Observations	1,182	1,182

Notes: Omitted categories: male, age under 35, low education (primary and lower secondary education), household gross monthly income under €2,500, retired, and not having a partner. Since the CCEI is a number between zero and one, we repeated the estimations reported in columns 1 and 2 using a fractional regression model (Papke and Wooldridge 1996). The two specifications yield similar results. Standard errors in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Skinner, and Weinberg (2001) and Ameriks, Caplin, and Leahy (2003) show substantial differences in wealth even among households with very similar lifetime incomes and provide evidence that differences in decision-making ability drive wealth differentials. Scholz, Seshadri, and Khitatrakun (2006) counter this by showing that, with detailed data on household-specific earnings, a standard life-cycle model with homogeneous preferences can account for more than 80 percent of the cross-sectional variation in wealth.

Our analysis of wealth proceeds in four steps. We first establish the correlation between the CCEI and household wealth by estimating regressions of the log of household wealth on socioeconomic variables (including a flexible function of age), the log of household contemporaneous income, and the consistency score of the person who is primarily responsible for household financial matters. Importantly, we follow a well-established tradition in life-cycle analysis and treat income, education, and family structure as predetermined. We also evaluate the robustness of the conditional correlation between the CCEI and wealth to changes in the functional form of the estimating wealth equation.

Second, we demonstrate that this correlation is quantitatively robust to the inclusion of additional controls for unobserved constraints, preferences, and beliefs. If these unobservables were important sources of the observed correlation, then adding the controls should have a substantial effect on the estimated correlation between the CCEI and wealth. This analysis does not seek the impossible goal of "controlling for everything" that might influence wealth. Instead, in the spirit of Altonji, Elder, and Taber (2005), we examine whether the conditional correlation we see between CCEI and wealth in the basic specifications is much affected by the inclusion of additional controls that, a priori, should be correlated with wealth through their correlation with unobserved or misspecified variables.[13]

Third, we show that alternative measures of decision-making quality (from the experiment) and decision-making ability (from the CentERpanel survey) are not substitutes for the CCEI. The available alternatives either have no independent power to predict wealth, or are not well-correlated with consistency with GARP. Last, to account for the sources of the correlation and to better understand which real-world decisions cause those with higher CCEI scores to accumulate more wealth, we investigate the relationship between the CCEI and the details of household saving allocations.

A. *Establishing the Correlation between CCEI and Wealth*

The CentERpanel collects information about wealth on an annual basis. Panel members are asked to identify a financial respondent who is "most involved with the financial administration of the household." All panel members age 16 and older respond to questions about the assets and liabilities that they hold alone. The financial respondent also provides information about assets and liabilities that are jointly held by more than one household member. The inventory covers checking and saving accounts, stocks, bonds and other financial assets, real estate, business assets, mortgages, loans, and lines of credit. Our analysis focuses on nonpension household wealth, calculated by summing net worth over household members

[13]The reference to Altonji, Elder, and Taber (2005) underscores an analogy to the literature on education. Economists have long struggled to identify and interpret the various effects of education. The principal difficulty is that exogenous variation in education is rare and limited in scope. But the topic is so important that it justifies many approaches to inference and interpretation. We view our article in a similar vein, though, in the case of decision-making quality, both the empirical and the theoretical literatures are much less mature. Even the unconditional correlations presented above were previously unknown.

and taking the household's average over 2008 and 2009. The 703 households with wealth data and a CCEI score from the household's financial respondent had an average household wealth of €164,130. Percentile values (in thousands of euros) are provided below.[14]

Min	1	5	10	25	50	75	90	95	Max
−180.7	−68.2	−4.8	0.0	10.8	93.0	242.1	523.8	955.6	3,984.2

In our baseline specification, the sample size drops from 703 to 517 households (73.5 percent). This decline derives largely from three sources. First, 54 households (7.7 percent) have negative or missing household income in 2008, and 74 households (10.5 percent) have negative wealth and, thus, a missing dependent variable. Second, younger households face incentives to hold less wealth as they borrow in order to invest or to smooth lifetime consumption. With that in mind, we drop the 49 households (7.0 percent) whose financial respondent is less than 35 years old. Finally, to reduce the importance of extreme outliers, we drop the seven households that represent the top and bottom half of 1 percent of the wealth distribution and the bottom half of 1 percent of the CCEI distribution. Two additional households are dropped due to missing data on education. Our basic estimation results are reported in Table 3.

Baseline.—In column 1, we present estimates from our baseline specification using the sample of 517 households described above. The point estimate of 1.35 for the coefficient on the CCEI indicates that a standard deviation increase in the CCEI score of the household's financial respondent is associated with 18 percent more household wealth. As one might expect from a relatively small sample of data on self-reported wealth, the standard error on this point estimate is fairly large. Nevertheless, we can reject a null hypothesis of no relationship at the 5 percent level (p-value $= 0.017$) with standard errors robust to heteroskedasticity.[15]

Life Cycle.—In column 2, we repeat the estimation reported in column 1 with the sample not restricted to households with financial respondents who are at least 35 years old. Using the entire analysis sample, we find that the point estimate on the CCEI is somewhat smaller, so a standard deviation increase in the CCEI score of the household's financial respondent is associated with about 15 percent more household wealth. The standard error on this point estimate implies that we can reject a null hypothesis of no relationship with considerable confidence (p-value $= 0.038$), but

[14]The CentERpanel data do not include information on pension wealth. Nearly all of the Dutch population is covered by the public pension system, whose benefits are a relatively simple function of family structure. A large majority of workers is also covered by private pensions associated with their employment. Nearly all of these employment-based plans are defined benefit, the vast majority of which pay benefits as a function of earnings. Conditioning on family structure and earnings should, therefore, do much to control for the incentives these pensions create for nonpension wealth accumulation. See Alessie and Kapteyn (2001) and OECD (2009) for details about the pension systems in the Netherlands. While it is a necessity, studying nonpension wealth has the advantage of better isolating discretionary wealth accumulation.

[15]The individual coefficients on age, education, and occupation are economically large, but not statistically distinguishable from zero at standard levels of significance. A test of their joint significance, however, rejects a null hypothesis of no relationship with a p-value of 0.0002.

TABLE 3—THE RELATIONSHIP BETWEEN CCEI SCORES AND WEALTH

	(1)	(2)	(3)
CCEI	1.351**	1.109**	101,888.0*
	(0.566)	(0.534)	(52,691.9)
log 2008 household income	0.584***	0.606***	
	(0.132)	(0.126)	
2008 household income			1.776***
			(0.4)
Female	−0.313*	−0.356**	−32,484.3*
	(0.177)	(0.164)	(17,523.9)
Partnered	0.652***	0.595***	46,201.9***
	(0.181)	(0.171)	(17,173.7)
Number of children	0.090	0.109	14,078.6*
	(0.093)	(0.086)	(8,351.5)
Age	−0.303	−0.008	−19,148.5
	(0.347)	(0.208)	(30,164.4)
Age^2	0.007	0.002	468.7
	(0.006)	(0.004)	(523.6)
Age^3	0.000	0.000	−2.9
	(0.000)	(0.000)	(2.9)
Education			
Prevocational	0.269	0.245	14,137.4
	(0.464)	(0.462)	(43,449.1)
Preuniversity	0.634	0.562	59,035.0
	(0.478)	(0.476)	(44,746.1)
Senior vocational training	0.416	0.421	28,318.7
	(0.474)	(0.468)	(42,419.2)
Vocational college	0.490	0.527	31,341.2
	(0.451)	(0.449)	(42,046.8)
University	0.725	0.685	77,578.8
	(0.473)	(0.465)	(47,709.4)
Occupation			
Paid work	0.206	0.226	−12,657.2
	(0.322)	(0.321)	(26,597.8)
House work	0.552	0.603	16,876.8
	(0.406)	(0.413)	(31,114.3)
Retired	0.131	0.190	16,753.1
	(0.318)	(0.318)	(35,165.2)
Constant	6.292	0.469	76,214.4
	(6.419)	(3.598)	(559,677.5)
R^2	0.179	0.217	0.188
Observations	517	566	568

Notes: The groupings of different levels of education are based on the categorization of Statistics Netherlands (Centraal Bureau voor de Statistiek). For a complete description see http://www.centerdata.nl/en/centerpanel. Standard errors in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

we cannot reject a null hypothesis that the point estimates of the coefficient on the CCEI reported in columns 1 and 2 are the same.

Levels.—The log specification in columns 1 and 2 excludes households with negative wealth and may also cause small differences at positive but very low levels of wealth to have large effects on point estimates. To evaluate the sensitivity of the results to the log specification, in column 3 we estimate the regression in levels (of wealth and income) for the sample age 35 and older. We again see an economically large correlation between the CCEI and levels of wealth, though this relationship is estimated somewhat less precisely; the coefficient on the CCEI is significant only at the 10 percent level (p-value $= 0.054$).

B. *Evaluating a Causal Interpretation*

We find an economically large and statistically significant correlation between the financial respondent's CCEI score in the experiment and household wealth. This lends a basic level of support to the idea that our measure of decision-making quality from the experiment can proxy for decision-making ability that applies across multiple real world choice domains. The correlation between the CCEI and wealth may, however, not reflect decision-making ability, but instead be due to a correlation between the CCEI and other standard, but so far unobserved or misspecified, sources of heterogeneity in choice that affect wealth. To evaluate a causal relationship, we study the robustness of the correlation with respect to the inclusion of additional controls for unobserved constraints, preferences, and beliefs. Tables 4 and 5 present the estimates of the coefficients of interest. The full-length tables are contained in online Appendix VI.

Constraints.—We begin by investigating the correlation of the CCEI with unobserved or misspecified constraints that affect the accumulation of wealth. The estimation results are reported in Table 4. In standard life-cycle models, wealth at a given age is a function of the constraints imposed by the path of income over a lifetime. There is a variety of reasonable specifications for a wealth regression, and our baseline specification reported in Table 3 above is an especially simple benchmark. In column 1 of Table 4, we assess the importance of the baseline linear-in-contemporaneous-income specification by allowing income to enter in the form of a cubic. We see virtually no change in the point estimate of the coefficient on the CCEI. We thus find no evidence that the simple specification of contemporaneous income drives the estimated relationship between the CCEI and wealth.

Another concern is that contemporaneous income is measured with error, and the estimated coefficient on this variable is therefore biased toward zero. The bias of this estimate then biases estimates of the coefficients on other variables, including the CCEI. Standard life-cycle models predict constant saving rates across lifetime income groups, and thus a unit elasticity of wealth with respect to income (Dynan, Skinner, and Zeldes 2004). If contemporaneous income is a good proxy for lifetime income, then these theories predict the coefficient on the log of contemporaneous income should equal one in the baseline specification. In column 2 of Table 4, we impose this restriction and see virtually no change in the point estimate of the coefficient on the CCEI.[16] There is, thus, no evidence that measurement error in contemporaneous income drives the main result.

[16] Brown (1976) shows that if theory makes a prediction about the coefficient on a variable that is measured with error then restricting that coefficient to take on the value predicted by theory will reduce the bias on the other coefficients being estimated.

Table 4—The Robustness of the Correlation between CCEI Scores and Wealth to the Inclusion of Controls for Unobserved Constraints

	(1)	(2)	(3)	(4)	(5)
CCEI	1.322**	1.318**	1.925***	1.888***	1.441**
	(0.570)	(0.574)	(0.672)	(0.652)	(0.578)
log household income					
2008	19.770	1.000	0.544***	0.285*	0.616***
	(14.629)		(0.137)	(0.165)	(0.128)
2008^2	−2.194				
	(1.533)				
2008^3	0.082				
	(0.053)				
2006				0.232	
				(0.231)	
2004				0.215	
				(0.174)	
Demographics	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	No
Occupation	Yes	Yes	Yes	Yes	Yes
Constant	−47.059	0.864	5.354	3.016	6.398
	(46.275)	(6.545)	(6.93)	(7.109)	(6.484)
R^2	0.187		0.205	0.217	0.177
Observations	517	517	449	449	517

Notes: Standard errors in parentheses.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

A related concern is that contemporaneous income is a poor proxy for the *path* of lifetime income, and unobserved aspects of that path are correlated with the CCEI. We can evaluate this concern with some of the limited panel data available on household income. To strike a balance between capturing more income information and maintaining reasonable sample sizes, we go back five years and use household income information for every other year.[17] In column 3 of Table 4, we repeat the baseline specification reported in column 1 of Table 3, this time restricting attention to the 449 households (86.8 percent) for whom we have household income data from 2004 and 2006, as well as from 2008. In this smaller sample, the point estimate on the CCEI remains economically large and statistically different from zero (p-value $= 0.004$). In column 4 of Table 4, we add controls for the log of household income in 2004 and 2006. As a result, the magnitude of the coefficient on the CCEI declines only slightly (by 0.037). We interpret this to indicate that, while some of the correlation between the CCEI and wealth may be attributable to a correlation between the CCEI and unobserved past income, the available CentERpanel data on income provide little evidence that this is the case.

[17] The CentERpanel has been operating since 1993. However, income data for most households who responded to the 2009 survey and completed our experiment do not go back nearly that far. In cases where we have two out of the three income measures, we use linear extrapolation to fill in the third.

An alternative approach is to take completed education as a proxy for lifetime income. All of our specifications so far include indicators for each level of education completed. It may be, however, that unobserved aspects of education, such as the quality of schooling, are correlated with unobserved elements of lifetime income which are, in turn, correlated with the CCEI score in the experiment. If so, and if these unobserved constraints are important sources of the observed correlation between CCEI and wealth, then conditioning on completed education should have a substantial effect on the estimated coefficient on the CCEI. In column 5 of Table 4, we repeat the baseline specification reported in column 1 of Table 3 after omitting the controls for the education of the financial respondent. Comparing the estimates from these two specifications, we see that removing the education controls increases the estimated coefficient on the CCEI only modestly (by 0.090). In this way, we find little evidence that unobserved aspects of education are driving the correlation between the CCEI and wealth.

Preferences.—We have evaluated whether the correlation between the CCEI and wealth is due to a relationship between the CCEI and unobserved or misspecified income constraints. The results on education, which we took to proxy for unobserved income but could also proxy for attitudes toward risk and time, suggest that the relationship between the CCEI and unobserved preferences is unlikely to play an important role. Nevertheless, we next turn to analyze the possibility that the correlation between the CCEI and wealth is driven by a relationship between the CCEI and unobserved preferences that determine wealth. The estimation results are reported in Table 5 below.

Our experimental task delivers individual-level measures of both decision-making quality and risk preferences from a single realm of decision-making. In column 1 of Table 5 we add to the baseline specification reported in column 1 of Table 3 a control for risk attitudes by including a nonparametric measure from the experiment discussed above—the average fraction of points the financial respondent allocated to the cheaper account.[18] The point estimate on this *quantitative* measure indicates that risk tolerance in the experiment is negatively associated with wealth. The coefficient is economically large—a standard deviation increase in the fraction placed in the cheaper account is associated with about 7 percent less household wealth—but imprecisely estimated. We cannot reject a null hypothesis of no relationship (p-value $= 0.282$) or a null of an economically large and positive relationship. Given that risk attitudes and CCEI scores in the experiment are effectively uncorrelated ($\rho = 0.113$), it is unsurprising that the inclusion of this control leaves the point estimate of the coefficient on the CCEI little changed.

It may be, however, that risk attitudes that influence wealth are not well correlated with risk preferences revealed over the small stakes of the experiment. If so, *qualitative* measures of risk tolerance taken from the CentERpanel survey instead of the experiment may do better. In column 2, we evaluate this possibility by also

[18] To avoid the influence of extreme outliers, and to place this variable on more equal footing with the CCEI, we also estimated a specification that omitted the top and bottom half of 1 percent of the distribution of this risk attitude measure, a total of six households. The results are qualitatively similar.

TABLE 5—THE ROBUSTNESS OF THE CORRELATION BETWEEN CCEI SCORES AND WEALTH TO THE INCLUSION OF CONTROLS FOR UNOBSERVED PREFERENCES AND BELIEFS

	(1)	(2)	(3)	(4)	(5)
CCEI	1.379**	1.396**	1.404**	1.214*	1.237**
	(0.568)	(0.568)	(0.569)	(0.625)	(0.623)
Risk tolerance					
Quantitative (experiment)	−0.768	−0.808	−0.766		
	(0.714)	(0.711)	(0.718)		
Qualitative (survey)		0.017	0.023		
		(0.074)	(0.076)		
Qualitative (survey) missing		−0.190	−0.162		
		(0.335)	(0.482)		
Conscientiousness			0.089		
			(0.072)		
Conscientiousness missing			−0.040		
			(0.668)		
Longevity expectations					−0.034
					(0.040)
log 2008 household income	0.589***	0.578***	0.572***	0.443***	0.434***
	(0.132)	(0.131)	(0.133)	(0.123)	(0.123)
Demographics	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes	Yes
Constant	6.840	6.883	6.496	3.777	4.411
	(6.361)	(6.357)	(6.395)	(15.258)	(15.256)
R^2	0.179	0.176	0.176	0.163	0.163
Observations	517	517	517	414	414

Notes: Risk aversion in the experiment is measured by the average fraction of tokens allocated to the cheaper asset. Standard errors in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

including a normalized measure of risk-taking in investments.[19] To preserve sample size, we also include a variable to indicate whether the respondent provided a complete answer to these questions. The results reinforce those from the previous specification. The point estimate of the coefficient on the qualitative risk tolerance measure is economically small but imprecisely estimated. As important, the inclusion of a qualitative measure of risk attitudes leaves the estimated coefficient on the CCEI virtually unchanged. We thus find no evidence that these qualitative measures of risk attitudes from the survey are better able to capture unobserved preferences, correlated with the CCEI, that influence wealth.

In a final effort to evaluate the importance of a correlation between the CCEI and unobserved preferences, in column 3 we add to the list of controls a conscientiousness

[19] The CentERpanel survey contains six statements related to investment risk and return such as "I think it is more important to have safe investments and guaranteed returns, than to take a risk to have a chance to get the highest possible returns," and "I want to be certain my investments are safe." Respondents are asked to evaluate the accuracy of these statements as descriptions of themselves on a seven point scale. We sum the responses for each respondent, when necessary reordering them so that higher scores reflect greater risk tolerance. We then normalize the scores to have sample mean 0 and standard deviation 1.

measure from the "Big Five" test used for personality research in psychology.[20] Again, to preserve sample size, we also include a variable to indicate whether the respondent completed the conscientiousness questions. If not, we set his score to the sample mean (zero). The magnitude of the coefficient on conscientiousness is large. A standard-deviation increase in conscientiousness is associated with about 9 percent more wealth. The standard error on the conscientiousness coefficient is also relatively large, however, and we cannot reject a null hypothesis of no correlation. Most important, adding the control for conscientiousness has almost no effect on the coefficient on the CCEI. Thus, we again find no evidence that the relationship between the CCEI and wealth is driven by a correlation between the CCEI and unobserved preferences that influence wealth.

Beliefs.—Standard life-cycle models predict that beliefs, such as expectations for longevity, income, or asset returns, should affect household wealth levels. The CentERpanel collects relatively little information about respondents' beliefs, but the survey does ask questions about expected longevity. We can therefore use these data to evaluate the extent to which the correlation between the CCEI and wealth accumulation is attributable to a correlation between the CCEI and some unobserved beliefs that influence wealth. To strike a balance between capturing more information and maintaining the sample sizes, we consider a measure of longevity expectations based on the question answered by the largest number of respondents.[21]

In column 4 of Table 5, we repeat the baseline specification reported in column 1 of Table 3, this time restricting attention to the 414 households (80.0 percent) for whom the financial respondent answered this question about longevity. In this smaller sample, the point estimate of the coefficient on the CCEI remains economically large, though a larger standard error reduces the statistical significance (p-value $= 0.053$). In column 5 of Table 5, we add the control for longevity expectations. This measure, itself, has little power to predict wealth levels, and including it increases the estimate of the coefficient on the CCEI very slightly (by 0.023). Thus, while we have limited ability to explore this issue with available data, we find no evidence that a relationship between the CCEI and unobserved beliefs drives the correlation between the CCEI and wealth.

[20] The other Big Five personality traits are openness, extraversion, agreeableness, and neuroticism. Among the Big Five, conscientiousness has the strongest correlation with economic success (see, for examples, Barrick and Mount 1991, and Tett, Jackson, and Rothstein 1991). Conscientious people are described as "thorough, careful, reliable, organized, industrious, and self-controlled" (Duckworth et al. 2007). These terms suggest more patience, less risk tolerance, and less taste for leisure. Conscientiousness may thus proxy for unobserved preferences that influence wealth. The CentERpanel survey contains ten statements related to conscientiousness. The statements include: "I do chores right away," "I am accurate in my work," among others. Respondents are asked to evaluate the accuracy of these statements as descriptions of themselves on a five-point scale. For each respondent, we sum his or her responses to the ten statements, and then normalize the scores to have sample mean 0 and standard deviation 1. When necessary, we reordered the responses so that higher scores reflect greater conscientiousness. Simultaneously adding other measures from the Big Five yields the same conclusion. We omit those results for the sake of brevity.

[21] The question answered by the largest number of respondents asks "How likely is it that you will attain (at least) the age of 80?" Responses are recorded on a scale from 0 to 10, and respondents are instructed to interpret 0 to mean "no chance at all" and 10 to mean "absolutely certain."

TABLE 6—EVALUATING ALTERNATIVE MEASURES OF DECISION-MAKING QUALITY

	(1)	(2)	(3)	(4)
CCEI	1.253*	1.401*	1.269*	1.177**
	(0.712)	(0.729)	(0.729)	(0.583)
CCEI (combined dataset)	0.099			
	(0.380)			
von Gaudecker et al. (2011)			0.927*	
			(0.485)	
Cognitive Reflection Test				0.120*
				(0.071)
Cognitive Reflection Test missing				−0.203
				(0.237)
log 2008 household income	0.586***	0.388*	0.383*	0.577***
	(0.132)	(0.155)	(0.154)	(0.132)
Demographics	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes
Constant	6.237	10.056	8.355	6.855
	(6.424)	(6.976)	(6.990)	(6.464)
R^2	0.177	0.225	0.232	0.181
Observations	517	326	326	517

Notes: The CCEI scores for the combined dataset are computed after combining the actual data from the experiment and the mirror-image data. Standard errors in parentheses.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

C. *Evaluating Alternatives to the CCEI*

We have found an economically large, statistically significant, and quantitatively robust relationship between the CCEI scores in the experiment and wealth. We next evaluate whether alternative laboratory- and survey-based measures can substitute for the CCEI for the purposes of explaining wealth. Measuring decision-making quality by consistency with GARP has strong theoretical and methodological justifications, but augmenting GARP with additional, normative criteria might better capture decision-making quality. It is also possible that, while lacking in theoretical foundations, other proxies for decision-making ability are so well correlated with the CCEI that they may serve as substitutes for the CCEI. This would be especially useful if the other proxies are readily available on surveys or in administrative datasets. The estimation results are reported in Table 6. The full-length table is contained in online Appendix VI.

Stochastic Dominance.—We begin with consideration of a stronger notion of decision-making quality derived from our experiment. In column 1 of Table 6 we repeat the estimation of the baseline specification reported in column 1 of Table 3 after adding the CCEI scores for the combined dataset (combining the actual data from the experiment and the mirror-image data). As explained above, this test of decision-making quality is stronger because it demands both consistency with GARP and first-order stochastic dominance. We find no evidence that, conditional on the CCEI score from the actual data, the CCEI score for the combined dataset has an independent relationship with wealth. Adding the CCEI for the combined dataset as a regressor has only a modest effect on the point estimate of the coefficient on the CCEI, though the standard error on this estimate increases. The point estimate of the coefficient on the CCEI for the combined dataset is small but imprecisely estimated. These results are consistent with the idea that the CCEI for the combined dataset, while requiring a compelling and generally accepted notion of decision-making quality, merely represents a noisier measure of the aspects of decision-making ability captured by the CCEI scores for the actual dataset.

Trembling.—von Gaudecker, van Soest, and Wengström (2011) conducted risk experiments with CentERpanel members using a multiple price list design (Andersen et al. 2006). They estimated a flexible parametric model that includes an individual "trembling" parameter ω_i measuring "the propensity to choose randomly rather than on the basis of preferences." von Gaudecker, van Soest, and Wengström (2011) conclude that "while many people exhibit consistent choice patterns, some have very high error propensities." That parameter can be interpreted as a measure of decision-making quality as it captures the degree to which an individual's choices are consistent both with rationality and with some assumptions about the functional form of utility. This contrasts with the CCEI, which makes no assumptions about the structure of preferences. The CCEI and the "trembling" parameter of von Gaudecker, van Soest, and Wengström (2011) are only moderately correlated ($\rho = 0.178$) in the overlapping sample of 624 subjects (43.9 percent) who participated in both experiments. As a result, we can gain some insight into the relationship between wealth and consistency with the utility-maximizing model *versus* consistency with a class of utility functions commonly employed in the empirical studies.

In column 2 of Table 6, we repeat the estimation of the baseline specification reported in column 1 of Table 3, this time restricting attention to the 326 households (63.1 percent) with a financial respondent who participated in both experiments.[22] In column 3 of Table 6 we add to the list of regressors a variable equal to $(1 - \omega_i)$ in von Gaudecker, van Soest, and Wengström (2011). The point estimate of the coefficient on this parameter is large—a standard-deviation increase is associated with 17 percent more household wealth—and we can reject a null hypothesis of no relationship at the 10 percent confidence level (p-value $= 0.057$). The estimated coefficient on the CCEI is reduced somewhat when we include this estimate but remains economically large and statistically significant at the 10 percent confidence level (p-value $= 0.083$). There are many possible reasons for the differences in these two measures and their relationship to wealth—they are based on different methods, derived from different designs, and elicited in different experiments. Intriguingly, however, the results suggest substantial differences between decision-making quality measured by the restrictions imposed by the utility-maximization model and additional restrictions imposed by various hypotheses concerning functional structure. This is an interesting avenue for future work with more data on both measures.

Cognitive Ability.—Tests of cognitive ability (IQ) might also capture aspects of decision-making ability (cf. Dohmen et al. 2010). Different from IQ tests,

[22] To place the two measures on more equal footing, we trim the lowest one-half of 1 percent of the distribution of each of the measures (two observations each) in the overlapping sample.

consistency with GARP offers a theoretically disciplined measure for decision-making quality that has a well-established economic interpretation. There is no comparable, theoretically disciplined means of using and interpreting an IQ test. Nevertheless, if the CCEI and IQ were very well correlated, then analysts interested in measuring decision-making ability might be able to replace the revealed preference tests with one of the many IQ tests and, in some circumstances, the conceptual distinctions between the measures would have little practical import. It seems likely that the capacity to make choices of high decision-making quality draws on skills of analysis and perception that also improve IQ test scores. But if the goal is to isolate the influence of these skills on decision-making ability, rather than on constraints, information, or beliefs, are the CCEI and IQ scores substitutes?

Many IQ tests are precluded from wide use by intellectual property rights or are impossible to implement in Internet panels. The CentERpanel has not implemented any of the well-known and wide-ranging IQ instruments. However, in connection with our and other researchers' projects, the CentERpanel asked a sample of respondents to complete the Frederick (2005) Cognitive Reflection Test and a brief Raven's Progressive Matrices Test.[23] We omit the Raven's test because it generated effectively no variation in responses. Among the 467 subjects who completed the Cognitive Reflection Test and participated in our experiment, the correlation between the CCEI and the Cognitive Reflection Test is positive, but the variables are far from collinear ($\rho = 0.193$). This result echoes Burks et al. (2009) who find a correlation of approximately 0.22 between IQ and compliance with monotonicity (measured by more than one switch point in multiple price list experiments regarding risk and time trade-offs). To assess the predictive content of the CCEI and the Cognitive Reflection Test, in column 4 of Table 6 we add the number of questions answered correctly in the Cognitive Reflection Test to the baseline specification. To preserve sample size, we also include a variable to indicate whether a Cognitive Reflection Test score was available for the household's financial respondent. For those who had no score, we substitute the mean of the distribution of the rest of the sample.

The point estimate of the coefficient on the Cognitive Reflection Test is economically large—answering one more of the three questions on the Cognitive Reflection Test correctly is associated with about 12 percent more wealth—and statistically significant at the 10 percent level (p-value $= 0.094$). Adding this measure, and the missing indicator, to the basic specification reduces the estimated coefficient on the CCEI somewhat, but about a fifth of the reduction is due to the inclusion of the indicator for a missing Cognitive Reflection Test score. These results suggest that the Cognitive Reflection Test captures some decision-making ability related both to decision-making quality in the experiment and wealth accumulation. The findings also indicate, however, that this measure of cognitive ability cannot be used as a simple substitute for the CCEI for the purposes of explaining wealth.[24]

[23]The Cognitive Reflection Test consists of three questions. Each question is designed to have an intuitive, but incorrect, answer. The intuitive answer tends to spring to mind and then require reflection in order to dismiss. One question asks "A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?" Here the intuitive answer is $0.10, but the correct answer is $0.05. Frederick (2005) shows that this test is well correlated with a 50-question test of general cognitive ability, as well as tests of achievement such as the Scholastic Assessment Test (SAT).

[24]Perhaps related to Cognitive Reflection Test and the tendency to reflect upon choices, adding a control for subjects' response times in the experiment has virtually no effect on the coefficient on the CCEI. This is expected

D. *The Sources of Correlation between CCEI and Wealth*

We found an economically large and statistically significant correlation between household wealth and the financial respondent's CCEI score in the experiment. We saw that this correlation is robust to the inclusion of controls for unobserved preferences, constraints, and beliefs, and that alternative measures of decision-making quality or decision-making ability elicited in an experiment or a survey are not substitutes for the CCEI. With these results in mind, we now turn to account for the sources of the correlation by investigating the relationship between the CCEI and the details of household saving allocations. Our goal is to better understand which important real-world decisions cause those who appear to have better decision-making ability to accumulate more wealth. These estimation results are reported in Table 7. The full-length table is contained in online Appendix VI.

Portfolio.—In columns 1–6 of Table 7, we present estimates that relate the CCEI score of the household's financial respondent to whether the household has a checking account, a savings account, owns stocks, and the fraction of the household's wealth held in each of these assets. To reduce the importance of extreme outliers, in all specification we drop households whose fraction of wealth in the relevant category (checking, saving, stocks, housing) is less than -0.15 or greater than 1.15. The results provide some evidence that, conditional on household characteristics, contemporaneous income, occupation, and education level, households with financial respondents with higher CCEI scores put less of their wealth in low-risk and low-return assets such as checking and savings accounts. The coefficients on the CCEI in columns 2 and 4 of Table 7 are modest in magnitude but statistically significant at the 10 percent level (p-values of 0.083 and 0.095, respectively). The results also provide some evidence that individuals with higher CCEI scores are somewhat more likely to participate in the stock market, though this relationship is not statistically distinguishable from zero.

Housing.—Finally, the coefficients on the CCEI in columns 7 and 8 of Table 7 show economically substantial and statistically significant correlations between the CCEI and decisions regarding home ownership. Households with financial respondents with higher CCEI scores are more likely to own a home, and they put a larger fraction of their household's wealth in a home. A standard-deviation increase in the CCEI of the financial respondent is associated with an increase of 0.047 in the probability of owning a home. Similarly, a standard deviation increase in the CCEI is associated with an increase of 0.043 in the fraction of wealth held in housing. The tendency for those with higher CCEI scores to own a home and put more of their wealth in housing is especially interesting given the favorable tax treatment of owner-occupied housing in the Netherlands, which gives home ownership an important advantage over renting and, other things equal, means wealth placed in

given the modest unconditional correlation between the CCEI scores and response times ($\rho = -0.075$). We also cannot reject a null hypothesis of no correlation between the response time and wealth. These results are omitted in the interest of brevity.

TABLE 7—THE SOURCES OF THE RELATIONSHIP BETWEEN HOUSEHOLDS' NET WORTH AND CCEI SCORES

	(1)	(2)	(3)	(4)
	Have checking	Fraction in checking	Have saving	Fraction in saving
CCEI	0.03	−0.098*	−0.047	−0.162*
	(0.032)	(0.057)	(0.053)	(0.097)
log 2008 household income	0.001	−0.029**	0.003	−0.068***
	(0.002)	(0.013)	(0.010)	(0.021)
Demographics	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes
Constant	0.998***	0.106	1.126	1.448
	(0.172)	(0.822)	(0.848)	(1.288)
R^2	−0.007	0.021	−0.011	0.083
Observations	512	512	502	502
	(5)	(6)	(7)	(8)
	Have stocks	Fraction in stocks	Have a house	Fraction in house
CCEI	0.167	0.001	0.352**	0.324**
	(0.163)	(0.050)	(0.152)	(0.129)
log 2008 household income	0.148***	0.013	0.134***	0.096***
	(0.031)	(0.009)	(0.029)	(0.024)
Demographics	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes
Constant	−3.152*	−0.317	−1.047	−1.151
	(1.856)	(0.398)	(1.760)	(1.419)
R^2	0.079	0.002	0.148	0.123
Observations	514	514	479	479

Notes: Standard errors in parentheses.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

mortgaged housing pays a substantial premium.[25] So long as housing supply is somewhat elastic, and thus the incidence of these tax benefits is shared between buyers and sellers, this suggests that owning a home and placing more wealth in mortgaged housing are often high decision-making quality financial choices. If so, the positive correlation between the CCEI and these decisions is what we would expect if the CCEI captured a general tendency toward higher (financial) decision-making ability.

[25] About 69 percent of households in the sample own a home, and the average fraction of wealth held in housing is approximately 54 percent. In the Netherlands, assets held in owner-occupied housing are not subject to the usual capital income tax. If they were, 4 percent of housing value would be treated as implicit income and taxed at 30 percent. Instead, imputed rent is presumed to be very low (0.55 percent of housing value), is subject to the progressive tax on labor income, and that tax is not due unless the household claims a deduction for mortgage interest. Nominal mortgage interest is, in turn, fully deductible from taxable income. Thus, for purposes of federal taxation, housing assets underwritten by a mortgage will typically pay a negative rate of return. In this way, according to van Ewijk, Jacobs, and de Mooij (2007), the Netherlands offers by far the most favorable tax treatment of owner occupied housing in Western Europe.

V. Related Literature

This paper is primarily concerned with a relatively new economics literature that emphasizes a gap between what people actually choose and what they would have chosen if they were fully attentive to their choices and had the skills and knowledge necessary to weigh costs and benefits in often complex settings. This research provides evidence that some choices are better than others and some individuals are better decision-makers than others.

This new literature contrasts standard economic analysis which takes a libertarian approach; in the absence of the complete data required, standard analysis assumes that all choices are of good decision-making quality and that everyone has sufficient decision-making ability. The libertarian approach has obvious appeal, and economists in the new literature have struggled to measure decision-making quality and to separately identify decision-making ability from standard sources of heterogeneity in important economic outcomes. That is, the new studies have been subject to identification and measurement problems and confronted them to different degrees.

Observational studies of market data have shown that some individuals make choices of such poor decision-making quality that they clearly leave "money on the table." Agarwal et al. (2009) is a prominent example. That paper provides evidence of strictly dominated choices regarding the use of credit. Importantly, research like this requires an uncommonly high-quality (administrative) dataset that makes the relevant trade-offs very clear. The comprehensive nature of the data makes classifying some decisions as "mistakes" immediate and indisputable, and thus allows the analyst to avoid both the measurement and the identification problem. However, such administrative datasets provide information about the activity at just one retailer, or on just a few credit cards, or in just one form of saving. They thus capture only a slice of the economic activities of the individuals involved.[26] As a result, they cannot reveal whether a lower decision-making quality choice represents a minor bobble in otherwise sound decision-making, or a more fundamental problem in evaluating economic choices due to lack of decision-making ability.

Experiment- or survey-based studies take a different approach. They confront the identification problem using measures of education, cognitive and noncognitive skills, or financial literacy as proxies for decision-making ability and either instrumental variables or experimental manipulation to infer causal effects. A prominent example of this research is Ameriks, Caplin, and Leahy (2003). That paper provides evidence that differences in individuals' planning abilities, rather than more standard sources of heterogeneity, explain important variation in wealth. Other examples of significant research in this vein include Bernheim and Garrett (2003) and Duflo and Saez (2003), who study the effect of employer-based financial education on saving. Research of this kind relaxes the imposing data requirements of observational studies that investigate decision-making quality directly, but it is

[26]Other examples of strictly observational studies of decision-making quality that focus on "slices" of economic activity include Miravete (2003) on cell phone calling plans, Choi, Laibson, and Madrian (2011) on employer-sponsored saving, Ketcham et al. (2012) on health insurance, and Lacetera, Pope, and Sydnor (2012) on car purchases.

silent on the measurement problem. It does not seek a portable, quantitative, and economically interpretable measure of decision-making quality.

Other related research relies importantly on surveys and is more strictly descriptive. Restricting attention just to financial decision-making, this research includes Lusardi and Mitchell (2007) who document very low levels of financial planning, financial literacy, and a positive correlation between literacy, financial planning and wealth; and Fang, Keane, and Silverman (2008), who find that, rather than measures of risk preferences, cognitive functioning stands out as a significant predictor of Medigap purchase. Agarwal and Mazumder (2013) correlate proxies of decision-making ability, namely mathematical and nonmathematical cognitive tests, with making low quality financial decisions involving the use of credit.[27]

Our approach follows the strictly observational studies by defining decision-making quality and measuring it directly. Different from that literature, we define decision-making quality by the consistency of choices with GARP, and we measure it by the extent of GARP violation using the CCEI and other standard indices that have been proposed for this purpose. Like the experimental literature, we seek to isolate decision-making ability from standard sources of heterogeneity in choice, in our case by presenting individuals with a theoretically grounded economic choice experiment designed to provide a stringent test of consistency with GARP.

Echenique, Lee, and Shum (2011) measure consistency with revealed preference conditions in individual-level data on grocery store expenditures and correlate the degree of consistency with some indicators of family size, income, age, and education. Their measure is based on the idea that an individual who violates GARP can be exploited as a "money pump." We refer the interested reader to online Appendix II for the construction of the money pump index. To our knowledge, no other papers analyze the correlation between consistency with GARP and socioeconomic characteristics in a broad population. Echenique, Lee, and Shum (2011) conclude that "the hypothesis of consumer rationality cannot be rejected." Such consumption data can, however, lack power to reject violations of GARP (Blundell, Browning, and Crawford 2003, 2008). The power of the test depends on the range of prices consumers face (the frequency with which budget lines cross) and the number of choices each consumer makes.

VI. Concluding Remarks

We offered a new large-scale experiment—employing graphical representations of standard consumer decision problems and using a diverse pool of subjects—that enables us to collect richer data than has been possible in the past. These data allow us to say that some sets of choices are of better decision-making quality than others, in that some choices are more rational than others. Because the data are provided by

[27] Special attention has been paid in this literature to older populations, reflecting a concern about decreasing decison-making ability later in life. The papers in the November 2010 issue of the *Economic Journal* (Volume 120, Issue 548) offer nice reviews: Banks (2010) summarizes research on the relationships between cognitive function, financial literacy, and financial outcomes at older ages; Smith, McArdle, and Willis (2010) and Banks, O'Dea, and Oldfield (2010) show that wealth and retirement saving patterns are associated with numerical and other cognitive abilities at middle and older ages; and van den Berg et al. (2010) and Jappelli (2010) explore causes of the differences in cognitive function and financial literacy in later life.

a large and heterogenous sample, we can analyze the correlates of decision-making quality in the laboratory and relate it to important economic outcomes like wealth. We find that differences in the experimental measures of decision-making quality across subjects independently and robustly explain differential patterns of wealth across households. Since wealth accumulation is determined by countless decisions, made over time in many environments, and involving a host of different trade-offs, our findings indicate that our measure of decision-making quality captures aspects of decision-making ability that apply across many sorts of economic choice problems. This study therefore suggests a new path toward solving the twin problems of identification and measurement.

We conclude by underscoring three key features of the approach we have taken here. First, our measure of decision-making quality—the consistency of choices with GARP—dictates the experimental task: a canonical problem of selecting an allocation from a budget line. Given the task, the measure also provides the benchmark level of consistency necessary to provide a rigorous test. There are no comparable, theoretically disciplined means of quantifying, interpreting, and evaluating laboratory or survey measures of decision-making ability, namely tests of cognitive and noncognitive skills. Second, informed by economic theory, the single experimental task delivers measures of both decision-making quality and preferences from a unified realm of decision-making. Relevant preferences cannot be recovered from performance on standard psychological tests. Third, unlike many tests of cognitive abilities or personality traits, revealed preference tests are applicable to, and comparable across, all sorts of economic choice problems. Our approach can thus be transported, with relative ease, to different decision domains. We can make domain-specific predictions and provide a unified measure of decision-making quality across domains. In all of these ways, the theoretical foundation of our approach drives the design of the experiment and allows diverse and disciplined use of the resulting data.

Taken together, our findings provide new evidence on three important issues: (i) the validity of measuring decision-making quality by the consistency of choices with economic rationality; (ii) the feasibility of testing for consistency with rationality through a web-based survey on a large scale; (iii) the relative importance of heterogeneity in decision-making ability for understanding important economic outcomes. This last issue commands special attention because decision-making ability, unlike preferences, may be justifiably manipulated. If differences in decision-making ability are important sources of the heterogeneity in economic outcomes, then even quite costly policy changes aimed at "soft" or "libertarian" paternalism may hold substantial promise.[28]

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The Power of Revealed Preference Tests: Ex-Post Evaluation of Experimental Design*

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Abstract

Revealed preference tests are elegant nonparametric tools that ask whether choice data conforms to optimizing behavior. These tests present a vexing tension between goodness-of-fit and power. If the test finds violations, is there an acceptable tolerance for goodness-of-fit? If no violations are found, was the test demanding enough to be powerful? This paper complements the many on goodness-of-fit by presenting several new indices of power. By focusing on the underlying probability model induced by sampling, we attempt to unify the two approaches. We illustrate applications of the indices, and provide a field guide to applying them to experimental data.

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Contents

1 Introduction

One of the most elegant tools to test theories of optimizing behavior is revealed preference. The core axioms of revealed preference are presented in a remarkable series of papers by Hal Varian (1982, 1983, 1984, 1985), which built on earlier work by Afriat (1967, 1972), Hauthakker (1950) and Samuelson (1938). Given a vector of prices p_t and choices x_t at time t, we know that the bundle x_t is preferred to another bundle x if x was affordable when x_t was chosen, $p_t x_t \geq p_t x$. Relying on transitivity of preferences, we can string together chains of these inequalities to rank bundles, even those that were never directly compared by the consumer, and bound possible indifference curves that could have generated this data. Of course, if these chains of inequalities cannot all be mutually satisfied, then the data fail to conform with a model of utility maximization. Hence, revealed preference is both a descriptive and a diagnostic tool.[1]

We begin with a few definitions:

Definition: Directly Revealed Preferred: x_t is directly revealed preferred to x if $p_t x_t \geq p_t x$, and is *strictly* directly revealed preferred if $p_t x_t > p_t x$.

Definition: Revealed Preferred: x_t is revealed preferred to x if there is a chain of directly revealed preferred bundles linking x_t to x.

The revealed preference relation is thus the transitive closure of direct revealed preference and revealed preference tests evaluate the validity of the following axioms:

Definition: Weak Axiom of Revealed Preference (WARP): If x_t is directly revealed preferred to x, then x is not directly revealed preferred to x_t.

Definition: Strong Axiom of Revealed Preference (SARP): If x_t is revealed preferred to x, then x is not revealed preferred to x_t.

Definition: Generalized Axiom of Revealed Preference (GARP): If x_t is revealed preferred to x, then x is not strictly directly revealed preferred to x_t.

[1] Note the same notions can be applied to optimizing by firms, as Varian (1984) demonstrates. For brevity, we will confine our discussion to consumer theory, but it all can be applied to producer theory as well.

The most commonly applied notion of a revealed preference test focuses on Varian's (1982) Generalized Axiom. If the data is consistent with GARP, then there exists a utility function that would have generated the data. That is, the data conforms with a theory of optimizing behavior. A failure to satisfy GARP, on the other hand, precludes the existence of a utility representation for the observed choices.

Two obvious issues arise in interpreting revealed preference tests from data. The first is that the test is extremely sharp – a single violation of GARP results in a rejection of the model. One can naturally ask whether there is some tolerance that can be applied to the data to account for errors in either measurement or choice that can allow some "minor" violations to be accepted within the theory. This is the notion of goodness of fit of the model. There have been several important attempts in the literature to formalize approaches to goodness of fit, the most prominent of which is the Afriat Critical Cost Efficiency Index (CCEI) proposed by Varian (1990, 1991).[2] These techniques allow researchers to not only identify the event that choices violate GARP, but also characterize the welfare loss due to these violations.

The other issue arises when the data fail to reject GARP, leaving researchers to interpret a negative result. If the optimizing model is not, in fact, the correct model, would the revealed preference test applied be sensitive enough to detect it or is the negative result a consequence of weak design? This is a question of the power of the revealed preference test, one closely related to the empirical content of the theory in the testing environment.

In contrast to substantial efforts characterizing the goodness of fit for GARP studies, there have been few formal attempts to characterize the power of these revealed preference tests. The earliest contribution in this vein was Bronars (1987), who proposes an alternative hypothesis that individual choices are randomly distributed uniformly over the choice set. In investigating the empirical content of GARP tests generally, Beatty and Crawford (2011) incorporate Bronars alternative hypothesis with Selton (1991)'s measure of predictive success

[2]We formally define Afriat's CCEI in Section 5.2, but intuitively, the measure can be thought of as the proportion of an agent's wealth that is preserved despite violations of GARP. Several alternative goodness of fit measures have been proposed in the recent literature, including Echenique, Lee, and Shum (2012)'s Money Pump and Dean and Martin (2011)'s modification of Houtman and Maks (1985)'s goodness of fit measure.

to define the Difference Power Index which is quite similar to our Optimal Placement Index. Dean and Martin (2012) extend Beatty and Crawford's approach to incorporate observed choice information by deploying a bootstrap of budget shares across budget set. In a novel application, Polisson (2012) compares the power of GARP tests over goods and aggregated features of those goods.

When measuring power for a fixed experimental design, the central challenge lies in specifying the alternative hypothesis that characterizes choice behavior. To that end, we introduce a nonparametric panel regression model in Section 3 that provides a flexible characterization of choice behavior without imposing a utility representation. Given an observed sample of choice behavior, our analysis in Section 4 then illustrates ways of estimating the distribution over choice in that regression model using nonparametric methods. We present intuitive sampling strategies to generate these distributions from observed choice behavior and, in so doing, relate these measures to prior approaches adopted by experimental researchers to measure power.

Estimating the probability of observing GARP violations provides a first step toward characterizing the power of an experimental design. Still further information is available by exploiting design features specific to tests for revealed preference to create intuitive measures of the efficiency of the experimental design. For example, we can ask how severely the observations or design would have to be perturbed in order to observe GARP violations. Exploring this sort of question in Section 5 doesn't lead to a probabilistic measure of power, but rather statistics we refer to as power "indices." We introduce three new power indices: the Jittering Index, the Afriat Power Index, and the Optimal Placement Index.

Our analysis presents a series of different approaches to measure the power of an experiment's design. How these different power measures and indices behave depends on the characteristics of choices in the population and the experimental design. For example, if behavior in the cross-section is extremely concentrated around modes (for instance, due to unobserved types that favor equity or fairness), a fixed power measure may behave differently than if that behavior is rather diffuse. Throughout the exposition, we explore these properties of different power measures by presenting the power indices and measures from two very different experimental settings. Before concluding, we provide some suggestions for how researchers seeking to use these measures should choose the order and depth with which to explore their design's power.

2 Background on Testing Revealed Preference

A vast empirical literature uses revealed preference axioms to build new and better price indices. Mansur and McDonald (1988) examined 27 years of aggregate consumption data. By assuming preferences are homothetic, they improved the power of GARP tests and narrowed the bias in constructing exact prices indices, finding the aggregate data to be broadly consistent with both GARP and with homothetic preferences.

Other researchers have used repeated samples from cross-sectional surveys such as the Consumer Expenditure Survey and the British Family Expenditure Survey. Famulari (1995)'s analysis of the Consumer Expenditure Survey (CEX) from 1982–1985 focused on testing the common preferences assumption. Aggregating choices across representative households to increase power, she found almost all of these "groupings" satisfied GARP. Blundell, Browning and Crawford (2003) note that GARP tests may actually be quite weak when applied to annual data, since incomes expand over time and relative prices are somewhat stable. They propose adopting "flexible parametric models over regions where the nonparametric tests do not fail" to enhance the power of revealed preference tests. Using sophisticated semi-parametric methods to estimate expansion paths for preferences, they then project observed choices into an optimal test setting, finding the data largely fails to reject the optimizing model while also deriving much tighter bounds on consumer price indices.

Not all empirical studies show uniform support for revealed preference axioms and numerous violations of the optimizing model have been discovered using disaggregated consumer panels. A recent study by Echenique, Lee, and Shum (2012) uses scanner data from supermarket food expenditures and find individuals' consumption decisions are often inconsistent with WARP. In order to evaluate the magnitude of these violations, they introduce a "money-pump" measure corresponding to the profits an arbitrageur would be able to generate by exploiting these violations. By this measure, they show that the widespread violations do not impose great costs to the consumer. In another analysis, Dean and Martin (2011) propose a modification of Houtman and Maks (1985) efficiency that finds the least expensive way in which to resolve GARP violations. Using a large panel of household consumption choices, they find pervasive violations of GARP, most of which are of relatively small magnitude. Further, by focusing on substantial heterogeneity in preferences across households, Dean and Martin (2011) underscore that care must be taken in aggregating choices in cross-sectional evaluations of revealed preference.

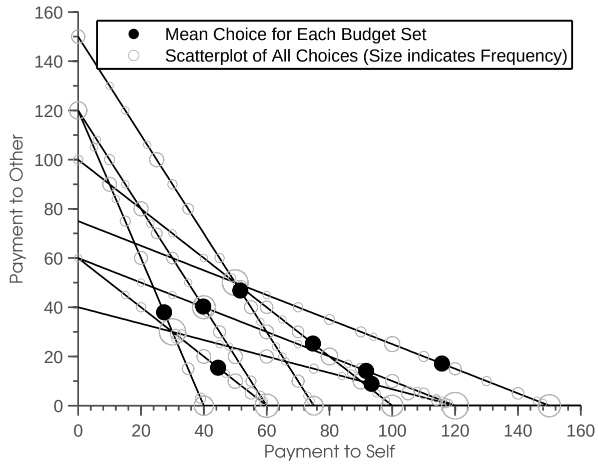
A parallel literature has developed around controlled laboratory experiments. An important first study is by Cox (1997), who evaluates revealed preference in a field experiment using subjects who were residents at a psychiatric hospital. This hospital had a functioning "token economy" with a local currency that could only be traded for goods at a hospital store. Cox found almost all patients' consumption choices to be consistent with the revealed preference axioms, despite potential problems with some goods being storable.

To address issues relating to choices over storable goods, Sippel (1997) provided a test with 10 budget sets over eight commodities, all of which had to be consumed over the course of the experiment. Sippel found 57% of the subjects violated GARP, although very few of these violations were severe in terms of the Afriat Efficiency Index. Fevrier and Visser (2004) used five budgets of six goods, all of which were different varieties of orange juice. They found that 30% of subjects were inconsistent with GARP, with 15% having Afriat Effeciency below 0.95.

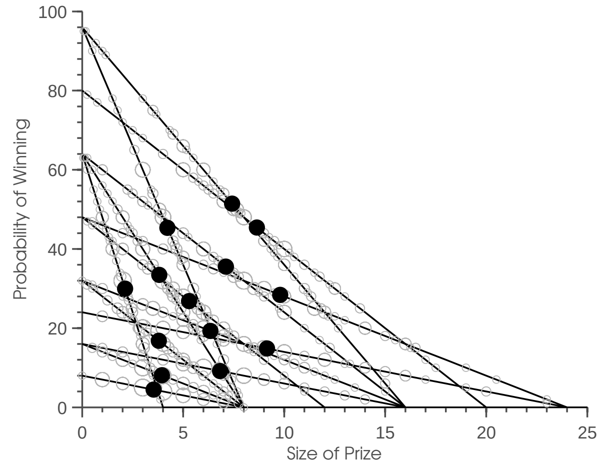
Other experimental settings have focused on evaluating the degree to which GARP applies in different populations. Mattei (2000) used 20 budgets with 8 goods (mostly school supplies), conducting the experiment with three different populations, including 20 undergraduates, 100 graduate students, and 320 readers of a consumer affairs magazine. Applying Afriat Efficiency threshold of 0.95, he found fewer than 4% of subjects violated GARP in each of these populations. Harbaugh, Krause and Berry (2001) test the rationality of children and young adults at different stages of development by offering them choices from budget sets over chips and juice boxes. While second graders performed relatively poorly, sixth graders and college students tended to perform equally well in choosing consumption bundles consistent with GARP. More recent work by Burghart, Glimcher, and Lazzaro (2012) explores the degree to which alcohol impairs an individual's ability to choose consistently with GARP. Surprisingly, even highly inebriated subjects' choice behavior appears to be fairly consistent with axioms of revealed preference.

We illustrate the power measures and indices proposed in the paper using data from two experiments. In the first, Andreoni and Miller (2002) evaluated the extent to which subjects' generosity in a dictator game is consistent with revealed preference. Participants chose from linear budget sets over wealth kept by the dictator and passed to their partner, revealing modal choices corresponding to monotonicity and equity with less than 10% of subjects' choices violating of GARP. The second, Andreoni and Harbaugh (2009), presents subjects with choices over gambles where they faced a linear trade off between the size of the prize and the probability of winning. Behavior in this setting is much more diffuse and, while choices over gains are largely consistent with revealed preference, choices over losses have many more violations of GARP.

The protocols for these two studies are discussed in more detail in Appendix A1. Figure 1's left panel shows the choice sets offered in Andreoni and Miller (2002), as well as a scatterplot of the actual choices and the average allocation chosen on each budget set. The right panel shows a similar perspective of the Andreoni and Harbaugh (2009) experimental treatment. Our objective in the paper is to answer the question of which experiment provided a more powerful test of GARP. The study on risk preferences includes more budget sets and is characterized by more diffuse choices, so we expect to (and do) find more GARP violations in that sample. However, choice behavior in the rational altruism experiment is concentrated around the intersection of budget sets, so the design of that experiment can be considered more efficient even if it reveales fewer ex post violations of GARP in the sample.



(a) Budgets for Testing Altruistic Preferences Andreoni and Miller (2002)



(b) Budgets for Testing Risk Preferences Andreoni and Harbaugh (2009)

Figure 1: Budget Sets and Distribution over Choices from Experimental Data
This figure presents the budget sets and distribution over observed choices in the two experimental treatments we use to illustrate the properties of our ex post power measures and indices. The solid lines present budget sets, the solid black dots represent the mean choice on each budget set, and the light circles illustrate the distribution over choices along each set. Larger circles indicate bundles that were chosen with relatively high frequency.

3 Power and Experimental Design

In this section, we illustrate the tight link between the pattern of choice behavior and the structure of the experiment's design. We define the power of an experiment, conditional on the experiment's design, and develop a nonparametric characterization for the distribution over choices.

3.1 Characterizing the Distribution over Choice

We begin by presenting a probability model for studies evaluating the rationality of observed choice under differing budget sets. Using this context, we will be able to discuss alternative specifications for the data generating process that are not necessarily consistent with rational choice and how to easily sample from these alternative hypotheses.

Assume the econometrician observes a panel of N individuals choosing allocations among K goods and represent each of individual i's choices of consumption bundles on $t = 1, \ldots, T$

budget sets by the vector $x_{i,t}$. The budget sets are denoted $B_1, \ldots, B_T$ with B_t defined by the price vector p_t and income m_t. Denoting individual i's choice function by r_i and using ε as an error term, we can represent the data generating process using a nonparametric panel regression model:

$$x_{i,t} = r_i(B_t) + \varepsilon_{i,t},\ \ E[\varepsilon_{i,t}|r_i(B_t)] = 0 \tag{1}$$

Note that there are two underlying sources of randomness in the observed consumption decisions. Across subjects, variation in choices arises from the individual's "true" choice function, r_i, that could result from an underlying random utility model whose error terms are fixed across all budget sets. The second source of randomness is generated by the term $\varepsilon_{i,t}$, which reflects noise in the individual's observed choices, whether due to measurement error, optimization errors, or time-variation in preferences. The realized choices for all individuals in the population forms the outcome for which our probability model is defined. The sampling of individuals and choices induces a population joint distribution for $X_i \equiv \{x_{i,1}, \ldots, x_{i,T}\}$ conditional on the budget sets included in the experimental design, $B = \{B_1, \ldots, B_T\}$. We denote this measure P^* and use it to characterize the power properties of a given test.[3]

3.2 Power under Different Designs

Our power measures explicitly state the probability that the null hypothesis will be rejected when observing the choice behavior of a single subject from the population *for a specified experimental design*. Fixing the budget sets included in the experiment, the null hypothesis here is simply:

$$H_0\colon\ P^*\{X_i(B)\text{, such that } X_i(B) \text{ violates GARP}\} = 0 \tag{2}$$

[3]Additional regularity and exogeneity conditions are needed to ensure identification of the model. Such an exercise is not in the scope of the current work but would be necessary for projecting choices onto unobserved budget sets. An alternative would be to specify a random utility model where an individual receives a preference shock at every budget set. Further, as a referee pointed out to us, a line of research has explored set-valued extrapolation, including Manski (2007, forthcoming) as well as Blundell, Browning, & Crawford (2003). Current research by Blundell, Kristensen, & Matzkin (2013) and Kitamura & Stoye (2013) considers the problem of directly extrapolating choice behavior without complete identification. Our purpose in adopting the regression framework is to frame our analysis in a setting similar to the framework established by Epstein and Yatchew (1985) to study testing in nonparametric models.

This specification of the null hypothesis is rather flexible, in that it allows us to adopt alternative characterizations for a choice profile that "violates GARP." This flexibility will be useful in allowing for different thresholds, perhaps in terms of Afriat's CCEI, to satisfy goodness of fit. While the null hypothesis can satisfy different thresholds of goodness of fit, it applies for any experimental design.[4]

Unfortunately, feasibility of implementation prevents the experimenter from completely spanning the set of all possible budget sets. Consequently, the probability of rejecting the null hypothesis depends both on the data generating process and the experimental design itself. Note that, while conceptually similar notions, it is important to distinguish this definition of power as a feature of the design of an experiment from the classical definition of power characterizing the properties of a statistical test. In particular, we are interested in comparing the power of *potentially different* experimental designs. For a given choice setting, some experimental designs may be more likely to reveal violations of GARP than others. Here, we use the regression model to characterize choice behavior in the experiment under these different designs.

The power of a statistical test states the probability of rejecting the null hypothesis based on a the distribution for a summary statistic for a single experiment when the data is generated according to an alternative specification inconsistent with the null. Evaluating the power of a statistical test requires a probability model, whether or not it satisfies the null hypothesis, for the sampling error of the test statistic.

The power of the experiment states probability of rejecting a behavioral model based on observed behavior within a fixed design. Here the sampling error comes from selection within the population as well as potentially random individual behavior. In order to evaluate experimental power, we need to specify a probability model for that population selection process and the randomness of individual behavior.

The goal of the current exercise is to evaluate the power of a fixed design specification

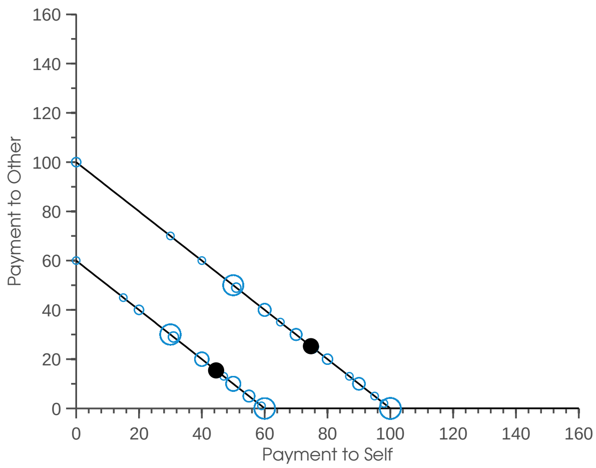
[4]Many natural specifications for the distribution of the error component in the generative choice model, such as the normal error considered in jittering measures below, would lead to observed choice profiles that violate GARP. However, the frequency with which such violations are observed will still depend on the budget sets presented in the experiment.

conditional on the distribution of choices observed in the experiment. This objective corresponds to a counterfactual estimation problem of characterizing the probability that we would observe a violation in other samples or other designs based on what we observe about behavior in a given sample for a fixed design. Under the null hypothesis that every individual of the population makes choices consistent with GARP, any experiment or testing strategy would have zero power by definition. Instead, our goal is to specify the distribution of choices using the ex-post observed choice data. We can then use the structure implied by that distribution over choices to characterize the counterfactual power under different designs.

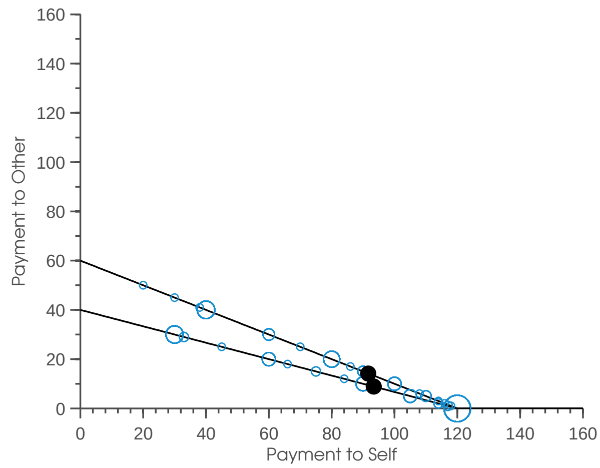
To illustrate the role of design in experimental power, suppose that, rather than using the full set of budget choices presented in the Andreoni and Miller (2002) experiment presented in Figure 1, the experimenter could include only two budget sets. Figure 2 presents four possible designs. In the design presented by Panel (a), none of the budget sets cross one another, and, as such, the design has no power to generate violations of GARP. That is, no matter what choices a subject makes in Experiment (a), the experimenter will not observe a violation of GARP. In Panel (b), the budget sets meet only at their corners, making GARP violations possible but exceedingly unlikely. The budget sets in Panel (c) cross in the middle of the choice plane, but choices are concentrated away from the crossing. The budget sets in Panel (d) cross away from the mid point of the budget sets, but near where many choices are observed.

3.3 Power Measures and Power Indices

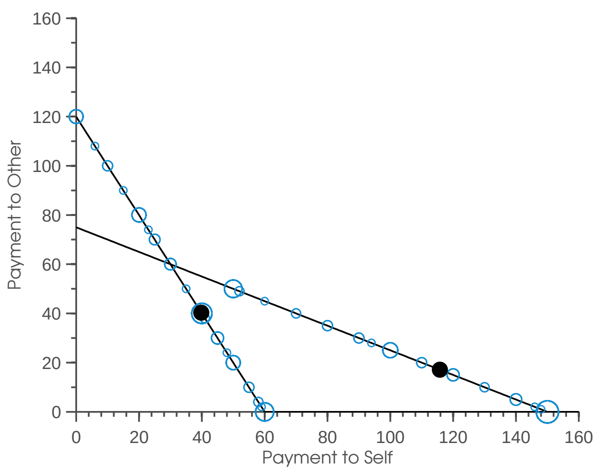
As evidenced by the examples in Figure 2, the placement of budget sets and the distribution over choices on those budget sets jointly determine the power of the experiment. Conditioning on the budget sets, the power of an experiment's design is entirely determined by the distribution over observed choices. The inference objective here is to characterize this distribution over choices to evaluate ex post the degree of probability of rejecting revealed preference. Since distributional estimation typically involves some form of smoothing the



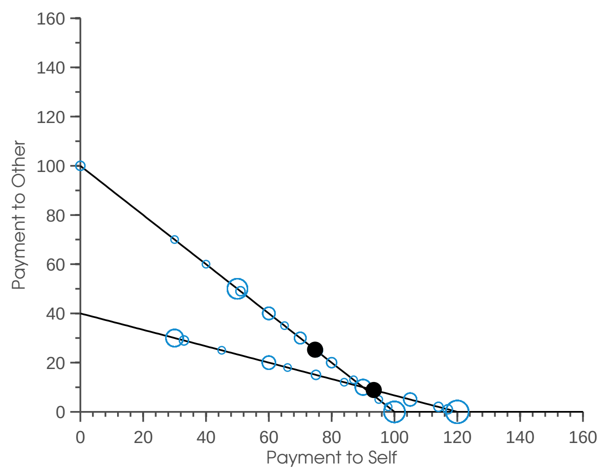
(a) Uninformative Design



(b) Design with Possible SARP Violations



(c) Budget Sets Cross Away From Most Choices



(d) Budget Sets Cross Near Many Choices

Figure 2: Experimental Design and Ability to Detect GARP Violations
Panels (a) through (d) present hypothetical experiments in which subjects faced only two budget sets. The probability of these hypothetical experiments detecting violations of GARP in the population depends both on the point at which the budget sets cross and the distribution of choices in the population.

observed sample distribution, different assumptions on that smoothness will generate different distributions over choice and different measures of power. Defining the alternative hypothesis distribution over choices as a function of the smoothing parameter, we refer to such direct probabilities of revealed preference violations as power measures.

Power indices differ from direct power measures by evaluating ex post how the data generating process must be perturbed to generate violations of the null hypothesis. For example, we may wish to find the "smallest" change that would be necessary for a set of observed choices, which are consistent with the restrictions of revealed preference, to generate violations. Here, indices will vary in terms of how one defines the distance used to characterize "smallest." While they are not probabilistic statements, power indices provide ordinal measures of power by exploiting features of the model that characterize the degree of rigor in the test.

4 Power Measures

In this section, we characterize different nonparametric approaches to estimating the ex post distribution of choices implied by model 1 for use as the alternative hypothesis in measuring experimental power. Perhaps the simplest formulation of this alternative hypothesis is to adopt the sample distribution of choices observed from the $N \subset \mathcal{I}$ subjects in the experiment, which we'll denote P_N. We can then define:

$$H_{\text{A, Sample}}\colon\ r_i\left(B_t\right) = x_{it};\ \varepsilon_{it} = 0 \tag{3}$$

The key limitation of a purely sample-based measure is that it may be overfit in finite samples, understating the true variation in choice behavior, and is difficult to extend to choices outside the observed budget sets, limiting its application in counterfactual analysis. To this end, we propose nonparametric strategies for smoothing the observed measure P_N along with simple sampling algorithms that facilitate calculating the power properties under these smoothed measures.

The ex post results for observed choices from the two experimental samples are displayed in the bottom row of each panel for Table 1 along with the Bronars power measures that we present in the next subsection. The rational altruism study uncovered a small frequency of violations with 9% of subjects selecting choice profiles inconsistent with GARP and only 2% of subjects choices implying an Afriat Critical Cost Efficiency Index (CCEI) less than 0.95 and an average CCEI of 0.998. The study evaluating risk preferences over gains reveals a higher frequency of violations, with 44% of the subjects including at least one violation and

Table 1: Sample Ex Post Results and Bronars' Power Measures

Panel A: Altruistic Preferences

	Budget Share Avg St Dev	Violation Frequency	CCEI Average	CCEI St Dev	Frequency of CCEI < 0.50	0.75	0.90	0.95	0.99	1.00
Bronars M1	0.289	75%	0.88	0.122	0%	16%	46%	59%	69%	72%
Bronars M2	0.238	59%	0.93	0.095	0%	7%	27%	40%	52%	55%
Bronars M3	0.220	44%	0.95	0.082	0%	4%	18%	27%	37%	40%
Sample	0.278	9%	1.00	0.017	0%	0%	1%	2%	3%	3%

Panel B: Risk Preferences over Gains

	Budget Share Avg St Dev	Violation Frequency	CCEI Average	CCEI St Dev	Frequency of CCEI < 0.50	0.75	0.90	0.95	0.99	1.00
Bronars M1	0.289	91%	0.82	0.133	1%	27%	68%	80%	88%	90%
Bronars M2	0.238	80%	0.89	0.112	0%	11%	44%	60%	74%	78%
Bronars M3	0.237	86%	0.87	0.120	1%	15%	52%	68%	81%	84%
Sample	0.185	44%	0.98	0.057	0%	2%	7%	14%	25%	27%

This table reports the Sample and Bronars Power Measures for choices observed in the experimental studies by Andreoni and Miller (2002) and Andreoni and Harbaugh (2009). The cross-sectional standard deviation of budget shares is averaged across budgets and the Violation Frequency reports the frequency with which a subjects' choice profile violates GARP. The table also presents the cross-sectional average, standard deviation, and quantiles for the distribution of the Afriat Critical Cost Efficiency Index (CCEI).

14% of subjects' CCEI's falling below 0.95. Interestingly, while there was more variation in the realized CCEI values for individuals in this experiment, these were generated with a smaller average variance in the budget shares.

4.1 Bronars' Power Measures

Bronars (1987) developed the first and most lasting index for the power of revealed preference tests, specifying an alternative hypothesis based on Becker's (1962) notion that individual choices are made at random and uniformly distributed on the frontier of the budget set. In a data generating model consistent with this behavior, the alternative hypothesis can be stated as:

$$H_{\text{A, Bronars M1}}\colon\; r_{i(k)}\left(B_t\right) = \frac{1}{K}\frac{m_t}{p_{t(k)}}, k = 1, \ldots, K;\; \varepsilon_{i,t} \sim \mathcal{U}\left(B_t\right) \tag{4}$$

where $\mathcal{U}\left(B_t\right)$ denotes the uniform distribution over the frontier budget set B_t recentered at zero.

With this alternative, one can analytically calculate the exact probability that a random set of choices will violate GARP. Perhaps more sensibly, one can conduct a series of Monte

Carlo experiments on the budgets under the alternative hypothesis and calculate the probabilities of GARP violations. Then the power of a particular GARP test is the chance that random choices will violate GARP. Bronars calls this Method 1.[5] Bronars also considered two modification of Method 1. His Method 2 first derives random budget shares in which the expected share is $1/n$, where n is the number of goods. Method 3 finds random budget shares in which the randomness is centered on actual budget shares. Method 1, however, has come to dominate the literature.[6]

The three Bronars' power measures are presented in Table 1 for the altruism and risk preference experimental designs. Bronars' Method 1 provides the least structured behavioral model and, as such, imparts the highest power to each of the experiments. Under this measure, approximately 75% of the choice samples for altruistic preferences included at least one violation of GARP, generating an average CCEI of 0.88, with 59% of the samples generating a CCEI less than 0.95. The Bronars' power measures rate the risk preference study somewhat higher, mainly due to the larger number of budget sets available, with 91% of the samples including at least one GARP violation and 80% of samples generating a CCEI less than 0.95 for an average CCEI of 0.82. Bronars' Methods 2 and 3 impart more structure on the data and, in doing so, yield weaker power properties. Still, across the two experimental designs, all of the Bronars' measures impart a higher power to the risk preference study.

An advantage of Bronars' approach is that it is both natural and simple, motivated by the representation of the alternative hypothesis as a minimally informative prior in the Bayesian sense. A disadvantage is that the alternative hypothesis is perhaps too unconditional and takes no advantage of the information in observed choices about the distribution

[5] One should also note the paper by Aizcorbe (1991) that argued that using Bronars' method to search for WARP violations in all pairs of observations may misstate power in that violations over pairs is not independent (comparing bundle a vs. b is not independent of the comparison of b vs. c). She then suggests a lower bound estimate of power based on independent sets of comparisons. We propose an alternative method for addressing this dependence using a weighted bootstrap algorithm below.

[6] Famulari (1995) and Cox (1997) offered variants of the Bronars method in which observed prices and quantities were randomly paired, and these pseudo-random budget choices are tested for GARP violations. As these are not formal measures of power, we do not discuss them further here, but mention them for completeness. Both approaches involve sampling from both observed budget sets as well as observed choices in a manner that projects choices from one budget set onto another. While a helpful technique for addressing settings with stochastic budget sets, our focus on a fixed sample of budget sets allows us to avoid such issues.

over behavior. [7] Suppose, for instance, the budgets offered did not intersect near the points where individuals are actually choosing. Then if preferences do not conform to utility maximization, the test would be unlikely to discover it. This is true even if Bronars' analysis shows that randomly made choices provide a high probability of violations. Dean and Martin (2012) present a similar critique in a comment on Beatty and Crawford (2011)'s difference power index, proposing a bootstrap technique loosely related to the approaches we propose in the next section. What would be preferred, though, is an index of power based on an alternative that takes account of the choices exhibited.

4.2 Bootstrapped Power Measures

In the ex post setting where we have choice data from a panel of subjects, we can use the multiple observations to get additional information about the distribution over choices that will actually be made within these budget sets. In particular, we can ask whether the organization put on the data by the subjects themselves—by matching individuals with choices—is superior to another method that would have randomly assigned choices to individuals from the universe of choices actually made.

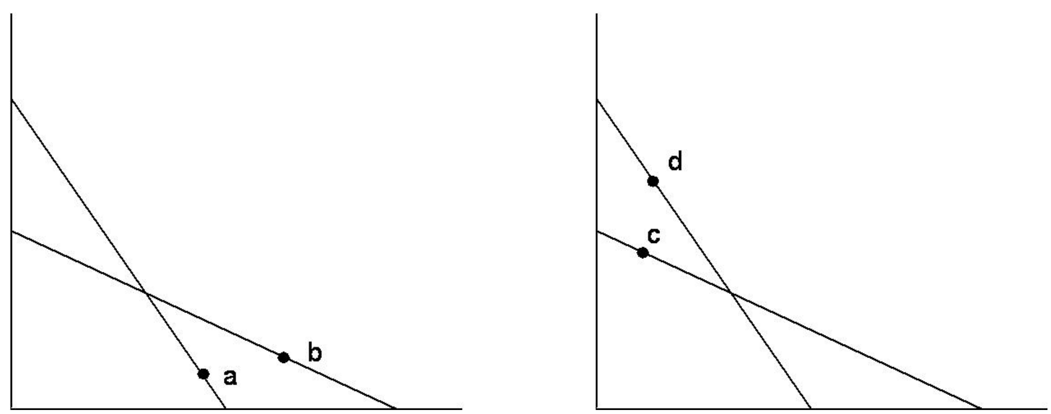


Figure 3: Individual Choices without GARP Violations

For simplicity, consider an example of two experimental subjects given the same two budgets. Suppose the data are like that shown in Figure 3. Here there are no violations of

[7]Note, however, that Bronars' Method 3 does take into consideration the average choices observed in the population at each budget set.

revealed preference. Suppose that, on each budget, we were to pool the choices made by the subjects and then create new synthetic subjects by randomly drawing from the universe of choices actually made. That is, we use bootstrapping techniques to generate a measure of power. In the example of Figure 3, $x_1 \in \{a, d\}$, and $x_2 \in \{b, c\}$. Then there would be a 25% chance that the synthetic subject would be assigned choices a and c, hence violating GARP, which is the maximum probability possible with two budgets and no initial violations of revealed preferences.

Compare these choices to those in Figure 4. Here there would be no chance that we could create a synthetic subject that would violate GARP. In this sense, the test has more power if the study generates data like that in Figure 3 rather than Figure 4.

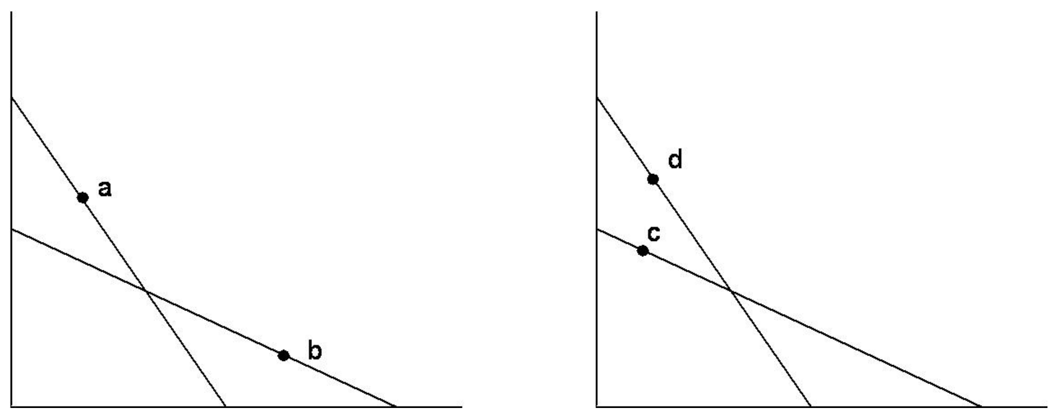


Figure 4: Individual Choices without GARP Violations and Zero Bootstrap Power

Note that this technique can reveal either greater or lesser power than a simple Bronars methods. For instance, in the budgets shown in Figures 3 and 4 a Bronars (Monte Carlo) test would show only about 12% of the cases finding violations, whereas the bootstrapping test will get exactly 25% violations (Figure 3) or 0% violations (Figure 4).

This algorithm maps to an alternative hypothesis that choices are drawn independently across budgets from the empirical marginal distribution of choices on each budget. As such, the bootstrapped alternative hypothesis maintains the Bronars' Method 3 hypothesis that the function $r_i(\cdot)$ is a constant equal to the cross-sectional average consumption bundle chosen on the budget set. However, instead of $\varepsilon_{i,t}$ being uniformly distributed over the zero-centered budget set, as in Bronars' measures, here $\varepsilon_{i,t}$'s distribution gives rise to the empirical distribution of observed choices on the budget set (which, we recall, is denoted by P). That is:

$$H_{\text{Bootstrap}}\colon\ r_i\left(B_t\right) = \bar{x}_t \equiv \frac{1}{N}\sum_{i=1}^{N} x_{i,t}, \qquad (5)$$
$$\tilde{P}\left(\varepsilon_{i,t} = x_{j,t} - \bar{x}_t\right) = P\left(x_{j,t}\right)$$

With this alternative, the probability of violations among the synthetic subjects is the power of the test.[8]

4.3 Weighted Bootstrap: Sampling from the Conditional Distribution over Choices

The main strength of the unconditional bootstrap as compared to Bronars' method lies in its ability to measure how the test's design was suited to the population studied. Like the Bronars method, however, the alternative hypothesis specified is still subject to the Aizcorbe (1991) critique of ignoring dependence in choices across budget sets and consequently ascribing too much randomness to each subject, especially in populations with heterogeneous preferences.

We can address this shortcoming by exploiting continuity in preferences to group the behavior of different subjects. Consider the example from the previous section but, instead of there being two subjects, there are four subjects who make the choices depicted in Figure 5. Visually, it appears the preferences for subjects A and B and for subjects C and D are similar to one another, but the two sets of subjects appear to have very different preferences. Under the bootstrapped power measure, after drawing an observation of $x_{A,1}$ on budget set 1, we'd be equally likely to impute the selection of $x_{A,2}$, $x_{B,2}$, $x_{C,2}$, and $x_{D,2}$ on budget set

[8] This bootstrapping technique was introduced by Andreoni and Miller (2002) and applied by Harbaugh, Krause, and Berry (2001). Dean and Martin (2012) adopt a bootstrapping strategy where they sample from budget shares across budget sets, effectively implying that the distribution of budget shares on unobserved budget sets is equivalent to the unconditional distribution of chosen budget shares. The Dean and Martin (2012) approach is particularly helpful when projecting observed choices onto budget sets that are not observed in the experiment.

2 as the anticipated decision for that type. However, given the obvious pattern in choices, it seems relatively unlikely that an indvidual who selects $x_{A,1}$ really would select $x_{C,2}$ or $x_{D,2}$ and much more likely that she would select either $x_{A,2}$ or $x_{B,2}$.

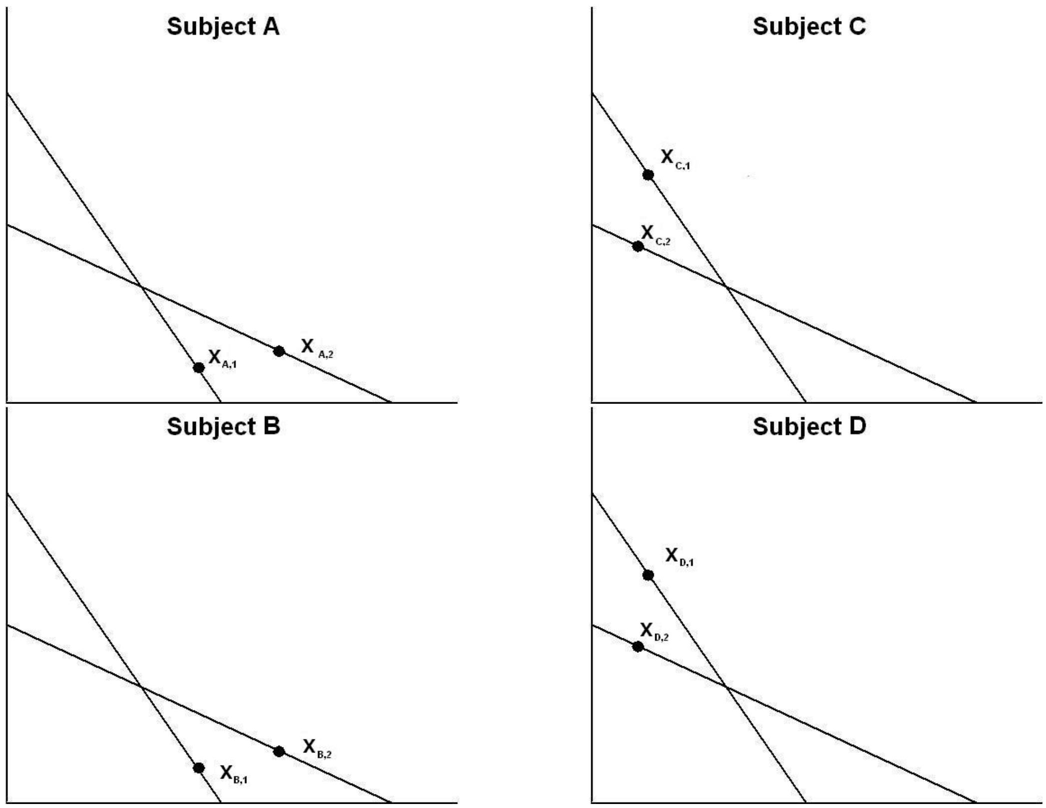


Figure 5: Dependence in Choices Across Budget Sets
When individual choices indicate dependence across budget sets, an unconditional bootstrap can overstate the degree of variation in the data. In contrast, the weighted bootstrap samples from choices in a way that preserves this dependence structure. In this example, individuals tend to prefer one of the two goods, but are unlikely to choose a bundle concentrated in good 1 on one budget and in good 2 on another budget.

For these reasons, we may wish to account for this dependence structure of the choices for an individual across budget sets in the bootstrap. Specifically, for budget set B_1, we want to identify the conditional distribution of selected consumption bundles for subject i given the observed choices subject i has made on the budget sets $2, \ldots, T$. That is, we want to identify the distribution of $x_{i,1}|x_{i,2}, \ldots, x_{i,T}$ and use that distribution to characterize the probability of a WARP violation along the B_1 budget set. Further, denoting by $x_{i,-t}$ the array of observed consumption bundles $x_{i,1}, \ldots, x_{i,t-1}, x_{i,t+1}, \ldots, x_{i,T}$, we can iteratively identify each of the T_i marginal conditional distributions for subject i. We can then use these marginal distributions to sample from the set of budget choices conditional on drawing subject i from the population, using the frequency of GARP violations in these draws to measure the power of the test for each individual in the study.

4.3.1 Sampling for an Individual Budget Set

To link choices on budget set B_τ with the choices observed on other budget sets, we add additional structure to the random function $r_i(B_\tau)$. In particular, letting $\eta_{i,t}$ be a mean-zero error term, assume

$$r_i(B_t)\,|x_{i,-t} = g_t(x_{i,-t}) + \eta_{i,t} \tag{6}$$

Importantly, the function g does not depend on the actual individual i, but only the choices made by individual i on other budget sets. Then, subject to exogeneity conditions on $\eta_{i,t}$, we could use nonparametric regression in the cross-section to estimate the g function and characterize the distribution for $r_i(B_t)\,|X_{i,-t}$. Adapting this estimation strategy, however, is not necessary when a weighted bootstrap allows us to sample directly from this distribution.

Define the weighting function $w(x_{i,-t}, x_{j,-t})$ so that, given the sample $x_1, \ldots, x_N$ and a choice profile on the $-t$ budget sets $x_{i,-t}$ we assign as the choice profile on the t budget set the choice $x_{j,t}$ with probability $\frac{w(x_{i,-t},x_{j,-t})}{\sum_{n=1}^{N} w(x_{i,-t},x_{n,-t})}$. The weighting function is analogous to the kernel in nonparametric regression, smoothing out variation across the population, motivating a Gaussian weighting function. Denoting the normal p.d.f. by $\mathcal{N}$, the sample covariance matrix for choices on the budget sets other than budget set t by Σ_{-t}, and a bandwidth parameter by h, we propose the weighting function:

$$w(x_{i,-t}, x_{j,-t}) \propto \mathcal{N}\left(x_{i,-t} - x_{j,-t}, h^2\Sigma_{-t}\right) \tag{7}$$

The weighted bootstrap nests the unconditional bootstrap as h becomes large and each observation is drawn with equal probability. Such a bandwidth would imply individual preferences on a given budget set are not well-characterized by their choices on other budget sets and decision is driven by idiosyncrasies at the individual and budget set level. As h becomes small, the distribution implied by the weighted bootstrap becomes very close to the sample distribution over choices.[9] The weighted bootstrap implies an alternate hypothesis measure over choices $\tilde{P}$ as:

$$H_{\text{A, WBS}}(h): \; r_i(B_t) = \sum_{j=1}^{N} w(x_{i,-t}, x_{j,-t})\, x_{j,t} \equiv \bar{x}_{i,t}; \tag{8}$$
$$\tilde{P}(\varepsilon_{i,t} = x_{j,t} - \bar{x}_{i,t}) \propto w(x_{i,-t}, x_{j,-t})\, P(x_{j,t})$$

4.3.2 Strengths and Weaknesses of the Weighted Bootstrap

The weighted bootstrap generalizes the unconditional bootstrap method from Andreoni and Miller (2002) for characterizing the distribution over choices. The main benefit to doing so comes by exploiting the cross-section to account for dependence across an individual's choices on multiple budget sets. In this regards, the weighted bootstrap effectively addresses the Aizcorbe (1991) critique of Bronars' and similar power measures, such as the unconditional bootstrap, that ignore this dependence.

The weighted bootstrap also decomposes variation in choices as arising from between subject and within subject variation, sampling from the conditional distribution of $r_i(B_t) + \varepsilon_{i,t}$ given that individual i has been drawn from the population. As such, a standard law of large numbers implies the bootstrap sample average converges to $r_i(B_t)$ and the bootstrap sample variance converges to $Var[\varepsilon_{i,t}]$. Aggregating across individuals, we can take advantage of the exogeneity of errors to decompose the unconditional variance of the choice for a randomly selected individual at budget set t:

$$Var[x_{i,t}] = Var[r_i(B_t)] + Var[\varepsilon_{i,t}] \tag{9}$$

Denoting the total number of bootstrap samples drawn by M, it is straightforward to show the following convergence results hold *within* subjects:

$$\bar{x}_{i,t} \equiv \frac{1}{M}\sum_{m=1}^{M} x_{i,t}^{(m)} \xrightarrow[M,N\to\infty]{} r_i(B_t)\,, \text{ and, } \frac{1}{M}\sum_{m=1}^{M}\left(x_{i,t}^{(m)} - \bar{x}_{i,t}\right)^2 \xrightarrow[M,N\to\infty]{} Var[\varepsilon_{i,t}]$$

[9] In this limit, the only within subject sampling variation in choice for an individual would come from other subjects whose choices matched that individual along each of the $-t$ budget sets but differed at the t-th choice set. In the example above, if $X_{C,2} = X_{D,2}$ but $X_{C,1} \neq X_{D,1}$, then the weighted bootstrap for subject C would draw both $X_{C,1}$ and $X_{D,1}$ with equal probability for the choices on budget set B_1. While minor, aggregating this variation across budget sets can be sufficient to generate choice profiles that do not appear in the sample.

With the following convergence results also holding *between* subjects:

$$\bar{x}_t \equiv \frac{1}{N}\sum_{i=1}^{N} x_{i,t} \xrightarrow[N\to\infty]{} E\left[r_i\left(B_t\right)\right], \text{ and, } \frac{1}{N}\sum_{n=1}^{N}\left(\bar{x}_{i,t}-\bar{x}_t\right)^2 \xrightarrow[N\to\infty]{} Var\left[r_i\left(B_t\right)\right]$$

As in most nonparametric analysis, a central indeterminacy in the weighted bootstrap arises from the need to specify the weighting function and bandwidth. Unfortunately, there is no clear definition of an "optimal" bandwidth in this setting, as such a bandwidth would depend on the true conditional dependence among choices. If the bandwidth is too small, the weighted bootstrap would understate the degree of variation in the data, essentially considering the power of the test to be the frequency of violations in the observed data. Too high of a bandwidth overstates the degree of variation in the data, exaggerating the power for the test. As such, we propose evaluating the weighted bootstrap in terms of its power function determined by the bandwidth.

4.3.3 Empirical Properties of the Unconditional and Weighted Bootstrap

For the altruism and risk preferences experiments, we generate one million samples for each budget using the unconditional bootstrap and 100,000 draws for each subject using the weighted conditional bootstrap for a variety of bandwidths ranging from $h = 0.1$ to $h = 10$. The results summarizing the power properties of the GARP tests using the bootstrapped sample appear in Table 2.

Comparing the two experiments, the unconditional bootstrap illustrates similar power properties for both, the only difference being that we'd expect slightly more severe violations of GARP to be observed in the risk preferences study. Violations occur in approximately 75% of the samples with about 43% (23%) of the risk preference (altruism) samples generating a CCEI of less than 0.95.

As expected for the highest bandwidths, the weighted bootstrap gives almost the exact same results as the unweighted bootstrap. As we decrease the bandwidth, the frequency of violations and the distribution over CCEI's drops from that implied by the unconditional bootstrap to that observed in the sample. Further, as the bandwidth is tightened, the amount of variability in choices and CCEI attributed to variation between individuals increases

Table 2: Bootstrapped Power Measures

	Violation Frequency	CCEI Average	Frequency of CCEI < 0.75	0.90	0.95	1.00	St Dev	CCEI Variance Analysis Within Subject	Between Subjects
			Panel A: Altruistic Preferences						
Simple Bootstrap	77%	0.93	6%	29%	33%	34%	0.109	100%	0%
Weighted Bootstrap									
$h = 10$	73%	0.94	5%	24%	29%	30%	0.102	100%	0%
$h = 5$	64%	0.96	3%	17%	22%	22%	0.087	98%	2%
$h = 1$	36%	0.99	0%	3%	6%	6%	0.034	78%	22%
$h = 0.5$	27%	1.00	0%	1%	3%	3%	0.022	59%	41%
$h = 0.1$	15%	1.00	0%	1%	2%	2%	0.017	18%	82%
Sample	9%	1.00	0%	1%	2%	3%	0.017	0%	100%
			Panel B: Risk Preferences over Gains						
Simple Bootstrap	75%	0.93	6%	29%	45%	63%	0.094	100%	0%
Weighted Bootstrap									
$h = 10$	66%	0.95	3%	16%	30%	51%	0.072	99%	1%
$h = 5$	61%	0.96	2%	12%	25%	44%	0.063	95%	5%
$h = 1$	49%	0.97	2%	8%	15%	32%	0.058	14%	86%
$h = 0.5$	45%	0.98	2%	7%	14%	28%	0.057	1%	99%
$h = 0.1$	44%	0.98	2%	7%	14%	27%	0.057	0%	100%
Sample	44%	0.98	2%	7%	14%	27%	0.057	0%	100%

This table reports properties of the Bootstrap Power Measures for choices observed in the experimental studies by Andreoni and Miller (2002) and Andreoni and Harbaugh (2009). The Simple Bootstrap corresponds to the alternative hypothesis presented in equation 5 while the Weighted Bootstrap corresponds to the alternative hypothesis in equation 8 for varying bandwidths (h). The cross-sectional standard deviation of budget shares is averaged across budgets and the Violation Frequency reports the frequency with which a subjects' choice profile violates GARP. The table presents the distributional properties for the Afriat Critical Cost Efficiency Index (CCEI), including the percentage of the variance in the CCEI that is due to within- and between-subject variation in choices based on the decomposition in equation 9.

as each individual's choice samples become less variable, with the residual variation being attributed to between subject variation in preferences.

4.4 Jittering Measure: Sampling from the Smoothed Probability Distribution

As the bandwidth of the weighted bootstrap goes to zero, the weighted bootstrap sampling algorithm gives very similar results to the sample measure P_N. However, the collection of atoms representing the empirical distribution P_N will overfit the true measure P^* in the same way that a zero-bandwidth nonparametric kernel density estimator for a sample overfits the distribution generating that sample. To address this overfit, kernel density estimators smooth the sample distribution, taking a weighted average of the frequency of "nearby" observations to represent the probability of a given observation. Denoting the kernel weighting function (for example, the standard normal p.d.f.) by $\kappa(\cdot)$, the kernel density estimate for the probability that a given choice profile will be drawn from P^* is:

$$\tilde{P}(x_i = x) = \frac{1}{Nh} \sum_{j=1}^{N} \kappa\left(\frac{x_j - x}{h}\right) \tag{10}$$

Analogous to the weighted bootstrap, we could compute the kernel density estimator for $\tilde{P}$ above and use that distribution to characterize the probability of observing a sample that violates GARP. However, an easier calculation is to implement a sampling strategy that "jitters" the data by introducing white noise to the sample. Let ν_m be a T-dimensional vector of independently drawn standard normally distributed noise terms, or jitters, we generate a large number, say M, such jitters and project each of the t noise terms onto their respective budget lines. We then create the jittered sample for subject i by adding this noise to their observed choices. We repeat this process for each of the N subjects in the sample, so that the probability of drawing a given observation in the synthetic sample is identical to that given in equation 10.[10] The frequency of GARP violations observed in this synthetic sample can then characterize the probability of observing GARP violations under the measure P^*. With the sampling interpretation in hand, we can express the jittering alternative hypothesis as:

$$H_{\text{A, Jitter}}(h): \; r_i(B_t) = x_{i,t}; \; \varepsilon_{i,t} \sim \mathcal{N}_{i,t}(0, h) \tag{11}$$

where $\mathcal{N}_{i,t}(0, h)$ is the truncated normal distribution that ensures $x_{i,t} + \varepsilon_{i,t} \in B_t$.

Using jittering to make inferences about P^* requires addressing two challenges: irregularities in the distribution P^* itself that may require boundary corrections and selecting the

[10] This sampling strategy is equivalent to the smoothed bootstrap, so the distributional equivalence arises from standard results (for example, in Efron and Tibishirani, 1993). Here, we are simply enforcing the uniform frequency of bootstrap draws by holding M constant across each individual in the sample. Note that, while the kernel density estimation strategy could be generalized to project choices onto budget sets not included in the experiment's design, the sampling strategy can only be directly implemented on budget sets for which a cross-section of choices are observed.

kernel and bandwidth parameter for implementation. A priori, one could expect atoms to exist in the distribution P^*, particularly at the corner solutions. The smoothed sample may overstate the degree of variation in choices around such focal points. Also, the constrained support of the budget set requires censoring or truncating the distribution for the jitters to restrict the sample to the support of the budget set. Censoring would tend to overstate the frequency of observed corner solutions, though truncating is known to bias the frequency of corner solutions downward. In our implementation, we consider truncated errors, as these are most consistent with the density estimation strategy described above, although both approaches are feasible.

As in the weighted bootstrap measure, there is room for debate about the choice of kernel, as different kernels will undoubtedly imply slightly different population properties, but the normal kernel is well-suited and widely adopted in the literature. The bandwidth parameter h, however, will have a much more substantial impact on the estimated distribution over choices. For arbitrarily small values of h, the jittered distribution $\tilde{P}$ will converge to the sample distribution P. For extremely large values of h, the jittered distribution will converge to the uniform, Bronars' Method 1 distribution over choices. In this sense, jittering provides a bridge between the Bronars' Method 1, which takes no account of observed choices, and sample based inference, which assigns unobserved choice profiles measure zero.

From an implementation perspective, there are two obvious ways of specifying the standard error for the normal distribution used to jitter the data. The first is to define the standard error in proportion to the length of the budget line ($\tilde{\sigma}_t = \tilde{\sigma}\ell_t$), which we call relative errors. This sampling strategy is equivalent to drawing from a smoothed kernel density over the sample distribution for budget shares in the experiment. The second is to let the distribution be the same for all budget lines regardless of length ($\tilde{\sigma}_t = \tilde{\sigma}$), which we call absolute errors. Jittering with absolute errors is equivalent to drawing from a smoothed kernel density over the sample distribution for choices themselves in the experiment. Because the units for the relative errors bandwidth is constant across budget sets and experiments, the power function from relative jittering is well-suited for comparing experiments. In analyzing the behavior within an experiment, however, jittering with absolute errors provides a measure that can be directly related to the underlying choices.

Table 3 reports the distributional properties of choices and CCEI at variable levels of $\tilde{\sigma}$ for the jittered data. Focusing on the experiment analyzing altruistic prefences, we see a remarkably different effect of the bandwidth in characterizing power using jittering and the weighted bootstrap. In the weighted bootstrap, even minor increases in the bandwidth lead to a substantial increase in GARP violations due to the presence of differing behavioral types that prevented a smooth distribution of choices on the budget set. This same agglomeration of choice requires relatively large jitters in order to bridge the gap between modal choices, so that jittering does not impart a large frequency of GARP violations until the bandwidth exceeds the relevant threshold.

The risk preferences experiment, with its smoother distribution of choices on the budget sets, does not gain much power by relaxing cross-sectional dependence in the weighted bootstrap but does respond to even slight jitters in the data. Interestingly, for the experiment on risk preferences over gains at very low bandwidths, the jittered frequency of violations actually drops compared to the sample violation frequency. This result arises for subjects whose choices violate GARP while maintaining a Critical Cost Efficiency Index of unity. In these cases, jittering the data slightly actually removes violations in 75% of the draws by moving a choice profile off the intersection of two budget sets.

5 Power Indices

In this section, we look at power indices characterizing three properties of the experiment's design. We begin with the Jittering Index to characterize the amount of noise that we would need to add to an individual subject's choices to generate GARP violations. Next, we consider the Afriat Power Index, which is defined by the degree to which GARP would need to be strengthened in order to generate violations. We close with the Optimal Placement Index as a measure of the efficiency of the experiment's design relative to a maximally efficient design for testing violations of WARP at each individual budget set.

Table 3: Jittered Measure Properties

	Violation Frequency	CCEI Average	Frequency of CCEI < 0.75	0.90	0.95	1.00	St Dev	CCEI Variance Analysis Within Subject	Between Subjects
Panel A: Altruistic Preferences									
Bronars M1	75%	0.88	16%	46%	59%	72%	0.122	100%	0%
Absolute Jittering									
$\sigma = 50$	58%	0.92	9%	31%	44%	58%	0.104	85%	15%
$\sigma = 25$	38%	0.96	4%	17%	26%	38%	0.083	64%	37%
$\sigma = 15$	27%	0.97	1%	10%	17%	27%	0.059	59%	41%
$\sigma = 5$	22%	0.99	0%	2%	6%	22%	0.024	41%	59%
Relative Jittering									
$\sigma = 1$	73%	0.88	16%	47%	60%	72%	0.120	99%	1%
$\sigma = 0.5$	65%	0.91	11%	38%	51%	65%	0.111	94%	6%
$\sigma = 0.1$	26%	0.98	1%	8%	15%	25%	0.053	56%	44%
$\sigma = 0.05$	23%	0.99	0%	2%	8%	22%	0.028	48%	53%
Sample	9%	1.00	0%	1%	2%	3%	0.017	0%	100%
Panel B: Risk Preferences over Gains									
Bronars M1	91%	0.82	27%	68%	80%	90%	0.133	100%	0%
Absolute Jittering									
$\sigma = 25$	87%	0.85	20%	60%	75%	87%	0.127	96%	4%
$\sigma = 5$	57%	0.95	5%	19%	33%	57%	0.089	53%	48%
$\sigma = 2.5$	45%	0.97	3%	11%	21%	44%	0.072	28%	73%
$\sigma = 0.5$	36%	0.98	3%	8%	15%	35%	0.059	2%	99%
Relative Jittering									
$\sigma = 1$	91%	0.82	26%	68%	81%	90%	0.132	100%	0%
$\sigma = 0.5$	89%	0.84	23%	64%	78%	89%	0.128	99%	1%
$\sigma = 0.1$	58%	0.95	3%	17%	32%	58%	0.079	51%	49%
$\sigma = 0.05$	44%	0.97	2%	10%	19%	43%	0.066	22%	79%
Sample	44%	0.98	2%	7%	14%	27%	0.057	0%	100%

This table reports properties of the Jittered Power Measures corresponding to the alternative hypothesis in equation 11 for choices observed in the experimental studies by Andreoni and Miller (2002) and Andreoni and Harbaugh (2009). The varying bandwidths, σ, correspond to the standard deviation of errors of actual goods (Absolute Jittering) or budget shares (Relative Jittering). The cross-sectional standard deviation of budget shares is averaged across budgets and the Violation Frequency reports the frequency with which a subjects' choice profile violates GARP. The table presents the distributional properties for the Afriat Critical Cost Efficiency Index (CCEI), including the percentage of the variance in the CCEI that is due to within- and between-subject variation in choices based on the decomposition in equation 9.

5.1 Jittering Index

To motivate this approach, suppose a person was offered the five budget constraints pictured in Figure 6 and all the choices involved equal quantities of both goods, as in the left panel of the figure. These choices do not violate GARP and are consistent with preferences that have a kink at the 45-degree line, however, adding only the slightest shift in choices along the budget constraint could result in a GARP violation. Compare this to the data shown in the right panel of Figure 6 where the data look as though they are consistent with a perfect substitutes utility function and requiring very big perturbations added to the data in order to generate violations of revealed preference. Since both samples imply similar predictability in individual choices we could conclude that the left panel provides a more powerful test of rationality due to its relative sensitivity to perturbations.

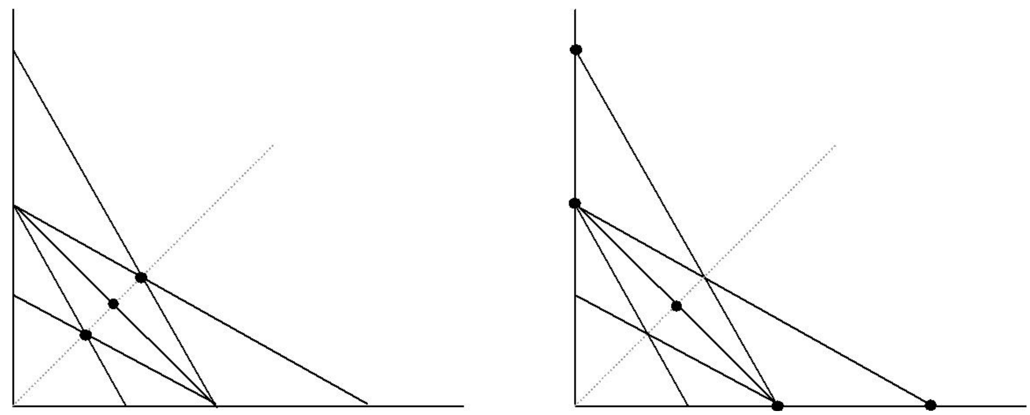


Figure 6: Different Choice Profiles for the Same Budget Sets

To formalize the approach, we need to construct two measures. The first is a measure of how much error we need to add to the data in order to generate a predetermined severity of GARP violations. The second is a measure of the amount of variance or error naturally occurring in the data. By comparing the variation we need to add with the naturally occurring variation, the jittering index can be normalized to account for the number of budget sets in the design so that indices can be compared across experiments.

We can get the first measure from the power function using the jittered power measure. The bandwidth parameter of the jittered sample corresponds to the standard deviation of the noise added to the observed sample. For each subject, we can vary this smoothing parameter to find the minimum bandwidth for which 5% of the jittered experiments find at least one GARP violation. Denoting this bandwidth $\overline{\sigma}$, it gives a direct measure of how close the chosen budgets came to finding a violation of rationality—the closer $\overline{\sigma}$ is to zero, the sharper the test of rationality.[11]

To normalize the Jittering Index across experiments, consider tests of whether the noise added to create the jittered data, $\tilde{\varepsilon}$, is significantly bigger than the noise naturally occurring in the data, ε, under the null hypothesis that $\tilde{\varepsilon}$ and ε both have the same variance of σ^2. For each individual in the sample, the statistic:

$$\phi = \frac{\frac{1}{T(K-1)} \sum (x_{i,t} - \tilde{x}_{i,t})^2 / \sigma^2}{\frac{1}{T(K-1)} \sum (x_t - r_i(B_t))^2 / \sigma^2} \approx \frac{\overline{\sigma}^2}{\sigma^2}$$

is characterized by the F distribution under the null hypothesis. If there are K goods on each of T budgets, then this F–test has $T(K-1)$ degrees of freedom in both the numerator and denominator.[12] Fixing the significance level at the customary 5%, we can find the critical values from the quantiles of the F distribution, denoting these $c_{T,K-1,0.05}$. The Jittering Index is then defined as $\sigma^* = \overline{\sigma} c_{T,K-1,0.05}^{-1/2}$. Then, any $\sigma \geq \sigma^*$ would be enough natural variance to satisfy the desired confidence in the power of our test.

For interpretation, we must appeal to intuitions about whether σ^* is "small." The lack of an objective definition for a "powerful" test presents the greatest limitation for the Jittering Index, though having to specify a σ is tempered by being able to state a needed σ^* threshold for variance in the data. If σ^* is a number that all would agree is small given the nature of the data, then arguments over σ may be avoided.[13]

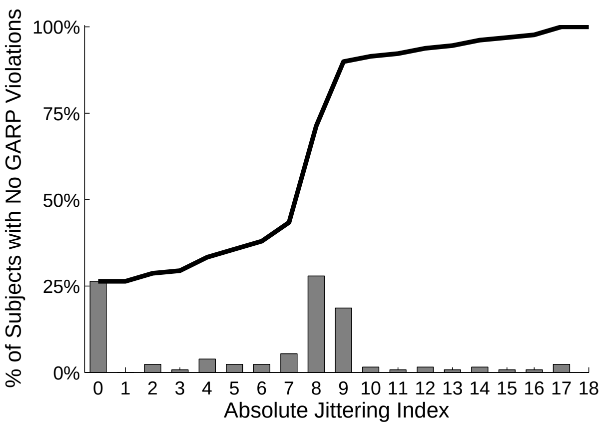
In Figure 7, we show the values of $\overline{\sigma}$ for all subjects who had no violations of GARP. The bars are the marginal frequency and the lines are the cumulative frequency. Panel (a) shows that, under absolute error, a similar degree of power holds if the natural error exceeds

[11] Note that this method even works to find power when there are violations of GARP, but just relatively few. We may still want to jitter the data to see how much noise we need to add to bring violations up to some critical value. However, in an experiment whose design has a Bronars power measure less than 5%, the Jittering Index would be unbounded (though this is not necessarily a bad result as such a design would have very weak power under any data generating process).

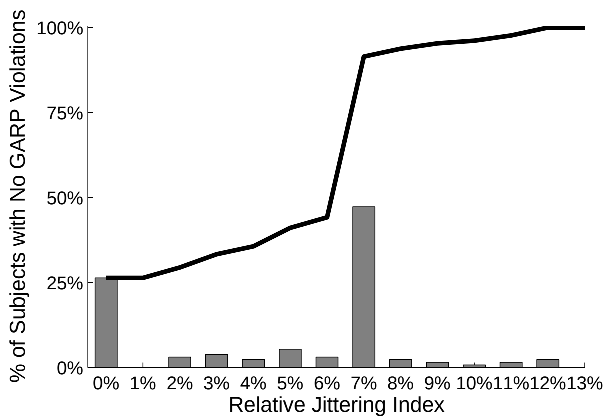
[12] Recall that we are thinking of K as a point on a budget plane. Thus there are only $K-1$ independent values in the vector x, and $m-1$ elements in ε. Note also that the vector notation implies that $\sum_{t=1}^{T}(x_t - z_t)^2 = \sum_{t=1}^{T}\sum_{i=1}^{K-1}(x_{ti} - z_{ti})^2$.

[13] This question is reminiscent of that encountered by Varian (1985) in his goodness-of-fit analysis, and the answers are thus similar. One option is to find a parametric estimate of a utility function and let the standard error of the regression stand for σ. This, obviously, dilutes the value of nonparametric analysis with parametric analysis. Moreover, there often may be too few observations from a single agent to estimate such a function, leaving one to postulate σ from some other ad hoc means.

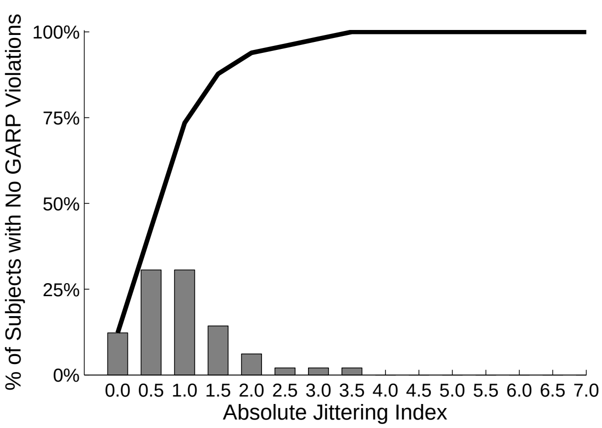
$\sigma = 10$. Panel (b) shows that, under relative errors for the altruistic study, if the natural error in the data exceeds $\sigma_i = 0.08\ell_i$, then 90% of the subjects would have been given significantly powerful tests of GARP. The results for relative jittering in the risk preference experiments imply a slightly lower level of natural error required to generate violations, even after conditioning on those subjects who did not violate GARP.



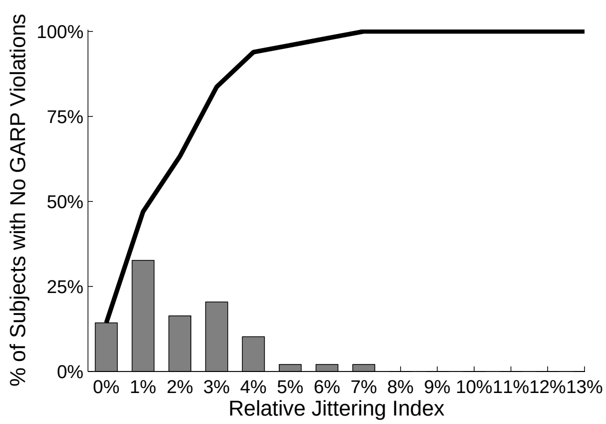
(a) Altruistic Preferences



(b) Altruistic Preferences



(c) Risk Preferences over Gains



(d) Risk Preferences over Gains

Figure 7: Distribution of Jittering Index for Subjects with No GARP Violations
This figure presents the cross-sectional distribution of the absolute and relative Jittering Indices calculated for the individual subjects in the two experiments whose choices revealed no GARP violations. The Jittering Index corresponds to the maximal standard deviation of within-subject variation in choices that could be rejected by an F-Test when compared to the minimal standard deviation required to generate violations in at least 5% of the jittered samples for that subject.

This leads naturally to the question, how much natural error exists in the data? Looking at the data from the study of altruistic preferences, one sees immediately that one source of natural error is rounding.[14] Perhaps for cognitive ease, subjects have an overwhelming tendency to choose numbers divisible by 10. This is true for both the hold and pass amounts. In fact, over 85% of all choices had both the hold and pass values divisible by 10. Another 11% were divisible by 5, but not 10. Only 4% of choices made were not divisible by either 10 or 5. Suppose we assume subjects restrict choices to those where both hold and pass amounts are divisible by 10, and that "rational rounding" would choose the point that yields the highest utility. This means that the maximum error would be at least 5, assuming convex preferences. These rounding errors alone would provide enough natural variance in the data to make at least 38% of our GARP tests have sufficient power.[15] If we were to believe that there is some other independent variation in the data, either from measurement, reporting or learning, that is roughly equal to noise from rounding so the expected absolute error was about 5 tokens on each budget, then $\sigma_i \approx 0.1$ for relative and $\sigma_i \approx 13$ for absolute errors. If this were the case, then about 95% of the GARP tests would have sufficient power.

Comparing the Relative Jittering Indices across the two studies, the experiment evaluating risk preferences over gains appears to have greater power than the experiment evaluating altruistic preferences. This enhanced power comes from the difficulty of testing rigid preferences, as evidenced by the spikes in the centers of Panels A & B in Figure 7. These are due to subjects who always chose corner solutions: 28% always kept everything, and 11% had apparently linear preferences (see Andreoni and Miller (2002), Table III). Note also that one would not expect even rounding error to be present at corner solutions, so there's very little naturally occurring variation in the data at these points. As is evident, when testing revealed preference in settings with such stark preferences, it's exceedingly difficult to design

[14]Rounding is commonly observed as a feature of choice in continuous problems in a broad array of forms. In particular, Pollison & Quah (2013) and Cosaert & Demuynck (2012) explore the power of revealed preference in aggregated or discretized choice spaces.

[15]To be conservative, assume a uniform distribution of absolute rounding errors between 0 and 5, and thus an expected absolute error of 2.5 tokens. Under the assumption of relative errors, this implies $E|\varepsilon_i| = 0.43$ and for absolute errors, this implies of $E|\varepsilon_i| = 5.7$. It is easy to show that our assumptions imply the standard error of $\sigma \approx 1.15E|\varepsilon_i|$. For the assumption of relative errors, this means $\sigma_i = 0.049$, while for absolute errors it means $\sigma_i = 6.53$.

an experiment with a viable amount of power.

5.2 The Afriat Power Index

Although the index proposed in this section was not suggested by Afriat, it seems natural to give it his name given its similarity to the Afriat Critical Cost Efficiency Index. To characterize the severity of a violation of revealed preference, Varian (1990, 1991) builds on Afriat (1967, 1972) to construct the Afriat Critical Cost Efficiency Index. Varian first relaxes the directly revealed preferred relation by defining, $R^d(e)$, so that: $x_j R^d(e) x$ iff $ep_j x_j \geq p_j x$, where $0 \leq e \leq 1$. It follows to define $R(e)$, a relaxed revealed preference relation, as the transitive closure of $R^d(e)$. Varian defines a version of GARP, which we call L-GARP(e) ("L" for lower), as

Definition: *L-GARP(e):* If $x_j R(e) x_k$, then $ep_k x_k \leq p_k x_j$, for $e \leq 1$.

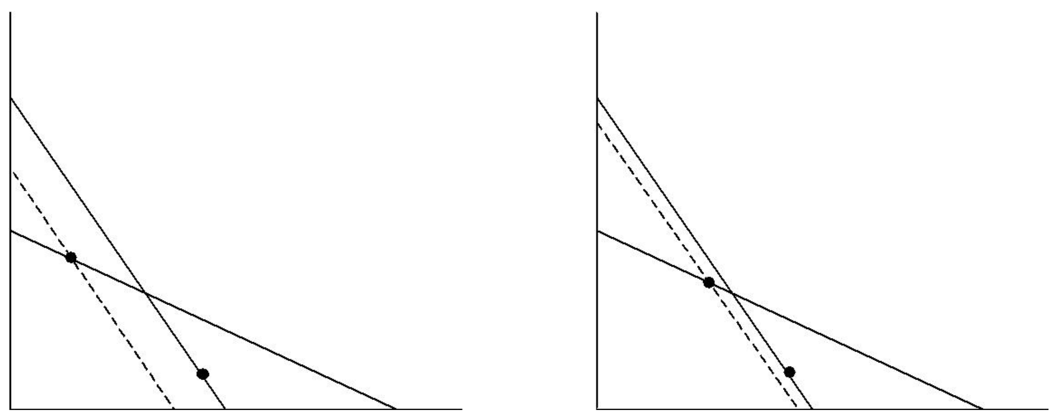


Figure 8: The Afriat Critical Cost Efficiency Index
The Afriat Critical Cost Efficiency Index (CCEI) measures how far budget sets would need to be shifted to remove any violations of GARP from the data. A choice of consumption bundles with a low CCEI (as in the left panel) indicates a more severe violation of GARP than one with a CCEI near unity (the right panel).

Afriat's Critical Cost Efficiency Index, or the Afriat Efficiency Index for short, is the *largest* value of $e \leq 1$, say e^*, such that there are no violations of L-GARP(e). If $e^* = 1$ then there are no violations of GARP in the original data, but for $e^* < 1$ there are violations. Traditionally, researchers begin their analysis of consumer behavior by setting some critical level of e^*, say $\overline{e}$, such that they would consider any $e^* \geq \overline{e}$ a small or tolerable violation of

GARP. Varian (1991), for instance, suggests a value of $\overline{e} = 0.95$.[16]

Suppose a set of choices *does not* violate GARP. If the budget constraints cross near the area that subjects are actually choosing, then we can think of that set of budgets as being more diagnostic than a different set in which the choices are far from the intersections. For instance, Figure 9 shows two budgets without violations of revealed preference. However, the frame on the right gives us more confidence that the person choosing these goods satisfies utility maximization. If there were a violation or rationality, we would be more likely to uncover it in the right panel since even a small change in choices would have been enough to violate GARP. In the frame on the left, by contrast, there would have to be much larger violations of rationality before we could uncover them with this test.

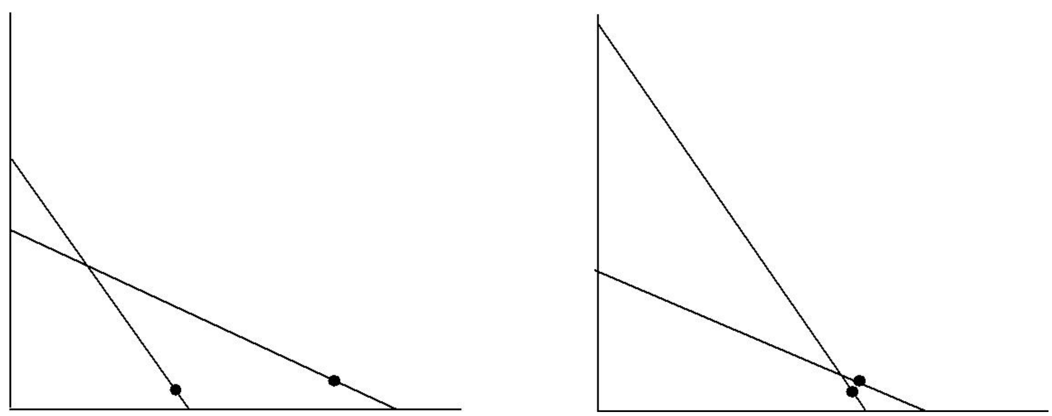


Figure 9: Consumption Choices that do not Violate GARP

To capture the intuition behind Figure 9, define a stronger direct revealed perference relation, $\widetilde{R}^d(g)$, as $x_j \widetilde{R}^d(g) x_k$ iff $gp_j x_j \geq p_j x_k$, where $g \geq 1$. Thus, if $g = 1$ we have the standard notion of directly revealed preferred. Then let $\widetilde{R}(g)$ be the transitive closure of $\widetilde{R}^d(g)$. Given this stronger notion of revealed preference, we can define a new concept H-GARP ("H" for higher) as

Definition: *H-GARP(g):* If $x_j \widetilde{R}(g) x_k$, then $gp_k x_k \leq p_k x_j$, for $g \geq 1$.

[16]Note that, since the Afriat Efficiency Index is just a transformation of individual choices, it can be defined as a random variable on the probability space for the experiment. As such, its distributional properties can be inferred using the sampling techniques presented in section 4. Further, we can extend the variance decomposition from individual choices to separate a priori variation in the Afriat Efficiency Index into individual and cross-sectional components.

Using this inverted notion of the Afriat Efficiency Index, we can define the Afriat Power Index as the *infimal* value of $g \geq 1$, say g^*, such that there is *at least one* violation of H-GARP(g). If $g^* > 1$ there are no violations of GARP in the data, but if g^* is close to 1 the choices are near where the budget constraints intersect. An example of the Afriat Power Index is shown in Figure 10. The choices on the left are less informative about rationality than those on the right, and the Afriat Power Index is closer to 1 in the panel on the right. Hence, while the Afriat Efficiency Index told us how much we need to "relax" the budgets to avoid violations, the Afriat Power Index tells us how much we need to "expand" budgets in order to generate violations.[17]

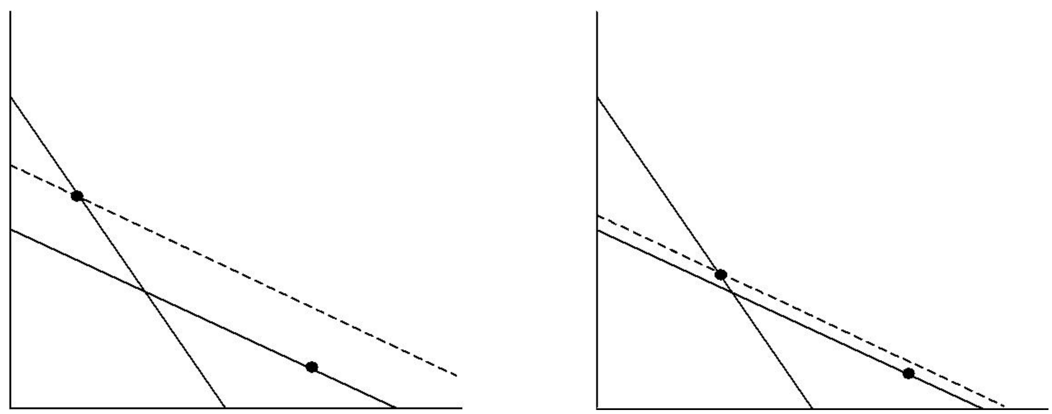


Figure 10: The Afriat Power Index

The Afriat Power Index measures how much budget sets would need to be shifted outward for a violation of GARP to be observed in the choice sample. Subjects whose choices would require greater shifts in the budget set to generate a violation (as in the left panel) face a less demanding test than those whose choices can generate violations with only small shifts in the budget sets (as in the right panel).

When can we say that the g^* found from the Afriat Power Index is "too big" and thus has too little power? One obvious approach is to switch our perspectives. If, under the Afriat Efficiency Index we were willing to accept any $e^* \geq \bar{e}$ as an acceptably small violation of GARP, then any $g^* \leq 2 - \bar{e}$ should also be an acceptably powerful test of GARP.

As the original Afriat Efficiency Index has some shortcomings that have been highlighted

[17]Consider the special case where a single choice is made at the point where two budgets intersect but that there is no violation of GARP. Then the smallest shift in one budget constraint will create a violation, in which case $g^* = 1 + \delta$, where δ is infinitely small. For ease of discussion, we will refer to this as a case of $g^* = 1$.

in the literature, the Afriat Power Index is subject to similar critiques. For instance, the Afriat Efficiency Index is defined by only one violation, and does not give credit to an individual who may otherwise have large numbers of perfectly rational choices. In other words, it is not very forgiving of a single error. By the same token, it can potentially mask the troubling nature of a large number of small errors. Similarly, the Afriat Power Index will score well if there is a single pair of budget constraints which cross near the choices, even if all other budget constraints cross far from the choices. As with the Jittering Index, this limitation of the Afriat Power Index highlights the challenge of testing rationality in settings with sharp preferences.

5.3 The Afriat Confidence Index

Assign an individual i a number $A_i = e_i^* g_i^*$, where e_i^* is the Afriat Efficiency Index and g_i^* is the Afriat Power Index.[18] Call A_i person i's *Afriat Confidence Index*. If $A_i < 1$ the person has at least one violation of GARP and this number can be interpreted as indexing the severity of the violation. If $A_i > 1$ then the person has no violations of GARP, and the number can index the stringency of the GARP test. An $A_i = 1$ corresponds to the ideal data—the person could not have been given a sharper test. By selecting an $\overline{e}$ prior to analysis we gain a "confidence interval" on A_i, that is $\overline{e} \leq A_i \leq 2 - \overline{e}$. An A_i in this interval can be seen as a successful test of GARP.

Figure 11 shows the distribution of Afriat Confidence Indices, A_i, for the experimental samples. The left hand side of the graph presents results for subjects who had at least one violation of GARP (and, as such, an ACI ≤ 1) and the right hand side of the graph presents results for subjects with no violations of GARP (and an ACI ≥ 1).

To focus on the power properties of the experiments' designs, consider those subjects who had no GARP violations. In the rational altruism study, more than two-thirds of these

[18] Alternatively, we could derive A_i from a unified framework. Define $R_A^d(a)$ as $x_j R_A^d(a) x_k$ iff $ap_j x_j \geq p_j x_k$, for some $a > 0$, and let R_A be the transitive closure of R_A^d.
Define *A-GARP(a):* If $x_j R_A(a) x_k$, then $ap_k x_k \leq p_k x_j$, for $a \geq 0$.
Then let $a_i^* = inf\{a$: there exits a single violation of *A-GARP(a)*, or at which the smallest change in a would remove all violations of *A-GARP(a)*$\}$. Then $A_i = a_i^*$.

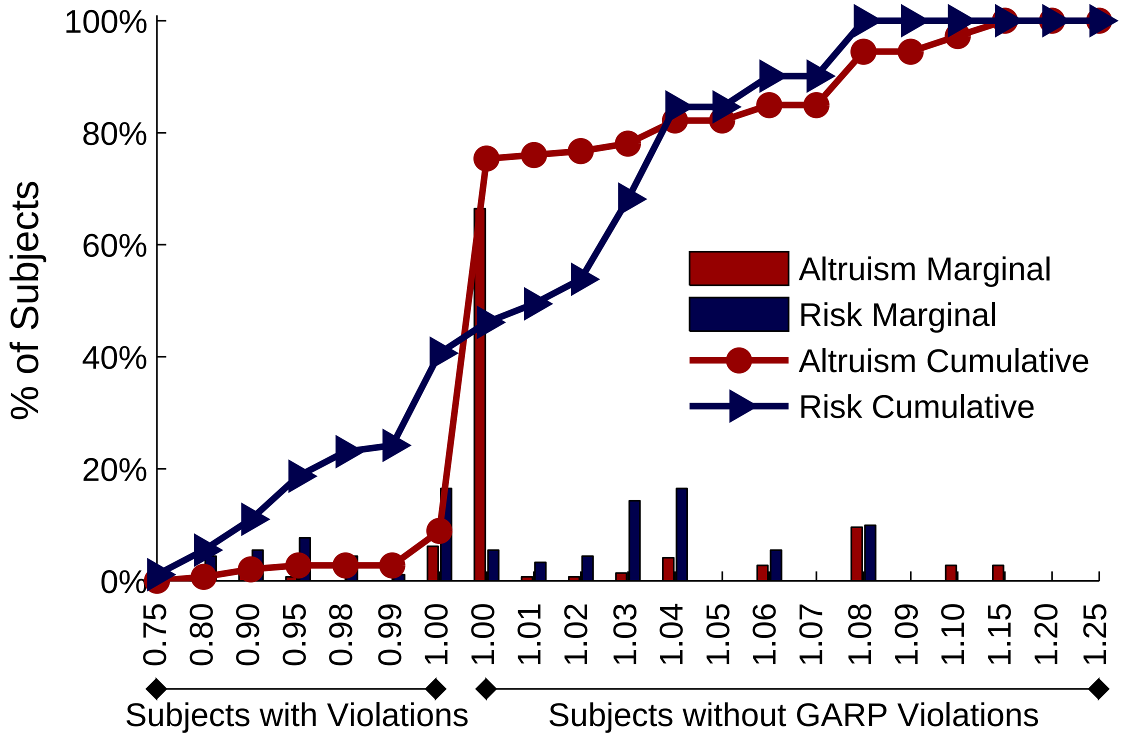


Figure 11: Afriat Confidence Index, A_i

This chart presents the cross-sectional distribution of the Afriat Confidence Index for choices observed in the experimental studies by Andreoni and Miller (2002) and Andreoni and Harbaugh (2009). Subjects whose scores are far above unity did not encounter a sufficiently stringent test of their preferences. Subjects whose scores are far below unity severely failed to pass the GARP test. The population of subjects with A_i near unity either passed a stringent test of their choice behavior or failed within the tolerance of the Critical Cost Efficiency Index.

(71%) had Afriat Confidence Indices of 1, indicating that the GARP test could not have been sharper. If we apply the same criterion for "high power" that we do to "small violation" then 107 (83%) of the non-violators have $A_i \leq 1.05$. Then defining an "acceptably stringent success range" for Afriat Confidence Indices such that $0.95 \leq A_i \leq 1.05$ means that 82% of subjects in the sample were given stringent tests of GARP and passed, 2% of subjects had significant violations of GARP ($A_i < 0.95$) and 16% were given GARP tests that were not sufficiently diagnostic ($1.05 < A_i$).

Comparing the results from altruism study to those from the experiment on risk preferences, the Afriat Confidence Indices have a much smoother distribution, reflecting the fact that the budget line crossings were less "focal" compared to the altruism study. The "acceptably stringent success range" for rational risk preferences means 75% of subjects can be characterized as passing stringent GARP tests, with 15% presenting significant violations of GARP and 10% revealing the test was not sufficiently diagnostic.

As such, mainly through the higher frequency of violations in the risk preferences experiment, the altruism experiment yielded greater confidence in the validity of GARP. However, the budget sets in the altruism experiment were placed in such a way that most observed choices would fail any stronger notion of GARP. As such, the lower frequency of violations is not due to faulty design, but rather arises from the structure imposed by individuals on their choices.

5.4 The Optimal Placement Index

Consider the choices a on budget A in the left panel of Figure 12 and suppose that, *ex post*, we wanted to alter the placement of budget set B without changing relative prices to test whether the choice a satisfies WARP. In this case, we would obviously choose a budget that would intersect A at point a, corresponding to budget set C. How much more efficient is C than B at testing rationality? As seen on the right panel of Figure 12, on budget B there is a fraction d/D of choices available that would violate WARP, while on budget C there is a fraction e/E of available choices that would violate WARP. Hence, we define the Optimal Placement Index to indicate the relative efficiency of the placement for budget set B to test WARP for choice a as

$$\theta_{a,b} = \frac{d/D}{e/E} = \frac{d}{e}\frac{E}{D}$$

How about choice b on budget B in Figure 12? Here the budget A has no ability to find a violation of WARP, conditional on the observation of b. In this sense, the test has no power to show that b was chosen irrationally. Hence, we can say $\theta_{ba} = 0$. As such, we can calculate two *directed* Optimal Placement Indices for any pair of budget sets. We discuss how to aggregate across these directed power measures in the next subsection.

To relate this calculation to the power measures introduced in the previous section, notice that the numerator of the index $\theta_{\tau,-\tau}$ corresponds to the Bronars' WARP Power Measure

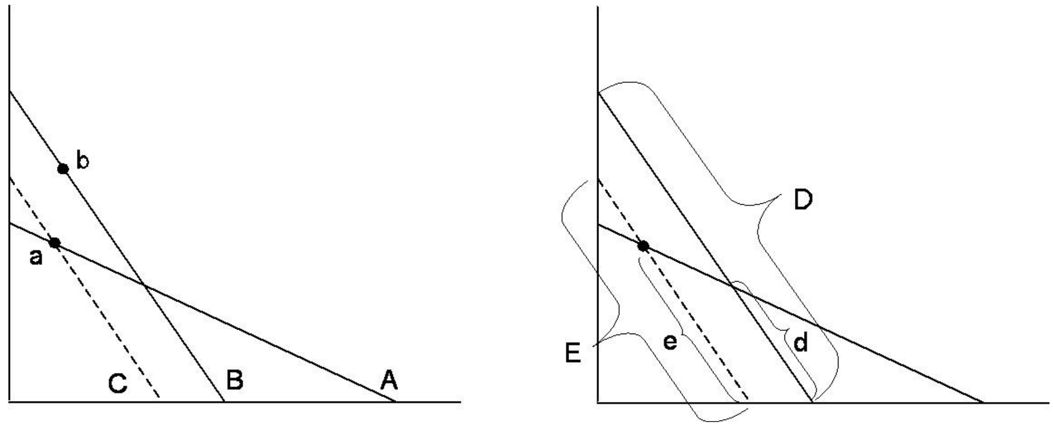


Figure 12: The Optimal Placement Index
The Optimal Placement Index measures the power of the implemented test (d/D) relative to the power of an optimally designed test (e/E) assuming the Bronars' M1 alternative hypothesis of choice behavior on unobserved budget sets.

(BWPM) for budget set $B_{-\tau}$ conditional on the choice x_τ. Further, denoting by $\tilde{B}_{-\tau}$ the budget set with prices $p_{-\tau}$ and wealth $\tilde{m}_{-\tau} = p_\tau x_\tau$, we can see that the denominator of the index $\theta_{\tau,-\tau}$ corresponds to the BWPM for budget set $\tilde{B}_{-\tau}$ conditional on the choice x_τ. The Optimal Placement Index then represents a ratio of the BWPM measure under the experiment as currently designed to the maximum BWPM that would be attainable by shifting the income levels for a single budget set in the experiment.[19]

Note that if a choice lies on two budget constraints, then this approach will assign a directed OPI to that point of $\theta_{\tau,tau} = 1$. As shown above when calculating $\theta(b, a$ in the left hand panel of Figure 12, it is also possible for the index to take on a value of zero. Such a value arises here given choice b on budget set B because, emphex post, any choice along budget set A would be consistent with GARP. However, as with the Afriat Power Index, the Optimal Placement Index will always show power if at least one pair of choices can be ranked by revealed preference.

[19]As formulated, the Optimal Placement Index is only operable for WARP, not GARP. While it could in principle be generalized to GARP, taking the perspective of shifting all budget sets relative to one another would not be feasibly for computational reasons. Moreover, with only two goods it is impossible to have a violation of GARP without also having a violation of WARP, although this is not the case with more goods. As a result, this power index is more demanding of the budgets than GARP requires, implying that the true power of the test is likely to be higher than this index might imply.

Extending this notion to a setting with two budget sets and more than two goods is immediate by comparing the BWPM with the supremum attainable BWPM. By comparing the relative magnitude of the rejection region with the acceptance region, this analysis provides an analog to the difference power index presented in Beatty and Crawford (2011).

5.4.1 Aggregating the Optimal Placement Index Across Budget Sets

Since GARP tests analyze choices at more than two budgets, so we must aggregate the directed placement indices to provide a summary characterization of a given test's efficiency. To begin, for every budget t, calculate the value of $\theta_{t,\tau}$ for all $\tau \neq t$ other budgets (if a budget τ does not cross budget t or has zero power given the relative prices for any income level then $\theta_{t,\tau} = 0$).

There are two natural aggregations for the $\theta_{t,\tau}$ indices across τ for budget t. The first is to define $\theta_t^{MAX} = \max\{\theta_{t,1}, \theta_{t,2}, ..., \theta_{t,t-1}, \theta_{t,t+1}, ..., \theta_{t,T_i}\}$. This θ_t^{MAX} represents the least possible improvement in the test's power at that budget set, or equivalently, the maximum efficiency of those tests given the observed choice x_t. An alternative way to define the index would be to begin by averaging across the other budgets, defining $\theta_t^{AVG} = \frac{1}{T_i-1}\sum_{\tau\neq t}\theta_{t,\tau}$. This θ_t^{AVG} is the expected ratio of the BWPM under the currently designed experiment to the maximum of the BWPM attainable by changing the income for one randomly selected budget set given the observed choice x_t.

To construct an overall power index for individual i, we propose averaging across θ_t, defining $\theta^{\cdot} = \frac{1}{T_i}\sum_{t=1}^{T_i}\theta_t^{\cdot}$. This average represents the expected benefit to randomly selecting a reference budget set to use as the basis for shifting the experimental design.[20]

In aggregate, then, we have two definitions for an individual subject's Optimal Placement Index: the Average Budget OPI and the Maximum Budget OPI. The Average Budget OPI

[20] We also considered an aggregation that takes the maximum across θ_t, defining $\theta^{\cdot} = \max\{\theta_t^{\cdot}\}$. This aggregated $\theta^{\cdot}$ would represent the highest efficiency in terms of the BWPM for the experiment as currently designed compared to the maximum BWPM attainable by shifting one level of income for one budget set conditional on all observed choices. In practice, this maximum is often truncated at unity and is consequently relatively uninformative. Further, the aggregated index's value would be determined by the placement of other budget sets relative to only a single budget set, whereas taking the average incorporates information about the placement of all budget sets.

presents the most diffuse measure of power, but may be a bit too conservative as it requires every budget set to have power against every other budget set, weighing a heavy penalty against designs that include budget sets that are shifted outward. The Maximum Budget OPI balances this by evaluating the efficiency of the most efficient WARP test for all budgets, though at the cost of ignoring the frequency with which such an efficiency is achieved.

5.4.2 Strengths and Weaknesses of the Optimal Placement Index

The analysis above is based on the optimal placement of budget constraints only for predetermined price vectors. As Bronars has shown, the test that will expose the individual to the greatest chance for a GARP violation is the budget that puts the most area under the budget in question. If preferences are normal, then this same conclusion follows naturally from Proposition 1 of Blundell, Browning and Crawford (2003), while Beatty and Crawford (2011) establish a similar result by invoking Selton (1991)'s measure of predictive success. Since the price vector could be shifted to produce a budget set with a greater chance for a GARP violation, this constraint prevents the placement of these budget sets from being truly optimal.[21]

Underlying its definition, the optimal placement index implicitly assumes the Bronars measure for choices over non-observed budget sets. This assumption is maintained in Beatty and Crawford's difference power index. As Dean and Martin (2012) do with a bootstrap approach, we could leverage an approach discussed in the section 4 to take observed choices into account when defining the relative power under different placements.

Lastly, as with the Jittering and Afriat Power Indices, there is no objective magnitude for which the Optimal Placement Index indicates an "efficient test." As such, its interpretation requires an intuitive notion of efficiency, with Optimal Placement Index values across studies

[21]If we were to optimally choose relative prices as well as placement, the maximally efficient budget set would not be well defined as it would be arbitrarily close to the original budget set. We could define the supremum of the power for such an optimally placed budget set as the largest budget share in the consumption bundle chosen on that budget set. We could also define WARP violations subject to a maximum Afriat Confidence Index of some critical value. With more than two goods, such an optimally placed budget set itself would not be uniquely defined as there will be a continuum of budget sets with equivalent power. Despite this multiplicity, the maximum such power will be attained by any of these budget sets and Optimal Placement Index itself would remain well-defined.

being informative solely as an ordinal ranking for the efficiency of test designs.

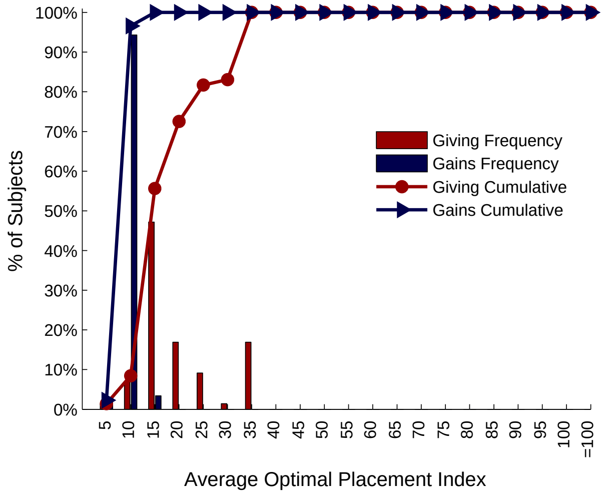
5.4.3 The Distribution of the Optimal Placement Index

Figure 13 shows the sample distributions of the Optimal Placement Index. The Average Optimal Placement Indices for both studies are quite small, with the median Average OPI for both experiments falling under 15%. The Maximum OPI shows the median participant in each experiment faced at least one budget with 55%-60% efficiency. Comparatively, the Average OPI indicates the experiment analyzing altruistic behavior was slightly more efficient than the study of risk preferences. The Maximum OPI is less stark in its ranking, but does have substantially more subjects whose Maximum OPI was above 75%. Again, subjects who keep all of the tokens for themselves appear as a mass in the distribution. With an OPI of 100%, these subjects having been given the most stringent test possible at all budget sets, but still have not violated GARP.

The Optimal Placement Index reveals very similar power properties to the Afriat Confidence Index. The experiment evaluating preferences over altruism, by taking account of focal features in that space (such as equality, equity, and selfishness), is able to generate a very tight test of GARP. In exploring preferences over risk, however, the lack of focal features results in a "looser" test. Under the Optimal Placement Index, this latter experiment would seem to have less power. However, the heterogeneity in preferences results in much more frequent violations, which is why the Afriat Confidence Index reveals these experiments to have relatively strong power despite this inefficiency.

6 A Field Guide to Characterizing Experimental Power

While the indices presented above all focus on measuring the power of a test's design, each does so from a rather different perspective. Bootstrapping the data helps characterize the role of heterogeneous preferences as potentially driving GARP violations. The Afriat Confidence Index and Jittering Index both characterize how close choice behavior in the experiment is to generating violations, though they differ in the metric that measures this closeness. The



(a) Average Optimal Placement Indices (b) Maximum Optimal Placement Indices

Figure 13: Distribution of Optimal Placement Indices
This chart presents the cross-sectional distribution of the Optimal Placement Indices (OPIs) for choices observed in the experimental studies by Andreoni and Miller (2002) and Andreoni and Harbaugh (2009). The Average OPI corresponds to the average ex post efficiency of the design in terms of BWPM for each budget. The Maximum OPI corresponds to the maximum ex post efficiency for each budget. For each individual, the OPI measures are averaged across all budgets, indicating the average efficiency of the design for that individual across budget sets.

Optimal Placement Indices only incorporates observed choices as an anchor to measure the efficiency of the design.

Considering multiple perspectives in evaluating the power of these tests gives a more detailed characterization of the features of observed choices that result in violations of GARP. However, these different perspectives, while all informative, may be redundant for some experimental contexts. As such, some guidance as to which measures to implement, and when, could help researchers seeking to take advantage of these measures.

The empirical results presented throughout the paper highlight the role of underlying preferences in determining the degree to which a power index accurately reflects the power of the experimental test of GARP. When preferences are concentrated around modal types, power measures that ignore individual heterogeneity (such as the unconditional bootstrap) can overstate the design's power. Similarly, if choices are clustered at corner solutions, the Optimal Placement Index may consider a relatively easy test to be perfectly efficient whenever the corner solution sits at the intersection of two budget sets. When choice behavior is more diffuse, as is the case with most experimental choice settings, it is more difficult to design "sharp" tests that maximize the power. However, even sight perturbations to observed choices can yield violations of revealed preference axioms, so these designs will still be rated highly by the Jittering Index and the Afriat Confidence Index.

Ideally, the power index should be matched with the source of variation in the data. For choice settings in which decisions are exposed to idiosyncratic noise, perhaps due to rounding or complexity in the problem, the Jittering Index or Afriat Confidence ought to provide similar characterizations of power. In contexts where choice behavior is relatively well structured but not very noisy, perhaps due to the presence of latent types with strong preferences, Optimal Placement Indices can characterize the efficiency of the experimental design and the weighted Bootstrap can illustrate how far you would need to relax this structure to yield violations.

As an initial metric to evaluate an experiment's power, we recommend the Afriat Confidence Index due to its ease of computation and close relationship with the Critical Cost Efficiency Index, a statistic that's already commonly reported for revealed preference tests. If an experiment's power is found to be lacking due to relatively high Confidence Indices, designers may wish to dig further, possibly starting with the Optimal Placement Index to characterize the design's efficiency. While the jittering and bootstrap measures are the most informative characterizations of power, their computational intensity would argue that they are best used in those settings where neither the Afriat Confidence nor the Optimal Placement Indices yield conclusive results.

As ex-post metrics, the techniques we've proposed are designed to evaluate the effectiveness of a design after its completion. To deploy these tools in the process of designing experiments, researchers could apply them to the outcomes of early pilot studies. Indices like the Optimal Placement Index can be particularly helpful in identifying adjustments to budget sets that may help enhance experimental efficiency. Similarly, measures like the Jittering Measure and the Afriat Power Index could indicate when and where additional budget sets could improve power. In this context, of course, samples will be very small, warranting a caveat that researchers should take care not to infer too much precision from small samples.

7 Discussion and Conclusion

This paper presents, analyzes, and compares several approaches to measuring the power of revealed preference tests. We first characterize different measures of choices by a representative individual in the population as well as conditional on the observed decisions and propose sampling strategies for drawing representative choice profiles from the population. We then distill these measures into statistics that characterize the power of the test both overall and at an individual-subject level.

In terms of measures over choice, our generalized conditional bootstrap and nonparametric kernel-based jittering measures provide novel mechanisms for characterizing the distribution of choice from observed behavior. Several approaches could extend these measures to budget sets that do not occur in the population, for instance, by sampling from budget shares in a manner similar to Dean and Martin (2012) but weighting the sampling measure by characteristics of the budget sets themselves.

In translating these measures into indices, we seek to average over the underlying choice process to develop a more intuitive characterization of power. Our most straightforward index inverts the well-known Afriat Efficiency Index into the Afriat Power and Confidence Indices, allowing us to characterize the degree to which our theory would need to be strengthened to be violated by observed choice behavior. The Optimal Placement Index, which asks how well the experimental design performed relative to the best possible design that could have been dynamically generated after each choice, provides a nice tool for comparing the efficiency of two different GARP tests. The Jittering index adopts a modicum of structure on the distribution of within-subject variation in choice, addressing the question of how noisy the data must be in order for a design to have power. The intuitive appeal of each of these metrics is tempered by the fact that there is no clear guidance on power or a natural threshold to appeal to as "high power." As such, beyond intuitive characterizations of when a test is “good enough,” the metrics are better thought of as ordinal rather than cardinal measures of power.

In sum, the tension in controlling for, and the parallels between measuring, goodness-of-fit and power are clear in revealed preference tests, whether using survey or experimental data. In this paper, we hope to have provided some guidance to researchers to both design and analyze tests that maximize our ability to make the correct inferences about economic models of maximizing behavior.

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Appendix: Design Details for Experimental Data

In this section, we describe the experimental studies used in the paper to illustrate the properties of different power measures and indices. The first experiment considers individual preferences for altruism, which are characterized by a set of "modal" preferences, where an individual's choices are commonly consistent with heterogeneous types. The second experiment looks at risk preferences, which are much more diffuse, as an individual's choices tend not to be driven by any sense of normative behavior. As such, the two studies illustrate two different settings in which to evaluate measurement of power for GARP: the first where the type of an individual puts a lot of structure on the data, the second where the individual choices are more fungible across budget sets.

Altruistic Preferences

The first sample we use is described in detail in Andreoni and Miller (2002) and Andreoni and Vesterlund (2001). Briefly, the experiment was designed to explore individual preferences for altruism by asking subjects to make a series of choices in a Dictator game, under varying incomes and costs of giving money to another subject. In particular, subjects made eight choices by filling in the blanks in statements like this: "Divide M tokens: Hold ____ at X points, and Pass ____ at Y points (the Hold and Pass amounts must sum to M)," where the parameters M, X, and Y were varied across decisions. The subject making the choice would receive the "Hold" amount times X, and another subject would receive the "Pass" amount times Y. All points were worth \$0.10.

Let π_s be payoff to self, and π_o be payoff to other. The hypothesis is that individuals have well-behaved preferences $U_s = U(\pi_s, \pi_o)$.The experimental parameters imply a budget constraint for any choice of

$$\frac{1}{X}\pi_s + \frac{1}{Y}\pi_o = M.$$

The parameters chosen provided the budgets shown in Figure 9. As can be seen, the pie to be divided ranged from \$4 to \$15 and the relative prices ranged from 3 to 1/3. After subjects made all 8 choices, one choice was selected at random by the experimenter and carried out.

Data was collected on 142 subjects and each subject's choices were tested for violations of GARP.[22] The result was that 13 of the subjects (9.1%) had violations of GARP. Applying the Afriat Efficiency Index, only 3 of these were found to be large violations (as we show below). This is a rather striking failure to contradict the neoclassical model of preferences, but leaves open the question of how discriminating the GARP test was at uncovering potential violations.[23]

Risk Preferences over Gains

In addition to the altruism study, we analyze the power properties for one of the treatments in Andreoni and Harbaugh (2009), who explore rationality of risk preferences and aversiveness over gains and losses. In their experiment, individuals face a lottery that has a probability p of winning $x > 0$, and wins zero otherwise. The subjects are offered to choose p and x from a linear budget, say

$$r_1 p + r_2 x = m, \text{ where } r_1, r_2, \text{ and } m > 0. \tag{12}$$

That is, to get a bigger prize, one has to accept a smaller chance of winning it. If these preferences over risk are rational and well-behaved, then $(p; x)$ choices should satisfy the axioms of revealed preference.

The experiment is split into two treatments. In the first treatment, the prize x is positive (representing gains) and subjects are asked to choose their most prefered combination of probability of winning and magnitude of the prize. In this setting, the standard formulation of revealed preferences and their implications hold and so we focus our analysis on that treatment. In the second treatment, the prize is negative (representing losses) and subjects are asked to choose their least prefered combination of risk and loss. Since the power properties of both treatments are quite similar, our analysis focuses on the first treatment, allowing us to forgo a discussion on the parallel axioms of revealed aversiveness.

[22] Andreoni and Miller (2002) report data on 176 subjects, but their session 5 is set aside here for brevity.

[23] Andreoni and Miller (2002) reported both the Bronars Method 1 power index and the panel index. We repeat them here for completeness.

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A THEORY OF DISAPPOINTMENT AVERSION

BY FARUK GUL[1]

An axiomatic model of preferences over lotteries is developed. It is shown that this model is consistent with the Allais Paradox, includes expected utility theory as a special case, and is only one parameter (β) richer than the expected utility model. Allais Paradox type behavior is identified with positive values of β. Preferences with positive β are said to be disappointment averse. It is shown that risk aversion implies disappointment aversion and that the Arrow-Pratt measures of risk aversion can be generalized in a straight-forward manner, to the current framework.

KEYWORDS: Preferences over lotteries, expected utility theory, independence axiom, risk aversion, Arrow-Pratt measures of risk aversion.

INTRODUCTION

THE PURPOSE OF THIS PAPER is to develop an axiomatic model of decision making under uncertainty that (i) includes expected utility theory as a special case, (ii) is consistent with the Allais Paradox, and (iii) is the most restrictive possible model that satisfies (i) and (ii) above.

The difficulty is in providing a precise sense in which (iii) can be satisfied. We propose to do this as follows: We will present an intuitive explanation of the Allais Paradox. Then we will replace the independence axiom of expected utility theory with an alternative axiom which explicitly incorporates our intuitive explanation. An additional axiom which does not conflict with the intuitive explanation or with expected utility maximization will also be imposed. Analysis of the resulting model will reveal that it does indeed satisfy (i) and (ii) above and that no further qualitative restriction can be imposed without violating either (i) or (ii). Our aim is to show that the type of behavior exhibited by a large number of subjects in Allais' original experiment can be interpreted intuitively and justified within the framework of a reasonable model.

With this in mind, in what follows we characterize preferences that are described completely by a real-valued function u on the set of prizes and a real number $\beta > -1$ (Theorem 1). We show that u is unique up to an affine transformation and β is unique. Hence we isolate a class of preferences that is one parameter richer than von Neumann-Morgenstern preferences. We further show that $\beta = 0$ corresponds to the case of expected utility theory (where u is the von Neumann-Morgenstern utility function). We describe preferences with $\beta \geqslant 0$ as disappointment[2] averse and establish the equivalence of strict disap-

[1] I am indebted to Elchanan Ben-Porath, Eddie Dekel, Darrell Duffie, David Kreps, Mark Machina, Dilip Mookherjee, Ariel Rubinstein, Hugo Sonnenschein, Robert Wilson, two anonymous referees, and especially Outi Lantto for their help and criticism.

[2] The term *disappointment* was first used by Bell (1985) and Loomes and Sugden (1986). While we have borrowed the word from them, our motivation and the class of preferences that we consider are different.

pointment aversion (i.e. $\beta > 0$) and Allais Paradox type behavior (Theorem 2). Finally we show the relationship between risk aversion and disappointment aversion (Theorems 3–5). In particular we show that in this model risk aversion implies disappointment aversion.

THE ALLAIS PARADOX[3]

Consider an individual who is faced with the following two choice problems:

PROBLEM 1: Choose either p_1 or p_2 where p_1 is a degenerate lottery which yields 200 dollars for sure and p_2 is a lottery that yields 300 dollars with probability .8 and 0 dollars with probability .2.

PROBLEM 2: Choose either $\bar{p}_1$ or $\bar{p}_2$ where $\bar{p}_1$ is a lottery which yields 200 dollars with probability .5 and 0 dollars with probability .5 and $\bar{p}_2$ is a lottery which yields 300 dollars with probability .4 and 0 dollars with probability .6.

The propensity of decision makers to choose p_1 if confronted with the first problem and $\bar{p}_2$ if confronted with the second, is a phenomenon that is now widely known as the Allais Paradox. The term *paradox* is due to the fact that such preferences are not consistent with expected utility maximization.[4] In particular, this pair of choices is inconsistent with the independence axiom, which is a necessary condition for expected utility maximization. The independence axiom states that given any three lotteries p_1, p_2, and r and a number $\alpha \in (0, 1]$, p_1 is preferred to p_2 implies $\alpha p_1 + (1-\alpha)r$ is preferred to $\alpha p_2 + (1-\alpha)r$ (where $\alpha p_1 + (1-\alpha)r$ denotes the lottery which yields any prize x with probability $\alpha p_1(x) + (1-\alpha)r(x)$).

Letting r be the lottery which yields 0 dollars for sure, α equal $\frac{1}{2}$, and observing that $\bar{p}_1 = \alpha p_1 + (1-\alpha)r$, $\bar{p}_2 = \alpha p_2 + (1-\alpha)r$ establishes that the Allais Paradox above constitutes a violation of the independence axiom. Observe that in Problem 1, lottery p_1 has no chance of yielding a disappointing outcome whereas lottery p_2 has a .2 chance of yielding a disappointing outcome. One possible explanation of why the independence axiom fails in this particular example is that the lottery with a lower probability of disappointment suffers more when it is mixed with an inferior lottery (i.e., $r = 0$ dollars for sure); that is, if the lotteries were nearly indifferent initially, the lottery with the higher probability of disappointment becomes preferred after being mixed with the inferior lottery.

[3] This is not Allais' (1979) most famous example. This particular version is sometimes called the "Allais Ratio Paradox." It is also referred to as the common ratio effect or common consequence effect by Kahnemann and Tversky (1979). We use it here because the intuitive explanation of the type we wish to isolate is easier to express in terms of this slightly simpler example. However, the same intuitive argument applies to both this and the original version of the Allais Paradox and the notion of disappointment aversion resolves both versions.

[4] To check this note that $p_1 \succ p_2$ implies $u(200) > .8u(300) + .2u(0)$ and $\bar{p}_1 \prec \bar{p}_2$ implies $.5u(200) + .5u(0) < .4u(300) + .6u(0)$; i.e. $u(200) < .8u(300) + .2u(0)$, a contradiction.

In the words of Savage (1972, page 102), "Many people prefer Gamble 1 (p_1) to Gamble 2 (p_2) because, speaking qualitatively, they do not find the chance of winning a very large fortune in place of receiving a large fortune outright adequate compensation for even a small risk of being left in the status quo. Many of the same people prefer Gamble 4 ($\bar{p}_2$) to Gamble 3 ($\bar{p}_1$); because, speaking qualitatively, the chance of winning is nearly the same in both gambles, so the one with the much larger prize seems preferable." While 300 and 200 dollars hardly qualify as very large and large fortunes, it is clear that Savage's interpretation of the original version is closely related to our intuitive explanation here. What Savage calls the chance of winning is one minus what we have called the probability of disappointment which we will define formally in our model.

Before we begin our formal analysis two basic questions need to be addressed. First, why concentrate on the Allais Paradox as opposed to other systematic violations for the expected utility hypothesis? Second, what distinguishes our approach from other axiomatic models of choice under uncertainty that allow for Allais Paradox type behavior?

There is a large body of work on observed violations of the expected utility model. Historically the Allais Paradox has played a very significant role in the development of this literature. This is no doubt in part due to the intuitive appeal of the Allais Paradox choices. Hence it would appear that the Allais Paradox is a natural starting point for any attempt at reconciling the normative theory of choice under uncertainty with the existing empirical evidence.

One can identify at least three distinct ways that the non-expected utility literature has dealt with observed violations of the expected utility theory:

(a) By emphasizing the need for a purely descriptive theory. Such work has either attempted to describe the actual decision making process that is used by the subjects (see Kahneman and Tversky (1979) and Rubinstein (1988)) or to identify useful (i.e., consistent with the existing empirical evidence) functional forms. Regret theory (Bell (1982), Loomes and Sugden (1962)), the disappointment theory of Bell (1985) and Loomes and Sugden (1982), the subjective expected value models used in the psychology literature (see Edwards (1953) and Tversky (1967) among others) are some of the many examples that can be included under this category. What is common to this particular body of work is the emphasis on descriptive aspects and skepticism regarding relevance of a normative theory. Hence the models mentioned above often violate even the most basic desiderata of choice under uncertainty (transitivity, stochastic dominance, etc.).

(b) By rejecting the normative appeal of the independence axiom. Allais (1979) and Machina (1982) belong in this category. Allais argues for a cardinal measure of utility over sure prospects and postulates that individuals' utility for uncertain prospects will depend on the distribution of the cardinal measure, typically its first three moments. Machina (1982) considers preferences that can be represented by a "smooth" preference functional and develops the machinery for analyzing the local properties of a preference functional. He offers two empirical hypotheses which he states in terms of these local properties. The first is risk aversion. The second which is called Hypothesis II is shown to imply behavior consistent with the Allais Paradox and a number of other observed violations of the expected utility theory. A more detailed comparison between the model of this paper and Machina's generation of expected utility theory will be provided after the formal analysis of the next section.

(c) By modifying the independence axiom. This class of papers starts by offering similar (typically weaker) alternatives to the independence axiom. The resulting model is defended by pointing out that it is consistent with observed violations of expected utility theory and by arguing that the alternative assumption is more compelling than the independence axiom. Some examples of this type of work are: Chew and MacCrimmon (1979), Dekel (1986), Fishburn (1983), and Yaari (1987).

This paper belongs among the work cited under (c) above. What distinguishes the model of this paper is our emphasis on the Allais Paradox and the direct role it plays in our axiomatization. Hence we provide a narrow interpretation of the Allais Paradox and search for a generalization of expected utility theory which is consistent with this interpretation and yet allows us to retain as much of the insight offered by expected utility theory as possible.

The Model

For some b, w such that $b > w$, let $X = [w, b]$ be the set of all prizes. Let $\mathscr{L}$ be the set of all simple lotteries over these prizes. That is, $p \in \mathscr{L}$ implies that $\operatorname{supp}(p)$, the support of p is finite. For any $p, q \in \mathscr{L}$ and $\alpha \in [0,1]$, $\alpha p + (1-\alpha)q$ denotes the lottery $r \in \mathscr{L}$ such that for all $x \in X$, $r(x) = \alpha p(x) + (1-\alpha)q(x)$. When there is no risk of confusion we use $x \in X$ to denote the lottery p such that $p(x) = 1$. $\succsim$ is a binary relation on $\mathscr{L}$. We use $p \succ q$, "p is strictly preferred to q", to denote $p \succsim q$ and not $q \succsim p$. We use $p \sim q$, "p is indifferent to q", to denote $p \succsim q$ and $q \succsim p$.

Since, typically, we want to interpret $x \in X$ as a quantity of money, $x > y$ iff $x \succ y$, will be a maintained assumption throughout this paper.

Definition 1: For any $\succsim$ and p, let

$$B(p, \succsim) = \{q \in \mathscr{L} \mid x \in \operatorname{supp}(q) \text{ implies } x \succsim p\};$$

$$W(p, \succsim) = \{q \in \mathscr{L} \mid x \in \operatorname{supp}(q) \text{ implies } p \succsim x\}.$$

We sometimes use $B(p), W(p)$ instead of $B(p, \succsim)$ and $W(p, \succsim)$.

Thus $B(p)$ and $W(p)$ denote the set of lotteries with supports consisting of prizes respectively, better than and worse than p.

DEFINITION 2: (α, q, r) is an *elation/disappointment decomposition* (EDD) of p iff $q \in B(p)$, $r \in W(p)$ and $\alpha q + (1-\alpha)r = p$.

Thus an EDD of p is constructed as follows: The lottery is divided into two parts, those prizes which are preferred to the certainty equivalent of p (called elation prizes) and those prizes which are less preferred to the certainty equivalent of p (called disappointment prizes). Then we normalize by dividing the probability of all elation prizes by α, the sum of all elation prize probabilities and obtain q. Similarly we divide all disappointment prize probability by $1-\alpha$ and obtain r. Hence $\alpha q + (1-\alpha)r = p$ (note that $\alpha q + (1-\alpha)r$ is p, not just indifferent to p). Obviously if the certainty equivalent of p is not in the support of p there is a unique EDD for p. Otherwise there will be an infinity of EDD's for p. To see this note that $(.2, x, x)$, $(.7, x, x)$ and $(0, b, x)$ are all EDD's of x.

Next we define elation, $e(p)$, and disappointment, $d(p)$, probabilities for a lottery p. Note that if p does not yield its certainty equivalent with positive probability, then $e(p) + d(p) = 1$ and $D(p) = \{(e(p), q, r)\}$ for some $q, r \in \mathscr{L}$.

DEFINITION 3: $e(p) \equiv \Sigma_{x \succ p} p(x)$ and $d(p) \equiv \Sigma_{p \succ x} p(x)$.

We use $D(p)$ to denote the set of all EDD's of p. Instead of $(\alpha, q, r) \in D(\alpha q + (1-\alpha)r)$ or equivalently $(\alpha, q, r) \in D((\alpha, q, r))$ we simply write $(\alpha, q, r) \in D$ where $D = \bigcup_{p \in \mathscr{L}} D(p)$. (Note that by definition if (α, q, r) is an EDD it must be an EDD of $\alpha q + (1-\alpha)r$.)

AXIOM 1—Preference Relation: $\succsim$ *is complete and transitive.*

AXIOM 2—Continuity: *For all* $p \in \mathscr{L}$ *the sets* $\{q \in \mathscr{L} \mid q \succsim p\}$ *and* $\{q \in \mathscr{L} \mid p \succsim q\}$ *are closed (under the topology generated by the* L^1 *metric).*[5]

Axiom 2 implies that the function CE: $\mathscr{L} \to [w, b]$ such that $CE(p) \sim p$ (i.e. CE is the certainty equivalent of p) is well-defined.

Next we will present a restriction of the independence axiom to the case in which the disappointment probabilities of the lotteries p_1 and p_2 are the same and no elation (disappointment) prize of p_i switches over to being a disappointment (elation) prize of $ap_i + (1-a)x$. The motivation for this is the intuitive explanation of the Allais Paradox that was presented earlier. Consider the lotteries $p_i^t = tp_i + (1-t)x$ as t decreases from 1 to a. If $p_1 \succsim p_2$ and no elation (disappointment) prize of p_i^t switches to being a disappointment (elation) prize, then the disappointment probabilities of p_1^t and p_2^t are always the same; hence our intuitive explanation of the Allais Paradox is not applicable so we would expect the conclusion of the independence axiom to be valid.

[5] To be more precise, let f: $\mathscr{L} \to \mathscr{L}^1$ where $f(p)$ is the cdf associate with p. Then, Axiom 2 requires that $f(\{q \in \mathscr{L} \mid q \succsim p\})$ and $f(\{q \in \mathscr{L} \mid p \succsim q\})$ are closed (for every $q \in \mathscr{L}$) in the relative topology on $f(\mathscr{L})$ generated by the L^1 metric.

Axiom 3—Weak Independence: *$p_1 \succsim p_2$, $a \in [0,1]$, $z \in X$ implies $ap_1 + (1-a)z \succsim ap_2 + (1-a)z$ whenever there exists $(\lambda, q_i, r_i) \in D(p_i)$ such that $q_i \in B(ap_i + (1-a)z)$ and $r_i \in W(ap_i + (1-a)z)$ for $i = 1, 2$.*

It can be seen from the proof of Theorem 1, that Axioms 1–3 imply betweenness. That is if $\succsim$ satisfies Axioms 1–3 and $p \succ q(p \sim q)$, then $p \succ \alpha p + (1-\alpha)q \succ q(p \sim \alpha p + (1-\alpha)q)$ for all $\alpha \in (0,1)$. Hence preferences which satisfy Axioms 1–3 belong to the class studied by Dekel (1986).

Axiom 3 captures our intuitive explanation of the Allais Paradox by enabling the independence axiom to fail when disappointment effects are present. However, in order to verify the "minimality" criterion (iii) discussed in the introduction, we need to determine if the class of preferences which are characterized by Axioms 1–3 can be restricted further without excluding expected utility preferences of our intuitive explanation. To put it differently, are there situations in which the independence axiom would be applicable but our intuitive explanation would not? Consider the following example:

Let $\alpha x + (1-\alpha)w \succsim \alpha p + (1-\alpha)w$ and $p \in B(\alpha p + (1-\alpha)w)$. Thus the decision-maker prefers substituting x in place of p in $\alpha p + (1-\alpha)w$ when p consists of elation prizes of $\alpha p + (1-\alpha)w$. Now assume that $p \in W(\alpha b + (1-\alpha)p)$. Hence p consists of disappointment prizes of $\alpha b + (1-\alpha)p$. Note that the independence axiom would imply that $\alpha b + (1-\alpha)x \succsim \alpha b + (1-\alpha)p$ whenever $\alpha x + (1-\alpha)w \succsim \alpha p + (1-\alpha)w$. Furthermore observe that $\alpha b + (1-\alpha)x$, $\alpha b + (1-\alpha)p$, $\alpha x + (1-\alpha)w$, and $\alpha p + (1-\alpha)w$ all have the same disappointment probability $(1-\alpha)$. Thus substituting x in place of p does not result in the type of effect discussed in our intuitive explanation of the Allais Paradox. Hence we would again expect the independence axiom to hold (i.e., $\alpha b + (1-\alpha)x \succsim \alpha b + (1-\alpha)p$). This will be Axiom 4. To see why this particular application of the independence axiom is not covered by Axiom 3, note that by requiring that no elation prize switches to being a disappointment prize, Axiom 3 severs the connection between the individual's evaluation of elation prizes and his evaluation of disappointment prizes.

Axiom 4—Symmetry: *For $i = 1, 2$, $(\alpha, p_i, w), (\alpha, b, p_i) \in D$ implies*

$$\alpha p_1 + (1-\alpha)w \succsim \alpha p_2 + (1-\alpha)w \qquad \textit{iff}$$

$$\alpha b + (1-\alpha)p_1 \succsim \alpha b + (1-\alpha)p_2.$$

Theorem 1: *$\succsim$ satisfies Axioms 1–4 if and only if there exist functions u: $X \to \Re$ and γ: $[0,1] \to [0,1]$ such that*: (i) *$(\alpha_i, q_i, r_i) \in D(p_i)$ for $i = 1, 2$ implies $p_1 \succsim p_2$ iff $\gamma(\alpha_1)\Sigma_x u(x)q_1(x) + (1-\gamma(\alpha_1))\Sigma_x u(x)r_1(x) \geqslant \gamma(\alpha_2)\Sigma_x u(x)q_2(x) + (1-\gamma(\alpha_2))\Sigma_x u(x)r_2(x)$*; (ii) *$\gamma', u'$ satisfy* (i) *above implies $u' = au + b$ for some $a > 0$, $b \in \Re$ and $\gamma' = \gamma$*; (iii) *u is continuous and there exists $\beta \in (-1, \infty)$ such* *that*

$$\gamma(\alpha)=\frac{\alpha}{1+(1-\alpha)\beta} \qquad \textit{for all} \qquad \alpha\in[0,1].$$

PROOF: See Appendix.

Theorem 1 establishes that if Axioms 1–4 hold, then there exists a utility function V: $\mathscr{L}\to\mathfrak{R}$ which represents $\succsim$ and furthermore $V(p)$ can be calculated by taking an EDD (α,q,r) of p, computing the expected utilities of the elation and disappointment parts (q and r respectively) with respect to the utility index u, and taking a $\gamma(\alpha)$ weighted average of these utilities. Hence u and β are parameters of the individual's preferences and $V(p)$ is defined implicitly by the procedure above. To see that $V(p)$ is not explicitly defined note that the certainty equivalent of p needs to be known in order to determine an EDD of p.

However, a simple and finite algorithm (see Appendix) will enable us to construct all EDD's of p and commute $V(p)$ for arbitrary u and β.

The fact that $V(p)$ is well defined for any $\succsim$ which satisfies Axioms 1–4 is guaranteed by Theorem 1. However, this does not preclude the possibility that there might be no non-expected utility preference which satisfies Axioms 1–4. Defining $V(u,\beta,p)$ implicitly by $V(u,\beta,p)=\gamma(\alpha)Eu(q)+(1-\gamma(\alpha))Eu(r)$ for some α,q,r such that $\alpha q+(1-\alpha)r=p$ and $x\in\operatorname{supp}(q)$ implies $u(x)\geqslant V(u,\beta,p)$ and $x\in\operatorname{supp}(r)$ implies $u(x)\leqslant V(u,\beta,p)$ and showing that $V(u,\beta,\cdot)$ is a well defined function for arbitrary strictly increasing, continuous u and $\beta\in(-1,\infty)$ would establish that Axioms 1–4 characterize a rich class of preferences. This can be done using simple manipulations of the definition of $V(u,\beta,p)$.

Observe that expected utility theory corresponds to the special case $\gamma(\alpha)=\alpha$; that is, $\beta=0$. Furthermore, if $\beta>0$, then $\gamma(\alpha)<\alpha$ for all $\alpha\in(0,1)$ and $\gamma(\alpha)$ is convex. If $-1<\beta<0$ then $\gamma(\alpha)>\alpha$ for all $\alpha\in(0,1)$ and $\gamma(\alpha)$ is concave. We say that $\succsim$ is disappointment averse if $\beta\geqslant 0$ and $\succsim$ is elation loving if $\beta\in(-1,0]$. Note that unlike risk aversion, disappointment aversion is, by definition, a global property. Theorem 2 below (and its proof) reveals that β is a measure of the extent to which $\succsim$ is prone to Allais Paradox type behavior. Since the preferences which satisfy Axioms 1–4 are only one-parameter (β) richer than expected utility preferences, it would appear that no additional qualitative restrictions can be imposed without excluding either our intuitive explanation of the Allais Paradox or certain expected utility preferences.

THEOREM 2: *Let $\succsim$ satisfy Axioms* 1–4 *and $p\sim q$. Then if $\beta>0$ ($\beta<0$) there exists $\bar{a}>0$ such that* (i) *$a<\bar{a}$, $e(p)>e(q)$ implies $ax+(1-a)p\succ(\prec)ax+(1-a)q$ for $x\succ p$;* (ii) *$a<\bar{a}$, $d(p)>d(q)$ implies $ax+(1-a)p\succ(\prec)ax+(1-a)q$ for $p\succ x$.*

PROOF: See Appendix.

Let (u, β) denote the generic preference satisfying Axioms 1–4.

Define

$$\phi(x,v)=\begin{cases} u(x) & \text{for } x \text{ such that } u(x)\leqslant v,\\ \dfrac{u(x)+\beta v}{1+\beta} & \text{for } x \text{ such that } u(x)>v.\end{cases}$$

Observe that by using the definition of $v(p)$ provided in Theorem 1 we obtain that $\Sigma_x \phi(x,v)p(x)=v$ iff $V=v(p)$, hence ϕ is the local utility function for the preference (u,β) (see Dekel (1986) for the definition and analysis of local utility functions of this form).

Roughly speaking, given Axioms 1, 2, Axiom 3 guarantees that the local utility function has the following property: All elation prizes are evaluated with respect to one utility function and all disappointment prizes are evaluated with respect to another utility function. Symmetry (Axiom 4) guarantees that these utility functions represent the same preferences. That is, the utility function for elation prizes $(u(x)+\beta v)/(1+\beta)$ is a (positive) affine transformation of the utility function for disappointment prizes, $u(x)$.

Abandoning the symmetry (Axiom 4) assumption would lead to the following local utility function:[6]

$$\phi(x,v)=\begin{cases} u_d(x) & \text{for } x \text{ such that } u_d(x)\leqslant v,\\ u_e(x)-u_e\left(u_d^{-1}(v)\right)+v & \text{for } x \text{ such that } u_d(x)>v,\end{cases}$$

where u_d, u_e are two distinct functions from $[w,b]$ to $\Re$. Note that Axiom 4 implies $u_e-u_e(u_d^{-1}(v))+v$ and hence u_e, is an affine transformation of u_d and expected utility implies $u_e=u_d$.

It can be shown that for preferences which satisfy Axioms 1–4, $\beta\geqslant 0$ iff $d(\Sigma\phi(x,v)p(x))/dv$ is an increasing function of $d(p)$, the probability of disappointment (among p such that $V(p)=v$). This observation can be used to extend the notion of disappointment aversion to preferences which satisfy Axioms 1–3. For such preferences $d(\Sigma\phi(x,v)p(x))/dv$ is a decreasing function of $d(p)$ iff $(u_e'(y)/u_d'(y))<1$ for y such that $u_d(y)=v$. But now disappointment aversion has become a local property and global disappointment aversion can be imposed by requiring disappointment aversion at every point y. With this extended definition of disappointment aversion the results that Allais Paradox implies disappointment aversion (Theorem 2) and that risk aversion implies disappointment aversion (Theorem 3) can be generalized to preferences which satisfy Axioms 1–3.

We end this section by noting that imposing Axiom 4 is consistent with our objective of seeking a model in which any deviation from expected utility theory can be ascribed to disappointment aversion. Furthermore adding Axiom 4

[6] The existence of some local utility function is guaranteed by Dekel's (1986) Proposition 1 and the fact that Axioms 1–3 imply betweenness. Furthermore Axiom 3 implies that (local) preferences over disappointment outcomes and elation outcomes (but not combinations of disappointment and elation outcomes) are independent of v. Then a suitable normalization yields the representation provided above.

enables us to analyze a particular simple subclass of the preferences that satisfy Axioms 1–3 for which we can obtain a nearly closed form representation.

DISAPPOINTMENT AVERSION AND RISK AVERSION

In this section we analyze the relationship between disappointment aversion and risk aversion and develop measures of risk aversion for preferences satisfying Axioms 1–4. Hence, in what follows we will concentrate only on preferences satisfying Axioms 1–4 and sometimes use (u, β) to denote such preferences.

THEOREM 3: *(u, β) is risk averse (in the sense of weakly not preferring mean-preserving spreads) iff $\beta \geqslant 0$ and u is concave.*

PROOF: Dekel (1986) establishes that $\succsim$ is risk averse iff the local utility function is concave. Note that $\phi(x, v)$ is concave if $\beta \geqslant 0$ and u is concave. *Q.E.D.*

There are two main implications of Theorem 3. The first one is that risk aversion implies disappointment aversion. The second is that disappointment aversion and the concavity/convexity of u determine the individual's attitude towards risk.

The possibility of having concave u and $\beta < 0$ or convex u and $\beta > 0$ enables us to obtain preferences that display risk aversion with respect to certain types of gambles and risk loving with respect to others. For example, if $\beta = 4$ and $u(x) = x$ for $x \leqslant 0$ and $u(x) = 5x$ when $x > 0$ (hence x is convex), then the individual will be risk averse with respect to even chance gambles and gambles which yield a large loss with small probability but will be risk loving with respect to gambles that involve winning a large prize with small probability if his initial income is low. Hence, there are preferences consistent with Axioms 1–4 such that at all income levels, the individual would not accept fair even chance gambles, yet would still be willing to buy less than fair insurance. Furthermore such an individual would be willing to buy, at certain income levels, tickets to the state lottery.

It is possible to develop measures of absolute, relative, and comparative risk aversion for preferences satisfying Axioms 1–4, similar to those developed by Arrow and Pratt for expected utility theory. Of course, these measures coincide with the corresponding Arrow-Pratt measures when $\beta = 0$. However, it is interesting that essentially the same Arrow-Pratt measures are appropriate even when $\beta \neq 0$. More specifically, let $R_u^a(x) = -u''(x)/u'(x)$ and $R_u^r(x) = -xu''(x)/u'(x)$; then by noting that β does not depend on x and essentially replicating the corresponding proofs of expected utility theory we obtain the following theorem.

THEOREM 4: *(u, β) is increasingly (decreasingly, constant) absolute (relative) risk averse iff $R^a(R^r)$ is increasing (decreasing, constant).*

DEFINITION 4: $\succsim_1$ is more risk averse than $\succsim_2$ iff $p \succsim_1 x$ implies $p \succsim_2 x$ for all $p \in \mathscr{L}$, $x \in X$.

THEOREM 5: *(u_1, β_1) is more risk averse than (u_2, β_2) if $\beta_1 \geqslant \beta_2$ and $R^a_{u_1}(x) \geqslant R^a_{u_2}(x)$ for all $x \in (w, b)$. Furthermore if (u_1, β_1) is more risk averse than (u_2, β_2), then $\beta_1 \geqslant \beta_2$.*

PROOF: See Appendix.

Machina (1984) provides the following stronger notion of comparative risk aversion: $\succsim_1$ is more risk averse than $\succsim_2$ iff for all $\lambda \in [0,1]$, $p, p' \in \mathscr{L}$; $\lambda p' + (1-\lambda)p \sim_2 \lambda p' + (1-\lambda)\bar{x}$ and $\lambda p' + (1-\lambda)p \sim_1 \lambda p' + (1-\lambda)x$ implies $x \leqslant \bar{x}$.

Given Theorem 3 and the observation that β and the curvature of u determines the curvature of the local utility function, we would expect that being more risk averse corresponds to having a higher β and a more concave u. This is almost correct in the sense that (u_1, β_1) is more risk averse (in the stronger sense) than (u_2, β_2) iff $R^a_{u_1}(x) \geqslant R^a_{u_2}(x)$ for all $x \in (w, b)$ and $\beta_1 \geqslant 0 \geqslant \beta_2$. To see why $\beta_1 \geqslant 0 \geqslant \beta_2$ cannot be replaced by $\beta_1 \geqslant \beta_2$, note that if $\beta_1 \geqslant \beta_2 > 0$,[7] then both (u_1, β_1) and (u_2, β_2) are risk averse (Theorem 3). Furthermore the certainty equivalent y_2, of $\lambda p + (1-\lambda)p'$ for (u_2, β_2) is greater than its certainty equivalent y_1, for (u_1, β_1) (Theorem 5). Then, at y_2, the local utility function ϕ_1 is smooth whereas the local utility function ϕ_2 has a concave kink. This means that ϕ_1 is less concave than ϕ_2 at y_2. Machina (1982), however, shows that more risk aversion is equivalent to the greater concavity of the local utility function.[8] Thus the comparison fails. Intuitively the fact that $y_2 > y_1$ makes it possible for (u_2, β_2) to view p' as increasing the probability of disappointment while (u_1, β_1) views p' as decreasing the probability of disappointment and therefore is less reluctant to accept it.

OTHER MODELS ON DECISION MAKING UNDER UNCERTAINTY AND EXPERIMENTAL EVIDENCE

It should be noted that although we have so far concentrated on the case where X is a compact interval, it is clear that the type of preference that we have been considering can just as well be defined on $\mathscr{L}$ when X is either finite or an unbounded interval and $\mathscr{L}$ includes nonsimple lotteries. All of the qualitative conclusions of this paper (Theorems 2–5) would still hold. If X is finite, however, Axioms 1–4 are not sufficient to characterize (u, β) preferences. This is due to the fact that our proof necessitates that certainty equivalents be well defined. The case of unbounded X and/or nonsimple lotteries can be dealt with as is done in expected utility theory. For nonsimple lotteries a finite algorithm for explicitly computing $V(p)$ would no longer exist. Instead an algorithm which converges to $V(p)$ can be constructed.

The most striking feature of these preferences is that by adding only one new variable, β, to the expected utility model, we are able to construct a class of

[7] A symmetric argument applies to the case $0 > \beta_1 \geqslant \beta_2$.

[8] Theorem 4 of Machina (1982) assumes the global differentiability of local utility functions. However, similar arguments can be used for the preferences considered here.

preferences which include the EU model as a special case, are compatible with many of the observed violations of this model, and enable us to explain these violations in terms of the notion of disappointment aversion. Observe that when we are comparing binary lotteries with each other or binary lotteries with sure things, then the functional form we have considered can be expressed as

$$V(ax+(1-a)z)=\pi_1(a)u(x)+\pi_2(1-a)u(z)$$
$$=\frac{a}{1+(1-a)\beta}u(x)+\frac{(1-a)(1+\beta)}{1+(1-a)\beta}u(z)$$

where $x \geqslant z$ and obviously $\pi_2(1-a)=1-\pi_1(a)$.

But this is very similar to the subjective expected utility models that have been used extensively in the psychology and economics literature (see, for example, Kahneman and Tversky (1979)) and is a special case (for binary lotteries) of Quiggin (1982).

Note also that for the case of $\beta > 0$, the weight function of the good prize is convex. Hence, a small increase in the probability of the good prize increases utility much more, when the chance of getting the good prize is already high. This is very suggestive of what Kahneman and Tversky (1979) refer to as the tendency for "people [to] overweight outcomes that are considered certain, relative to outcomes that are merely probable," i.e., the so-called certainty effect. The class of (u, β) preferences also have the following feature: If X is finite and $|X|=n$, then the preferences of any individual can be determined uniquely by asking him $n-1$ simple questions and solving a quadratic equation. This is only one more than the number of questions one would need to ask under the assumptions of expected utility theory.

Consider a person, with preferences (u, β), who will receive income x in state 1 and income y in state 2. Let α and $(1-\alpha)$ denote the probabilities of states 1 and 2 respectively (see Figure 1). An indifference curve for such a person will be described by the equation

$$\frac{\alpha}{1+(1-\alpha)\beta}u(x)+\frac{(1-\alpha)(1+\beta)}{1+(1-\alpha)\beta}u(y)=v \qquad \text{for } x \geqslant y$$

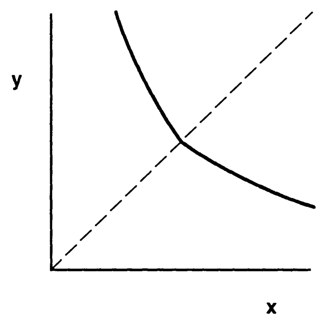

FIGURE 1

and

$$\frac{1-\alpha}{1+\alpha\beta}u(y)+\frac{\alpha(1+\beta)}{1+\alpha\beta}u(x)=v \qquad \text{for } y>x.$$

Hence

$$\frac{dy}{dx}=-\frac{\alpha u'(x)}{(1-\alpha)u'(y)(1+\beta)} \qquad \text{if } y<x,$$

$$\frac{dy}{dx}=-\frac{\alpha u'(x)(1+\beta)}{(1-\alpha)u'(y)} \qquad \text{if } y>x.$$

Thus if u is concave and $\beta>0$, then these preferences would look as in Figure 1 above. Note that the shape of the indifference curve on either side of the 45° line is determined by the curvature of u and the nature of the kink is determined by β. In particular there will be a kink so long as $\beta \neq 0$. Figure 1 shows why $\beta>0$ is necessary for risk aversion (see Theorem 3). This particular shape of the indifference curve has two important implications. The first is that risk averse individuals will purchase full insurance at less than fair odds; the second is that these preferences are not "differentiable" when $\beta \neq 0$ and hence do not belong to the class of preferences considered by Machina (1982).

Figure 2 illustrates the indifference map of (u,β) for lotteries over three prizes x, y, z where $x<y<z$. Hence any point (p_x, p_z) in Figure 2 corresponds to the lottery p such that $p(x)=p_x$, $p(y)=1-p_x-p_z$, and $p(z)=p_z$. After normalizing u so that $u(z)=1$ and $u(x)=0$, the indifference curve through any lottery p such that $p \succ y$ is defined by equation (1) and the indifference curve through any p such that $p \precsim y$ is defined by equation (2):

$$(1) \qquad p_z=\frac{(1+\beta)[u(y)p_x+v-u(y)]}{1+\beta v-(1+\beta)u(y)} \qquad \text{for } v>u(y),$$

$$(2) \qquad p_z=\frac{(u(y)+\beta v)p_x+v-u(y)}{1-u(y)} \qquad \text{for } v \leqslant u(y).$$

Both 1 and 2 can be derived from the definition of $V(p)$ provided in Theorem 1.

Equations 1 and 2 imply that the indifference curves on the top half of Figure 2 all intersect at the point $((1-u(y))/u(y)\beta, (1+\beta)/\beta)$ and indifference curves on the bottom half of Figure 2 all intersect at the point $(-1/\beta, -(1+\beta)u(y)/\beta(1-u(y)))$. Hence for $\beta>0$ all indifference curves in the bottom half are "fanning out" and all indifference curves in the top half are coming together from left to right (as depicted in Figure 2). For $\beta<0$ the opposite is true. Hence for the case of lotteries over three prizes, taking the preference (u,β) and "flipping" half of its indifference map, we obtain the type of preference considered by Chew and MacCrimmon (1979) and Fishburn

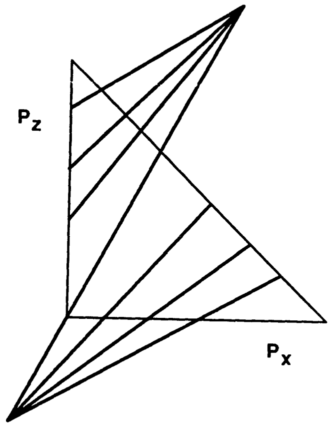

FIGURE 2

(1985) which are defined by the property that all indifference curves originate from the same point.

Machina (1982, 1987) considers a large class of systematic violations of the expected utility model and shows that these violations, which are frequently encountered in experiments, all imply that indifference curves in Figure 2 are fanning out. Since any individuals with preferences (u, β) will have indifference curves that fan out either in the top half or the bottom half (but not both) of Figure 2, no such individual can exhibit all of the violations considered by Machina.

It is undeniably true that fanning out over some range is necessary for Allias Paradox type behavior. The preference (u, β) will display the fanning out properly over the range concerning the Allais Paradox if and only if $\beta > 0$. Hence there is no conflict between Machina's observation regarding fanning out and Theorem 2. There are however substantial differences in the two interpretations of the Allais Paradox and the underlying approaches to violations of expected utility theory. Whereas Machina views the Allais Paradox as a special case of (global) fanning out, we have emphasized our narrower intuitive explanation and sought to provide a model which incorporates this intuitive explanation while retaining many of the features of expected utility theory. In the case of lotteries over three outcomes no two distinct lotteries can be indifferent and have the same disappointment probability. Thus Figure 2 understates the similarity between (u, β) preferences and expected utility theory. To see this, consider the following example: Suppose $\succsim$ satisfies

$$(3) \qquad \frac{1}{2} \times \left(\frac{1}{2} \times 1000 + \frac{1}{2} \times 800\right) + \frac{1}{2} \times 100 \sim \frac{1}{2} \times 880 + \frac{1}{2} \times 100 \sim 400;$$

then Axioms 1–4 imply that $\succsim$ satisfies

$$(4) \qquad \frac{1}{2} \times \left(\frac{1}{2} + 1000 + \frac{1}{2} \times 800\right) + \frac{1}{2} \times 50 \sim \frac{1}{2} \times 880 + \frac{1}{2} \times 50.$$

Under Axioms 1–4 the necessity of satisfying (4) whenever (3) is satisfied does not conflict in the displaying Allais Paradox type behavior at every income level. Under Machina's interpretation any individual who displays Allais type behavior at every income level and satisfies (3) must violate (4). This follows immediately from fanning out.

Not surprisingly there is some empirical and experimental evidence conflicting with Axioms 1–4 and Hypothesis II. In particular for $\beta > 0$ (which we consider to be the more important case), (u, β) will be consistent with the common ratio effect, partly consistent with the common sequence effect (including the Allais Paradox), and inconsistent with the common ratio effect with negative numbers (see Machina (1987)). Conversely for $\beta < 0$, (u, β) will be consistent with the common ratio effect with negative numbers, partly consistent with the common consequence effect (excluding the Allais Paradox), and inconsistent with the common ratio effect.

Neilson (1989) considered lotteries over three prizes and concludes that existing empirical evidence suggests the need for preferences which fan in on the top part and fan out on the bottom part of the probability triangle (i.e., exactly the situation depicted by Figure 2). The model of this paper (for $\beta > 0$) always has this property. To put it another way, we have identified Allais Paradox with precisely this mixed-fanning property.

CONCLUSION

We have taken what is considered to be the most compelling argument against the independence axiom and attempted to find an alternative to expected utility theory which is immune to this particular argument and yet retains as much of the expected utility theory as possible. The notion of disappointment aversion offers good intuition as to why the independence axiom is so often violated. Axioms 1–4 aim to capture the notion of disappointment aversion that lead to a rather restricted class of preferences with acceptable normative properties capable of accommodating many of the experimental results. The simple characterization of these preferences suggest that they might constitute a useful step in better understanding the failure of the independence axiom.

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APPENDIX

PROOF OF THEOREM 1: The proof will make use of the following two lemmas.

LEMMA 1: (I) $x > y$, $\lambda \in (0,1)$ *implies* $x \succ \lambda x + (1-\lambda)y \succ y$.

(II) $\lambda, a \in (0,1)$, $y > z$, $p = ay + (1-a)z$ *imply* (1) $\lambda x + (1-\lambda)p \succ p$ *whenever* $x \succ p$; (2) $p \succ \lambda x + (1-\lambda)p$ *whenever* $p \succ x$; (3) $x \sim \lambda x + (1-\lambda)p$ *whenever* $x \sim p$; (4) $x \succ \lambda x + (1-\lambda)p$ *whenever* $x \succ p$; (5) $\lambda x + (1-\lambda)p \succ x$ *whenever* $p \succ x$.

(III) II *above holds for arbitrary* p.

PROOF: (I) Assume the contrary; then there exists λ such that $\lambda x + (1-\lambda)y \succsim x$ or $y \succsim \lambda x + (1-\lambda)y$. Take the first case and let $\alpha = \inf\{\lambda \in [0,1] | \lambda x + (1-\lambda)y \succsim x\}$. By Axiom 2 $\alpha \in (0,1)$ and $\alpha x + (1-\alpha)y \sim x$. Then Axiom 3 implies that $\alpha^2 x + (1-\alpha^2)y \sim \alpha x + (1-\alpha)y$ if $x \succsim \alpha^2 x + (1-\alpha^2)y \succsim y$. But this follows immediately from Axiom 3. Hence $\alpha^2 x + (1-\alpha^2)y \succsim \alpha x + (1-\alpha)y \succsim x$. But $\alpha^2 < \alpha$ which contradicts the fact that α is the infimum. A similar argument establishes a contradiction for the $y \succsim \alpha x + (1-\alpha)y$ case.

(II) (1) Assume the contrary; hence there exists $\lambda, a \in (0,1)$, $p = ay + (1-a)z$, and $x \in X$ such that $x \succ p$ and $p \succsim \lambda x + (1-\lambda)p$. Then by Axiom 2 there exists $\lambda^* \in (0,1)$ such that $\lambda^* x + (1-\lambda^*)p \sim p$. Let $T = \{(y,z) | ay + (1-a)z \sim p\}$ and $\Delta = \{y^* - z^* > 0 | y^* \succsim \lambda^* x + (1-\lambda^*)(ay^* + (1-a)z^*) \succsim z^*$ for $(y^*, z^*) \in T\}$. By assumption Δ is nonempty. Let $\delta = \inf \Delta$. Then by Axiom 2 there exist y^* such that $ay^* + (1-a)(y^* - \delta) \sim p$ and then, by Axiom 3, $\lambda^* x + (1-\lambda^*)(ay^* + (1-a)(y^* - \delta)) \sim p$. Therefore, by Axiom 2, $\delta = 0$; otherwise we can find $\bar{y}$ such that $\bar{y} \succsim \lambda^* x + (1-\lambda^*)(a\bar{y} + (1-a)(\bar{y} - \bar{\delta})) \succsim \bar{y} - \delta$, $a\bar{y} + (1-a)(\bar{y} - \bar{\delta}) \sim p$, and $0 < \bar{\delta} < \delta$, which contradicts the fact that $\delta = \inf \Delta$. But if $\delta = 0$, by Axiom 2, $\lambda^* x + (1-\lambda^*)y^* \sim p$ and $y^* \sim p$ which contradicts (I) above.

(2) follows from a symmetric argument.

(3) follows from (1) above and Axiom 2.

(4) If $x > y$, then set

$$\hat{p} = \frac{\lambda}{\lambda + (1-\lambda)a} x + \frac{(1-\lambda)a}{\lambda + (1-\lambda)a} y.$$

Then by (I) and (II) (2) above we have $\hat{p} \succ tz + (1-t)\hat{p}$ whenever $t \in (0,1)$. Set $t = (1-\lambda)(1-a)$ to obtain $\hat{p} \succ tz + (1-t)\hat{p} = \lambda x + (1-\lambda)p$. But $x \succ \hat{p}$ by (I), so $x \succ \lambda x + (1-\lambda)p$.

If $y \geqslant x$ then let $\alpha^* x + (1-\alpha^*)p \sim x$ and $T = \{(y^*, z^*) | y^* \succsim \alpha^* x + (1-\alpha^*)(ay^* + (1-a)z^*) \succsim z^*$ and $ay^* + (1-a)z^* \sim p\}$. Define $\bar{y} = \inf\{y | (y,z) \in T$ for some $z \in X\}$. Observe that $\bar{y} = x$. Otherwise by Axiom 3 (and Axiom 2) $\alpha^* x + (1-\alpha^*)(a\bar{y} + (1-a)\bar{z}) \sim p$ for some $\bar{z}$. This would imply that there exists $y' < \bar{y}$ and z' such that $(y', z') \in T$, a contradiction to the fact that $\bar{y}$ is the infimum. But if $\bar{y} = x$ we have $\alpha^* x + (1-\alpha^*)(ax + (1-a)\bar{z}) \sim x$ and $\bar{z} < x$ which contradicts (I) above.

(5) follows from a symmetric argument to the one provided in (4) above.

(III) The results of (II) can be generalized to p with arbitrary supports as follows: assume that (1)–(5) hold for all p such that $|\mathrm{supp}(p)| \leqslant n$. Then for p such that $|\mathrm{supp}(p)| = n+1$ we can conclude by Axiom 2 that there exists $v \in (w,b)$ such that $v \sim p$. Let $\Delta = \{\lambda \in (0,1) | p \succsim \lambda x + (1-\lambda)p\}$ for some $x \succ p$. If (1) is false, then Δ is nonempty. If $\inf \Delta = 0$, then choose $(a,q,r) \in D(p)$ such that $x' \sim p$ implies $x' \in \mathrm{supp}(r)$. Next let $(a,y,z) \sim p$ for some $y > z$. By Axiom 2 such y and z exist. Finally choose $\lambda^* \in \Delta \cap \{\lambda > 0 | y \succsim \lambda x + (1-\lambda)(ay + (1-a)z) \succsim z,\ r \in W(\lambda x + (1-\lambda)p)\}$. By Axiom 2 such λ^* exist. Then Axiom 3 implies $\lambda^* x + (1-\lambda^*)p \sim \lambda^* x + (1-\lambda^*)(ay + (1-a)z)$. But by (II) $\lambda^* x + (1-\lambda^*)(ay + (1-a)z) \succ p$, hence a contradiction.

If $\inf \Delta \neq 0$ there exists some $\hat{v} \in (w,x)$ such that $\hat{v} = CE(ax + (1-\alpha)p)$ for some $\alpha \in (0,\lambda)$ and for all $\varepsilon > 0$ there exists $\alpha' \in (\alpha, \alpha+\varepsilon)$ such that $CE(\alpha' x + (1-\alpha')p) < CE(\alpha x + (1-\alpha)p)$.

Set $\hat{p} = \alpha x + (1-\alpha)p$ and observe that $\inf \hat{\Delta} = 0$ where $\hat{\Delta} = \{\hat{\lambda} \in (0,1) | \hat{p} \succsim \hat{\lambda} x + (1-\hat{\lambda})p\}$. Hence we can use the argument above to establish the desired conclusion.

(2) follows from a symmetric argument.

(3) follows from Axiom 2 and (1) above.

(4) Assume the contrary; then choose α such that $0 < \alpha < \lambda^*$, $a \in (0,1)$, $q, r \in \mathscr{L}$ and $z < x$ such that $(a,q,r) \in D(\alpha x + (1-\alpha)p)$, $\lambda^* = \inf\{\lambda | \lambda x + (1-\lambda)p \succsim x\}$ and $x' \in \mathrm{supp}(p)$ and $x' < x$ implies $x' < V$. By Axiom 2 all of this is possible. Then set $c = (\lambda^* - \alpha)/(1-\alpha)$ and observe that

$$cx + (1-c)(\alpha x + (1-\alpha)p) = \lambda^* x + (1-\lambda^*)p \sim x, \qquad \text{by Axiom 2,}$$

$$cx + (1-c)(\alpha x + (1-\alpha)p) \sim cx + (1-c)(ax + (1-a)z), \qquad \text{by Axiom 3.}$$

Hence $x \sim (cx + (1-c)ax + (1-c)(1-a)z)$, contradicting (I) above.

(5) follows from a symmetric argument. *Q.E.D.*

Next we will define a binary relation R on $\mathscr{L}^0 = \{p \in \mathscr{L} | w \in \mathrm{supp}(p)$ implies $b \notin \mathrm{supp}(p)\}$.

sRs' iff $(\alpha, s, r) \in D, (\alpha, s', r) \in D$ implies $\alpha s + (1-\alpha)r \succsim \alpha s' + (1-\alpha)r$. We write *sIs'* to denote *sRs'* and *s'Rs*.

It is easy to see that Axiom 2, Axiom 3, and Lemma 1 (III) imply the following, stronger version of A3:

AXIOM 3: For $i = 1,2$ $(\alpha, q_i, r_i) \in D(p_i)$, $q_i \in B(\lambda x + (1-\lambda)p_i)$ and $r_i \in W(\lambda x + (1-\lambda)p_i)$, $\lambda \in [0,1]$ implies $p_1 \succsim p_2$ iff $\lambda x + (1-\lambda)p_1 \succsim \lambda x + (1-\lambda)p_2$.

But Axiom 1, Axiom 3*, and Lemma 1 (III) imply that R is preference relation on $\mathscr{L}^0$.

LEMMA 2: *sIy implies for all $\lambda \in [0,1]$, $\lambda s + (1-\lambda)p \sim \lambda y + (1-\lambda)p$ whenever*

$$s \in B(\lambda s + (1-\lambda)p) \cup W(\lambda s + (1-\lambda)p).$$

PROOF: It follows from Axiom 3* and Lemma 1 (II) that $s \in B(\lambda s + (1-\lambda)p)$ implies sIy iff $\lambda s + (1-\lambda)p \sim \lambda y + (1-\lambda)p$. Assume that $s \in W(\lambda s + (1-\lambda)p)$. Then by Axiom 3* and Lemma 1 (III) we have $\lambda s + (1-\lambda)p \sim \lambda y + (1-\lambda)p$ iff $\alpha s + (1-\alpha)b \sim \alpha y + (1-\alpha)b$ for all α sufficiently small. But then Axiom 4 implies that $\lambda s + (1-\lambda)p \sim \lambda y + (1-\lambda)p$ iff $\alpha s + (1-\alpha)w \sim \alpha y + (1-\alpha)w$ for all α such that $s \in B(\alpha s + (1-\alpha)w)$; that is, $\lambda s = (1-\lambda)p \sim \lambda y + (1-\lambda)p$ iff sIy. *Q.E.D.*

PROOF OF THEOREM 1: Define w_0 such that $w < w_0 < b$ and a function α_0: $[w_0, b] \to (0,1]$ such that $\alpha_0(x)x + (1-\alpha_0(x))w \sim w_0$ for all $x \in [w_0, b]$. Axiom 2 and Lemma 1 establish that α_0 is well-defined and continuous. It is easy to show, using Lemma 1, that α_0 is strictly decreasing. Next define u_0: $[w_0, b] \to [0,1]$ by

$$u_0(x) = \frac{\alpha_0(b)(1-\alpha_0(x))}{(1-\alpha_0(b))\alpha_0(x)}.$$

If follows from the continuity and strict decreasingness of α_0 that u_0 is continuous and strictly increasing. We will now show that (*) sRr iff $\Sigma_x u_0(x)s(x) \geqslant \Sigma_x u_0(x)r(x)$ for all $s, r \in \mathscr{L}(w_0)$ where $\mathscr{L}(w_0) = \{p \in \mathscr{L} | x \in \operatorname{supp}(p)$ implies $x \geqslant w_0\}$. To do this first we will show that $\alpha_0(x)[u(x)b + (1-u(x))w_0] + (1-\alpha_0(x))w \sim \alpha_0(x)x + (1-\alpha_0(x))w$. By Lemma 1 (III), part (3), $t\alpha_0(b)b + (1-t)w_0 + t(1-\alpha_0(b))w \sim w_0$. Set

$$t = \frac{1-\alpha_0(x)}{1-\alpha_0(b)}.$$

So,

$$\frac{\alpha_0(b)(1-\alpha_0(x))}{1-\alpha_0(b)}b + \frac{\alpha_0(x)-\alpha_0(b)}{1-\alpha_0(b)}w_0 + (1-\alpha_0(x))w \sim w_0;$$

hence,

$$\alpha_0(x)\left[\frac{\alpha_0(b)(1-\alpha_0(x))}{(1-\alpha_0(b))\alpha_0(x)}b + \left(1 - \frac{\alpha_0(b)(1-\alpha_0(x))}{(1-\alpha_0(b))\alpha_0(x)}\right)w_0\right] + (1-\alpha_0(x))w \sim w_0;$$

i.e.,

$$\alpha_0(x)\left[u_0(b)b + (1-u_0(x))w_0\right] + (1-\alpha_0(x))w \sim w_0.$$

Therefore

$$\alpha_0(x)\left[u_0(x)b + (1-u_0(x))w_0\right] + (1-\alpha_0(x))w \sim \alpha_0(x)x + (1-\alpha_0(x))w.$$

Then, applying Lemma 2 yields that for all s such that $\operatorname{supp}(s) = 2$,

$$\binom{*}{*} \qquad s \in B(\alpha s + (1-\alpha)p) \cup W(\alpha s + (1-\alpha)p)$$

implies $\alpha s + (1-\alpha)p \sim \alpha y + (1-\alpha)p$ iff $\Sigma_x u(x)s(x) = u(y)$. Hence, applying $\binom{*}{*}$ repeatedly establishes that $\binom{*}{*}$ holds for arbitrary s. A3 and Lemma 1 imply that $\alpha y + (1-\alpha)p \succsim \alpha y' + (1-\alpha)p$. Therefore $\binom{*}{*}$ establishes the desired conclusion.

Choose some sequence $\{w_i\}_{i=1,2\ldots}$ such that $w < w_{i+1} < w_i$ for $i = 1, 2\ldots$ and $\lim_{i\to\infty}\{w_i\} = w$. Define α_i: $[w_i, b] \to [0,1]$ by substituting w_i in place of w_0 in the definition of α_0. Furthermore let

$$u_i(x) = \frac{\alpha_i(b)}{1-\alpha_i(b)}\,\frac{(1-\alpha(x))}{\alpha(x)}.$$

By the argument above, u_t satisfies (*). Note that on the interval $[w_t, b]$ both u_t and u_{t+1} satisfy (*); hence $u_{t+1} = au_i + c$ by a familiar argument from expected utility theory. $u_{t+1}(b) = u_t(b) = 1$, so $a = 1 - c$. Furthermore $u_i(w_t) = 0$; hence $0 \leqslant c$ and therefore $0 < c < 1$ and $u_{t+1} = (1-c)u_t + c \geqslant u_t$. Hence $u_t(x)$ is an increasing sequence. Let $j^*(x) = \inf\{j | w_j \leqslant x\}$ for all $x \in (w, b]$. Define $u(x)$ by $u(x) = \lim_{t \geqslant j^*(x)} u_t(x)$. Clearly $u(x)$ is well-defined for all $x \in (w, b]$; furthermore for all i, u_t satisfies strict monotonicity, (*), and continuity, and hence u satisfies those properties also. Note that since $\lim_{x \to w} u(x) = 0$, u can be extended to $[w, b]$ with all of those properties by setting $u(w) = 0$.

Next define γ: $[0,1] \to [0,1]$ by $\gamma(\alpha) = u(x)$ where $\alpha b + (1-\alpha)w \sim x$. It follows from Axiom 2 that γ is well-defined and continuous. From Lemma 1 it follows that γ is strictly increasing. Furthermore $\gamma(1) = 1$, $\gamma(0) = 0$. We will show that $\gamma(\alpha) = \alpha/(1 + (1-\alpha)\beta)$ for some $\beta \in (-1, \infty)$. Let $\alpha > \hat{\alpha}$ and $\gamma(\alpha) = u(x)$, $\gamma(\hat{\alpha}) = u(y)$. By definition $\alpha b + (1-\alpha)w \sim x$. By Lemma 1 (II) part (3) $\lambda\alpha b + (1-\lambda)x + \lambda(1-\alpha)w \sim x$. Choose λ such that $(1-\lambda)u(x) = u(y)$. Then $\alpha b + (1-\alpha)w \sim \alpha[\lambda b + (1-\lambda)x] + (1-\alpha)[(1-\lambda)x + \lambda w]$. From (*) and Axiom 3 we have

$$\alpha b + (1-\alpha)w \sim \alpha[\lambda b + (1-\lambda)x] + (1-\alpha)y.$$

By taking a $\hat{\alpha}/\alpha$ convex combination of both sides with w and applying Axiom 3 we get

$$\hat{\alpha} b + (1-\hat{\alpha})w \sim \hat{\alpha}[\lambda b + (1-\lambda)x] + \hat{\alpha}\frac{(1-\alpha)}{\alpha}y + \frac{\alpha - \hat{\alpha}}{\alpha}w.$$

Let a satisfy $a + (1-a)u(y) = u(x)$; then Axiom 3 and (*) yields

$$\hat{\alpha} b + (1-\hat{\alpha})w \sim \hat{\alpha}(\lambda + (1-\lambda)a)b + \left[\hat{\alpha}(1-\lambda)(1-a) + \frac{\hat{\alpha}(1-\alpha)}{\alpha}\right]y + \frac{\alpha - \hat{\alpha}}{\alpha}w.$$

Then by Axiom 3

$$\hat{\alpha} b + (1-\hat{\alpha})w \sim \frac{\hat{\alpha}(\lambda + (1-\lambda)a)}{D}b + \left[1 - \frac{\hat{\alpha}(\lambda + (1-\lambda)a)}{D}\right]w,$$

where $D = 1 - \hat{\alpha}(1-\lambda)(1-a) - (\hat{\alpha}(1-\alpha)/\alpha)$. But this implies that

$$\hat{\alpha} = \frac{\hat{\alpha}(\lambda + (1-\lambda)a)}{D}.$$

Substituting the value for λ and a, and some simplifying, yields:

$$u(y) = \frac{u(x)\hat{\alpha}(1-\alpha)}{\alpha(1-u(x)) - \hat{\alpha}(\alpha - u(x))}.$$

Substituting $\gamma(\alpha)$ for $u(x)$ and $\gamma(\hat{\alpha})$ for $u(y)$, we obtain

$$\gamma(\hat{\alpha}) = \frac{\gamma(\alpha)\hat{\alpha}(1-\alpha)}{\alpha(1-\gamma(\alpha)) - \hat{\alpha}(\alpha - \gamma(\alpha))};$$

that is,

$$\gamma(\hat{\alpha}) = \frac{\hat{\alpha}}{\dfrac{\gamma(\alpha)(1-\alpha) + \alpha - \gamma(\alpha)}{\gamma(\alpha)(1-\alpha)} - \hat{\alpha}\dfrac{\alpha - \gamma(\alpha)}{\gamma(\alpha)(1-\alpha)}}.$$

Define

$$\beta(\alpha) = \frac{\alpha - \gamma(\alpha)}{\gamma(\alpha)(1-\alpha)};$$

then,

$$\gamma(\hat{\alpha}) = \frac{\hat{\alpha}}{1 + (1-\hat{\alpha})\beta(\alpha)}.$$

If we can show that $\beta(\alpha)$ is a constant, we are done. For $0<\hat{\alpha}<\alpha<1$,

$$\beta(\hat{\alpha})=\frac{\hat{\alpha}-\gamma(\hat{\alpha})}{\gamma(\hat{\alpha})(1-\hat{\alpha})}.$$

Substituting

$$\gamma(\hat{\alpha})=\frac{\hat{\alpha}}{1+(1-\hat{\alpha})\beta(\alpha)}$$

into the above equation yields $\beta(\hat{\alpha})=\beta(\alpha)$. Hence β is constant. $\beta>-1$ follows from the fact that $b \succ \alpha b+(1-\alpha)w$ for all $\alpha \in(0,1)$ (by Lemma 1). Next we will show that the function

$$V(p)=\gamma(\alpha)\sum_{x} u(x)q(x)+(1-\gamma(\alpha))\sum_{x} u(x)r(x)$$

represents $\succsim$ for $(\alpha, q, r) \in D(p)$. To show this we will prove that $(\alpha, x, z) \in D$ implies $(\alpha, x, z) \sim y$ iff $\gamma(\alpha)u(x)+(1-\gamma(\alpha))u(z) \sim u(y)$. Then, the fact that V represents $\succsim$ will follow from Lemma 2 and the observation that $ax^{*}+(1-a)y^{*} \succ ax^{*}+(1-a)z^{*}$ iff $y^{*}>z^{*}$.

$$(\alpha, x, z) \sim y \quad \text{iff} \quad \alpha(cb+(1-c)y)+(1-\alpha)(dy+(1-d)w) \sim y$$

for $c=(u(x)-u(y))/(1-u(y))$ and $d=u(z)/u(y)$ (by Lemma 2).

Hence $(\alpha, x, z) \sim y$ iff $\alpha cb+[\alpha(1-c)+(1-\alpha)d]y+(1-\alpha)(1-d)w \sim y$. Hence $(\alpha, x, z) \sim y$ iff $tb+(1-t)w \sim y$ where

$$t=\frac{\alpha c}{1-\alpha(1-c)-(1-\alpha)d}.$$

But by construction

$$tb+(1-t)w \sim y \quad \text{iff}$$

$$\gamma(t)=u(y), \quad \text{i.e. iff}$$

$$\frac{t}{1+(1-t)\beta}=u(y), \quad \text{i.e. iff}$$

$$t=\frac{u(y)(1+\beta)}{1+u(y)\beta}.$$

Substituting for t, c, and d yields

$$(\alpha, x, z) \sim y \quad \text{iff}$$

$$\alpha=\frac{(1+\beta)(u(y)-u(z))}{u(x)-u(z)+\beta[u(y)-u(z)]} \quad \text{which holds iff}$$

$$\gamma(\alpha)u(x)+(1-\gamma(\alpha))u(z)=u(y).$$

Hence we have proven part (i) and (iii) of the theorem. That u is unique up to an affine transformation follows from the familiar argument of expected utility theory. The uniqueness of γ is obvious. *Q.E.D.*

An Algorithm for Computing V(p)

Let $\{x_1, x_2, \ldots x_k\}$ be the support for some lottery p. Without loss of generality assume $x_j>x_{j-1}$ for all $j=2,3, \ldots k$. If $CE(p) \notin\{x_1, x_2, \ldots x_k\}$, then there exists a unique j^{*} such that $\{x_1, x_2, \ldots x_{j^{*}}\}$ constitute all the disappointment prizes of p and $\{x_{j+1}, x_{j+2} \ldots x_k\}$ constitute all the elation prizes of p. Hence there are $k-1$ candidates for an EDD of p. These are (α_j, q_j, r_j) (for $j=1,2, \ldots, k-1$) where $\alpha_j=\sum_{i=j+1}^{k} p(x_i)$, $q_j(x)=0$ for $x \notin\{x_{j+1}, x_{j+1}, \ldots x_k\}$, $q_j(x)=p_j(x)/\alpha_j$ for $x \in \{x_{j+1}, x_{j+2} \ldots x_k\}$, $r_j(x)=0$ for $x \notin\{x_1, x_2 \ldots x_j\}$, and $r_j(x)=p(x)/(1-\alpha_j)$ for $x \in\{x_1, x_2 \ldots x_j\}$. Let $\hat{V}(\alpha_j, q_j, r_j)=\gamma(\alpha_j)Eu(q_j)+(1-\gamma(\alpha_j))Eu(r_j)$ where $\gamma(\alpha)=\alpha/(1+(1-\alpha)\beta)$. The condition $u(x_j)<\hat{V}(\alpha_j, q_j, r_j)<u(x_{j+1})$ will be satisfied if and only if $j=j^{*}$. For j^{*}, $\hat{V}(\alpha_{j^{*}}, q_{j^{*}}, r_{j^{*}})=V(p)$ and $\{(\alpha_{j^{*}}, q_{j^{*}}, r_{j^{*}})\}=D(p)$. Hence in at most $k-1$ steps we can isolate the unique EDD of p and

determine $V(p)$. If $CE(p) \in \text{supp}(p)$ then there will exist a unique j^* such that $\hat{V}(\alpha_{j^*-1}, q_{j^*-1}, r_{j^*-1}) = \hat{V}(\alpha_{j^*}, q_{j^*}, r_{j^*}) = u(x_{j^*})$. For $j < j^* - 1$, $\hat{V}(\alpha_j, q_j, r_j)$ and for $j > j^*$, $\hat{V}(\alpha_j, q_j, r_j) < u(x_j)$. Thus $D(p) = \{(\alpha, q, r) | \alpha q + (1-\alpha) r = p$ and $q(x) = 0$ if $x \notin \{x_{j^*}, x_{j^*-1}, \dots x_k\}$, $r(x) = 0$ if $x \notin \{x_1, x_2, \dots x_{j^*}\}\}$. Furthermore for all $(\alpha, q, r) \in D(p)$, $\hat{V}(\alpha, q, r) = u(x_{j^*}) = V(p)$. So again in at most $k-1$ steps, $V(p)$ and $D(p)$ can be determined.

Proof of Theorem 2: Let $e(p) > e(q)$, $x \succ p$, and $\beta > 0$. Then there exists $(\alpha, s, r) \in D(p)$ such that $\alpha = e(p)$. Furthermore, by Axiom 2, there exists $\bar{a} > 0$ such that $s \in B(ax + (1-a)p)$ and $r \in W(ax + (1-a)p)$ for all $a < \bar{a}$. Then for $a < \bar{a}$,

$$V(ax + (1-a)p) = \gamma(a + (1-a)\alpha) \cdot \frac{au(x) + (1-a)\alpha c}{a + (1-a)\alpha} + (1 - \gamma(a + (1-a)\alpha))d$$

where $c = \Sigma_y u(y) s(y)$ and $d = \Sigma_y u(y) r(y)$. Hence,

$$\left.\frac{dV(ax + (1-a)p)}{da}\right|_{a=0} = \gamma'(\alpha)(1-\alpha)c + \gamma(\alpha)\frac{u(x) - c}{\alpha} - \gamma'(\alpha)(1-\alpha)d.$$

Substituting $\gamma'(\alpha) = 1 + \beta/(1 + (1-\alpha)\beta)^2$ and $\gamma(\alpha) = \alpha/1 + (1-\alpha)\beta$ and rearranging terms yields

$$\left.\frac{dV(ax + (1-a)p)}{da}\right|_{a=0} = \frac{u(x)}{1 + (1-\alpha)\beta} - \frac{\alpha c + (1-\alpha)(1+\beta)d}{(1 + (1-\alpha)\beta)^2}$$
$$= \frac{u(x) - V(p)}{1 + (1-\alpha)\beta} = \frac{u(x) - V(p)}{1 + (1-e(p))\beta}.$$

Repeating the same argument for q yields

$$\left.\frac{dV(ax + (1-a)q)}{da}\right|_{a=0} = \frac{u(x) - V(q)}{1 + (1-e(q))\beta}.$$

By assumption $V(p) = V(q)$ and $u(x) - V(p) > 0$. Hence $e(p) > e(q)$ implies

$$\left.\frac{dV(ax + (1-a)p)}{da}\right|_{a=0} > \left.\frac{dV(ax + (1-a)q)}{da}\right|_{a=0},$$

which establishes that $ax + (1-a)p \succ ax + (1-a)q$ for sufficiently small a. All remaining cases follow from symmetric arguments. *Q.E.D.*

Proof of Theorem 5: First we will prove that $\beta_2 > \beta_1$ implies $\succsim_1$ is not more risk averse than $\succsim_2$. Choose $x \in (w, b)$ and $\varepsilon > 0$. Let α solve

$$u_1(x) = \frac{\alpha u_1(x + \varepsilon) + (1-\alpha)(1+\beta_1) u_1(x - \varepsilon)}{1 + (1-\alpha)\beta_1}.$$

Then obviously $x \sim p$ where $p(x + \varepsilon) = \alpha$ and $p(x - \varepsilon) = 1 - \alpha$. Using Taylor series expansion of u_1 around x we obtain

$$(1 + (1-\alpha)\beta_1) u_1(x) = \alpha[u_1(x) + \varepsilon u_1'(x)]$$
$$+ (1-\alpha)(1+\beta_1)[u_1(x) - \varepsilon u_1'(x)] + o(\varepsilon);$$

hence

$$\alpha = \frac{1 + \beta_1}{2 + \beta_1} + o(\varepsilon).$$

Similar argument establishes that if $a'(x + \varepsilon) + (1 - \alpha')(x - \varepsilon) \sim_2 x$, then

$$\alpha' = \frac{1 + \beta_2}{2 + \beta_2} + o(\varepsilon).$$

Hence for ε sufficiently small $\alpha' > \alpha$. That is $x \succ_2 \alpha(x + \varepsilon) + (1-\alpha)(x - \varepsilon) = p$.

Next assume that $R_1^a(x) \geqslant R_2^a(x)$ for all $x \in (w, b)$, $\beta_1 \geqslant \beta_2$ and $p \succsim_1 \bar{x}$. Then, in particular $p \sim_1 x$ for some $x > \bar{x}$. Since u_1 and u_2 are unique only up to affine transformations we can, without loss of generality, assume that $u_1(x) = u_2(x)$ and $u_1'(x) = u_2'(x)$. Then note that $u_1(y) \leqslant u_2(y)$ for all $y \in [w, b]$. Let $(\alpha, q, r) \in D(p, \succsim_1)$. Then we have

$$\gamma_1(\alpha) \sum_y u_1(y) q(y) + (1 - \gamma_1(\alpha)) \sum_y u_1(y) r(y) = u_1(x).$$

But $u_1(x) = u_2(x)$, $\gamma_1(\alpha) \leqslant \gamma_2(\alpha)$ and $u_1(y) \leqslant u_2(y)$; hence

$$\gamma_2(\alpha) \sum_y u_2(y) q(y) + (1 - \gamma_2(\alpha)) \sum_y u_2(y) r(y) \geqslant u_2(x).$$

Therefore

$$u_2(x) \leqslant \sum_y u_2(y) p(y) + \beta_2 \sum_{y \leqslant x} (u_2(y) - u_2(x)) p(y).$$

Since $V_2(p) = \Sigma_y u_2(y) p(y) + \beta_2 \Sigma_{y \leqslant p}(u_2(y) - V_2(p)) p(y)$, $V_2(p) \geqslant u_2(x)$, which establishes that $p \succsim_2 x \succsim_2 \bar{x}$ and proves that $\succsim_1$ is more averse than $\succsim_2$. *Q.E.D.*

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Parametric Recoverability of Preferences

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Revealed preference theory is brought to bear on the problem of recovering approximate parametric preferences from consistent and inconsistent consumer choices. We propose measures of the incompatibility between the revealed preference ranking implied by choices and the ranking induced by the considered parametric preferences. These incompatibility measures are proven to characterize well-known inconsistency indices. We advocate a recovery approach that is based on such incompatibility measures and demonstrate its applicability for misspecification measurement and model selection. Using an innovative experimental design, we empirically substantiate that the proposed revealed-preference-based method predicts choices significantly better than a standard distance-based method.

I. Introduction

This paper studies the problem of recovering stable preferences from individual choices. The renewed interest in this problem emerges from the

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recent availability of relatively large data sets composed of individual choices made from linear budget sets. These rich data sets allow researchers to recover approximate individual stable utility functions and report the magnitude and distribution of behavioral characteristics in the population. We bring revealed preference theory to bear on the problem of recovering approximate parametric preferences from both consistent and inconsistent consumer choices.

Classical revealed preference theory studies the conditions on observables (choices) that are equivalent to the maximization of some utility function. If a data set is constructed from consumer choice problems in an environment with linear budget sets, Afriat (1967) proves that no revealed preference cycles among observed choices, a condition known as the generalized axiom of revealed preference (henceforth GARP), is equivalent to assuming that the consumer behaves as if she maximizes some locally nonsatiated utility function. In his proof, Afriat constructs a well-behaved piecewise linear utility function that is consistent with the consumer choices. Theorem 1 shows that similar reasoning may be applied for approximate preferences when GARP is not satisfied, by adjusting the revealed preference information to exclude cycles.

The method above requires recovering twice the number of parameters as there are observations, and therefore the behavioral implications of the constructed functional forms may be difficult to interpret and apply to economic problems. In many cases researchers assume simple functional forms with few parameters that lend themselves naturally to behavioral interpretations. The drawback of this approach is that simple functional forms are often too structured to capture every nuance of individual decision making. Thus, preferences recovered in this way are almost always misspecified. That is, the ranking implied by the recovered preferences may be incompatible with the ranking information implied by the decision maker's choices (summarized through the revealed pref-

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erence relation).[1] We argue that given a parametric utility specification, one should seek a measure to quantify the extent of misspecification and minimize it as a criterion for selecting from the functional family.

Our proposed measures of misspecification rely on insights gained from the literature that quantifies internal inconsistencies inherent in a data set. The Houtman and Maks (1985) inconsistency index searches for the minimal subset of observations that should be removed from a data set in order to eliminate cycles in the revealed preference relation. Similarly, the Varian (1990) inconsistency index is calculated by aggregating the minimal budget adjustments required to remove revealed preference relations that cause the data set to fail GARP. A special case of the Varian inconsistency index is the critical cost efficiency index (Afriat 1972, 1973) in which adjustments are restricted to be identical across all observations.

Theorem 2 provides the following novel theoretical characterization of these indices: for every utility function a loss can be calculated that aggregates budget adjustments required to remove incompatibilities between the ranking information induced by the utility function and the revealed preference information contained in the observed choices. The loss function corresponding to the Houtman-Maks inconsistency index is the binary incompatibility index (henceforth BII), which counts the observations that are not rationalized by a given utility function. Similarly, the loss function corresponding to the Varian inconsistency index is the money metric index (proposed by Varian [1990]; henceforth MMI), which aggregates the minimal budget adjustments required to remove all incompatibilities. We prove that the inconsistency indices equal the infimum of their corresponding loss functions taken over all continuous, acceptable, and locally nonsatiated utility functions.[2] Hence, the inconsistency indices lend themselves naturally as benchmarks for minimizing incompatibilities between the data set and all considered utility functions.

We argue that parametric recovery should generalize the principle introduced in characterizing the inconsistency indices by calculating the infimum of the loss function over a restricted subset of utility functions. If a data set is consistent (satisfies GARP), the incompatibility measures we propose quantify the extent of misspecification that arises solely from considering a specific family of utility functions rather than all continuous, acceptable, and locally nonsatiated utility functions. If the data set does not satisfy GARP, each measure can be additively decomposed into

[1] If choices are inconsistent, the "revealed preference relation" refers to the ranking remaining after excluding cycles in some "minimal" way (see definition 1 below).

[2] A utility function is acceptable if the zero bundle is weakly worse than every other nonnegative bundle. See also definition 2.3 in Sec. 1 of the online appendix.

the respective inconsistency index and a misspecification index. Since for a given data set the inconsistency index is constant, the incompatibility measures can be minimized to recover parametric preferences within some parametric family.

This discussion continues a line of thought proposed by Varian (1990), who was unsatisfied with the standard approach that relies on parametric specification when testing for optimizing behavior. Varian suggested separating the analysis into two parts. The first part, which does not rely on a parametric specification, tests for consistency and quantifies how close choices are to being consistent using an inconsistency index. The second part uses the money metric as a "natural measure of how close the observed consumer choices come to maximizing a particular utility function" (133) and employs it as a criterion for recovering preferences. Varian argued that measuring differences in utility space has a more natural economic interpretation than measuring distances between bundles in commodity space.

We augment Varian's intuition by providing theoretical and practical substance for the use of loss functions as measures of misspecification. First, we relate the budget adjustments implied by the proposed loss functions to the Houtman-Maks, Varian, and Afriat inconsistency indices. Second, we advocate recovery methods that utilize as much ranking information encoded in observed choices rather than distance-based methods, since making a choice from a menu reveals that the chosen alternative is preferred to every other feasible alternative, not only to the predicted one. Therefore, our rationale for using the MMI is different from Varian's and could be equally applied to other loss functions, as the BII. Third, since we show that the goodness of fit can be decomposed into an inconsistency index and a misspecification measure, it lends itself naturally to several novel applications including evaluating parametric restrictions and model selection. Thus, ultimately we show that the two parts proposed by Varian (1990) are closely related, as the difference between them can be attributed to the sets of utility functions considered. Finally, while Varian takes the theory to representative agent data, we use individual-level data gathered in the laboratory to provide evidence for the predictive superiority of the MMI.

As an illustration of a practical application, we use the MMI to recover parameters for the data set collected by Choi et al. (2007) in which subjects choose a portfolio of Arrow securities. Using the disappointment aversion model of Gul (1991) with the constant relative risk aversion (CRRA) functional form, we recover parameters using nonlinear least squares (NLLS) and MMI. We find substantial numerical differences with respect to the recovered parameters that in some cases imply significant quantitative and qualitative differences in preferences.

However, the data collected by Choi et al. (2007) were not designed to compare the accuracy in which different recovery methods represent the decision maker's preferences. Therefore, we propose a general empirical-experimental methodology whereby recovery methods are evaluated on the basis of their predictive success and apply it in an experimental setting similar to that of Choi et al. The experiment utilizes a unique two-part design. In the first part of the experiment we collect choice data from linear budget sets and instantaneously recover individual parameters from these data using the two different parametric recovery methods (MMI and NLLS). We use the individually recovered parameters to construct a sequence of pairs of portfolios (per individual) such that one of the portfolios in each pair is preferred according to the parametric preferences recovered by the MMI and the other is preferred by the parametric preferences recovered by the NLLS. Then, in the second part of the experiment, subjects are presented with these individually constructed pairs of portfolios, and their choices are used to evaluate the predictive success of each recovery method.

This methodology enables us not only to compare the relative predictive success of the recovery methods but also to observe subjects' choices in regions that may otherwise be unobservable. In particular, when subjects choose from linear budget sets, nonconvex preferences imply the existence of bundles that are never chosen if the subject chooses optimally. This may make it difficult to identify different sets of parameters that may nevertheless imply substantially different behavior (e.g., the extent of local risk seeking). By offering the subjects pairwise choices located in the region of nonconvexity, we can directly observe their true preferences in this region and identify which set of recovered parameters more accurately represents their underlying preferences.

For our sample of 203 subjects, we find that the MMI recovery method predicted subjects' choices significantly more accurately than the NLLS recovery method. At the aggregate level, approximately 54 percent of pairwise choices are predicted by the MMI recovery method. At the individual level, consider those subjects for whom one of the methods correctly predicted more than two-thirds of the pairwise choices. The choices of almost 60 percent of those subjects were more accurately predicted by the MMI recovery method. Moreover, when we focus our attention on only those subjects for whom the recovered parameters imply nonconvex preferences (i.e., local risk-seeking behavior), the MMI recovery method predicted more accurately in 62.5 percent of pairwise choices and for 75 percent of subjects for whom more than two-thirds of the choices are correctly predicted. We interpret these results as suggesting that our proposed MMI recovery method is more reliable than measures based on the distance between observed and predicted choices in commodity space, especially in decision-making environments in which closeness does not necessarily imply similarity.

We use the data from the experiment and the data collected by Choi et al. (2007) to show that the preferences of approximately 40 percent of the subjects are well approximated by expected utility compared to the general disappointment aversion functional form. In addition, we demonstrate nonnested model selection by providing evidence that the choices of most subjects are better approximated by the disappointment aversion model with the CRRA utility index than by the disappointment aversion model with the constant absolute risk aversion (CARA) utility index.

In the next section we generalize the standard definitions of revealed preference relations and extend Afriat's (1967) theorem to inconsistent data sets (theorem 1). In Section III, we introduce the main inconsistency indices discussed in the paper, and in Section IV, we introduce the money metric and the binary incompatibility measures and use them to characterize the inconsistency indices (theorem 2). In Section V, we analyze the data gathered by Choi et al. (2007) and point out the need for an external criterion to decide between the recovery methods. The experimental design is described in Section VI, while the results are reported in Section VII. Section VIII demonstrates the use of our theoretical results for hypothesis testing and model selection. Section IX presents conclusions.

II. Preliminaries

Consider a decision maker (henceforth DM) who chooses bundles $x^i \in \mathfrak{R}^K_+$ $(i \in 1, \dots, n)$ from budget menus $\{x : p^i x \leq p^i x^i, p^i \in \mathfrak{R}^K_{++}\}$. Let $D = \{(p^i, x^i)^n_{i=1}\}$ be a finite data set, where x^i is the chosen bundle at prices p^i. The following definitions generalize the standard definitions of revealed preference (for similar concepts, see Afriat [1972, 1987], Varian [1990, 1993], and Cox [1997]).

DEFINITION 1. Let D be a finite data set. Let $\mathbf{v} \in [0, 1]^n$.[3] An observed bundle $x^i \in \mathfrak{R}^K_+$ is

1. $\mathbf{v}$–*directly revealed preferred* to a bundle $x \in \mathfrak{R}^K_+$, denoted $x^i R^0_{D,\mathbf{v}} x$, if $v^i p^i x^i \geq p^i x$ or $x = x^i$;
2. $\mathbf{v}$–*strictly directly revealed preferred* to a bundle $x \in \mathfrak{R}^K_+$, denoted $x^i P^0_{D,\mathbf{v}} x$, if $v^i p^i x^i > p^i x$;
3. $\mathbf{v}$–*revealed preferred* to a bundle $x \in \mathfrak{R}^K_+$, denoted $x^i R_{D,\mathbf{v}} x$, if there exists a sequence of observed bundles $(x^j, x^k, \dots, x^m)$ such that $x^i R^0_{D,\mathbf{v}} x^j$, $x^j R^0_{D,\mathbf{v}} x^k$, ..., $x^m R^0_{D,\mathbf{v}} x$.

[3] Throughout the paper we use bold fonts (as $\mathbf{v}$ or $\mathbf{1}$) to denote vectors of scalars in $\mathfrak{R}^n$. We continue to use regular fonts to denote vectors of prices and goods. For $\mathbf{v}, \mathbf{v}' \in \mathfrak{R}^n$, $\mathbf{v} = \mathbf{v}'$ if for all i, $v_i = v'_i$, $\mathbf{v} \geqq \mathbf{v}'$ if for all i, $v_i \geq v'_i$, $\mathbf{v} \geq \mathbf{v}'$ if $\mathbf{v} \geqq \mathbf{v}'$, and $\mathbf{v} \neq \mathbf{v}'$ and $\mathbf{v} > \mathbf{v}'$ if for all i, $v_i > v'_i$.

When $\mathbf{v} = \mathbf{1}$, definition 1 reduces to the standard definition of revealed preference relation. When $\mathbf{v}$ decreases, more revealed preference information is being relaxed as summarized in the following observation (for a proof see Sec. 1.1 of the appendix).

FACT 1. Let $\mathbf{v}' \leq \mathbf{v}$. Then $R^0_{D,\mathbf{v}'} \subseteq R^0_{D,\mathbf{v}}$, $P^0_{D,\mathbf{v}'} \subseteq P^0_{D,\mathbf{v}}$, and $R_{D,\mathbf{v}'} \subseteq R_{D,\mathbf{v}}$.

Consider the following notion of consistency for data sets (Varian 1990):

DEFINITION 2. Let $\mathbf{v} \in [0,1]^n$. D satisfies the general axiom of revealed preference given $\mathbf{v}$ (GARP$_{\mathbf{v}}$) if for every pair of observed bundles, $x^i R_{D,\mathbf{v}} x^j$ implies not $x^j P^0_{D,\mathbf{v}} x^i$.

When $\mathbf{v} = 1$, definition 2 is equivalent to Afriat's (1967) cyclical consistency (GARP; see Varian 1982). Practically, the vector $\mathbf{v}$ is used to generate an adjusted relation $R^0_{D,\mathbf{v}}$ that contains no strict cycles while $R^0_{D,\mathbf{1}}$ may contain such cycles. Obviously, usually there are many vectors such that D satisfies GARP$_{\mathbf{v}}$. Following are two useful and trivial properties of GARP$_{\mathbf{v}}$ (proofs are in Secs. 1.2 and 1.3 of the appendix, respectively):

FACT 2. Every D satisfies GARP$_{\mathbf{0}}$.

FACT 3. Let $\mathbf{v}, \mathbf{v}' \in [0,1]^n$ and $\mathbf{v} \geq \mathbf{v}'$. If D satisfies GARP$_{\mathbf{v}}$, then D satisfies GARP$_{\mathbf{v}'}$.

The following definition of $\mathbf{v}$-*rationalizability* relates the revealed preference information implied by observed choices to the ranking induced by a utility function.

DEFINITION 3. Let $\mathbf{v} \in [0,1]^n$. A utility function $u(x)$ $\mathbf{v}$-*rationalizes* D if for every observed bundle $x^i \in \Re^K_+$, $x^i R^0_{D,\mathbf{v}} x$ implies that $u(x^i) \geq u(x)$. We say that D is $\mathbf{v}$-*rationalizable* if such $u(\cdot)$ exists.

That is, the intersection between the set of bundles that are ranked strictly higher than an observed bundle x^i according to u and the set of bundles to which x^i is revealed preferred when the budget constraint is adjusted by v^i is empty. Hence, $\mathbf{1}$-rationalizability reduces to the standard definition of rationalizability (Afriat 1967).[4]

Notice that $\mathbf{v}$-rationalizability does not imply uniqueness. There could be different utility functions (not related through monotonic transformation) that $\mathbf{v}$-rationalize the same data set. Afriat's (1967) celebrated theorem provides tight conditions for the rationalizability of a data set.[5] Afriat's theorem was generalized in many directions. For example, Reny (2015) extended it to infinite data sets, Forges and Minelli (2009) to general budget sets, and Fujishige and Yang (2012) to indivisible goods. The following theorem generalizes Afriat's result to inconsistent data sets.

THEOREM 1. The following conditions are equivalent:

[4] Throughout the paper *rationalizability* means $\mathbf{1}$-*rationalizability*, D is *rationalizable* if it is $\mathbf{1}$-*rationalizable*, and D satisfies GARP if it satisfies GARP$_1$.

[5] For discussion and alternative proofs of the original theorem, see Diewert (1973), Varian (1982), Teo and Vohra (2003), Fostel, Scarf, and Todd (2004), and Geanakoplos (2013).

1. There exists a nonsatiated utility function that **v**-rationalizes the data.
2. The data satisfy $\text{GARP}_{\mathbf{v}}$.
3. There exists a continuous, monotone, and concave utility function that **v**-rationalizes the data.

Proof. See Section 1.4 of the appendix.[6]

III. Inconsistency Indices

For some of the following inconsistency measures we make use of a general aggregator function across observations.[7]

DEFINITION 4. $f_n : [0, 1]^n \to [0, M]$, where M is finite, is an *aggregator function* if $f_n(\mathbf{1}) = 0$, $f_n(\mathbf{0}) = M$, and $f_n(\cdot)$ is continuous and weakly decreasing.[8]

Varian (1990) proposed an inconsistency index that measures the minimal adjustments of the budget sets that remove cycles implied by choices. While Varian suggests to aggregate the adjustments using the sum of squares, we define this index with respect to an arbitrary aggregator function.[9]

DEFINITION 5. Let $f : [0, 1]^n \to [0, M]$ be an aggregator function. *Varian's inconsistency index* is[10]

[6] Afriat (1973) uses the theorem of the alternative to provide a nonconstructive proof for the special case in which the coordinates of the adjustment vector are equal. Afriat (1987) states theorem 1 without a proof (theorem 6.3.I on p. 179). In his unpublished PhD dissertation, Houtman (1995, theorem 2.5) considers nonlinear pricing and monotone adjustments. While the proof in Afriat (1973) can be easily generalized to our case, we preferred to adapt the construction suggested in Houtman (1995) for the case of scale adjustments of linear budget sets. In addition, while Afriat (1973) does not require the chosen bundle to remain feasible following an adjustment, our proof (as the one in Houtman [1995]) respects this requirement.

[7] In most of this paper we assume a fixed data set of size n; therefore, we will abuse notation by omitting the subscript, unless required for clarity.

[8] An aggregator function f_n is weakly decreasing if for every $\mathbf{v}, \mathbf{v}' \in [0, 1]^n$,

$$\mathbf{v} \geq \mathbf{v}' \Rightarrow f_n(\mathbf{v}) \leq f_n(\mathbf{v}'),$$
$$\mathbf{v} > \mathbf{v}' \Rightarrow f_n(\mathbf{v}) < f_n(\mathbf{v}').$$

One may wish to restrict the set of potential aggregator functions to include only separable functions that satisfy the cancellation axiom. The results do not require the richness of possible aggregator functions.

[9] Alcantud, Matos, and Palmero (2010) follow Varian (1990) to suggest the Euclidean norm of the adjustments vector. Tsur (1989) uses $\Sigma_{i=1}^n (\log v_i)^2/n$, while Varian (1993) and Cox (1997) mention the maximal adjustment and Smeulders et al. (2014) consider the generalized mean $\Sigma_{i=1}^n (1 - v_i)^\rho$, where $\rho \geq 1$.

[10] Consider a data set of two points $D = \{(p^1, x^1); (p^2, x^2)\}$ such that $p^1 x^2 = p^1 x^1$ but $p^2 x^1 < p^2 x^2$. The data set D is inconsistent with GARP (since $x^1 R_{D,1} x^2$ and $x^2 P^0_{D,1} x^1$), but consider the sequence $\mathbf{v}_l = (1 - (1/l), 1)$, where $l \in N_{>0}$. It is easy to verify that for every $l \in N_{>0}$, D satisfies $\text{GARP}_{\mathbf{v}_l}$. Therefore, $I_V(D, f) = 0$.

$$I_V(D,f) = \inf_{\mathbf{v}\in[0,1]^n : D \text{ satisfies } \mathrm{GARP}_\mathbf{v}} f(\mathbf{v}).$$

Varian (1990) suggested this index as a nonparametric measure for the extent of utility-maximizing behavior implied by a data set of consumer choices. Varian's inconsistency index is a generalization of the critical cost efficiency index (suggested earlier by Afriat [1972, 1973]) that is restricted to uniform adjustments. Denote the set of vectors with equal coordinates by $\mathcal{I} = \{\mathbf{v} \in [0,1]^n : \mathbf{v} = v\mathbf{1}, \forall\ v \in [0,1]\}$ and a coordinate of a typical vector $\mathbf{v} \in \mathcal{I}$ by v.

Definition 6. The *Afriat inconsistency index* is

$$I_A(D) = \inf_{\mathbf{v}\in\mathcal{I} : D \text{ satisfies } \mathrm{GARP}_\mathbf{v}} 1 - v.$$

Houtman and Maks (1985) proposed an inconsistency index based on the maximal subset of observations that satisfies GARP. This is identical to restricting the adjustments vector to belong to $\{0, 1\}^n$ (see also Smeulders et al. 2014; Heufer and Hjertstrand 2015) and to aggregate using the sum $n - \Sigma_{i=1}^n v_i$. Again, we define this index with respect to an arbitrary aggregator function.

Definition 7. Let $f : [0,1]^n \to [0, M]$ be an aggregator function. The *Houtman-Maks inconsistency index* is

$$I_{HM}(D,f) = \inf_{\mathbf{v}\in\{0,1\}^n : D \text{ satisfies } \mathrm{GARP}_\mathbf{v}} f(\mathbf{v}).$$

Fact 4. $I_V(D, f)$, $I_A(D)$, and $I_{HM}(D, f)$ always exist.

Proof. See Section 1.5 of the appendix.

Afriat's and Houtman-Maks's inconsistency indices are considerably more prevalent in the empirical-experimental literature than Varian's inconsistency index, mainly because of computational considerations (discussed in Sec. 2.1 of the appendix).[11] However, definitions 5, 6, and 7 demonstrate that Afriat's and Houtman-Maks's inconsistency indices are merely reductions of Varian's inconsistency index to subsets of adjustment vectors (and a specific functional form in the case of Afriat's inconsistency index). Moreover, in Section 2.1 of the appendix we claim that, practically, for most individual-level data sets, the Varian inconsistency index can be computed exactly or with an excellent approximation.

In the consistency literature, Afriat (1973) and Varian (1990, 1993) view the extent of the adjustment of the budget line as the amount of income

[11] The money pump inconsistency index proposed by Echenique, Lee, and Shum (2011), the minimum cost inconsistency index suggested by Dean and Martin (2015), and the area inconsistency index mentioned in Heufer (2008, 2009) and Apesteguia and Ballester (2015) are discussed and compared to the Varian inconsistency index in Sec. 2.2 of the appendix. In Sec. 2.3 of the appendix we discuss an inconsistency index based on Euclidean distance rather than on revealed preference, related to an index mentioned in Beatty and Crawford (2011).

wasted by a DM relative to a fully consistent one (hence the term "inefficiency index"). An alternative interpretation (due to Manzini and Mariotti [2007, 2012], Masatlioğlu, Nakajima, and Ozbay [2012], and Cherepanov, Feddersen, and Sandroni [2013]) views the adjusted budget set as a consideration set that includes only the alternatives from the original budget menu that the DM compares to the chosen alternative. By construction, those bundles not included in the attention set are irrelevant for revealed preference consideration. Houtman (1995), for example, holds that the DM overestimates prices and hence does not consider all feasible alternatives. Another line of interpretation for inconsistent choice data is measurement error (Varian 1985; Tsur 1989; Cox 1997). These errors could be the result of various circumstances as (literally) trembling hand, indivisibility, omitted variables, and so forth. All the interpretations above take literally the existence of underlying "welfare" preferences that generate the data (Bernheim and Rangel 2009). In addition, there exist other plausible data-generating processes that may result in approximately (and even exactly) consistent choices (Simon 1976; Rubinstein and Salant 2012).

We do not find a clear reason to favor one interpretation over the other and would rather remain agnostic about the nature of the adjustments required to measure inconsistency. Moreover, this paper takes the data set as the primitive and the utility function as an approximation. As such, the adjustments serve as a measurement tool ("ruler") for quantifying the extent of misspecification.

IV. Parametric Recoverability

The proof of theorem 1 is constructive: if a data set D of size n satisfies GARP$_{\mathbf{v}}$, then finding a utility function that $\mathbf{v}$-rationalizes the data reduces to finding $2n$ real numbers that satisfy a set of n^2 inequalities (see the proofs of lemma 4 and theorem 1 in Sec. 1.4 of the appendix).[12] Although the constructed utility function does not rely on any parametric assumptions, the large number of parameters makes it difficult to directly learn from it about behavioral characteristics of the DM, which are typically summarized by few parameters (e.g., attitudes toward risk, ambiguity, and time). Moreover, generically, a data set can be $\mathbf{v}$-rationalized by more than

[12] Varian (1982) builds on the celebrated Afriat (1967) theorem to construct nonparametric bounds that partially identify the utility function, assuming that preferences are convex (see Halevy, Persitz, and Zrill 2017). His approach has been extended and developed in Blundell, Browning, and Crawford (2003, 2008) (see also sec. 3.2 in Cherchye et al. [2009]). However, to the best of our knowledge, it has not been expanded to include treatment of inconsistent data sets. The parametric approach developed in the current paper extends naturally to inconsistent data sets and easily accommodates nonconvex preferences.

a single utility function. Hence, if one can find a "simpler" (parametric) utility function that rationalizes the data set, it will have an equal standing in representing the ranking information implied by the data set. If one accepts that "simple" may be superior, then one should consider the tradeoff between simplicity and misspecification. We pursue this line of reasoning by considering the minimal misspecification implied by certain parametric specifications.

The problem of parametric recoverability is to approximately rationalize observed choice data by a parametric utility function. We approach this problem by acknowledging that in the case in which the data set is consistent (satisfies GARP), the representation of choice data by utility function almost always entails some tension between two rankings over alternatives. The first is the ranking implied by choices, which is captured by the revealed preference (partial) relation, and the other is the complete ranking induced by the parametric utility function. If the utility function rationalizes the data, then the two rankings are compatible. Otherwise, the two rankings are incompatible and we say that the utility function is misspecified with respect to the data. The incompatibility is manifested by the existence of a pair of alternatives on which the two rankings disagree.

In Section IV.A we propose two loss functions that measure the incompatibility between the two rankings. Obviously, there are other loss functions that are not based on the incompatibility between the suggested utility function and the revealed preference relation. For example, NLLS is a loss function that is based on the distance between the choice predicted by the suggested utility function and the observed choice. In Sections V and VII, we demonstrate empirically the difference between these two types of loss functions.

The main theoretical contribution of the paper is presented in Section IV.B. This result establishes that the loss functions we propose do not depend on the choice data being consistent. In the case of inconsistent choices, the loss functions capture both the extent of inconsistency and the misspecification of the parametric utility function with respect to the data. We prove that the loss functions can then be additively decomposed into a corresponding inconsistency index and a misspecification measure. Section VIII demonstrates the empirical implications of this decomposition to model selection.

A. Incompatibility Indices

1. The Money Metric Index

Consider a bundle x^i that is chosen at prices p^i and a utility function $u(\cdot)$. While x^i is revealed preferred to all feasible bundles, u may rank some of these bundles above x^i. The first loss measure for the incompatibility between a data set D and a utility function u is based on the money metric utility function (Samuelson 1974) and was suggested by Varian (1990; see also Gross 1995). It measures the minimal budget adjustment that makes bundles that u ranks above x^i infeasible, thus eliminating the incompatibility between the two rankings.

DEFINITION 8. The *normalized money metric vector for a utility function* $u(\cdot)$, $\mathbf{v}^*(D, u)$, is such that $v^{*i}(D, u) = m(x^i, p^i, u)/p^i x^i$, where

$$m(x^i, p^i, u) = \min_{\{y \in \Re_+^K \,:\, u(y) \geq u(x^i)\}} p^i y.$$

Let $f : [0, 1]^n \to [0, M]$ be an aggregator function. The *money metric index for a utility function* $u(\cdot)$ is $f(\mathbf{v}^*(D, u))$.

Let $\mathcal{U}^c$ denote the set of all locally nonsatiated, acceptable, and continuous utility functions on $\Re_+^K$.

PROPOSITION 1. Let $D = \{(p^i, x^i)_{i=1}^n\}$, $u \in \mathcal{U}^c$, and $\mathbf{v} \in [0, 1]^n$. The function $u(\cdot)$ $\mathbf{v}$-rationalizes D if and only if $\mathbf{v} \leqq \mathbf{v}^*(D, u)$.

Proof. See Section 1.6 of the appendix.

Proposition 1 establishes that $f(\mathbf{v}^*(D, u))$ may be viewed as a function that measures the loss incurred by using a specific utility function to describe a data set. The function $\mathbf{v}^*(D, u)$ measures the minimal adjustments to the budget sets required to remove incompatibilities between the revealed preference information contained in D and the ranking information induced by u. It also implies that each coordinate of $\mathbf{v}^*(D, u)$ is calculated independently of the other observations in the data set.[13]

If $\mathbf{v}^*(D, u) = 1$, then proposition 1 is merely a restatement of the familiar definition of rationalizability using the money metric as a criterion. A utility function $u \in \mathcal{U}^c$ rationalizes the observed choices if and only if there is no observation such that there exists an affordable bundle that u ranks above the observed choice. In this case we would say that the utility function is correctly specified.

Recall that given an aggregator function $f(\cdot)$, $f(\mathbf{v}^*(D, u))$ measures the incompatibility between a data set D and a specific preference relation represented by the utility function u. Given a set of utility functions $\mathcal{U} \subseteq \mathcal{U}^c$, the MMI measures the incompatibility between $\mathcal{U}$ and D.

DEFINITION 9. For a data set D and an aggregator function $f(\cdot)$ let $\mathcal{U} \subseteq \mathcal{U}^c$. The *money metric index of* $\mathcal{U}$ is

[13] One may intuitively believe that such independent calculation uses only the directly revealed preference information and may fail to rationalize the data based on the indirect revealed preference information. However, since $R_{D,\mathbf{v}}$ is the transitive closure of $R^0_{D,\mathbf{v}}$, it follows that a utility function is compatible with the directly revealed preference information if and only if it is compatible with all the indirectly revealed preference information. An additional implication of this property is that given m data sets D_i of n_i observations and utility function $u(\cdot)$, since u $\mathbf{v}^*(D_i, u)$–rationalizes D_i for every i, then u $\mathbf{v}^*(\cup_{i=1}^m D_i, u)$–rationalizes $\cup_{i=1}^m D_i$, where $\mathbf{v}^*(\cup_{i=1}^m D_i, u) = (\mathbf{v}^*(D_1, u)', \ldots, \mathbf{v}^*(D_m, u)')'$. Moreover, if $f_n(\cdot)$ is additive separable for every n, then $f_{\Sigma_{i=1}^m n_i}(\mathbf{v}^*(\cup_{i=1}^m D_i, u)) = \Sigma_{i=1}^m f_{n_i}(\mathbf{v}^*(D_i, u))$.

$$I_M(D, f, \mathcal{U}) = \inf_{u \in \mathcal{U}} f(\mathbf{v}^*(D, u)).$$

2. The Binary Incompatibility Index

In this subsection we introduce a new loss measure that treats all incompatibilities similarly, by assigning them a maximal loss value.

Definition 10. The *binary incompatibility vector for a utility function* $u(\cdot)$, $\mathbf{b}^*(D, u)$, is such that $b^{*i}(D, u) = 1$ when there does not exist x such that $p^i x^i \geq p^i x$ and $u(x) > u(x^i)$, and $b^{*i}(D, u) = 0$ otherwise. Let $f : [0, 1]^n \to [0, M]$ be an aggregator function. The *binary incompatibility index for a utility function* $u(\cdot)$ is $f(\mathbf{b}^*(D, u))$.

Consider a data set that includes only the ith observation from D. Then the ith element of the binary incompatibility vector tests whether the utility function rationalizes this data set. While the MMI is restricted to the classical environment of choice from linear budget sets, the BII may be easily applied to more general settings of choice from menus. The following proposition is the counterpart of proposition 1 for the BII.

Proposition 2. Let $D = \{(p^i, x^i)_{i=1}^n\}$, $u \in \mathcal{U}^c$, and $\mathbf{b} \in \{0, 1\}^n$. The function $u(\cdot)$ $\mathbf{b}$-rationalizes D if and only if $\mathbf{b} \leqq \mathbf{b}^*(D, u)$.

Proof. See Section 1.7 of the appendix.

Definition 11. For a data set D and an aggregator function $f(\cdot)$, let $\mathcal{U} \subseteq \mathcal{U}^c$. The *binary incompatibility index of* $\mathcal{U}$ is

$$I_B(D, f, \mathcal{U}) = \inf_{u \in \mathcal{U}} f(\mathbf{b}^*(D, u)).$$

3. Monotonicity of the Incompatibility Indices

The following observation follows directly from the definitions of $I_M(D, f, \mathcal{U})$ and $I_B(D, f, \mathcal{U})$ and concerns their monotonicity with respect to $\mathcal{U}$ (see the proof in Sec. 1.8 of the appendix).

Fact 5. For every $\mathcal{U}' \subseteq \mathcal{U}$, $I_M(D, f, \mathcal{U}) \leq I_M(D, f, \mathcal{U}')$ and $I_B(D, f, \mathcal{U}) \leq I_B(D, f, \mathcal{U}')$.

In particular, fact 5 implies that for every $\mathcal{U}' \subseteq \mathcal{U}^c$, $I_M(D, f, \mathcal{U}^c) \leq I_M(D, f, \mathcal{U})$ and $I_B(D, f, \mathcal{U}^c) \leq I_B(D, f, \mathcal{U})$. That is, the value of the loss measures calculated for all continuous, acceptable, and locally nonsatiated utility functions is a lower bound on the incompatibility indices for every subset of utility functions.

B. *Decomposing the Incompatibility Indices*

The methods we propose to construct $\mathbf{v}^*(D, u)$ and $\mathbf{b}^*(D, u)$ do not depend on the consistency of the data set D. Therefore, even if a DM does not satisfy GARP, we can recover preferences (within the parametric family $\mathcal{U}$) that approximate the consistent revealed preference information encoded in choices.[14] The difficulty with this approach arises from the fact that the loss indices include both the inconsistency with respect to GARP and the misspecification implied by the chosen parametric family.

We show that the suggested incompatibility indices can be decomposed into these two components. Our strategy in developing the decomposition is to use an inconsistency index as a measure of internal inconsistency, which is independent of the parametric family under consideration. We prove that the incompatibility indices calculated for all locally nonsatiated, acceptable, and continuous utility functions coincide with the respective inconsistency indices. That is, $I_M(D, f, \mathcal{U}^c)$ equals Varian's inconsistency index (in particular, using the minimum aggregator, $I_M(D, f, \mathcal{U}^c)$ equals Afriat's inconsistency index), and $I_B(D, f, \mathcal{U}^c)$ coincides with the Houtman-Maks inconsistency index. The proof of the theorem invokes theorem 1 and is provided in Section 1.9 of the appendix.

THEOREM 2. For every finite data set D and aggregator function f,

1. $I_V(D, f) = I_M(D, f, \mathcal{U}^c)$;
2. $I_{HM}(D, f) = I_B(D, f, \mathcal{U}^c)$;
3. if $f(\mathbf{v}) = 1 - \min_{i \in \{1, \dots, n\}} v^i$, then $I_A(D) = I_M(D, f, \mathcal{U}^c)$.

Theorem 2 enables us to decompose the loss indices into familiar measures of inconsistency and natural measures of misspecification that quantify the cost of restricting preferences to a subset of utility functions (possibly through a parametric form). By the monotonicity of I_M and I_B (fact 5), for every $\mathcal{U} \subseteq \mathcal{U}^c$ we can write the loss indices of $\mathcal{U}$ in the following way:

$$I_M(D, f, \mathcal{U}) = I_V(D, f) + [I_M(D, f, \mathcal{U}) - I_M(D, f, \mathcal{U}^c)],$$
$$I_B(D, f, \mathcal{U}) = I_{HM}(D, f) + [I_B(D, f, \mathcal{U}) - I_B(D, f, \mathcal{U}^c)].$$

In each decomposition, the first addend is a measure of the cost associated with inconsistent choices that is independent of any parametric restriction and depends only on the DM's choices, while the second addend measures the cost of restricting the preferences to a specific parametric form by the researcher who tries to recover the DM's preferences. A graphical demonstration of this decomposition appears in Section 3 of the appendix.

Two reasons lead us to believe that such decomposition is essential for any method of recovering preferences of a DM who is inconsistent. First,

[14] Andreoni and Miller (2002) and Porter and Adams (2016) find that a great majority of the subjects satisfy GARP. However, other experimental studies (Choi et al. 2007, 2014; Fisman, Kariv, and Markovits 2007; Ahn et al. 2014) report that more than 75 percent of the subjects did not satisfy GARP.

since for a given data set the inconsistency index is constant (zero if GARP is satisfied), the decomposition implies that minimizing $I_M(D, f, \mathcal{U})$ or $I_B(D, f, \mathcal{U})$) is equivalent to minimizing the misspecification within some parametric family $\mathcal{U}$. Second, only when the incompatibility measure can be decomposed can one truly evaluate the cost of restricting preferences to some parametric family compared to the cost incurred by the inconsistency in the choices. The following sections demonstrate the importance of these theoretical insights in analyzing experimental data.

V. Application to Choice under Risk

The goal of this section is to demonstrate the empirical applicability of the MMI as a criterion for recovering parametric preferences.[15] We show that the suggested method can be used to recover approximate preferences for both consistent and inconsistent decision makers. For the inconsistent subjects, we use theorem 2 to assess the degree to which these recovered preferences encode the revealed preference information contained in the choices. We compare the parameters resulting from employing the MMI and a recovery method that minimizes a loss function that is based on the Euclidean distance between observed and predicted choices in the commodity space (NLLS) and show that important qualitative differences arise.

As a starting point, we analyze in this section a data set of portfolio choice problems collected by Choi et al. (2007). In their experiment, subjects were asked to choose the optimal portfolio of Arrow securities from linear budget sets with varying prices. We focus our analysis only on the treatment in which the two states are equally probable. For each subject, the authors collect 50 observations and proceed to test these choices for consistency (i.e., GARP). Then they estimate a parametric utility function in order to determine the magnitude and distribution of risk attitudes in the population. Choi et al. estimate a disappointment aversion (DA) functional form introduced by Gul (1991) (for more details, see Sec. 4 of the appendix):

$$u(x_1^i, x_2^i) = \gamma w(\max\{x_1^i, x_2^i\}) + (1-\gamma) w(\min\{x_1^i, x_2^i\}), \tag{1}$$

where

$$\gamma = \frac{1}{2+\beta}, \quad \beta > -1, \quad w(z) = \begin{cases} \dfrac{z^{1-\rho}}{1-\rho} & \rho \geq 0 \ (\rho \neq 1) \\ \ln(z) & \rho = 1. \end{cases}$$

[15] In analyzing choices from budget menus, recovery based on the MMI retains more ranking information from the data than recovery based on the BII.

The parameter γ is the weight placed on the better outcome. For $\beta > 0$, the better outcome is underweighted relative to the objective probability (of .5) and the decision maker is *disappointment averse*. For $\beta < 0$, the better outcome is overweighted relative to the objective probability (of .5) and the decision maker is *elation seeking*. In the knife-edge case, where $\beta = 0$, the DA functional form reduces to expected utility.

The parameter β has an important economic implication: if $\beta > (=)\ 0$, the decision maker exhibits *first-order (second-order) risk aversion* (Segal and Spivak 1990). That is, the risk premium for small fair gambles is proportional to the standard deviation (variance) of the gamble. First-order risk aversion can account for important empirical regularities that expected utility (with its implied second-order risk aversion) cannot, such as in portfolio choice problems (Segal and Spivak 1990), calibration of risk aversion in the small and large, and disentangling intertemporal substitution from risk aversion (see Epstein [1992] for a survey). A negative value of β corresponds to a DM who is locally risk seeking. Figure 1 illustrates characteristic indifference curves for disappointment averse and elation seeking (locally nonconvex) subjects, respectively. Additionally, $w(x)$ is a standard utility function and is represented here by the CRRA functional form (we also report results in which the utility for wealth function is CARA, that is, $w(z) = -e^{-Az}$, where $A \geq 0$).

We recover parameters using two different methods. The first is the NLLS, which is based on the Euclidean distance between the predicted and the observed choices,

$$\min_{\beta,\rho} \sum_{i=1}^{n} \left\| x^i - \arg \max_{x:\, p^i x \leq p^i x^i} (u(x; \beta, \rho)) \right\|, \tag{2}$$

where $\|\cdot\|$ is the Euclidean norm. The second is the MMI, $I_M(D, f, \mathcal{U})$, using the normalized average sum of squares (henceforth, SSQ) aggregator,

$$f(\mathbf{v}) = \sqrt{\frac{1}{n} \sum_{i=1}^{n} (1 - v^i)^2}.$$

For both methods, we use an optimization algorithm that allows us to recover individual parameters from observed choices for each subject.[16]

[16] The recovery code implements an individual-level data analysis and includes four modules. The first module implements the GARP test and calculates various inconsistency indices (see Sec. 2.1 of the appendix). The other three modules implement the NLLS, MMI (with various aggregators), and BII recovery methods. Each of these three modules can recover preferences in the disappointment aversion (CRRA and CARA) functional family for portfolio choice data and in the constant elasticity of substitution functional family for other-regarding preferences data. The MATLAB code package is available online, and user instructions are included in the package. The disaggregated results (using NLLS, MMI-SSQ, and MMI-MEAN) of the Choi et al. (2007) data are available in a separate Excel file named Choi et al (2007)—Results in the online data archive.

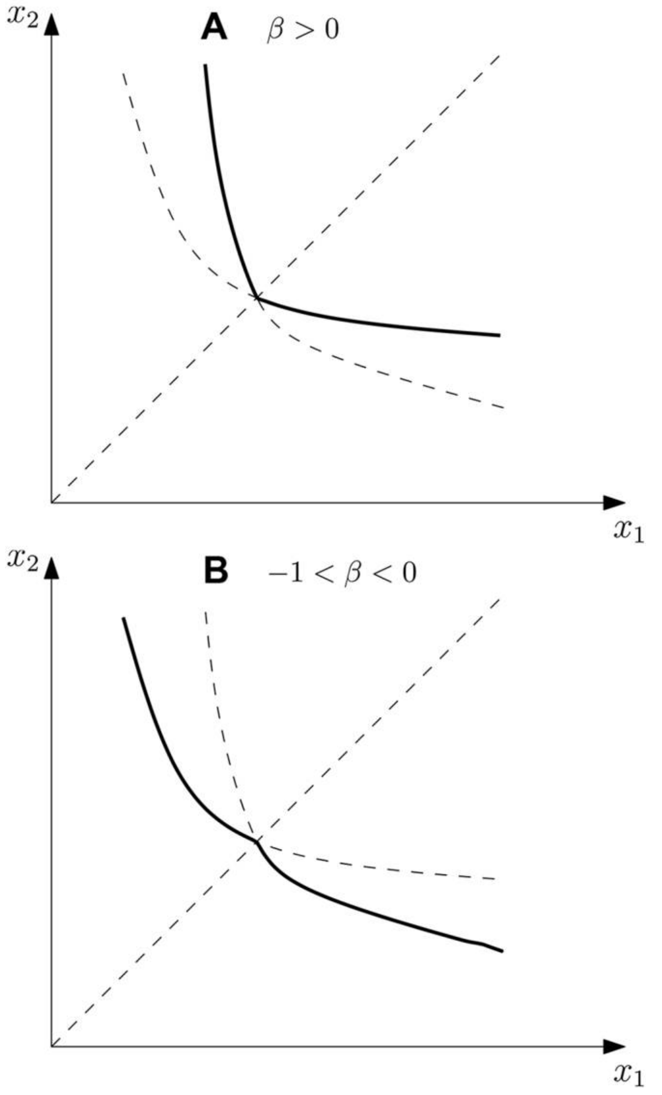

FIG. 1.—Typical indifference curves induced by Gul (1991) disappointment aversion function with $\beta \neq 0$. *A*, Disappointment aversion, $\beta > 0$. *B*, Elation seeking, $-1 < \beta < 0$.

A. Recovering Preferences for Inconsistent Subjects

In Section IV.B we prove the decomposition of the MMI into the Varian inconsistency index, which serves as a measure of inconsistency, and a remainder, which measures misspecification. As such, by using the MMI, we recover parameters that are closest to approximate preferences for those subjects who fail GARP.[17] Throughout the analysis, we exclude subjects with an unreliable Varian inconsistency index (nine out of 47 subjects).[18]

To illustrate, consider table 1, which compares the recovered parameters using the MMI with the SSQ aggregator for two subjects taken from Choi et al. (2007). Subject 320's choices are consistent with GARP, while subject 209's choices are inconsistent. In spite of the fact that subject 320 is consistent, the parametric preferences considered do not accurately encode the ranking implied by her choices, as it requires 13.22 percent wasted income on average. On the other hand, the revealed preference information implied by subject 209's choices is nicely captured by the parametric family, since it implies incompatibility of only 5.63 percent, in spite of the fact that her choices are inconsistent (114 violations of GARP). Additionally, since $I_V = 0.0288$, the decomposed misspecification for subject 209 amounts to only 2.75 percent $(I_M - I_V)$ wasted income, on average, with respect to her approximate preferences. The lesson from this example is that although subject 320 is consistent with GARP, the choices of subject 209 are better approximated using the disappointment aversion with CRRA functional form. As such, the MMI can be applied uniformly to all data sets, and the appropriateness of a certain functional form can be evaluated ex post (as will be further demonstrated in Sec. VIII).

B. Comparison of Recovered Parameters by Method

Figure 2 demonstrates graphically the difference between the recovered parameters by comparing the disappointment aversion parameter (β) as recovered by the NLLS and MMI (SSQ) recovery methods. When NLLS recovers convex preferences ($\beta > 0$), then usually MMI recovers convex preferences as well, although there may be considerable quantitative dif-

[17] Approximate preferences are defined by the set $\tilde{\mathcal{U}} = \{u \in \mathcal{U}^c : I_V(D, f) = I_M(D, f, \{u\})\}$. In general, this set is not a singleton as the vector of budget adjustments, $\mathbf{v}$, required by the calculation of the Varian inconsistency index, is not unique; nor is the utility function that rationalizes a given revealed preference relation, $R_{D,\mathbf{v}}$, for a particular vector of adjustments.

[18] Computing the Varian inconsistency index is a hard computational problem (see the discussion in Sec. 2.1.2 of the appendix). The data of Choi et al. (2007) include 47 subjects; 12 are consistent (pass GARP) and 35 are inconsistent. We take advantage of the sample size and calculate the exact index for 22 of the 35 inconsistent subjects (63 percent), and for four additional subjects we are able to provide a very good approximation. For the other nine subjects we report a weak approximation computed using an algorithm that overestimates the real index. The implication of overestimation is that the decomposition of the MMI overestimates the inconsistency component and underestimates the misspecification component. That said, while the extent of misspecification with respect to the approximate preferences may be underestimated, the recovered parameters are independent of the calculation of the Varian inconsistency index.

TABLE 1
Comparing Consistent and Inconsistent Subjects

Subject	I_V	β	ρ	I_M
320	0	−.509	.968	.1322
209	.0288	.164	.352	.0563

ferences between the recovered parameters. However, when the preferences recovered by NLLS are nonconvex ($\beta < 0$), there seems to be no qualitative relation between the recovered parameters by the two methods.[19]

Moreover, the parameters recovered by NLLS in some of the nonconvex cases imply extreme elation seeking. This property can also be seen clearly from the distribution of the disappointment aversion parameter (β) and the curvature of the utility function (ρ) across subjects, which is reported in Section 5 of the appendix.[20]

In light of the considerable differences between the recovered parameters, an essential next step is to compare these two recovery methods based on an out-of-sample criterion that is independent of the objective function of the candidate methods.

VI. Experimental Design and Procedures

In this section we propose and describe a controlled experiment designed to perform a comparison between NLLS and MMI based on predictive power. Specifically, in the first part of the experiment we used a design inspired by Choi et al. (2007) to collect individual-level portfolio choices from linear budget sets. From each subject's choices we instantaneously recovered approximate parametric preferences by each of the two recovery methods. Using this information, we constructed pairs of portfolios such that the rankings induced by each set of approximate preferences on these portfolios disagree. Therefore, each recovery method implied an opposite prediction on the subject's choice from each pair of constructed portfolios. In the second and final part of the experiment, the subject chose a portfolio from each of the constructed pairs of portfolios, thus providing an out-of-sample direct criterion for the relative predictive success of each method.

[19] When β_{NLLS} is positive, then β_{NLLS} and β_{MMI} are significantly positively correlated ($p =$ .0283), while when $\beta_{\text{NLLS}} < 0$, we cannot reject the null hypothesis of no linear correlation between them ($p = .1093$).

[20] Note that the recovered parameters for NLLS may differ from those reported in Choi et al. (2007) for several reasons: we allow for elation seeking ($-1 < \beta < 0$), we permit boundary observations ($x^i = 0$), we use Euclidean norm (instead of the geometric mean), and we use multiple initial points (including random) in the optimization routine (instead of a single predetermined point). We were able to replicate the results reported by Choi et al.

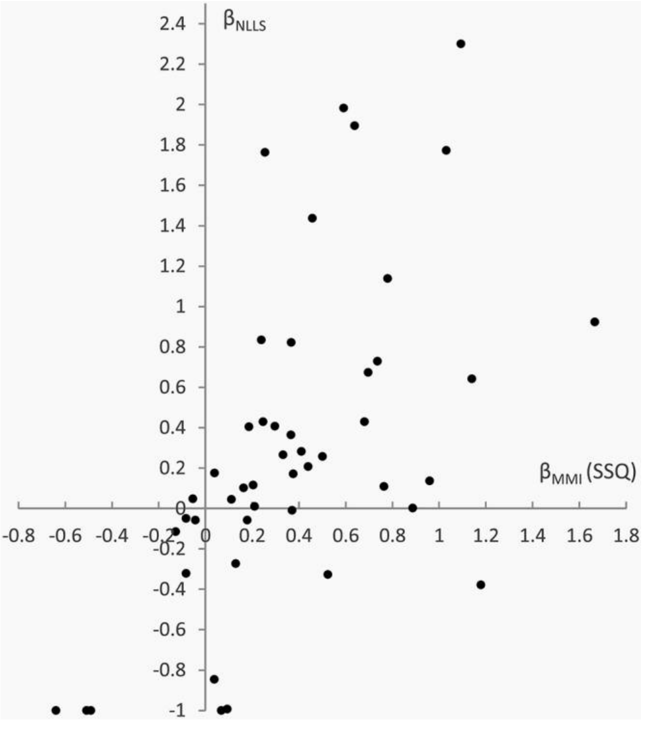

FIG. 2.—Disappointment aversion parameter: NLLS versus MMI (SSQ)

A. *Procedures and Details*

For the experiment we recruited 203 subjects using the ORSEE system (Greiner 2015), which is operated by the Vancouver School of Economics at the University of British Columbia. Subjects participated voluntarily and were primarily undergraduate students representing many disciplines within the university. Before subjects began the experiment, the instructions were read aloud as subjects followed along by viewing a dialog box on-screen (see Sec. 6.1 of the appendix for the instructions). The experiments were conducted over several sessions in October 2014 and February 2015 at the Experimental Lab at the Vancouver School of Economics. Each experimental session lasted approximately 45 minutes.

In the first part of the experiment, the subjects selected portfolios of contingent assets from a series of 22 linear budget sets with differing price ratios and/or relative wealth levels. These choices were used to instantaneously recover individual preferences using the two recovery methods introduced above. From these two sets of recovered parameters we constructed, uniquely for each subject, a sequence of nine pairs of portfolios from which subjects chose during the second part of the experiment.

Each pair included one *risky* portfolio, where outcomes differed across states, and one *safe* portfolio, where the subject obtained a certain payoff regardless of the state. Note that the subjects were unaware of the background calculation and the relation between the two parts of the experiment.

In total, each subject made 31 choices across the two parts of the experiment. After both rounds were completed, one of these rounds was selected randomly to be paid according to the subject's choice. For whichever round was selected, subjects were asked to flip a coin in order to determine for which state they would be paid. The choices were made over quantities of tokens, which were converted at a 2 : 1 exchange rate to Canadian dollars. Subjects were paid privately upon completion of the experiment, and their earnings averaged about C$19.53 in addition to a fixed fee of C$10.00 for showing up to the experiment on time.

B. *Part 1: Linear Budget Sets*

In this part of the experiment subjects chose portfolios of contingent assets from linear budget sets. Each portfolio, $x^i = (x_1^i, x_2^i)$, consisted of quantities of tokens such that subjects received x_1^i tokens if state 1 occurred and x_2^i tokens if state 2 occurred, with each state equally likely to occur. Portfolios were selected from a linear budget set, defined by normalized prices, p^i, and displayed graphically via a computer interface. All participants faced the same budget sets and in the same order; however, this was not known to the subjects.

The interface was a two-dimensional graph that ranged from 0 to 100 tokens on each axis. Subjects were able to adjust their choices in increments of 0.2 tokens with respect to the x-axis. Additionally, token allocations are rounded to one decimal place. Screen shots of the graphical interface are included in Section 6.1 of the appendix. Subjects chose a particular portfolio by left-clicking on their desired choice on the budget line and were asked to confirm their choice before moving on to the next round. Subjects were restricted to choose only those points that lie on the boundary of the budget set to eliminate potential violations of monotonicity.[21]

The budget sets, and associated prices, were specifically chosen to address two issues. First, a sufficient overlap between budget sets is required

[21] Two special cases were treated slightly differently by the interface. First, when subjects chose a point close to the certainty line, a dialog box appeared that asked them if they meant to choose the allocation in which the values in both accounts are equal, guaranteeing themselves a sure payoff, or if they prefer to stick with the point they chose. Second, when subjects chose a point that is close to either axis, a dialog box appeared that asked them if they meant to choose a corner choice or if they prefer to stick with the point they chose. This is done to overcome mechanical aspects of precision in the interface at points that have specific qualitative significance.

so that a GARP test will have sufficient power.[22] Second, an emphasis on moderate price ratios was required to identify the role of first-order risk aversion/seeking (represented by β) in the subject's preferences. For further details on the budget lines selection, see Section 6.2 of the appendix.

C. *Part 2: Pairwise Choices*

Upon completion of the tasks in part 1, the subject's choices were used to recover structural parameters for the disappointment aversion functional form with CRRA using both NLLS and MMI (SSQ). These two sets of parameters were used to construct a sequence of nine pairwise choice problems. In each pairwise comparison, subjects chose one of two portfolios—one *risky* portfolio (where payoffs differ across states) and one *safe* portfolio (where the payoff is certain)—represented as points in the coordinate system.[23]

As preferences are a binary relation over bundles, pairwise choices allow us to directly observe the subject's preferences in their most fundamental form. Therefore, we employed a pairwise choice procedure to adjudicate between the two sets of recovered parameters, $\hat{\theta}_{\rm NLLS} = \{\hat{\beta}_{\rm NLLS}, \hat{\rho}_{\rm NLLS}\}$ and $\hat{\theta}_{\rm MMI} = \{\hat{\beta}_{\rm MMI}, \hat{\rho}_{\rm MMI}\}$. Given a risky portfolio, x^R, we calculated the certainty equivalent, CE_i (CE_j), for both sets of parameters, $\hat{\theta}_i$ ($\hat{\theta}_j$), where i, $j \in \{{\rm NLLS}, {\rm MMI}\}$. In the case in which both $\hat{\beta}_{\rm NLLS} > 0$ and $\hat{\beta}_{\rm MMI} > 0$ (both recovered preferences are convex), we selected the safe portfolio to be the midpoint between the two certainty equivalents, $x^S = (CE_i + CE_j)/2$. Then if $CE_i > CE_j$, in ranking the risky portfolio x^R and the safe portfolio x^S, $\hat{\theta}_i$ induces a preference for the risky portfolio while $\hat{\theta}_j$ induces a preference for the safe one. Since pairwise choices reveal the DM's underlying preferences, choice of the risky portfolio reveals that the set of parameters $\hat{\theta}_i$ better approximates the DM's preferences, while choosing the safe portfolio reveals the opposite.

In the case in which at least one recovery method resulted in an elation seeking preference ($\hat{\beta}_{\rm NLLS} < 0$ or $\hat{\beta}_{\rm MMI} < 0$), part 2 of the experiment enabled us to identify the extent of nonconvexity of the underlying preferences, in addition to driving a wedge between the two sets of parameters. To achieve this additional goal we note that for locally nonconvex preferences the certainty equivalent may exceed the expected value for some risky portfolios. Therefore, the pairwise choice procedure searched for

[22] For a detailed analysis of a test that demonstrates that this set of budget sets is sufficiently powerful, see Sec. 6.2 of the appendix.

[23] A fundamental design requirement was that subjects would view the two related but distinct tasks in the same frame. Hence, the interface was designed so that the pairwise choice problems were presented in the same two-dimensional coordinate system as the budget lines task. Moreover, as most subjects view the pairwise choice as a more primitive task, the instructions were written so that part 1's interface was presented through a natural extension of a pairwise choice task. See the instructions in Sec. 6.1 of the appendix.

a risky portfolio x^R, such that $CE_j(x^R) < E[x^R] < CE_i(x^R)$, and chose the safe portfolio, x^S, such that $x^S = E[x^R]$.[24] Similarly to the midpoint design, choice of the risky portfolio reveals that the set of parameters $\hat{\theta}_i$ better approximates the DM's preferences, while choosing the safe portfolio reveals the opposite. In addition, the choice of the safe (risky) portfolio reveals local risk aversion (seeking) in the neighborhood of the portfolio x^R, providing direct evidence to the extent of nonconvexity of the underlying DM's preferences.[25]

To investigate the nature of local risk attitudes across subjects, the pairwise choice problems were constructed so that in six of them the risky portfolio was of *low variability* while in the other three problems, the risky portfolio was of *high variability*. For a detailed description of the algorithm that constructs the pairwise choices, see Section 6.3 of the appendix.

D. *Incentive Compatibility*

Finally, two comments regarding the incentive compatibility of this design. First, since this is a chained experimental design, had subjects been aware that parts of the experiment are connected and understood the precise structure of the pairwise choice procedure, they may have been able to manipulate their choices in order to maximize their expected gains. We are confident that this is not the case since the instructions and the experimental procedure were designed carefully not to reveal that the portfolios offered in part 2 were calculated on the basis of the choices in part 1. Moreover, an extremely detailed knowledge of the experimental design and the recovery procedures is essential in order to manipulate the choices successfully.

Second, subjects were paid according to their decision in a randomly selected problem. If subjects isolate their decisions in different problems, this payment system is incentive compatible. If they had integrated their decisions (by reducing the compound lottery induced by the random incentive system and their decisions), their choices would have been biased toward expected utility behavior ($\beta = 0$), a pattern observed for only about 40 percent of the subjects, as will be shown in Section VIII.B.

VII. Results: Pairwise Choice

The results of part 1 of the experiment exhibit patterns broadly similar to those reported in Section V for the data sets gathered by Choi et al. (2007)

[24] Since risk attitude depends on both β and ρ, it is possible to have $\beta < 0$ and have the associated utility function exhibit risk aversion with respect to some risky portfolio. However, $\beta < 0$ is sufficient for a utility function to display, at least locally, risk-seeking behavior with respect to portfolios with small variance.

[25] The safe portfolio was the preferred alternative by the MMI recovery method in 927 of the 1,827 pairwise choices in our sample (50.7 percent).

(see Sec. 7 of the appendix).[26] We use these results extensively (together with the results of Choi et al.) in Section VIII to demonstrate several important implications of theorem 2.

The current section, however, is devoted to the results from part 2 of the experiment. This part was designed so that in each pairwise comparison, one of the portfolios is preferred according to the recovered parameters of the MMI (SSQ) and the other is preferred according to the recovered parameters of the NLLS. Hence, in this section we analyze the choices of the subjects to infer the relative predictive accuracy of the two recovery methods.

The results provided here are based on the full sample. As the complete sample includes subjects and choices that arguably should not be included in such a comparison (as the choices in part 1 are too inconsistent or the algorithm could not meaningfully separate the recovery methods), Section 8 of the appendix reports similar results for a refined sample.

In the following, statistical significance is defined with respect to the null hypothesis that MMI predictions are not better than random predictions, which entails a one-sided binomial test. The p-values should be interpreted as the likelihood that the MMI correctly predicts x or more out of n choices correctly by chance alone. Results are reported at the aggregate and individual levels.

A. Results

1. Aggregate Results

In the aggregate analysis we treat all observations as a single data set. The first row of table 2 reports the predictive success of the MMI recovery method over all 1,827 observations (203 subjects times nine observations per subject). The next two rows report similar results for the low-variability and high-variability portfolios separately. These results suggest that the MMI is a significantly (p-value smaller than 1 percent) better predictor of subjects' choices both overall and for the two subclasses of portfolios separately (at an odds ratio of approximately 1.17).

2. Individual Results

For the individual-level analysis each subject is treated as a single data point. Denote the number of correct MMI predictions by X. With only nine choices per subject it may be difficult to declare one of the two methods as decisively better for moderate values ($X \in \{3, 4, 5, 6\}$), as the prob-

[26] The data gathered in the experiment are available in a separate Excel file named Halevy et al (2017)—Data. The disaggregated results of part 1 are available in a separate Excel file named Halevy et al (2017) Part 1—Results, both in the online data archive.

TABLE 2
Aggregate Results

	Observations	Correct Predictions by MMI	p-Value
Complete sample	1,827	986 (54.0%)	.0004
Low variability	1,218	652 (53.5%)	.0074
High variability	609	334 (54.8%)	.0093

ability of getting each one of these values at random is greater than 15 percent. Hence, table 3 reports the number of subjects for whom one method was decisively better—able to predict more than two-thirds of the choices correctly ($X \in \{0, 1, 2, 7, 8, 9\}$).

There are 103 subjects for which one recovery method was decisively better. The probability that one recovery method would be decisively better by random prediction alone for a single subject is approximately 18 percent, so the probability of having 103 decisive predictions out of 203 subjects is close to zero. One preliminary conclusion is that our design and algorithm were able to separate the predictions made by NLLS and MMI effectively.

The empirical distribution of correct MMI predictions is significantly different from a null hypothesis of random prediction.[27] As is evident from table 3, MMI is a significantly better predictor at the individual level as well (one-sided p-value .038), as it is a decisively better predictor for 45 percent more subjects than NLLS.[28]

B. *Disappointment Aversion*

1. Definite versus Indefinite Disappointment Aversion

To further our understanding of the results we divide the sample into two classes according to the recovered parameters. The definite disappointment averse (DDA) group is composed of those subjects for whom both methods recover $\beta \geq 0$, whereas the indefinite disappointment averse (IDA) group is composed of those subjects for whom β is negative for one or both recovery methods. The DDA group includes 150 subjects while the other 53 subjects belong to the IDA group.

[27] The statistic for the multinomial likelihood ratio test is $-2\ln(L/R) = -2\Sigma_{i=1}^{k} x_i \ln(\pi_i/p_i)$, where the categories are the number of correct predictions by the MMI, π_i is the theoretical probability of category i if the prediction is random, while p_i is the frequency of category i in the data. This statistic for the complete sample equals 85.523, which, by a chi-squared distribution with nine degrees of freedom, has a p-value of approximately zero. Pearson's chi-squared test provides similar results.

[28] The p-value in the third column is calculated for the group of 103 subjects for whom one recovery method was decisively better than the other, under the null hypothesis that each recovery method has an equal chance of being decisive.

TABLE 3
INDIVIDUAL-LEVEL RESULTS: 203 SUBJECTS

$X \geq 7$	$X \leq 2$	p-Value
61	42	.0378

In the aggregate analysis we treat the whole set of observations as a single data set with 1,350 observations for the DDA group and 477 for the IDA group. Table 4 demonstrates that the MMI recovery method remains a better predictor in both groups. When the sample includes only the DDA group, the advantage of the MMI is significant at the 5 percent level (but the advantage is not significant in the refined sample; see table 4 in Sec. 8 of the appendix). However, when the sample includes only the IDA group, the advantage of the MMI recovery method is highly significant in spite of the smaller sample size (and is robust to the refinement).

At the individual level table 5 shows that although the MMI recovery method predicts decisively better than NLLS in both DDA and IDA, the difference in predictive accuracy within the DDA group is insignificant. However, the difference within the IDA group is substantial and statistically significant as MMI predicts decisively for almost twice as many subjects for which NLLS predicts decisively.

2. Definite Elation Seeking

Further, we focus on a subset of the IDA group, referred to as the definite elation seeking (DES) group, that includes the 29 subjects for whom both recovery methods recover $\beta < 0$. The MMI recovery method predicted correctly 163 of the 261 choice problems these subjects encountered, which amount to 62.5 percent of the observations. Hence, the difference between the recovery methods within the DES group is even more substantial than in the whole IDA group, and it is highly significant (p-value < .0001).

The individual results are similar: for 20 out of the 29 subjects in the DES group, one recovery method predicted decisively better (more than two-thirds of pairwise choices) than the other, and for 75 percent of them (15 out of 20) the MMI produced the better prediction (p-value = .0207). These results suggest that the difference in predictive success between

TABLE 4
AGGREGATE RESULTS BY GROUP

	Observations	Correct Predictions by MMI	p-Value
DDA	1,350	706 (52.3%)	.0484
IDA	477	280 (58.7%)	.0001

TABLE 5
Individual-Level Results by Group

DDA (150)			IDA (53)		
$X \geq 7$	$X \leq 2$	p-Value	$X \geq 7$	$X \leq 2$	p-Value
38	30	.1981	23	12	.0448

the MMI and NLLS recovery methods can be attributed mostly (but not only) to subjects for whom the recovery methods resulted in apparent nonconvex preferences.

3. MMI versus NLLS When Preferences Are Nonconvex

The pairwise comparisons in part 2 of the experiment allow us to directly observe the subject's preferences in these nonconvex regions of their indifference curves. Our results imply that the MMI recovers a significantly more accurate representation of subject preferences when the underlying preferences are nonconvex.

Specifically, for 21 of the 29 subjects in the DES group (72.4 percent) the disappointment aversion parameter recovered by the NLLS is more negative than the one recovered by the MMI.[29] While we cannot conclude that NLLS systematically overstates the extent of elation seeking, this pattern of differences does correspond to particular patterns of choices observed in part 1 of the experiment. Figure 3 illustrates the choices from part 1 of the experiment for four characteristic subjects as well as their corresponding parameter estimates. Generally, as the subject's choices drift farther from the certainty line, the greater is the difference between the parameter recovered by the NLLS and the MMI recovery methods.

C. *Illustrative Discussion*

To conclude this section we wish to suggest an informal explanation for our finding. Briefly, when choices are induced by nonconvex preferences for which the model is misspecified, the NLLS recovery method will most probably pick a set of parameters that implies greater nonconvexity than implied by the set of parameters recovered by the MMI method. The results of part 2 of the experiment suggest that the parameters recovered by the MMI are considerably better in predicting the subjects' choices in the nonconvex region.

[29] For 19 of these 21 subjects the difference is more than 0.1. For six of the eight subjects for whom the parameter recovered by the NLLS is less negative than the one recovered by the MMI, the difference is less than 0.1.

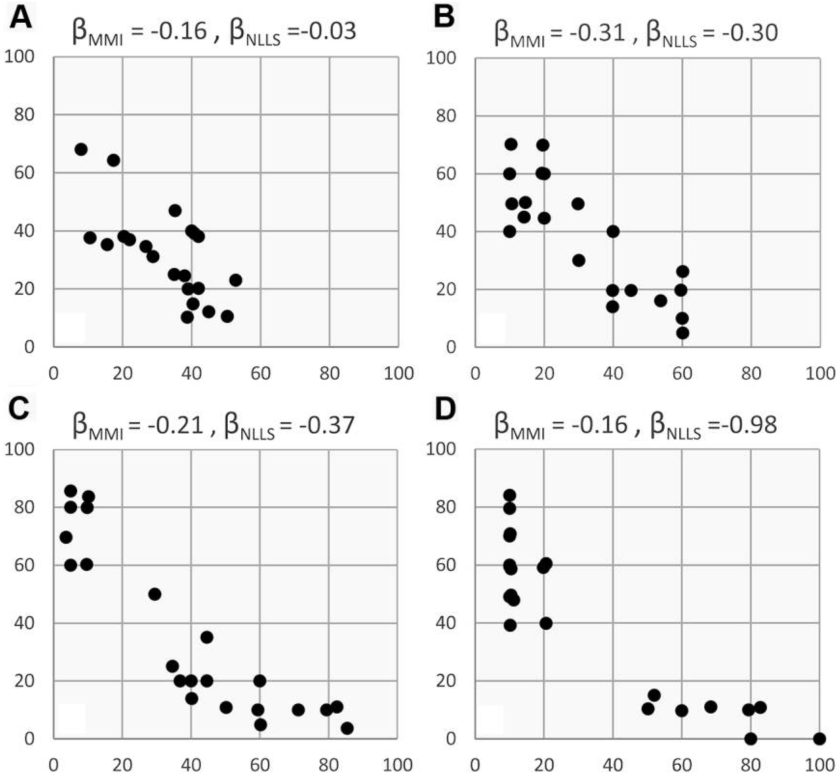

FIG. 3.—Patterns of choice: nonconvex preferences: *A*, subject 1203; *B*, subject 1512; *C*, subject 2203; *D*, subject 301.

To demonstrate the multiplicity of approximated preferences given the same data set, consider two simulated subjects with preferences represented by the utility functions u and u' with the characteristic indifference curves shown in figure 4*A*. Faced with the same sequence of linear budget sets as our subjects in part 1 of the experiment, the implied optimal choices for these simulated subjects are exactly the same and are illustrated in figure 4*B*.[30] This pattern of choices is highly structured and may result from a reasonable heuristic according to which the subject wants to guarantee a payment of 10 tokens but is willing to bet with the remainder of her income on the cheaper asset (unless the relative prices are extreme). In order to approximate this behavior within the DA model,

[30] Notice that the pattern of choice for these simulated subjects is very similar to that of subject 301 in fig. 3*D*. Not surprisingly, the recovered parameters for our simulated subject are also very similar to those of subject 301: $\beta_{\text{MMI}} = -0.24$, $\rho_{\text{MMI}} = 0.40$, $\beta_{\text{NLLS}} = -0.91$, and $\rho_{\text{NLLS}} = 1.55$.

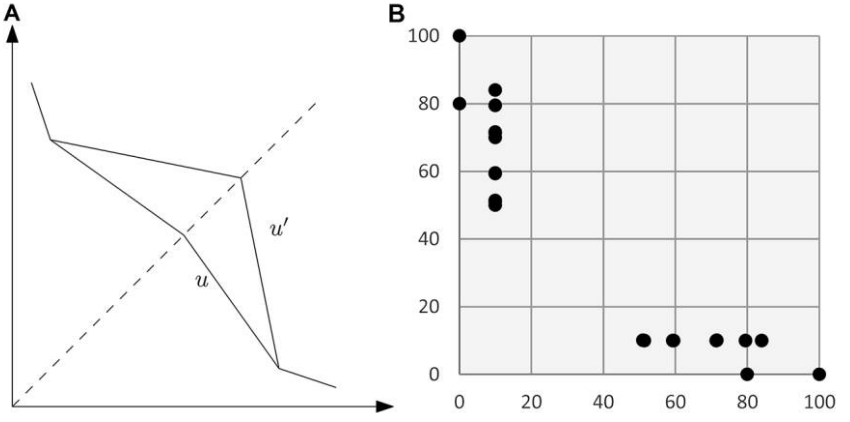

Fig. 4.—Two simulated subjects: *A*, typical indifference curves; *B*, choices given the linear choice problems presented in part 1 of the experiment.

which does not span this heuristic, NLLS resorts to substantial nonconvexity while the MMI can rationalize these choices without making strong claims on behavior that is unobservable using linear budget lines. For an informal demonstration, see Section 9 of the appendix.

VIII. Results: Choice from Budget Lines

The usage of the MMI as a recovery method relies on the observation that it can be decomposed into an inconsistency index, which is independent of the specific utility function evaluated, and a misspecification index, which depends on the subset of utility functions considered. Given two parametric families $\mathcal{U}$ and $\mathcal{U}'$, a researcher will calculate the value of the MMI loss index for each family ($I_M(D, f, \mathcal{U}')$ and $I_M(D, f, \mathcal{U})$), and since both incorporate the same inconsistency measure, $I_V(D, f)$, the data set D may be better approximated by $\mathcal{U}$ or $\mathcal{U}'$ depending on the magnitude of the loss index. Moreover, an important implication of fact 5 is that if we impose an additional parametric restriction on preferences, the misspecification will necessarily (weakly) increase. If $\mathcal{U}'$ is nested within $\mathcal{U}$, the difference between the value of the loss indices at $\mathcal{U}$ and $\mathcal{U}'$ is a measure of the marginal misspecification implied by the restriction of $\mathcal{U}$ to $\mathcal{U}'$.

In this section we demonstrate the application of these insights for evaluating nested and nonnested model restrictions in the two experimental data sets. We perform a subject-level analysis for the data collected in part 1 of the experiment and the data collected by Choi et al. (2007). We begin by evaluating the misspecification implied by the disappointment aversion functional form (with CRRA and CARA utility functions). Then we demonstrate the evaluation of nested parametric restrictions by measuring the misspecification implied by restricting the functional form to expected utility. Finally, we compare the CRRA and CARA functional forms as an example for the evaluation of nonnested model restrictions.[31]

A. *Evaluating Misspecification*

Using the decomposition of the MMI into the Varian inconsistency index (measure of consistency) and a residual that measures misspecification, we can calculate the misspecification for each subject.

One practical challenge is that the calculation of the Varian inconsistency index is computationally hard. However, as discussed in detail in Section 2.1 of the appendix, we are able to calculate the exact values (or very good approximations) of this index for most of the subjects in the two samples.

Table 6 provides some descriptive statistics on the misspecification in the recovered preferences of subjects for whom the Varian inconsistency index was calculated exactly or with tight approximation. It demonstrates that for approximately two-thirds of them, the disappointment aversion model entails less than 5 percent misspecification. In addition, table 6 provides a preliminary evidence that, on an aggregate level, the disappointment aversion may be more misspecified with CARA than with CRRA.

The bottom two rows of table 6 suggest that in both samples, the portion of misspecification in the loss index is considerably larger than the portion of inconsistency. In fact, there are almost no subjects for whom the portion of inconsistency is larger than the portion of misspecification.[32]

B. *Evaluating a Restriction to Expected Utility*

Expected utility is nested within the disappointment aversion model, satisfying the restriction that $\beta = 0$. We evaluate whether or not this restriction is justified by examining the additional misspecification implied by this restriction.[33] Given the choice of functional form (disappointment

[31] For conciseness, throughout this section we use the SSQ aggregator. Similar calculations are available using the MEAN aggregator in the results file Choi et al (2007)—Results and Halevy et al (2017) Part 1—Results in the online data archive.

[32] Since those subjects for whom the Varian inconsistency index could not have been calculated properly were dropped, the sample slightly overrepresents the less inconsistent subjects.

[33] In the results files in the online data archive (Choi et al (2007)—Results and Halevy et al. (2017) Part 1—Results), we include descriptive statistics of the parameter frequencies in 1,000 resamplings of each individual data set in every reported recovery scheme. Potentially, we could have used these distributions to evaluate whether the restriction can be rejected. However, since we do not provide any proof that these resamplings indeed recover confidence sets for the parameters, we merely interpret them as a measure for the sensitivity of the recovered parameters to extreme observations.

TABLE 6
Misspecification Using the Disappointment Aversion Functional Form

	Part 1 of the Experiment		Choi et al. (2007)	
	CRRA	CARA	CRRA	CARA
Number of subjects with at most 5% misspecification	136 (68%)	127 (63.5%)	26 (68.4%)	23 (60.5%)
Number of subjects with at least 10% misspecification	4 (2%)	10 (5%)	3 (7.9%)	6 (15.8%)
Subjects for whom misspecification is more than 90% of the MMI	149 (74.5%)	153 (76.5%)	26 (68.4%)	27 (71.1%)
Subjects for whom misspecification is less than 50% of the MMI	0 (0%)	0 (0%)	1 (2.6%)	1 (2.6%)
Original sample	203 subjects		47 subjects	
Consistent	92 (45%)		12 (26%)	
Dropped	3 (1.5%)		9 (195)	
Inconsistency level	At most 6%		At most 2.5%	

Note.—The sample includes all the subjects for whom the Varian inconsistency index was calculated exactly or with good approximation.

aversion with CRRA or CARA utility index), we use the ratio $[I_M(D, f, EU) - I_M(D, f, DA)]/[I_M(D, f, DA) - I_V(D, f)]$, where DA stands for the disappointment aversion (unrestricted) model, EU stands for the expected utility model, and f is the chosen aggregator.

If the restriction to expected utility implies a proportional increase in the misspecification of more than 10 percent, then we tend to reject the expected utility specification. Included in the sample are subjects whose Varian inconsistency index was calculated exactly or with good approximation and whose measured misspecification of the disappointment aversion model was less than 10 percent, implying that it is a reasonable model to capture their choices.

The results in table 7 demonstrate that choices of between one-third and one-half of the subjects are well approximated by the expected utility model, while for the others (more than half) the restriction to expected utility implies a substantial increase in misspecification.

C. *Comparison of Nonnested Alternatives*

The MMI also allows the researcher to evaluate nonnested alternatives. Here, we compare two utility indices for the disappointment aversion functional form—CRRA and CARA. We can calculate the extent of misspecification implied by each functional form and select the functional

TABLE 7
EVALUATING THE RESTRICTION TO EXPECTED UTILITY

	Part 1 of the Experiment	Choi et al. (2007)
CRRA	40.8% (80 of 196)	32.4% (11 of 34)
CRRA	44.7% (85 of 190)	45.2% (14 of 31)

NOTE.—The percentage of subjects for whom the additional misspecification implied by the expected utility restriction is less than 10 percent (the number of subjects who are well approximated by expected utility out of the number of subjects in the sample).

form that represents a decision maker's preferences best on a subject-by-subject basis.

Table 8 reports that choices made by about three-quarters of subjects are better approximated by the disappointment aversion model with CRRA than with the CARA utility index.

This result strengthens if we restrict the samples to include only those subjects who are not too inconsistent (i.e., the Varian inefficiency index was calculated exactly or with good approximation) and the difference between the models is substantial (i.e., the difference in misspecification between the two models is greater than 10 percent).

IX. Conclusions

This paper proposes a general methodology to structurally recover parameters (in the current study preferences) based on minimizing the incompatibility between the ranking information encoded in choices and the ranking induced by a candidate structural model (here utility function). We show that this incompatibility can be decomposed into an inconsistency index, which measures how far the data are from optimizing behavior (GARP), and a remainder that captures the model's misspecification, which is in the researcher's control. This approach is applicable to a variety of incompatibility indices and aggregator functions.

TABLE 8
CHOICE OF UTILITY INDEX

	Part 1 of the Experiment	Choi et al. (2007)
Full sample	71.4% (145 of 203)	80.9% (38 of 47)
Restricted sample	88% (103 of 117)	80% (24 of 30)

NOTE.—The percentage of subjects with lower misspecification using CRRA than CARA (number of subjects better approximated by CRRA than CARA out of the number of subjects in the sample). The full sample includes all subjects for whom the loss function was calculated. The restricted sample includes subjects whose Varian inefficiency index was calculated exactly or with good approximation and the difference in misspecification between the two indices is greater than 10 percent.

We demonstrate the proposed method in an environment of choice under risk and show that it may lead to different recovered parameters than standard NLLS, which represents recovery methods that minimize the distance between the observed data and the model's prediction. In order to compare the two methods on the basis of an objective criterion, we design and execute an experiment that distinguishes between the methods on the basis of their predictive success in out-of-sample pairwise comparisons. The results demonstrate that the proposed recovery method does a better job in predicting choices, especially when choices imply nonconvex preferences—an environment in which minimizing the distance between observed and predicted choices is problematic. Although the goal of the experiment is to distinguish parametric recovery methods, it is fully based on a subject's choices: her choices in part 1 (choice from linear budgets sets) determine the pairwise comparisons she will face in part 2, and her choices in the latter part inform an outside observer which recovery method provides better predictions. Moreover, choices made in pairwise comparisons reveal preferences in their purest form and permit their identification in scenarios in which other elicitation methods can only provide bounds.

The empirical analysis followed the theoretical decomposition result, which allows a researcher to evaluate the change in misspecification implied by nested and nonnested models. In the context of choice under risk, we demonstrate the relative importance of misspecification relative to inconsistency and that although a nonnegligible minority of the subjects are well approximated by the expected utility model, the choices of the majority of subjects are better approximated by a more general model of non–expected utility.

The current investigation includes theoretical foundations, empirical implications, and experimental evaluation, but we view it only as a necessary first step in integrating insights from revealed preference theory into otherwise standard structural recovery problems in economics. The model selected here is simple (utility maximization) yet central in economics and finance. The implied nonconvexities are noncoincidental, as they result from a reasonable calculated procedure. We believe that an important next step in this research program is the integration of a stochastic component into the present deterministic model, while retaining the crucial distinction between inconsistency and misspecification.

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Risk Aversion and Incentive Effects: New Data without Order Effects

By CHARLES A. HOLT AND SUSAN K. LAURY*

Holt and Laury (2002) used a menu of ordered lottery choices to make inferences about risk aversion under various payment conditions. The main results of that paper were: (a) subjects are risk averse, even for relatively small payments of less than \$5; (b) risk aversion increases sharply with large increases in the scale of cash payoffs; and (c) there is no significant effect from increasing the scale of hypothetical payment. With a few exceptions noted in the paper, all treatments began with a low-payment choice, followed by a choice with hypothetical payments that had been scaled up (by 20×, 50×, or 90×), followed by a real-cash decision with the same high payment scale (20×, 50×, or 90×), followed by a single, final, low (1×) real payment choice. Those in the 90× treatment could earn amounts ranging from \$9.00 to \$346.50 in this task. As Glenn W. Harrison et al. (2004) correctly note, this design confounds order and treatment effects since the high real payment choice was always completed *after* the low real and high hypothetical payment tasks.

In a new experiment reported below, we first seek to replicate Harrison et al.'s finding that the order effect (participating in a low-payment choice before making a high-payment choice) magnifies the scale effect. In a second treatment, each subject completes the menu of lottery choices under just one payment condition (1× or 20×, real or hypothetical), thereby eliminating any order effects.

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I. New Data

The new experiment was conducted in 2004 using 216 subjects recruited from undergraduate economics classes at the University of Virginia.[1] As in our previous experiment, each session began with a lottery choice "trainer" and a second unrelated experiment. Results are presented in Table 1. For comparison, we include data from the Holt and Laury (2002) experiment, as shown in the top two rows of Table 1, and from Harrison et al.'s (2005) experiment in rows three and four.

In the first treatment of our new experiment, 48 subjects completed a real low-payment choice, followed by a real high-payment choice, in which all choices were scaled up by a factor of 20.[2] Results are presented in Table 1, row 5. The average number of safe choices for the low (1×) real treatment is shown in row five as 6.1. When real cash payments are scaled up by a factor of 20, the average number of safe choices made by these subjects increased to 7.1. As can be seen, subjects from the new experiment are somewhat more risk averse than those used in the earlier studies; however, the scale effect (from 1× to 20×) with cash payments is essentially the same as that of our previous experiment. In both cases, the average number of safe choices increased by approximately one safe

[1] Unlike the experiment reported in Holt and Laury (2002), decisions were recorded using a computer interface. The die-throw, however, was still done by the experimenter by hand. Also, the left/right order of the safe and risky options was alternated in successive 12-person sessions. The order of presentation did not matter, and so we pool the data from both presentation orders.

[2] As in our original experiments, as a rough control for wealth effects, a person had to agree to give up the payment from the first (1×) choice in order to participate in the high-payment choice. One subject did not agree to participate in the high-payment choice, stating that she felt she had earned enough in the experiment already. Omitting this subject from the following analysis has no effect on these results.

TABLE 1—AVERAGE NUMBERS OF SAFE CHOICES: ORDER AND INCENTIVE EFFECTS

Experiment	Treatment	1×	10×	20×	50×	90×
Holt and Laury (2002)	1. Real (ordered)	5.2[a]		6.0[c]	6.8[c]	7.2[c]
U.C.F., Ga. St., U. Miami 208 subjects	2. Hypothetical (ordered)			4.9[b]	5.1[b]	5.3[b]
Harrison et al. (2004)	3. Real (ordered)	5.3[a]	6.4[b]			
South Carolina 178 subjects	4. Real (unordered)		6.0[a]			
New experiments (2004)	5. Real (ordered)	6.1[a]		7.1[b]		
U. of Virginia 216 subjects	6. Real (unordered)	5.7[a]		6.7[a]		
	7. Hypothetical (unordered)	5.6[a]		5.7[a]		

Note: Superscripts indicate order ([a] = 1st, [b] = 2nd, [c] = 3rd).

choice as the scale increased by a factor of 20. We use a Kolmogorov-Smirnov test to identify differences between the distributions of the number of safe choices made at the low- and high-payment levels.[3] There is a significant difference between the distributions of safe choices between these two payoff-scale conditions ($p < 0.01$). This test does not, however, explore the extent to which this payoff-scale effect is due to the fact that the 20× choice was made after the 1× choice.

We conducted four additional treatments in which each subject completed a *single* lottery-choice menu that was identical to that described above. Unlike our first treatment (with ordered choices), however, these subjects participated in just one payoff treatment. The four (unordered) treatments tested were: low (1x) real payments, low hypothetical payments, high (20x) real payments, and high hypothetical payments. There were 48 subjects in each real-payment treatment, and 36 subjects in each hypothetical-payment treatment.[4] Instructions for all treatments were identical, except for the description of the actual choices the subjects faced.[5]

The data from the single-choice treatments are summarized in the bottom two rows of Table 1. Those subjects who completed the low-real-payment decision were slightly less risk averse than those who completed the ordered task reported above (those in the low-real-payment treatment made 5.7 safe choices compared with 6.1 for those who participated under both payment conditions). A Kolmogorov-Smirnov test, however, cannot reject the null hypothesis of equal distributions between these two low-payment treatments (two-sided p-value = 0.50). A Wilcoxon rank-sum test also fails to reject the null hypothesis of equal distributions and central tendency ($p = 0.33$).

The increase in the number of safe choices from the low-real to high-real payment conditions is identical (1 safe choice) between these treatments with ordered data in row 5 and those with unordered data in row 6, which indicates that real payoff-scale effects are important, whether or not decisions are made in an ordered or unordered manner. Again, a Kolmogorov-Smirnov test fails to reject the null hypothesis of

[3] The Kolmogorov-Smirnov test looks for differences in the two distributions, both in terms of shape and location. It has good power to test for general differences in distributions, and not just in the central tendency of two distributions.

[4] Each hypothetical-payment treatment had 24 subjects making decisions with the "safe" lottery on the left, and 12 subjects making decisions with the "safe" lottery on the right. The numbers were balanced in the real-payment treatments, with 24 subjects in each order.

[5] Instructions are available on line at http://veconlab.econ.virginia.edu/admin.htm. To view the on-line instructions, go to the Decisions Menu and then select Lottery Choice Menu program. You may use the session name "test" to set up an experiment. Instructions can be seen by pressing the Instructions button on the final Admin Results page in the setup sequence. In the hypothetical payment treatments, the final line of the instructions noted that payoffs would not be paid, and the experimenter added a verbal comment: "Unlike the other tasks that you have done so far today, the earnings for this part of the experiment are hypothetical and will not be added to your previous earnings." Under the real-payoff condition, the experimenter finished the instructions by announcing to participants, "Your earnings for this part of the experiment are real and will be paid in cash when we finish." Subjects recorded their own earnings on a receipt form, and there seemed to be no confusion between treatments about whether the earnings would be paid or not.

equal distributions of safe choices in the two high-real-payment treatments in rows 5 and 6, even if one has an a priori belief that the distribution of safe choices under the 20× treatment, when conducted alone, will lie to the left of that when the 20× choice follows the low-payment task (one-sided p-value = 0.18). A Wilcoxon test, however, rejects the null hypothesis at a 10-percent level of significance (one-sided p-value = 0.09).

While the effect of prior experience with a low-payment decision on subsequent choices is not clear-cut at the 20× level, these four unordered treatments confirm the primary conclusions of Holt and Laury (2002). Considering only those treatments in which subjects participated in a single payment condition (the last two rows of Table 1), there is a significant difference in the distribution of safe choices under low- and high-real-payment conditions (Kolmogorov-Smirnov, one-tailed p-value = 0.01). There is no significant difference, however, in the distribution of safe choices under the corresponding hypothetical payment conditions ($p = 0.42$). Therefore, even when order-effects are eliminated, scaling up payments by a factor of 20 leads to a significant increase in risk aversion, but only when using real payments.

II. Conclusion

Harrison et al. (2005) correctly note that the estimate of an individual's degree of risk aversion may be biased if the subject first completes the same decision-problem under a different payoff scale. In response, we conducted a new experiment in which subjects completed a menu of lottery choices under a single payment condition, in order to eliminate order effects. Both our new data, and Harrison et al., confirm that scaling up real payments results in a significant increase in risk aversion. Our new data further demonstrate that scaling up hypothetical payments by the same amount does not cause a significant difference in risk aversion when possible order effects are eliminated.

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ORIGINAL PAPER

A distance measure for choice functions

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Abstract This paper discusses and characterizes a distance function on the set of quasi choice functions. The derived distance function is in the spirit of the widely used Kemeny metric on binary relations but extends Kemeny's use of the symmetric difference distance to set functions and hence to a more general model of choice.

1 Introduction

The aggregation of individual preferences into a group preference is of major interest in areas such as social choice theory and computer science. Certain aggregation rules and many of the comparisons between different aggregation rules rely on the idea of measuring distances or the similarity between such preferences. In mathematics many different distance functions have been devised to measure distances between pairs of objects in many different domains. A well-known distance in that respect which is also commonly used in social choice theory is the symmetric difference distance, i.e. the cardinality of the symmetric difference of two sets of objects. As preferences are often represented by binary relations and those are nothing else than sets of ordered pairs, a natural way of measuring distance between preferences is to use the symmetric difference distance on the set of binary relations. This is what actually has been done and characterized by Kemeny (1959). Intuitively the Kemeny distance counts the minimal number of (pairwise) "inversions" of alternatives necessary to transform one binary relation into the other. Kemeny's original focus was on complete and transitive binary relations, i.e. weak orders. However, soon other authors applied the symmetric difference distance to other preference structures. In particular, Bogart (1973) characterized a distance on partial orderings, i.e. transitive and irreflexive relations, whereas

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Bogart (1975) extended this even further to not necessarily transitive relations. Other applications of the symmetric difference distance can be found in Mirkin and Chernyi (1970) who applied it to equivalence relations and in Margush (1982) who applied it to tree structures.[1]

Distances are also explicitly used in aggregation procedures which try to find median relations. One example is the Kemeny procedure whose median relation is the relation that minimizes the sum of the Kemeny distances to the individual preferences. A detailed discussion of this "median principle" has been provided by Barthelemy and Monjardet (1981).

The focus in this paper is on a more general model of choice by using choice functions instead of binary relations to represent preferences. We will provide a natural extension of the symmetric difference distance to set functions and provide a characterization of a distance function which measures the distance between pairs of quasi choice functions.

This is of interest insofar as choice functions are seen as an attractive way of dealing with aggregation problems whenever little structure is imposed on individual and/or group preferences (Xu 1996). As in certain situations the transitivity and/or completeness of preferences seems to be a strong assumption, using choice functions as primitives seems to be a reasonable alternative. A detailed discussion of the importance and the advantages of choice functions can be found in Aizerman and Aleskerov (1995) and Aleskerov and Monjardet (2002). The idea of measuring distance between choice functions has been introduced already in connection to convexity issues and the aggregation of individual choice functions by Albayrak and Aleskerov (2000) and Ilyunin et al. (1988). Distance aspects in social choice theory have also been discussed in a broader context by Nurmi (2002, 2004). In a more applied form, Brams et al. (2006) used a distance approach to analyse the 2003 Game Theory Society council election.

2 The characterization result

Let X be a finite set of m alternatives. The set of all non-empty subsets of X is denoted by K. A *quasi choice function* is a function $C : K \to K \cup \emptyset$ such that for all $S \in K$, $C(S) \subseteq S$, i.e. it assigns to any set $S \in K$ a (possibly empty) subset $C(S) \subseteq S$. The set of all quasi choice functions on domain K is denoted by $\hat{\mathcal{C}}$. A certain idea of a choice function lying "between" other choice functions or, based on this "betweenness", choice functions being "on a line" will be used. Following Albayrak and Aleskerov (2000) we give the following definitions:

Definition 1 For any $C, C', C'' \in \hat{\mathcal{C}}$, we say that C' lies between C and C'', written $[C, C', C'']$ if for all $S \in K$, $C(S) \cap C''(S) \subseteq C'(S) \subseteq C(S) \cup C''(S)$.

Definition 2 The quasi choice functions $C_1, C_2, \ldots, C_n \in \hat{\mathcal{C}}$ are on a line if for all $i < j < k \leq n$, C_j lies between C_i and C_k.

A distance function on set $\hat{\mathcal{C}}$ is a function $d : \hat{\mathcal{C}} \times \hat{\mathcal{C}} \to \mathbb{R}_+$. Consider the following properties for distance functions on $\hat{\mathcal{C}}$.

[1] Further applications and references can be found in Bogart (1982).

A1.1 $d(C, C') \geq 0$ where equality holds if and only if $C = C'$

A1.2 $d(C, C') = d(C', C)$

A1.3 $d(C, C'') \leq d(C, C') + d(C', C'')$ and equality holds if and only if C' is between C and C''

A2 If $\tilde{C}, \tilde{C}'$ result from C, C' by a permutation of the alternatives, then $d(C, C') = d(\tilde{C}, \tilde{C}')$

A3 If two choice functions $C, C' \in \hat{\mathcal{C}}$ agree except for a set $\bar{K} \subset K$ which is part of the domain in both choice functions, then the distance $d(C, C')$ is determined exclusively from the choice sets over $\bar{K}$.

A4 Let four choice functions $C, C', \tilde{C}, \tilde{C}' \in \hat{\mathcal{C}}$ disagree only on set $T \in K$ such that for some $S \subseteq T$, $C(T) = \tilde{C}(T) \cup S$, and $C'(T) = \tilde{C}'(T) \cup S$. Then the distance between C and C' should be equal to the distance between $\tilde{C}$ and $\tilde{C}'$.

A5 The minimal positive distance is 1.

Axioms A1.1 to A1.3 are the usual metric axioms with the addition of A1.3 using the idea of "betweenness" (as previously defined). Axiom A2 is a usual neutrality condition saying that if we permute the alternatives in any pair of quasi choice functions, the distance between those quasi choice functions does not change. Axiom A3 is some kind of separability condition as the distance between quasi choice functions is only based on sets on which they differ. Moreover, A4 is a sort of translation invariance condition, i.e. distances are invariant under special types of parallel translations. Finally, A5 can be seen as choosing a unit of measurement.

Our aim now is to show that the above axioms uniquely determine a distance between quasi choice functions. The first lemma shows that if n quasi choice functions are on a line, then the distance between the first and the nth quasi choice function is equal to the sum of the distances between all pairs of adjacent quasi choice functions.

Lemma 1 *If $C_1, C_2, \ldots, C_n \in \hat{\mathcal{C}}$ are on a line, then $d(C_1, C_n) = d(C_1, C_2) + d(C_2, C_3) + \cdots + d(C_{n-1}, C_n)$.*

Proof The lemma is proved by induction. Therefore we will repeatedly apply Axiom A1.3. For $n = 2$ it is of course trivially true that $d(C_1, C_2) = d(C_1, C_2)$. Hence assume that it is true for $n = k$ and let us show that it is also true for $k + 1$. As the quasi choice functions are on a line it is the case that $[C_1, C_k, C_{k+1}]$. Therefore from Axiom A1.3 we know that $d(C_1, C_{k+1}) = d(C_1, C_k) + d(C_k, C_{k+1})$. However, applied over the first k quasi choice functions, we know that $d(C_1, C_k) = d(C_1, C_2) + d(C_2, C_3) + \cdots + d(C_{k-1}, C_k)$ and thus the lemma follows by induction. □

Lemma 2 *If three quasi choice functions $C, C', C'' \in \hat{\mathcal{C}}$ only differ in the choice over a set $S \subseteq X$ such that $C(S) = \emptyset$, $C'(S) = \{x\}$ and $C''(S) = \{y\}$, then $d(C, C') = d(C, C'')$.*

Proof Let us slightly abuse the notation and instead of using quasi choice functions use only the choice sets on the set S, i.e. if $C(S) = \emptyset$ and $C'(S) = \{x\}$ then $d(C, C')$ will equivalently be written as $d(\emptyset, \{x\})$. Axiom A3 implies that the distance between the quasi choice functions is determined exclusively from the choice sets on S. Then, permuting x and y, we get—from A2—that $d(\emptyset, \{x\}) = d(\emptyset, \{y\})$. □

Now, since $d(\emptyset, \{x\})$ does not depend on x, we can set $d(\emptyset, \{x\})$ equal to a constant $u = 1$ (by A5). We now show that the distance between any two quasi choice functions that only differ in the choice over one subset—such that one chooses the empty set and the other chooses the whole set—will be a multiple of u.

Lemma 3 *If two quasi choice functions $C, C' \in \hat{C}$ only differ in the choice over a set $S \subseteq X$ such that $|S| = n$, $C(S) = \emptyset$ and $C'(S) = S$, then $d(C, C') = n \cdot u$.*

Proof Using the same notational abuse as in lemma 2 we know that $d(\{x\}, \emptyset) = d(\{y\}, \emptyset) = u$. Using axiom A4 we get $d(\{x, y\}, \{y\}) = d(\{x\}, \emptyset) = u$. As $\{x, y\}$, $\{y\}, \emptyset$ are on a line it follows from lemma 1 that $d(\{x, y\}, \emptyset) = d(\{x, y\}, \{y\}) + d(\{y\}, \emptyset) = 2u$. Assume that this holds for $|S| = k$, then we need to show that it holds for $|S| = k+1$. From lemma 1, $d(\{x_1, x_2, \dots, x_k, x_{k+1}\}, \emptyset) = d(\{x_1, x_2, \dots, x_k\}, \emptyset) + d(\{x_1, x_2, \dots, x_{k+1}\}, \{x_1, x_2, \dots, x_k\})$. By A4, $d(\{x_1, x_2, \dots, x_{k+1}\}, \{x_1, x_2, \dots, x_k\}) = d(\{x_{k+1}\}, \emptyset) = u$. Hence, if $d(\{x_1, x_2, \dots, x_k\}, \emptyset) = k \cdot u$ then $d(\{x_1, x_2, \dots, x_{k+1}\}, \emptyset) = (k + 1) \cdot u$. □

The final lemma generalizes lemma 3 in the sense that there now is no restriction whatsoever on the choice sets of the two quasi choice functions for the one subset on which they differ.

Lemma 4 *If two quasi choice functions in $C, C' \in \hat{C}$ only differ in the choice over a set $S \subseteq X$ then their distance is based on the cardinality of their symmetric difference, i.e. $d(C, C') = \left|C(S) \Delta C'(S)\right| \cdot u$.*

Proof Let $C(S) = \emptyset$ and consider that $C'(S) \cap C''(S) = \emptyset$. Then, by the definition of betweenness, we know that C', C, C'' are on a line. Now let $C'(S) = \{x_1, x_2, \dots, x_k\}$ and $C''(S) = \{y_1, y_2, \dots, y_h\}$ Then by lemma 3 it is the case that $d(C, C') = k$ and $d(C, C'') = h$. Hence by A3 and lemma 1, $d(C', C'') = (k+h) \cdot u = \left|C'(S) \Delta C''(S)\right| \cdot u$. If $C'(S) \cap C''(S) = T \neq \emptyset$ then by A4 it follows that for any $\tilde{C}', \tilde{C}''$ such that $C'(S) = \tilde{C}'(S) \cup T$ and $C''(S) = \tilde{C}''(S) \cup T$, $d(C', C'') = d(\tilde{C}', \tilde{C}'')$ and therefore the lemma is true. □

Based on the symmetric difference of choice sets on different sets $S \in K$ we will now define the following distance function[2] on the set of all quasi choice functions, $\hat{C}$.

Definition 3 For any $C, C' \in \hat{C}$, $d_F(C, C') = \sum_{S \in K} \left|C(S) \Delta C'(S)\right|$.

Hence, d_F measures the distance between two quasi choice functions as the sum of the cardinalities of the symmetric differences over all subsets S in K. As turns out and is shown in the following theorem, d_F is the only distance function that satisfies the reasonable axioms discussed above.

Theorem 1 *A distance function d on $\hat{C}$ is equal to d_F if and only if it satisfies the axioms* A1–A5.

[2] This distance function has previously been used in papers by Ilyunin et al. (1988) and Albayrak and Aleskerov (2000).

Table 1 Binary relations

R_1	R_2	R_3
x	y	x
y	x	z
z	z	y

Proof Consider two different quasi choice functions $C, C' \in \hat{\mathcal{C}}$. We can transform C into C' via a sequence of quasi choice functions $C_1, C_2, C_3, \ldots, C_{k-1}, C_k \in \hat{\mathcal{C}}$ with $C_1 \equiv C$ and $C_k \equiv C'$, where for each pair C_i, C_{i+1}, $i \in \{1, \ldots, k-1\}$, there is a different set $S_i \in K$ such that for all $T \in K \backslash S_i$, $C_i(T) = C_{i+1}(T)$, i.e. the choice functions only differ in the choice over one particular subset of X. This, however, means that $C_1, C_2, \ldots, C_k$ are on a line and hence, from lemma 1 it is the case that $d(C_1, C_k) = d(C_1, C_2) + d(C_2, C_3) + \cdots + d(C_{k-1}, C_k)$. However, for any pair C_i, C_{i+1}, the distance $d(C_i, C_{i+1})$ is determined by lemma 4, i.e. $d(C_i, C_{i+1}) = |C_i(S_i) \Delta C_{i+1}(S_i)| \cdot u$. Therefore $d(C_1, C_k) = \sum_{S \in K} |C_1(S) \Delta C_k(S)| \cdot u$. As this shows that all distance values are multiples of u, axiom A5 gives u=1. Hence, $d = d_{\mathrm{F}}$ and obviously d_{F} satisfies axioms A1–A5. □

3 The relationship between d_{F} and the Kemeny metric

As d_{F} is defined on the very general class of quasi choice functions it allows to measure distance between preferences with very little structure. Of course, using the concept of "rationalizability"[3] of choice functions we can transform preferences represented by binary relations into preferences represented by choice functions. The question now arises whether d_{F} provides the same distance information when applied on choice functions as the Kemeny metric does when applied on binary relations that rationalize those choice functions. It turns out, that this is not the case. Consider the following example (Klamler 2006):

Example 1 Let $X = \{x, y, z\}$ and R_1, R_2, R_3 represent three binary relations as stated in Table 1 (where less preferred alternatives are in lower rows).

The corresponding choice functions C_1, C_2, C_3 are given in Table 2 (for notational convenience the values of C on singletons are ommitted).

The Kemeny distance between R_1 and R_2 is the number of inversions necessary to transform R_1 into R_2 multiplied by 2. As to do so it takes exactly the inversion of alternatives x and y in R_1, the Kemeny distance between R_1 and R_2 is 2. The same distance is derived between R_1 and R_3 where we need to invert alternatives y and z. However, if we consider the distance between the choice functions that are rationalized by those binary relations we get $d_{\mathrm{F}}(C_1, C_2) = 4$ and $d_{\mathrm{F}}(C_1, C_3) = 2$.

In a nutshell, this difference in distance information is based on the fact that d_{F} attaches more "weight" to changes in the preference relation with respect to more

[3] On rationalizability of choice functions see Sen (1986).

Table 2 Choice functions rationalized by the linear orders in Table 1

	$C_1(\cdot)$	$C_2(\cdot)$	$C_3(\cdot)$
X	x	y	x
xy	x	y	x
xz	x	x	x
yz	y	y	z

preferred alternatives. A higher ranked alternative is contained in a larger number of choice sets than a lower ranked alternative and therefore any change of such an alternative in the relation would lead to more changes in choice sets than would occur in the case of a lower ranked alternative. This is represented in d_F. The Kemeny metric, on the other hand, is neutral with respect to where in the relation such changes occur.

Given this fact, d_F does indeed sound reasonable as in many situations changes in more preferred alternatives seem to be considered more relevant in both, an individual and social view, than changes in less preferred alternatives.

Of course, the Kemeny metric on binary relations could be adapted in such a way that those "weights" are taken into account. Such a distance function on binary relations has been presented in Klamler (2006).

4 Conclusion

In this paper we have characterized a distance function on the set of quasi choice functions based on the symmetric difference distance. In addition we have shown in what sense it differs from the well known Kemeny metric on binary relations.

Finally, the question arises in what sense such a distance function can lead to further insight into non-binary aggregation rules. Rules such as those devised by Dodgson (1876), Kemeny (1959) and Slater (1961) are based on binary relations and all explicitly depend on the Kemeny metric.[4] The distance function characterized in this paper seems to open the possibility to transfer the above aggregation rules into the space of quasi choice functions and therefore enables distance-based aggregation in more general models of choice.

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A Measure of Rationality and Welfare

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Evidence showing that individual behavior often deviates from the classical principle of preference maximization has raised at least two important questions: (1) How serious are the deviations? (2) What is the best way to analyze choice behavior in order to extract information for the purpose of welfare analysis? This paper addresses these questions by proposing a new way to identify the preference relation that is closest, in terms of welfare loss, to the revealed choice.

I. Introduction

The standard model of individual behavior is based on the maximization principle, whereby the individual chooses the alternative that maximizes a preference over the menu of available alternatives. This has two key advantages. The first is that it provides a simple, versatile, and powerful

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account of individual behavior. The second is that it suggests the maximized preference as a tool for individual welfare analysis.

Research in recent years, however, has produced increasing amounts of evidence documenting deviations from the standard model of individual behavior.[1] The violation in some instances of the maximization principle raises at least two important questions: (1) How serious are the deviations from the classical theory? (2) What is the best way to analyze individual choice behavior in order to extract information for the purpose of welfare analysis?

The successful answering of question 1 would enable us to evaluate how accurately the classical theory of choice describes individual behavior. This would shift the focus from whether or not individuals violate the maximization principle to how closely their behavior approaches this benchmark. Addressing question 2, meanwhile, should help us to distinguish alternatives that are good for the individual from those that are bad, even when the individual's behavior is not fully consistent with the maximization principle. This, of course, is useful for performing welfare analysis.

Although these two questions are intimately related, the literature has treated them separately. This paper provides the first joint approach to measuring rationality and welfare. Relying on standard revealed preference data, we propose the *swaps index*, which measures the welfare loss of the inconsistent choices with respect to the preference relation that comes closest to the revealed choices, the *swaps preference*. The swaps index evaluates the inconsistency of an observation with respect to a preference relation in terms of the number of alternatives in the menu that rank above the chosen one. That is, it counts the number of alternatives that must be swapped with the chosen alternative in order for the preference relation to rationalize the individual's choices. Then, the swaps index considers the preference relation that minimizes the total number of swaps in all the observations, weighted by their relative occurrence in the data.

To the best of our knowledge, the literature on rationality indices starts with the Afriat (1973) proposal for a consumer setting, which is to measure the amount of adjustment required in each budget constraint

[1] Some phenomena that have attracted a great deal of empirical and theoretical attention and that prove difficult, if not impossible, to accommodate within the classical theory of choice are framing effects, menu effects, dependence on reference points, cyclic choice patterns, choice overload effects, etc. For experimental papers, see May (1954), Thaler (1980), Tversky and Kahneman (1981), and Iyengar and Lepper (2000). Some theoretical papers reacting to this evidence are Kalai, Rubinstein, and Spiegler (2002), Bossert and Sprumont (2003), Masatlioglu and Ok (2005, 2014), Manzini and Mariotti (2007, 2012), Xu and Zhou (2007), Salant and Rubinstein (2008), Green and Hojman (2009), Masatlioglu, Nakajima, and Ozbay (2012), and Ok, Ortoleva, and Riella (2012).

to avoid any violation of the maximization principle. Varian (1990) extends the Afriat proposal to contemplate a vector of wealth adjustments, with different adjustments in the different observations.[2] An alternative proposal by Houtman and Maks (1985) is to compute the maximal subset of the data that is consistent with the maximization principle.[3] Yet a third approach, put forward by Swofford and Whitney (1987) and Famulari (1995), entails counting the number of violations of a consistency property detected in the data. Echenique, Lee, and Shum (2011) make use of the monetary structure of budget sets to suggest a version of this notion, the money pump index, which considers the total wealth lost in all revealed cycles. The swaps index contributes to the measurement of rationality in a singular fashion by evaluating inconsistent behavior directly in terms of welfare loss. It is also the first axiomatically based measure to appear in the literature. In Section III.A, we illustrate the contrast between the swaps index treatment of rationality measurement and these alternative proposals.

There is a growing number of papers analyzing individual welfare when the individual's behavior is inconsistent. Bernheim and Rangel (2009) add to the standard choice data the notion of ancillary conditions, which are assumed to be observable and potentially to affect individual choice but are irrelevant in terms of the welfare associated with the chosen alternative. Bernheim and Rangel suggest a welfare preference relation that ranks an alternative as welfare superior to another only if the latter is never chosen when the former is available.[4] The proposal of Green and Hojman (2009) is to identify a list of conflicting selves, aggregate them to induce the revealed choices, and then perform individual welfare analysis using the aggregation rule. Nishimura (2014) builds a transitive welfare ranking on the basis of a nontransitive preference relation.[5] The swaps index uses the revealed choices, as in the classical approach, to suggest a novel welfare ranking, the swaps preference, interpreted as the best approximation to the choices of the individual and complemented with a measure of its accuracy: the incon-

[2] Halevy, Persitz, and Zrill (2012) extend the approach of Afriat and Varian by complementing Varian's inconsistency index with an index measuring the misspecification with a set of utility functions.

[3] Dean and Martin (2012) suggest an extension that weights the binary comparisons of the alternatives by their monetary values. Choi et al. (2014) apply the measures of Afriat and Houtman and Maks to provide valuable information on the relationship between rationality and various demographics.

[4] Chambers and Hayashi (2012) extend Bernheim and Rangel's model to probabilistic settings.

[5] Other approaches include Masatlioglu et al. (2012), Rubinstein and Salant (2012), and Baldiga and Green (2013). There are also papers describing methods for ranking objects such as teams or journals, based on a given tournament matrix describing the paired results of the objects (see Rubinstein 1980; Palacios-Huerta and Volij 2004).

sistency value. In Section III.B, we illustrate by way of examples other differences between our proposal and these other approaches.

In Section IV, we study the capacity of the swaps index to recover the true preference relation from collections of observations that, for a variety of reasons, may contain mistakes and hence potentially reveal inconsistent choices. We show that this is in fact the case for a wide array of stochastic choice models.

In Section V, we propose seven desirable properties of any inconsistency index relying only on endogenous information arising from the choice data and show that they characterize the swaps index. Then, in Section VI, we characterize several generalizations of the swaps index, together with versions of the classical Varian and Houtman-Maks indices within our framework. Section VII applies the swaps index to the experimental data of Harbaugh, Krause, and Berry (2001).

In the online appendix we discuss the relaxation of three assumptions made in the setup. We first show that it is immediate to make the swaps index capable of considering classes of preference relations with further structure, such as those admitting an expected utility representation. We then show how to extend the swaps index to include the treatment of indifferences. Third, we argue that it is possible to construct a natural version of the swaps index ready for application in settings with infinite sets of alternatives.

II. Framework and Definition of the Swaps Index

Let X be a finite set of k alternatives. Denote by $\mathcal{O}$ the set of all possible pairs (A, a), where $A \subseteq X$ and $a \in A$. We refer to such pairs as *observations*. Individual behavior is summarized by the relative number of times each observation (A, a) occurs in the data. Then, a *collection of observations* f assigns to each observation (A, a) a positive real value denoted by $f(A, a)$, with $\sum_{(A,a)} f(A, a) = 1$, interpreted as the relative frequency with which the individual confronts menu A and chooses alternative a. We denote by $\mathcal{F}$ the set of all possible collections of observations. The collection f allows us to entertain different observations with different frequencies. This is natural in empirical applications, where exogenous variations require the decision maker to confront the menus of alternatives in uneven proportions.

Another key feature of our framework is preference relations. A preference relation P is a strict linear order on X, that is, an asymmetric, transitive, and connected binary relation. Denote by $\mathcal{P}$ the set of all possible linear orders on X. The collection f is *rationalizable* if every single observation present in the data can be explained by the maximization of the same preference relation. Denote by $m(P, A)$ the maximal element in A according to P. Then, formally, we say that f is rationalizable if there exists a preference relation P such that $f(A, a) > 0$ implies $m(P, A) = a$.[6] Let $\mathcal{R}$ be the set of rationalizable collections of observations that assign the same relative frequency to each possible menu of alternatives $A \subseteq X$. Notice that every collection $r \in \mathcal{R}$ is rationalized by a unique preference relation, which we denote by P^r.[7] Clearly, not every collection is rationalizable. An inconsistency index is a mapping $I : \mathcal{F} \to \mathbb{R}_+$ that measures how inconsistent, or how far removed from rationalizability, a collection of observations is.

We are now in a position to formally introduce our approach. Consider a given preference relation P and an observation (A, a) that is inconsistent with the maximization of P. This implies that there are a number of alternatives in A that, despite being preferred to the chosen alternative a according to P, are nevertheless ignored by the individual. We can therefore entertain that the inconsistency of observation (A, a) with respect to P entails consideration of the number of alternatives in A that rank higher than the chosen one, namely, $|\{x \in A : xPa\}|$. These are the alternatives that must be swapped with the chosen one in order to make the choice of a consistent with the maximization of P. If every single observation is weighted by its relative occurrence in the data, the inconsistency of f with respect to P can be measured by $\sum_{(A,a)} f(A, a) \mid \{x \in A : xPa\} \mid$. The swaps index I_S adopts this criterion and finds the preference relations P_S that minimize the weighted sum of swaps. We refer to P_S as the swaps preference relations. Formally,

$$I_S(f) = \min_P \sum_{(A,a)} f(A, a) \mid \{x \in A : xPa\} \mid,$$

$$P_S(f) \in \arg\min_P \sum_{(A,a)} f(A, a) \mid \{x \in A : xPa\} \mid .$$

In summary, the swaps index enables the joint treatment of inconsistency and welfare analysis. It discriminates between different degrees of inconsistency in the various choices, relying exclusively on the information contained in the choice data, and additively considers every single inconsistent observation weighted by its relative occurrence in the data. It identifies the preference relations closest to the revealed data, the swaps preferences, measuring their inconsistency in terms of the associated welfare loss. In Appendix B we show that almost all collections of observations have a unique swaps preference; that is, the measure of

[6] Notice that, since P is a linear order, if there exist $a, b \in A$ with $a \neq b$ such that $f(A, a) > 0$ and $f(A, b) > 0$, then f is not rationalizable.

[7] The purpose here is to create a bijection between $\mathcal{P}$ and a subset of the rationalizable collections. The set $\mathcal{R}$ is one way of creating this bijection, which comes without loss of generality.

all collections with a nonunique swaps preference is zero. We then typically talk about the swaps preference without further considerations unless the distinction is relevant.[8] We now illustrate the swaps index and the swaps preference by way of two examples.

EXAMPLE 1. Consider the set of alternatives $X = \{1, \ldots, k\}$. Suppose that the collection f contains observations involving all the subsets of X and is completely consistent with the preference relation P, ranking the alternatives as $1P2P \cdots Pk$. Now consider the collection of observations g involving the consistent evidence f with a high frequency $(1 - \alpha)$ and the extra observation (X, x), $x > 1$, with a low frequency α. That is, $g = (1 - \alpha)f + \alpha \mathbf{1}_{(X,x)}$, where $\mathbf{1}_{(X,x)}$ denotes the collection with all the mass centered on the observation (X, x). Clearly, the collection g is not rationalizable. In order to determine the swaps index and the swaps preference for g, notice that for any $P' \neq P$ there is at least one pair of alternatives, z and y, with $y < z$ and $zP'y$. Hence, the weighted sum of swaps for P' is at least $(1 - \alpha)f(\{y, z\}, y)$. Meanwhile, preference P requires $x - 1$ swaps in the observation (X, x), and hence the weighted sum of swaps for P is exactly $\alpha(x - 1)$. For small values of α, it is clearly the case that $\alpha(x - 1) < (1 - \alpha)f(\{y, z\}, y)$, and therefore, $I_S(g) = \alpha(x - 1)$ and $P_S(g) = P$. Hence, in such cases the swaps preference coincides with the rational preference P, and the inconsistency attributed to g by the swaps index is the mass of the inconsistent observation α weighted by the number of swaps required to rationalize the inconsistent observation.

EXAMPLE 2. Let

$$f(\{x, y\}, x) = f(\{y, z\}, y) = \frac{1 - 2\alpha}{2}$$

and

$$f(\{x, y, z\}, y) = f(\{x, z\}, z) = \alpha,$$

where α is small. That is, there is large evidence that x is better than y and that y is better than z and some small evidence that y is better than x and z and that z is better than x. Notice that any preference in which y is ranked above x or z is ranked above y has a weighted sum of swaps of at least $(1 - 2\alpha)/2$. There is only one more preference to be analyzed, namely, $xPyPz$. This preference requires exactly one swap in menu $\{x, y, z\}$, where y is chosen, and also one swap in menu $\{x, z\}$ where z is chosen. The weighted sum of swaps of P is therefore 2α, which, for small values of α, is smaller than $(1 - 2\alpha)/2$, and hence $I_S(f) = 2\alpha$ and

[8] In addition, in App. C, we deal with the computational complexity of obtaining P_S.

$P_S(f) = P$. That is, the swaps preference rationalizes the large evidence of data,

$$f(\{x, y\}, x) = f(\{y, z\}, y) = \frac{1 - 2\alpha}{2},$$

and incurs some relatively small errors in $f(\{x, y, z\}, y) = f(\{x, z\}, z) = \alpha$.

III. Comparison to Alternative Measures

A. *The Measurement of Rationality*

In a consumer setting, Afriat (1973) suggests measuring the degree of relative wealth adjustment that, when applied to all budget constraints, avoids all violations of the maximization principle. The idea is that, when a portion of wealth is considered, all budget sets shrink, thus eliminating some revealed information, and thereby possibly removing some inconsistencies from the data. Thus, Afriat's proposal associates the degree of inconsistency in a collection of observations with the minimal wealth adjustment needed to make all the data consistent with the maximization principle.

We now formally define Afriat's index for our setting. Let $w_x^A \in (0, 1]$ be the minimum proportion of income in budget set A that must be removed in order to make x unaffordable. Then, given a menu A, if a proportion w of income is removed, all alternatives $x \in A$ with $w_x^A \leq w$ become unaffordable. We say that a collection f is w-rationalizable if there exists a preference relation P such that $f(A, a) > 0$ implies that aPx for every $x \in A \setminus \{a\}$ with $w_x^A > w$. Notice that when $w = 0$, this is but the standard definition of rationalizability. Afriat's inconsistency measure is defined as the minimum value w^* such that f is w^*-rationalizable. Note that we can alternatively represent this index in terms of preference relations, making its representation closer in spirit to that of the swaps index. To see this, suppose that P^* is a preference that w^*-rationalizes f. Then, for all observations (A, a) with $f(A, a) > 0$, all alternatives xP^*a must be unaffordable at w^*. Hence

$$w^* = \max_{(A,a): f(A,a)>0} \max_{x \in A, xP^*a} w_x^A.$$

Since no other preference can w-rationalize f for $w < w^*$, it is clearly the case that we can define Afriat's index as[9]

[9] For notational convenience, let $\max_{x \in \varnothing} w_x^A = 0$.

$$I_A(f) = \min_P \max_{(A,a):f(A,a)>0} \max_{x\in A, xPa} w_x^A.$$

Varian (1990) considers vectors of wealth adjustments **w**, with potentially different adjustments in the various observations. Then, Varian's index identifies the closest vector **w** to 0 that, under a certain norm, **w**-rationalizes the data. Here, given the structure of the swaps index, we consider the 1-norm and define Varian's index as follows:

$$I_V(f) = \min_P \sum_{(A,a)} f(A,a) \max_{x\in A, xPa} w_x^A.$$

Houtman and Maks (1985) propose considering the minimal subset of observations that needs to be removed from the data in order to make the remainder rationalizable. The size of the minimal subset to be discarded suggests itself as a measure of inconsistency. It follows immediately that, in our setting, the Houtman-Maks index, which we denote by I_{HM}, is but a special case of Varian's index when $w_x^A = 1$ for every A and every $x \in A$.

Finally, rationality has also been measured by counting the number of times in the data a consistency property is violated (see, e.g., Swofford and Whitney 1987; Famulari 1995). Consider for instance the case of the weak axiom of revealed preference (WARP). In our context, WARP is violated whenever there are two menus A and B and two distinct elements a and b in $A \cap B$ such that $f(A, a) > 0$ and $f(B, b) > 0$. Hence, we can measure the mass of violations of WARP by means of

$$I_W = \sum_{(A,a),(B,b):\{a,b\}\subseteq A\cap B, a\neq b} f(A,a) f(B,b).$$

Recently, Echenique et al. (2011) made use of the monetary structure of budget sets to suggest a new measure, the money pump index, which evaluates not only the number of times the generalized axiom of revealed preference (GARP) is violated but also the severity of each violation. Their proposal is to weight every cycle in the data by the amount of money that could be extracted from the consumer. They then consider the total wealth lost in all the revealed cycles. To illustrate the structure of this index in our framework, let us contemplate only violations of WARP (i.e., cycles of length 2). Consider a violation of WARP involving observations (A, a) and (B, b). The money pump reasoning evaluates the wealth lost in this cycle by adding up the minimal wealth $\tilde{w}_b^A$ that must be removed to make b unaffordable in A and the minimal wealth $\tilde{w}_a^B$ that must be removed to make a unaffordable in B.[10] Then,

[10] Notice that $\tilde{w}_x^A$, assumed to be strictly positive, is measured in dollars while Afriat's and Varian's weights w_x^A are proportions of wealth.

$\tilde{w}_b^A + \tilde{w}_a^B$ represents the money that could be pumped by an arbitrager from the WARP violation. Now, given the vector of weights $\tilde{\mathbf{w}}$, the WARP money pump index can be defined as[11]

$$I_{W-MP} = \sum_{(A,a),(B,b):\{a,b\}\subseteq A\cap B, a\neq b} f(A,a)f(B,b)(\tilde{w}_b^A + \tilde{w}_a^B).$$

In order to illustrate the differences between all these indices and the swaps index, let us reconsider example 1. Consider then two different scenarios in which $x = k$ and $x = 2$, that is, $g_k = (1-\alpha)f + \alpha\mathbf{1}_{(X,k)}$ and $g_2 = (1-\alpha)f + \alpha\mathbf{1}_{(X,2)}$. Intuitively, collection g_k involves a more severe inconsistency, since the observation in question is one in which the individual chooses the worst possible alternative, alternative k, while ignoring all the rest. Collection g_2 also shows some inconsistency with the maximization principle, but this inconsistency is orders of magnitude lower, since it involves choosing the second-best available option, that is, option 2. It follows immediately from the discussion in example 1 that the swaps index ranks these two collections in accordance with the above intuition, that is, $I_S(g_k) = \alpha(k-1) > \alpha = I_S(g_2)$. Afriat's and Varian's judgment of these collections depends crucially on the monetary values of the alternatives, which need not necessarily coincide with the welfare ranking and hence may lead to counterintuitive conclusions. For example, if 1 is the least expensive alternative in menu X, that is, $w_1^X \geq w_t^X$ for all $t \leq k$, Varian's approach involves removing income until alternative 1 becomes unaffordable, regardless of the scenario. Hence, both collections would be equally inconsistent. Note that, for Afriat, the mass of violations is irrelevant, and hence if removing option 1 from X is costly and removing alternative k from all menus is cheaper, it may be the case that g_k is w-rationalizable for some value $w < w_1^X$, while g_2 is not. Therefore, Afriat's index may judge g_k as being less inconsistent than g_2. With respect to Houtman-Maks's index, since the inconsistencies in both scenarios are of identical size, α, I_{HM} does not discriminate between them. Finally, the assessment provided by WARP violation index I_W depends on the specific nature of f. To illustrate, consider, for example, that $k = 3$ and that

$$f(X,1) = f(\{1,3\},1) = f(\{2,3\},2) = \beta$$

and

$$f(\{1,2\},1) = 1 - 3\beta.$$

It follows immediately that

[11] It is immediate to extend this index to consider cycles of any length, something that we avoid here for notational convenience.

$$I_W(g_k) = 3\alpha\beta(1-\alpha) < (1-\alpha)\alpha(1-2\beta) = I_W(g_2)$$

whenever $\beta < 1/5$, and hence scenario 2 is regarded as the more inconsistent of the two. Although index I_{W-MP} weights both sides of the above inequality by $\tilde{\mathbf{w}}$, the inequality still holds for certain nonnegligible values of β.

B. *The Measurement of Welfare*

Let us illustrate our approach to welfare analysis by contrasting it first with two proposals: Bernheim and Rangel (2009) and Green and Hojman (2009). Although these two papers tackle the problem from different angles, they independently suggest the same notion of welfare. Let us denote by $\overline{P}$ the Bernheim-Rangel-Green-Hojman welfare relation, defined as $x\overline{P}y$ if and only if there is no observation (A, y) with $x \in A$ such that $f(A, y) > 0$. In other words, x is ranked above y in the welfare ranking $\overline{P}$ if y is never chosen when x is available. Bernheim and Rangel show that, whenever every menu A in X is present in the data, $\overline{P}$ is acyclic and hence consistent with the maximization principle.

We now examine the relationship between $\overline{P}$ and the swaps preference P_S. It turns out to be the case that the two welfare relations are fundamentally different. It follows immediately that P_S is not contained in $\overline{P}$ because P_S is a linear order, while $\overline{P}$ is incomplete in general. In the other direction, and more importantly, note that while $\overline{P}$ evaluates the ranking of two alternatives x and y by taking into account only those menus of alternatives in which both x and y are available, P_S takes all the data into consideration. Hence, P_S and $\overline{P}$ may rank two alternatives in opposite ways.

Nishimura (2014) has recently proposed a different approach, the transitive core. Given a complete nonnecessarily transitive relation $\succcurlyeq$, the transitive core declares an alternative x preferred to alternative y whenever, for every z, (i) $y \succcurlyeq z$ implies $x \succcurlyeq z$ and (ii) $z \succcurlyeq x$ implies $z \succcurlyeq y$. Like $\overline{P}$, the transitive core may be incomplete, and since relative frequencies are not considered, the transitive core may go in the opposite direction to P_S.

We illustrate the differences between the swaps preference and the proposals here presented, using example 2 above. We argued there that xP_SyP_Sz. Note now that $z\overline{P}x$ since x is never chosen in the presence of z, and hence $\overline{P}$ and P_S follow different directions. Moreover, if $\succcurlyeq$ is understood to be the revealed preference, y is ranked above x by the transitive core, and hence this is different from P_S too.

Finally, notice that the swaps preference P_S comes, by construction, with the associated inconsistency I_S, which provides a measure of the credibility of P_S. A low value of I_S naturally gives credit to P_S, while high values of I_S may call for more cautious conclusions regarding the true welfare of the individual, either by focusing on subsets of alternatives over which violations are less dramatic (in the spirit of the aforementioned approaches) or by adopting a particular boundedly rational model of choice.

IV. Recoverability of Preferences and the Swaps Index

Consider a decision maker who evaluates alternatives according to the preference relation P but when it comes to selecting the preferred option sometimes chooses a suboptimal alternative. Mistakes can occur for various reasons, such as lack of attention, errors of calculation, misunderstanding of the choice situation, trembling hand when about to select the desired alternative, inability to implement the desired choice, and so forth. Whatever the specific model, mistakes generate a potentially inconsistent collection of observations f. This raises the issue of whether the swaps index has the capacity to recover the preference relation P from the observed choices f.

We show below that the swaps index identifies the true underlying preference for models that generate collections of observations in which, for any pair of alternatives, the better one is revealed preferred to the worse one more often than the reverse. Formally, we say that the collection f generated by a model satisfies *P-monotonicity* if xPy implies that $\sum_{A\supseteq\{x,y\}} f(A,x) \geq \sum_{A\supseteq\{x,y\}} f(A,y)$, where the inequality is strict whenever $\sum_{a\in\{x,y\}} f(\{x,y\},a) > 0$. In order to assess the generality of this result, we first show that a diverse number of highly influential classes of stochastic choice models satisfy this property.

Random utility models.—Suppose that the individual evaluates the alternatives by way of a utility function $u: X \to \mathbb{R}_{++}$.[12] At the moment of choice, this valuation is subject to an additive random error component. That is, when choosing from A, the true valuation of alternative x, $u(x)$, is subject to a random independent and identically distributed (i.i.d.) term, $\epsilon_A(x)$, which follows a continuous distribution, resulting in the final valuation $U(x) = u(x) + \epsilon_A(x)$. Then, the probability by which alternative a is chosen from A is the probability of a being maximal in A according to U, that is, $\Pr[a = \arg\max_{x\in A} U(x)]$.[13] Let ρ denote the probability distribution over the menus of options available to the individual, where

[12] In consonance with our analysis, assume that $u(x) \neq u(y)$ for every $x, y \in X$, $x \neq y$. Also, notice that the preference relation P of the individual is simply the one for which $u(x) > u(y) \Leftrightarrow xPy$. This also applies for the utility function used in the choice control models below.

[13] Notice that, since $\epsilon_A(x)$ is continuously distributed, the probability of ties is zero and hence $\Pr[a = \arg\max_{x\in A} U(x)]$ is well defined. Classic references for this class of models are Luce (1959) and McFadden (1974). See also Gul, Natenzon, and Pesendorfer (2014).

$\rho(A)$ denotes the probability of confronting $A \subseteq X$. We can now define the collection of observations generated by a random utility model as

$$f_{\mathrm{RUM}}(A, a) = \rho(A)\Pr\left[a = \arg\max_{x \in A} U(x)\right]$$

for every $(A, a) \in \mathcal{O}$. While the most widely used random utility models (logit, probit) have menu-independent errors, our formulation allows for menu-dependent utility errors, as in the contextual utility model of Wilcox (2011).

Tremble models.—The mistake structure in random utility models depends on the cardinal utility values of the options. Another way to model mistakes is as constant probability shocks that perturb the selection of the optimal alternative. That is, an individual facing menu A chooses her optimal option with high probability $1 - \mu_A > 1/2$ and, with probability μ_A, trembles and overlooks the optimal option. In the spirit of the trembling hand perfect equilibrium concept in game theory, in the event of a tremble, any other option is selected with equal probability. Formally, $f_{\text{TM-per}}(A, a) = \rho(A)(1 - \mu_A)$ when $a = m(P, A)$, and $f_{\text{TM-per}}(A, a) = \rho(A)[\mu_A/(|A| - 1)]$ otherwise, where ρ is defined as above. Alternatively, in line with the notion of proper equilibrium in game theory, one may entertain that the perturbation process recurs among the surviving alternatives. That is, conditional on a shock involving the best option, with probability $1 - \mu_A$ the individual chooses the second-best option from A and with probability μ_A overlooks the second-best option, and so forth. In this case, the resulting collection of observations is $f_{\text{TM-pro}}(A, a) = \rho(A)(1 - \mu_A)\mu_A^{|\{x \in A : xPa\}|}$ for any alternative a other than the worst one in menu A, and $f_{\text{TM-pro}}(A, a) = \rho(A)\mu_A^{|A|-1}$ otherwise. We write f_{TM} to refer to both models, $f_{\text{TM-per}}$ and $f_{\text{TM-pro}}$.[14] As in the previous case, the class of tremble models that we are contemplating allows the error to depend on the particular menus.

Choice control models.—Consider the case in which being able to control the implementation of choice involves a cost. In such a situation, the agent evaluates the trade-off between the cost of control and the cost of deviating from her preferences and maximizes accordingly.[15] Following Fudenberg et al. (2014), consider a utility function $u : X \to \mathbb{R}_{++}$ and a continuous control function $c_A : [0, 1] \to \mathbb{R}$ that describes the cost of choosing any alternative from menu A with a given probability. The utility associated with the individual choosing a probability distribution p_A over A is therefore

[14] See Selten (1975) and Myerson (1978). See Harless and Camerer (1994) for a first treatment of the tremble notion in the stochastic choice literature.

[15] Alternative motivations for the models in this category include a desire for randomization, the cost of deviating from a social exogenous choice distribution, etc. See Mattsson and Weibull (2002) and Fudenberg, Iijima, and Strzalecki (2014) for a discussion.

$$\sum_{x\in A}[p_A(x)u(x) - c_A(p_A(x))].$$

The individual then selects a probability distribution p_A^* that maximizes this utility, that is,

$$p_A^* \in \arg\max_{p_A} \sum_{x\in A}[p_A(x)u(x) - c_A(p_A(x))].$$

Thus, by using ρ as above, we can define the collection generated by the choice control model as $f_{\mathrm{CCM}}(A, a) = \rho(A)p_A^*(a)$.

Proposition 1 establishes that all the above models satisfy P-monotonicity.

PROPOSITION 1. f_{RUM}, f_{TM}, and f_{CCM} satisfy P-monotonicity.

Proof. We first analyze random utility models. Consider a menu A and alternatives $x, y \in A$ with xPy. Take a realization of the error terms such that U is maximized at y over the menu A. That is, $u(y) + \epsilon_A(y) > u(x) + \epsilon_A(x)$ and $u(y) + \epsilon_A(y) > u(z) + \epsilon_A(z)$ for any other option $z \in A\setminus\{x, y\}$. Then, consider the alternative realization of the errors, where y receives the shock $\epsilon_A(x)$, x receives the shock $\epsilon_A(y)$, and z receives the same shock $\epsilon_A(z)$. Since $u(x) > u(y)$, $u(x) + \epsilon_A(y) > u(y) + \epsilon_A(y) > u(z) + \epsilon_A(z)$ for all $z \in A\setminus\{x, y\}$, and also $u(x) + \epsilon_A(y) > u(y) + \epsilon_A(x)$. Then, the continuous i.i.d. nature of the errors within menu A guarantees that

$$\Pr\left[x = \arg\max_{w\in A} U(w)\right] > \Pr\left[y = \arg\max_{w\in A} U(w)\right].$$

This implies that $f_{\mathrm{RUM}}(A, x) \geq f_{\mathrm{RUM}}(A, y)$ with strict inequality if the menu A is such that $\rho(A) > 0$. Consequently,

$$\sum_{A\supseteq\{x,y\}} f_{\mathrm{RUM}}(A, x) \geq \sum_{A\supseteq\{x,y\}} f_{\mathrm{RUM}}(A, y),$$

with strict inequality whenever $\rho(A) > 0$ for at least one set A containing x and y and, clearly, P-monotonicity holds.

We now study tremble models. Consider a menu A and alternatives $x, y \in A$ with xPy. In the case of $f_{\mathrm{TM\text{-}per}}$, notice that $x = m(P, A)$ implies that

$$\begin{aligned} f_{\mathrm{TM\text{-}per}}(A, x) &= \rho(A)(1-\mu_A) \geq \rho(A)\mu_A \geq \rho(A)\frac{\mu_A}{|A|-1} \\ &= f_{\mathrm{TM\text{-}per}}(A, y) \end{aligned}$$

while $x \neq m(P, A)$ implies that

$$f_{\mathrm{TM\text{-}per}}(A, x) = \rho(A)\frac{\mu_A}{|A|-1} = f_{\mathrm{TM\text{-}per}}(A, y).$$

In the case of $f_{\text{TM-pro}}$, if y is not the worst alternative in A,

$$f_{\text{TM-pro}}(A, x) = \rho(A)(1 - \mu_A)\mu_A^{|\{z\in A: zPx\}|} \geq \rho(A)(1 - \mu_A)\mu_A^{|\{z\in A: zPy\}|} \\ = f_{\text{TM-pro}}(A, y).$$

If y is the worst alternative in A,

$$f_{\text{TM-pro}}(A, x) = \rho(A)(1 - \mu_A)\mu_A^{|\{z\in A: zPx\}|} \geq \rho(A)\mu_A^{|A|-1} \\ = f_{\text{TM-pro}}(A, y).$$

Then

$$\sum_{A\supseteq\{x,y\}} f_{\text{TM}}(A, x) \geq \sum_{A\supseteq\{x,y\}} f_{\text{TM}}(A, y),$$

with strict inequality whenever $\rho(A) > 0$ for at least one set A such that (i) in the case of $f_{\text{TM-per}}$, x is the best alternative in $A \supseteq \{x, y\}$, and (ii) in the case of $f_{\text{TM-pro}}$, $A \supseteq \{x, y\}$. This is clearly the case for $\{x, y\}$, and hence P-monotonicity holds.

Finally, we analyze choice control models. Consider a menu A and alternatives $x, y \in A$ with xPy. We first prove that $f_{\text{CCM}}(A, x) \geq f_{\text{CCM}}(A, y)$. Suppose, by contradiction, that $f_{\text{CCM}}(A, x) < f_{\text{CCM}}(A, y)$ or, equivalently, $p_A^*(x) < p_A^*(y)$. Consider p_A' with $p_A'(x) = p_A^*(y)$, $p_A'(y) = p_A^*(x)$, and $p_A'(z) = p_A^*(z)$ for all $z \in A\setminus\{x, y\}$. Since, by assumption, $u(x) > u(y)$, it is the case that

$$\sum_{w\in A}[p_A'(w)u(w) - c_A(p_A'(w))] > \sum_{w\in A}[p_A^*(w)u(w) - c_A(p_A^*(w))],$$

thus contradicting the optimality of p^*. Since this is true for every menu,

$$\sum_{A\supseteq\{x,y\}} f_{\text{CCM}}(A, x) \geq \sum_{A\supseteq\{x,y\}} f_{\text{CCM}}(A, y)$$

holds. For the strict part, notice that continuity of $c_{\{x,y\}}$ prevents the optimal solution p^* from being constant in $\{x, y\}$, and hence P-monotonicity follows. QED

We now show that the swaps index always identifies the true underlying preference in models that satisfy P-monotonicity and, particularly, that the presence of all the menus in the data guarantees that the swaps index uniquely identifies the preference.

THEOREM 1. If f satisfies P-monotonicity, then P is a swaps preference of f. If, moreover, $\sum_{a\in A} f(A, a) > 0$ holds for every menu A, then P is the unique swaps preference of f.

Proof. Let f be P-monotone. Consider any preference P' different from P. Then, there exist at least two alternatives a_1 and a_2 that are consecutive in P', with $a_2P'a_1$ but a_1Pa_2. Define a new preference P'' by $xP''y \Leftrightarrow xP'y$ whenever $\{x, y\} \neq \{a_1, a_2\}$ and $a_1P''a_2$. That is, P'' is simply defined by changing the position of the consecutive alternatives a_1 and a_2 in P', reconciling their comparison with that of preference P and leaving all else the same. We now show that P'' rationalizes data with fewer swaps than P'. To see this, simply notice that the swaps computation will be affected only by menus A such that $A \supseteq \{a_1, a_2\}$. Also, for any of such sets, since both alternatives are consecutive in both P' and P'', the swaps computation will be affected only by observations of the form (A, a_1) and (A, a_2) and clearly,

$$\begin{aligned}\sum_{(A,a)} f(A, a) \mid \{x \in A : xP''a\} \mid \; &= \sum_{(A,a)} f(A, a) \mid \{x \in A : xP'a\} \mid \\ &\quad + \sum_{A \supseteq \{a_1, a_2\}} f(A, a_2) - \sum_{A \supseteq \{a_1, a_2\}} f(A, a_1).\end{aligned}$$

Since f is P-monotone, the latter is smaller than or equal to $\sum_{(A,a)} f(A, a) \mid \{x \in A : xP'a\} \mid$, as desired. Given the finiteness of X, repeated application of this algorithm leads to preference P and proves that

$$\sum_{(A,a)} f(A, a) \mid \{x \in A : xPa\} \mid \; \leq \sum_{(A,a)} f(A, a) \mid \{x \in A : xP'a\} \mid .$$

Hence, P is an argument that minimizes the swaps index. Whenever $\sum_{a \in A} f(A, a) > 0$ holds for every menu A, it is in particular satisfied for the menus $\{a_1, a_2\}$ involved in each step of the previous algorithm. By P-monotonicity, the corresponding inequalities are strict, and therefore P is the unique swaps preference. QED

Theorem 1 provides a simple test to guarantee that the swaps index identifies the true preference of a particular choice model. Two questions naturally arise at this point. The first is whether other indices may also systematically recover it when P-monotonicity holds. It is easy to see that the Afriat and Varian indices do not possess this recovery property in general, since they depend on the monetary structure of the alternatives, which is not necessarily aligned with preferences. To see this, consider the simplest case in which $X = \{x, y\}$ and suppose xPy. Notice that if $f(X, y) \neq 0$, I_A recovers P if and only if $w_x^X \leq w_y^X$. Similarly, I_V recovers P if and only if $w_x^X / w_y^X \leq f(X, x)/f(X, y)$. Without these extra conditions, I_A and I_V are unable to recover P. Moreover, indices based on the number of violations of a rationality property, such as I_W or the money pump index, are also unable to recover the preference, since these indices are not built to identify any particular preference, nor can they be written in this form.[16] Finally, since I_{HM} does not take into consideration the severity of the inconsistencies, it is also unable to recover P from P-monotone models. To see this, let $X = \{x, y, z\}$ with $xPyPz$, and consider a model generating a P-monotone collection f such that $f(\{y, z\}, y) < f(\{y, z\}, z)$. It is immediate that the mass of inconsistent observations in f with respect to $xP'zP'y$ is strictly lower than that of P, and hence the optimal preference for I_{HM} cannot be P.[17]

The next question concerns choice models not satisfying P-monotonicity for which I_S does not recover the true preference. A leading case is consideration set models.[18] In this setting, the individual considers each alternative with a given probability and then chooses the maximal alternative from those that have been considered, and hence good alternatives may be chosen with low probability. We can address this case by using a slight generalization of I_S, the nonneutral swaps index I_{NNS} proposed in Section VI.B.

V. Axiomatic Foundations for the Swaps Index

Here, we propose seven properties that shape the way in which an inconsistency index I treats different types of collections of observations. We then show that the swaps index is characterized by this set of properties.

Continuity (*CONT*).—The index I is a continuous function. That is, for every sequence $\{f_n\} \subseteq \mathcal{F}$, if $f_n \to f$, then $I(f_n) \to I(f)$.

This is the standard definition of continuity, which is justified in the standard fashion. That is, it is desirable that a small variation in the data does not cause an abrupt change in the inconsistency value.

Rationality (*RAT*).—For every $f \in \mathcal{F}$, $I(f) = 0$ if and only if f is rationalizable.

Rationality imposes that a collection of observations is perfectly consistent if and only if the collection is rationalizable. In line with the maximization principle, rationality establishes that the minimal inconsistency level of 0 is reached only when every single choice in the collection can be explained by maximizing the same preference relation.

Concavity (*CONC*).—The index I is a concave function. That is, for every $f, g \in \mathcal{F}$ and every $\alpha \in [0, 1]$,

[16] In Sec. V we discuss the axiom piecewise linearity, which allows for the recoverability of preferences.

[17] Again, in Sec. V we discuss the axiom disjoint composition, which allows us to account for the severity of the inconsistencies.

[18] See Masatlioglu et al. (2012) for a deterministic modeling and Manzini and Mariotti (2014) for a recent stochastic model.

$$I(\alpha f + (1 - \alpha)g) \geq \alpha I(f) + (1 - \alpha)I(g).$$

.

To illustrate the desirability of this property in our context, take any two collections f and g and suppose them to be rationalizable when taken separately. Clearly, a convex combination of f and g does not need to be rationalizable, and hence the collection $\alpha f + (1 - \alpha)g$ can take only the same or a higher inconsistency value than the combination of the inconsistency values of the two collections. The same idea applies when either f or g or both are not rationalizable. The combination of f and g can generate the same or a greater number of frictions only with the maximization principle and hence should yield the same or a higher inconsistency value.

Piecewise linearity (*PWL*).—The index I is a piecewise linear function over $|\mathcal{P}|$ pieces. That is, there are $|\mathcal{P}|$ subsets of $\mathcal{F}$, the union of which is $\mathcal{F}$ such that for every pair f, g belonging to the same subset and every $\alpha \in [0, 1]$,

$$I(\alpha f + (1 - \alpha)g) = \alpha I(f) + (1 - \alpha)I(g).$$

Piecewise linearity brings two features: the piecewise nature of the index and the linear structure of the index over each piece. Let us now elaborate on the desirability of these two features.

Notice that the piecewise assumption in piecewise linearity is attractive from the recoverability of preferences perspective and hence is critical for predicting behavior and enabling individual welfare analysis. An index satisfying the piecewise assumption divides the set of collections of observations $\mathcal{F}$ into $|\mathcal{P}|$ classes. Thus, as any preference is linked to one and only one of such classes, every single collection of observations, even the nonrationalizable ones, can be linked to a specific preference relation.

Within each of the pieces, piecewise linearity makes the index react monotonically with respect to inconsistencies, whether they are (i) of the same type, thus making the index react to the mass of an inconsistency, or (ii) of different types, thus making the index react to the accumulation of several different inconsistencies. To enable formal study of these implications, we introduce a useful class of collections of observations, which we describe as perturbed. Consider a rationalizable collection of observations $r \in \mathcal{R}$ and an observation $(A, a) \in \mathcal{O}$. An ϵ-perturbation of r in the direction of (A, a) involves replacing an ϵ-mass of optimal choices $(A, m(P^r, A))$ with the possibly suboptimal choices (A, a).[19] We denote

[19] Obviously, the value of ϵ must be lower than $r(A, m(P^r, A))$.

such a perturbed collection by $r^{\epsilon(A,a)} = r + \epsilon\mathbf{1}_{(A,a)} - \epsilon\mathbf{1}_{(A,m(P^r,A))}$ and the collection in which two different ϵ-perturbations take place by

$$r^{\epsilon(A,a)}_{\epsilon(B,b)} = r + \epsilon\mathbf{1}_{(A,a)} - \epsilon\mathbf{1}_{(A,m(P^r,A))} + \epsilon\mathbf{1}_{(B,b)} - \epsilon\mathbf{1}_{(B,m(P^r,B))}.$$

The following lemma, proved in Appendix A, establishes the above implications.

LEMMA 1. Let I be an inconsistency index satisfying PWL, CONT, and RAT. Consider any collection $r \in \mathcal{R}$ and any two different observations (A, a), (B, b) such that $a \neq m(P^r, A)$ and $b \neq m(P^r, B)$. For any two sufficiently small real values $\epsilon_1 > \epsilon_2 \geq 0$,

1. reactivity to the mass of an inconsistency: $I(r^{\epsilon_1(A,a)}) > I(r^{\epsilon_2(A,a)})$;
2. reactivity to several inconsistencies: $I(r^{\epsilon_1(A,a)}_{\epsilon_1(B,b)}) > \max\{I(r^{\epsilon_1(A,a)}), I(r^{\epsilon_1(B,b)})\}$.

The proof of lemma 1 explicitly shows how PWL implies reactivity of the index to both the mass and the types of inconsistencies in a linear fashion, namely, $I(r^{\epsilon_1(A,a)}) = (\epsilon_1/\epsilon_2)I(r^{\epsilon_2(A,a)})$ and $I(r^{\epsilon_1(A,a)}_{\epsilon_1(B,b)}) = I(r^{\epsilon_1(A,a)}) + I(r^{\varepsilon_1(B,b)})$.

Ordinal consistency (*OC*).—For every $(A, a) \in \mathcal{O}$ and every $r, \tilde{r} \in \mathcal{R}$ such that $r(\{x, y\}, x) = \tilde{r}(\{x, y\}, x)$ whenever $x, y \in A$, it is $I(r^{\epsilon(A,a)}) = I(\tilde{r}^{\epsilon(A,a)})$ for any sufficiently small $\epsilon > 0$.

Ordinal consistency is in the spirit of the classical properties of independence of irrelevant alternatives. A small perturbation of the type (A, a) generates the same inconsistency in two rationalizable collections r and $\tilde{r}$ that coincide in the ranking of the alternatives within A but may diverge in the ranking of alternatives outside A. In other words, the order of alternatives not involved in the inconsistency is inconsequential. In line with the standard justification for such a property, one may simply contend that when evaluating a perturbed collection, any alternative not involved in the perturbation at hand should not matter.

Disjoint composition (*DC*).—For every (A_1, a), $(A_2, a) \in \mathcal{O}$ such that $A_1 \cap A_2 = \{a\}$ and every $r \in \mathcal{R}$, it is $I(r^{\epsilon(A_1 \cup A_2,a)}) = I(r^{\epsilon(A_1,a)}_{\epsilon(A_2,a)})$ for any sufficiently small $\epsilon > 0$.

In words, disjoint composition states that, given a rationalizable collection r, a small perturbation of the type $(A_1 \cup A_2, a)$ can be broken down into two small perturbations of the form (A_1, a) and (A_2, a), provided that A_1 and A_2 share no alternative other than a. By iteration, an index having this property is able to reduce the inconsistency of the observation into inconsistencies involving binary comparisons. This property is desirable for several reasons. First, from a purely normative point of view, notice that the standard welfare approach is constructed precisely on the basis of binary comparisons. Hence, an index that aims to capture the severity of an inconsistency in terms of the welfare loss involved must likewise be based on binary comparisons. Second, from a practical point of view, this decomposition facilitates the tractability of the data by compacting it into a unique matrix of binary choices. To illustrate, notice that both $r^{\epsilon(A_1 \cup A_2, a)}$ and $r^{\epsilon(A_1,a)}_{\epsilon(A_2,a)}$ correspond to the following summary of binary revealed choices. Whenever $xP^r a$ and $x \in A_1 \cup A_2$, ϵ percent of the data is inconsistent with x being preferred to a. No inconsistencies arise in any other comparison of two alternatives. Disjoint composition implies that this summary is the only relevant information and hence declares the two collections equally inconsistent.

In order to introduce our last property, let us consider the following notation. Given a permutation σ over the set of alternatives X, for any collection f we denote by $\sigma(f)$ the permuted collection such that $\sigma(f)(A, a) = f(\sigma(A), \sigma(a))$.

Neutrality (*NEU*).—For every permutation σ and every $f \in \mathcal{F}$, $I(f) = I(\sigma(f))$.

Neutrality imposes that the inconsistency index should be independent of the names of the alternatives. That is, any relabeling of the alternatives should have no effect on the level of inconsistency.

Theorem 2 states the characterization result.

THEOREM 2. An inconsistency index I satisfies CONT, RAT, CONC, PWL, OC, DC, and NEU if and only if it is a positive scalar transformation of the swaps index.

Proof. It is immediate to see that any positive scalar transformation of the swaps index satisfies the axioms. By way of seven steps, we show that an index satisfying the axioms is a transformation of the swaps index.

Step 1: Following the proof of lemma 1, consider the convex hulls of the closure of the $|\mathcal{P}|$ subsets of collections. Reasoning analogously, for every $r \in \mathcal{R}$, there exists $\alpha^r \in (0, 1)$ such that, for every observation (A, a) and every $\alpha \in [0, \alpha^r]$, the collection $\alpha \mathbf{1}_{(A,a)} + (1 - \alpha) r$ belongs to the convex hull of r. We then define, for every r and (A, a), the weight

$$w(P^r, A, a) = \frac{I(\alpha^r \mathbf{1}_{(A,a)} + (1 - \alpha^r) r)}{\alpha^r}.$$

Now notice that, whenever $aP^r x$ for all $x \in A \setminus \{a\}$, the collection $\alpha^r \mathbf{1}_{(A,a)} + (1 - \alpha^r) r$ is rationalizable by P^r and RAT implies $w(P^r, A, a) = 0$. Otherwise, it follows that $r(A, x) > 0$ with $x \neq a$, which implies that observations (A, a) and (A, x) have positive mass in the collection $\alpha^r \mathbf{1}_{(A,a)} + (1 - \alpha^r) r$, and RAT guarantees that $w(P^r, A, a) > 0$.

Step 2: We now prove that, whenever $f \in \mathcal{F}$ and $r \in \mathcal{R}$ belong to the same convex hull, it is the case that $I(f) = \sum_{(A,a)} f(A, a) w(P^r, A, a)$. By RAT and PWL,

$$I(f) = \frac{\alpha^r I(f)}{\alpha^r} = \frac{\alpha^r I(f) + (1-\alpha^r)I(r)}{\alpha^r} = \frac{I(\alpha^r f + (1-\alpha^r)r)}{\alpha^r}.$$

Notice that

$$\begin{aligned}\alpha^r f + (1-\alpha^r)r &= \alpha^r\left(\sum_{(A,a)} f(A,a)\mathbf{1}_{(A,a)}\right) + (1-\alpha^r)r \\ &= \sum_{(A,a)} f(A,a)[\alpha^r \mathbf{1}_{(A,a)} + (1-\alpha^r)r].\end{aligned}$$

By definition of α^r, all collections $\alpha^r\mathbf{1}_{(A,a)} + (1-\alpha^r)r$ belong to the convex hull of r and all convex combinations of such collections must also lie in it. We can thus apply linearity repeatedly to obtain

$$\begin{aligned}I(f) &= \frac{I(\alpha^r f + (1-\alpha^r)r)}{\alpha^r} = \frac{\sum_{(A,a)} f(A,a) I(\alpha^r \mathbf{1}_{(A,a)} + (1-\alpha^r)r)}{\alpha^r} \\ &= \sum_{(A,a)} f(A,a) w(P^r, A, a).\end{aligned}$$

Step 3: Here, we prove that, for every $f \in \mathcal{F}$, $I(f) = \min_P \sum_{(A,a)} f(A,a)w(P,A,a)$. We first prove that, for every $r \in \mathcal{R}$, $I(f) \leq \sum_{(A,a)} f(A,a)w(P^r,A,a)$. By RAT and CONC,

$$I(f) = \frac{\alpha^r I(f)}{\alpha^r} = \frac{\alpha^r I(f) + (1-\alpha^r)I(r)}{\alpha^r} \leq \frac{I(\alpha^r f + (1-\alpha^r)r)}{\alpha^r}.$$

By definition of α^r, all collections $\alpha^r\mathbf{1}_{(A,a)} + (1-\alpha^r)r$ belong to the convex hull of r, and hence $\alpha^r f + (1-\alpha^r)r$ also belongs to the hull. By steps 1 and 2, we know that

$$I(\alpha^r f + (1-\alpha^r)r) = \alpha^r \sum_{(A,a)} f(A,a)w(P^r,A,a)$$

and hence,

$$I(f) \leq \sum_{(A,a)} f(A,a)w(P^r,A,a).$$

By the proof of lemma 1 we know that each convex hull contains one and only one collection in $\mathcal{R}$, and then for every $f \in \mathcal{F}$, there exists $\hat{r} \in \mathcal{R}$ such that f and $\hat{r}$ lie in the same convex hull. Hence, step 2 and the above reasoning guarantee that

$$I(f) = \sum_{(A,a)} f(A,a)w(P^{\hat{r}},A,a) = \min_P \sum_{(A,a)} f(A,a)w(P,A,a).$$

Step 4: We now prove that, for every (A, a), and every pair P^r and $P^{\bar{r}}$ such that $xP^r y \Leftrightarrow xP^{\bar{r}} y$ whenever $x, y \in A$, it is the case that

$w(P^r, A, a) = w(P^{\tilde{r}}, A, a)$. To see this, notice that there exists a sufficiently small α such that $r^{\alpha(A,a)}$ and $\tilde{r}^{\alpha(A,a)}$ belong to the convex hulls of r and $\tilde{r}$, respectively. By steps 1 and 2 and OC, it is the case that

$$\alpha w(P^r, A, a) = I(r^{\alpha(A,a)}) = I(\tilde{r}^{\alpha(A,a)}) = \alpha w(P^{\tilde{r}}, A, a)$$

or, equivalently, $w(P^r, A, a) = w(P^{\tilde{r}}, A, a)$.

Step 5: Here we prove that, for every (A, a) and P^r, $w(P^r, A, a) = \sum_{x \in A} w(P^r, \{x, a\}, a)$. To do this, we prove that for any two menus A_1, A_2 such that $A_1 \cap A_2 = \{a\}$ and $A_1 \cup A_2 = A$, it is the case that $w(P^r, A, a) = w(P^r, A_1, a) + w(P^r, A_2, a)$. The recursive application of this idea, given the finiteness of X, concludes the step. Again, there exists a sufficiently small α such that $r^{\alpha(A,a)}$, $r^{\alpha(A_1,a)}$, and $r^{\alpha(A_2,a)}$ all belong to the convex hull of r. By steps 1 and 2 and DC, it is the case that

$$\begin{aligned} \alpha w(P^r, A, a) &= I(r^{\alpha(A,a)}) = I(r^{\alpha(A_1,a)}_{\alpha(A_2,a)}) \\ &= \alpha w(P^r, A_1, a) + \alpha w(P^r, A_2, a), \end{aligned}$$

which implies $w(P^r, A, a) = w(P^r, A_1, a) + w(P^r, A_2, a)$.

Step 6: Here we prove that $w(P^r, \{x, y\}, y) = w(P^{\tilde{r}}, \{z, t\}, t)$ holds for every $x, y, z, t \in X$ and every pair P^r and $P^{\tilde{r}}$ such that the ranking of x (respectively, of y) in P^r is the same as the ranking of z (respectively, of t) in $P^{\tilde{r}}$. Consider the bijection $\sigma : X \to X$, which assigns, to the alternative ranked at s in P^r, the alternative ranked at s in $P^{\tilde{r}}$. Then, it is $\sigma(x) = z$ and $\sigma(y) = t$ and also, $\sigma(r) = \tilde{r}$. There exists a sufficiently small α such that $r^{\alpha(\{x,y\},y)}$ belongs to the convex hull of r and $\tilde{r}^{\alpha(\{z,t\},t)}$ belongs to the convex hull of $\tilde{r}$. By steps 1 and 2 and NEU, we have that

$$\begin{aligned} \alpha w(P^r, \{x, y\}, y) &= I(r^{\alpha(\{x,y\},y)}) = I(\sigma(r^{\alpha(\{x,y\},y)})) = I(\tilde{r}^{\alpha(\{z,t\},t)}) \\ &= \alpha w(P^{\tilde{r}}, \{z, t\}, t), \end{aligned}$$

that is, $w(P^r, \{x, y\}, y) = w(P^{\tilde{r}}, \{z, t\}, t)$.

Step 7: We finally prove that I is a positive scalar transformation of the swaps index. Let P and P' be any two preferences and consider $x, y, z, t \in X$ with xPy and $zP't$. Thanks to step 4, consider without loss of generality that x and y (respectively, z and t) are the first two elements of P (respectively, P'). Steps 1 and 6 guarantee that $w(P, \{x, y\}, y) = w(P', \{z, t\}, t) > 0$ and steps 3 and 5 lead to

$$\begin{aligned} I(f) &= \min_P \sum_{(A,a)} f(A, a) w(P, A, a) \\ &= \min_P \sum_{(A,a)} f(A, a) \sum_{x \in A : xPa} w(P, \{x, a\}, a) \\ &= K \min_P \sum_{(A,a)} f(A, a) \mid \{x \in A : xPa\} \mid, \end{aligned}$$

TABLE 1
Summary of the Relationship between Axioms and Inconsistency Indices

	CONT	RAT	CONC	PWL	OC	NEU	DC
I_S	✓	✓	✓	✓	✓	✓	✓
I_{HM}	✓	✓	✓	✓	✓	✓	X
I_V	✓	✓	✓	✓	✓	X	X
I_W	✓	✓	✓[a]	X	X	✓	X
I_{W-MP}	✓	✓	✓[a]	X	X	X	X
I_A	X	✓	✓	X	X	X	X

[a] I_W and I_{W-MP} do not satisfy CONC, but a simple transformation would do. See Sec. VII for how to build the transformation in an application.

with $K > 0$, which shows that I is a positive scalar transformation of the swaps index. QED

Table 1 illustrates the structural relationship of the swaps index with the other rationality indices discussed in Section III.A. We do this by stating which axioms, among those characterizing the swaps index, they satisfy.

VI. A General Class of Indices

A. *General Weighted Index*

The swaps index relies exclusively on the endogenous information contained in the revealed choices. On occasions, however, the analyst may have more information and may wish to use it to assess the consistency of choice and identify the optimal welfare ranking. We now offer a generalization of the swaps index that is able to incorporate other information. The *general weighted index* considers every possible inconsistency between an observation and a preference relation through a weight that may depend on the nature of the menu of alternatives, the nature of the chosen alternative, and the nature of the preference relation. Then, for a given collection f, the inconsistency index takes the form of the minimum total inconsistency across all preference relations:

$$I_G(f) = \min_P \sum_{(A,a)} f(A,a) w(P,A,a),$$

where $w(P, A, a) = 0$ if $a = m(P, A)$ and $w(P, A, a) \in \mathbb{R}_{++}$ otherwise.

It turns out that the general weighted index is characterized by the first four axioms used in the characterization of the swaps index.[20]

Proposition 2. An inconsistency index I satisfies CONT, RAT, CONC, and PWL if and only if it is a general weighted index.

[20] The proof of this result, and all the ones that follow, can be found in App. A.

B. Nonneutral Swaps Index and Positional Swaps Index

We now present two indices from the class of general weighted indices that may be especially relevant. We start by considering settings in which the analyst has information on the nature of the alternatives, such as their monetary values, attributes, and so forth. Under these circumstances, the property of NEU may lose its appeal, since one now may wish to treat different pairs of alternatives differently, using the exogenous information that is available on them. It turns out that the remaining six properties in theorem 2 characterize a class of indices that we call the *nonneutral swaps index*. Let $w_{x,a} \in \mathbb{R}_{++}$ denote the weight of the ordered pair of alternatives x and a; that is, $w_{x,a}$ represents the cost of swapping the preferred alternative x with the chosen alternative a. Then

$$I_{\mathrm{NNS}}(f) = \min_{P} \sum_{(A,a)} f(A, a) \sum_{x \in A: xPa} w_{x,a}.$$

Proposition 3. An inconsistency index I satisfies CONT, RAT, CONC, PWL, OC, and DC if and only if it is a nonneutral swaps index.

Now suppose that the analyst has information on the cardinal utility values of the different alternatives, based on their position in the ranking, and wants to use it. Then, OC, which completely disregards this type of information, immediately obliterates its appeal. We show that the elimination of OC from the system of properties characterizes the following index, which we call the *positional swaps index*:

$$I_{PS}(f) = \min_{P} \sum_{(A,a)} f(A, a) \sum_{x \in A: xPa} w_{\hat{x}(P), \hat{a}(P)},$$

where $w_{i,j} \in \mathbb{R}_{++}$ denotes the weight associated with positions i and j and $\hat{x}(P)$ is the ranking of alternative x in P. Again, $w_{i,j}$ is interpreted as the cost of swapping the preferred alternative, the one that occupies position i in the ranking, with the chosen alternative, that occupies position j in the ranking.

Proposition 4. An inconsistency index I satisfies CONT, RAT, CONC, PWL, DC, and NEU if and only if it is a positional swaps index.

C. Varian and Houtman-Maks

As introduced in Section III.A, two popular measures of the consistency of behavior are due to Varian (1990) and Houtman and Maks (1985). We have already shown that these indices satisfy the properties that, by theorem 3, characterize the general weighted indices. We now provide their complete characterizations.

Let us start with the case of Varian. Its characterization requires a structure related to the search for the maximum weight in a given upper contour set. Let us then consider the following notation. For any $r \in \mathcal{R}$ and any $(A, a) \in \mathcal{O}$, denote by $\mathcal{R}^r_{(A,a)}$ all rationalizable collections $\tilde{r}$ such that (i) the top two alternatives in $P^{\tilde{r}}$ belong to A, and (ii) the top alternative in $P^{\tilde{r}}$ belongs to the strict upper contour set of a with respect to P^r.

Varian's consistency (*VC*).—For every $(A, a) \in \mathcal{O}$ and every $r \in \mathcal{R}$, it is $I(r^{\epsilon(A,a)}) = \max_{\tilde{r} \in \mathcal{R}^r_{(A,a)}} I(\tilde{r}^{\epsilon(A,z_{\tilde{r}})})$ for any sufficiently small $\epsilon > 0$, where $z_{\tilde{r}}$ is the second-best alternative according to $P^{\tilde{r}}$.[21]

Varian's consistency imposes that the inconsistency generated by a small perturbation of r in the direction of (A, a) can be related to that of perturbed collections of observations in which the inconsistency involves only the top alternative, which is ranked higher than a according to P^r. Varian's consistency is stronger than ordinal consistency because, whenever r and r' treat all the alternatives in A equally, the classes $\mathcal{R}^r_{(A,a)}$ and $\mathcal{R}^{r'}_{(A,a)}$ are the same. The following result establishes the characterization of Varian's index I_V.

Proposition 5. An inconsistency index I satisfies CONT, RAT, CONC, PWL, and VC if and only if it is a Varian index.

We now turn to the analysis of Houtman-Maks's index, recalling that, in our setting, it is but a special case of Varian's index when $w^A_x = 1$ for every A and every $x \in A$. Consequently, the characterization of I_{HM} builds on that of I_V and imposes some additional structure. First, notice that I_{HM} does not discriminate between the alternatives, and hence any relabeling of the alternatives should have no effect on the level of inconsistency, thus reinstating the appeal of neutrality. However, I_{HM} requires further structure.

Houtman-Maks's composition (*HMC*).—For every $(A_1, a), (A_2, a) \in \mathcal{O}$ with $A_1 \cap A_2 = \{a\}$ and every $r \in \mathcal{R}$, $I(r^{\epsilon(A_1 \cup A_2, a)}) = \max\{I(r^{\epsilon(A_1,a)}), I(r^{\epsilon(A_2,a)})\}$ for any sufficiently small $\epsilon > 0$.

Houtman-Maks's composition establishes that, under the same conditions of disjoint composition, a small perturbation of type $(A_1 \cup A_2, a)$ is equal to the maximum of the two small perturbations that appear when breaking down the former observation into (A_1, a) and (A_2, a). We can now establish the characterization result of I_{HM}.

Proposition 6. An inconsistency index I satisfies CONT, RAT, CONC, PWL, VC, HMC, and NEU if and only if it is a scalar transformation of the Houtman-Maks index.

VII. An Application

In this section we use the experimental study of Harbaugh et al. (2001) to see the applicability of the swaps index.[22] The paper develops a test of

[21] Again, for notational convenience, let $\max_{r \in \varnothing} I(\cdot) = 0$.

[22] We are very grateful to the authors for sharing all their material with us.

consistency with rationality for three different age groups: 31 7-year-old participants, 42 11-year-old participants, and 55 21-year-old participants. The experimental choice task presents the participants with 28 different bundles of two goods confronted in 11 different menus.[23] By counting the number of GARP violations, the main result is that, although violations of rationality are significantly more frequent in the youngest age group, they are present in all three age groups: 74 percent, 38 percent, and 35 percent, in the 7-, 11-, and 21-year-old groups, respectively.

We now report on I_S, together with I_{HM} and I_W. Given that the alternatives are defined by the quantities of two different goods, we compute I_S and I_{HM} by considering the set of all linear orders that satisfy quantity monotonicity. Note that $f(A, a)$ is either $1/11$ or zero, given that the individuals make choices from 11 different menus. With respect to I_W, we say that there is a violation of WARP between observations (A, a) and (B, b) if there are alternatives x, y with $a \leq x \in B$ and $b \leq y \in A$. We normalize the number of WARP violations dividing them by the total number of observations.[24] The results for all 128 subjects are reported in table 2 in the online appendix. The main conclusions reached in Harbaugh et al. (2001) are reproduced here.

We now contrast I_S with the other indices. First, among the 128 subjects, 70 are rational, and clearly, I_S coincides with I_{HM}, I_W, and I_A over them, since all these indices satisfy RAT.[25] Over the remaining subjects, the Spearman's rank correlation coefficient between I_S and I_{HM} is .97, between I_S and I_W is .83, and between I_S and I_A is .51. We now illustrate the differences in the rationality judgment of the indices, by using some particular participants.

Swaps versus Houtman-Maks.—Consider individual 119.[26] It turns out that all the inconsistencies generated by this individual can be eliminated by dropping only two observations, $(A_6, (4, 1))$ and $(A_9, (2, 0))$, which leads us to $I_{HM}(f_{119}) = 2/11$. However, by focusing on the number of inconsistencies, I_{HM} disregards their severity, which can be seen to be relevant since $I_S(f_{119}) = 5/11$, which is one of the highest inconsistency levels (see app. table 2). In fact, it is easy to find other individuals with a higher I_{HM} index but still arguably less inconsistent than individual 119. One example

[23] $A_1 = \{(6, 0), (3, 1), (0, 2)\}$, $A_2 = \{(9, 0), (6, 1), (3, 2), (0, 3)\}$, $A_3 = \{(6, 0), (4, 1), (2, 2), (0, 3)\}$, $A_4 = \{(8, 0), (6, 1), (4, 2), (2, 3), (0, 4)\}$, $A_5 = \{(4, 0), (3, 1), (2, 2), (1, 3), (0, 4)\}$, $A_6 = \{(5, 0), (4, 1), (3, 2), (2, 3), (1, 4), (0, 5)\}$, $A_7 = \{(6, 0), (5, 1), (4, 2), (3, 3), (2, 4), (1, 5), (0, 6)\}$, $A_8 = \{(3, 0), (2, 2), (1, 4), (0, 6)\}$, $A_9 = \{(2, 0), (1, 3), (0, 6)\}$, $A_{10} = \{(4, 0), (3, 2), (2, 4), (1, 6), (0, 8)\}$, and $A_{11} = \{(3, 0), (2, 3), (1, 6), (0, 9)\}$.

[24] Notice that our definition in the text would divide it by 11×11 instead of 11. This normalization is vacuous when comparing the inconsistency of individuals.

[25] Notice that the computation of I_A (or I_V) would require the explicit assumption of certain weights. Harbaugh et al. (2001) provide a computation of I_A under certain assumptions regarding the budget sets. We use their computations here.

[26] The choices of the individual from menus A_1 to A_{11} are given in the following ordered vector: $((3, 1), (3, 2), (0, 3), (2, 3), (1, 3), (4, 1), (3, 3), (1, 4), (2, 0), (2, 4), (2, 3))$.

is subject 60, who presents three mild inconsistencies, and $I_{HM}(f_{60}) = 3/11 = I_S(f_{60})$.[27]

Swaps versus WARP.—Individual 28, with $I_W(f_{28}) = 6/11$, represents one of the cases with the largest number of cycles.[28] However, by merely counting the number of cycles, I_W is unable to determine the number and severity of the mistakes that need to be cancelled in order to break the cycles. Closer inspection shows that this can be done by eliminating only two mild inconsistencies. This is what the swaps index does, $I_S(f_{28}) = 2/11$, with inconsistent observations $(A_4, (2, 3))$ and $(A_{11}, (3, 0))$, where, according to P_S, only (4, 2) is preferred to (2, 3) in the first and (2, 3) ranks above (3, 0) in the second. In this respect, there are a number of individuals that are classed by I_W as less inconsistent than individual 28 but whose choices are nevertheless harder to reconcile with preference maximization, and whose inconsistency values in terms of I_S are therefore higher.

Swaps versus Afriat.—Consider subject 12, who according to Afriat has a relatively low inconsistency index, $I_A(f_{12}) = .125$.[29] By considering only the largest violation and, within it, focusing on nonwelfare information, Afriat ignores (i) that individual 12 commits a relatively large number of mistakes (three, to be precise, since $I_{HM}(f_{12}) = 3/11$) and (ii) that the subject is committing relatively serious mistakes by choosing alternatives that are dominated by many others in the menu (leading to $I_S(f_{12}) = 6/11$). Once again, it is easy to find cases that are incorrectly ordered by Afriat. Consider individual 28 or 119, for example, who requires larger income adjustments but, according to I_S, fewer preference adjustments.

Appendix A

Remaining Proofs

Proof of Lemma 1

PWL guarantees that there are $|\mathcal{P}|$ pieces of $\mathcal{F}$, over every one of which the index is linear. The repeated application of linearity and CONT guarantee that the index is also linear over the convex hull of the closure of each piece. We now prove that each of these convex hulls contains one and only one collection in $\mathcal{R}$. Suppose, by contradiction, that this is not the case. Since $|\mathcal{P}| = |\mathcal{R}|$, there must exist two distinct r, r' belonging to the same convex hull. Then, PWL and RAT

[27] The choices of individual 60 are ((3, 1), (3, 2), (2, 2), (4, 2), (3, 1), (4, 1), (4, 2), (2, 2), (2, 0), (3, 2), (3, 0)).

[28] The choices of individual 28 are ((3, 1), (9, 0), (2, 2), (2, 3), (2, 2), (3, 2), (3, 3), (2, 2), (2, 0), (3, 2), (3, 0)).

[29] The choices of individual 12 are ((3, 1), (3, 2), (2, 2), (2, 3), (3, 1), (3, 2), (3, 3), (0, 6), (1, 3), (3, 2), (3, 0)). Afriat's inconsistency is driven by the critical observation $(A_4, (2, 3))$, where (3, 2) is feasible and costs .875 times as much as the chosen element (2, 3), and in its counterpart $(A_{10}, (3, 2))$, where (2, 3) is feasible and costs .875 times as much as the chosen element.

guarantee that, for every $\alpha \in (0, 1)$, it is the case that $I(\alpha r + (1-\alpha)r') = \alpha I(r) + (1-\alpha)I(r') = 0$. However, since $r \neq r'$, there must exist at least one menu A and two distinct alternatives, a and b, such that $r(A, a) > 0$ and $r'(A, b) > 0$. Consequently, for $\alpha \in (0, 1)$, $[\alpha r + (1-\alpha)r'](A, a) > 0$ and $[\alpha r + (1-\alpha)r'](A, b) > 0$, and hence, $\alpha r + (1-\alpha)r'$ is not rationalizable. RAT implies that $I(\alpha r + (1-\alpha)r') \neq 0$, which is a contradiction. Now, given r and (A, a), the collections $r^{\epsilon(A,a)}$ converge to r as ϵ goes to zero, and since there is a finite number of hulls, CONT guarantees that these collections belong to the same convex hull as r for sufficiently small values of ϵ. In the same vein, given r and two different observations (A, a) and (B, b), the collections r and $r^{\epsilon(A,a)}_{\epsilon(B,b)}$ belong to the same convex hull for sufficiently small values of ϵ. Then, for sufficiently small perturbations $\epsilon_1 > \epsilon_2 \geq 0$, PWL guarantees that

$$I(r^{\epsilon_2(A,a)}) = I\left(\frac{\epsilon_2}{\epsilon_1} r^{\epsilon_1(A,a)} + \left(1 - \frac{\epsilon_2}{\epsilon_1}\right) r\right) = \frac{\epsilon_2}{\epsilon_1} I(r^{\epsilon_1(A,a)}) + \left(1 - \frac{\epsilon_2}{\epsilon_1}\right) I(r).$$

Under the assumption of RAT, whenever $a \neq m(P^r, A)$, this is but

$$I(r^{\epsilon_2(A,a)}) = \frac{\epsilon_2}{\epsilon_1} I(r^{\epsilon_1(A,a)}) < I(r^{\epsilon_1(A,a)}),$$

as desired. Now consider r, two different observations, (A, a) and (B, b), and a sufficiently small perturbation $\epsilon_1 > 0$. From PWL and the previous reasoning,

$$\begin{aligned} I(r^{\epsilon_1(A,a)}_{\epsilon_1(B,b)}) &= I\left(\frac{1}{2} r^{2\epsilon_1(A,a)} + \frac{1}{2} r^{2\epsilon_1(B,b)}\right) = \frac{1}{2} I(r^{2\epsilon_1(A,a)}) + \frac{1}{2} I(r^{2\epsilon_1(B,b)}) \\ &= I(r^{\epsilon_1(A,a)}) + I(r^{\epsilon_1(B,b)}). \end{aligned}$$

Whenever $a \neq m(P^r, A)$ and $b \neq m(P^r, B)$, the latter is strictly larger than $\max\{I(r^{\epsilon_1(A,a)}), I(r^{\epsilon_1(B,b)})\}$, as desired. QED

Proof of Proposition 2

Immediate from the proof of theorem 2. QED

Proof of Proposition 3

It is easy to see that nonneutral swaps indices satisfy the axioms. To prove the converse statement, we use steps 1–5 in the proof of theorem 2. By steps 1 and 5,

$$\sum_{(A,a)} f(A, a) w(P, A, a) = \sum_{(A,a)} f(A, a) \sum_{x \in A: xPa} w(P, \{x, a\}, a).$$

By steps 1 and 4, $w(P, \{x, a\}, a) > 0$ is independent of P, provided that xPa, and then we can write $\sum_{(A,a)} f(A, a) \sum_{x \in A: xPa} w_{x,a}$. Step 3 proves that the index is a nonneutral swaps index. QED

Proof of Proposition 4

It is easy to see that positional swaps indices satisfy the axioms. To prove the converse, we use the proof of theorem 2 except steps 4 and 7. By steps 1 and 5,

$$\sum_{(A,a)} f(A,a)w(P,A,a) = \sum_{(A,a)} f(A,a) \sum_{x\in A:xPa} w(P,\{x,a\},a).$$

By steps 1 and 6, $w(P, \{x, a\}, a) > 0$ depends only on the rank of alternatives x and a in P. This, together with step 3, shows that the index is a positional swaps index. QED

Proof of Proposition 5

It is easy to see that Varian's index satisfies the axioms. For the converse, we use the first three steps in the proof of theorem 2. Consider a set A, alternatives x, y, and z in A, and a pair, P^r and $P^{\bar{r}}$, such that (i) x and y are, respectively, the first- and second-best alternatives in P^r, and (ii) x and z are, respectively, the first- and second-best alternatives in $P^{\bar{r}}$. We claim that $w(P^r, A, y) = w(P^{\bar{r}}, A, z)$. There exists a sufficiently small α such that $r^{\alpha(A,y)}$ and $\bar{r}^{\alpha(A,z)}$ belong, respectively, to the convex hulls of r and $\bar{r}$. Since the upper contour sets of y in P^r and z in $P^{\bar{r}}$ are both equal to $\{x\}$, it is the case that $\mathcal{R}^r_{(A,y)} = \mathcal{R}^{\bar{r}}_{(A,z)}$, and hence, steps 1 and 2 in the proof of theorem 2 and VC imply

$$\begin{aligned}\alpha w(P^r, A, y) = I(r^{\alpha(A,y)}) = \max_{\tilde{r}\in\mathcal{R}^r_{(A,y)}} = \max_{\tilde{r}\in\mathcal{R}^{\bar{r}}_{(A,z)}} = I(\bar{r}^{\alpha(A,z)}) \\ = \alpha w(P^{\bar{r}(A,z)}).\end{aligned}$$

We then denote this value by w^A_x. Now, given (A, a) and r, there exists α sufficiently small such that $r^{\alpha(A,a)}$ belongs to the convex hull of r and for every $\tilde{r} \in \mathcal{R}^r_{(A,a)}$, $\tilde{r}^{\alpha(A,a_{\tilde{r}})}$ belongs to the convex hull of $\tilde{r}$, where $a_{\tilde{r}}$ is the second-best alternative in $P^{\tilde{r}}$. We can apply steps 1 and 2 in the proof of theorem 2 and VC to see that

$$\begin{aligned}\alpha w(P^r, A, a) = I(r^{\alpha(A,a)}) = \max_{\tilde{r}\in\mathcal{R}^r_{(A,a)}} I(\tilde{r}^{\alpha(A,a_{\tilde{r}})}) = \max_{\tilde{r}\in\mathcal{R}^r_{(A,a)}} \alpha w(P^{\tilde{r}}, A, a_{\tilde{r}}) \\ = \alpha \max_{x\in A, xP^r a} w^A_x.\end{aligned}$$

This proves that the index is Varian's index. QED

Proof of Proposition 6

Clearly, Houtman-Maks's index satisfies the axioms. To see the converse, from the proof of proposition 5, we now show that for every menu A and any pair of alternatives x and y belonging to A, it is the case that $w^A_x = w^{\{x,y\}}_x$. To see this, consider any r such that x is the top alternative in P^r and $y \in A$ is the second top alternative in P^r. For a sufficiently small α, it is the case that $r^{\alpha(A,y)}$, $r^{\alpha(\{x,y\},y)}$, and

$r^{\alpha(A\setminus\{y\},y)}$ all belong to the convex hull of r. By steps 1 and 2 in the proof of theorem 2, HMC, and RAT, it is the case that

$$\alpha w_x^A = I(r^{\alpha(A,y)}) = \max\{I(r^{\alpha(\{x,y\},y)}, I(r^{\alpha(A\setminus\{y\},y)})\} = I(r^{\alpha(\{x,y\},y)}) = \alpha w_x^{\{x,y\}}.$$

NEU guarantees that $w_x^{\{x,y\}} = w_z^{\{z,t\}}$ for every $x, y, z, t \in X$, and given the strict positivity of these weights, the index is a scalar transformation of the Houtman-Maks index. QED

Appendix B

Uniqueness

Here we establish that almost all collections of observations have a unique swaps preference.

PROPOSITION 7. The Lebesgue measure of the set of all collections of observations for which P_S is not unique is zero.

Proof. The set of all collections $\mathcal{F}$ is the simplex over all possible observations (A, a). Consider two different preference relations, P_i and P_j, over X. We describe the set of collections $\mathcal{F}_{ij}$, for which the number of swaps associated with preference P_i is equal to the number of swaps associated with preference P_j, that is,

$$\sum_{(A,a)} f(A,a)\,|\{x\in A : xP_i a\}| = \sum_{(A,a)} f(A,a)\,|\{x\in A : xP_j a\}|$$

or, equivalently,

$$\sum_{(A,a)} f(A,a)(|\{x\in A : xP_i a\}| - |\{x\in A : xP_j a\}|) = 0.$$

Consider the interior of the simplex $\mathcal{F}$ and notice that, since P_i and P_j are different, there exists at least one observation such that $(|\{x\in A : xP_i a\}| - |\{x\in A : xP_j a\}|) \neq 0$. Hence, the interior of $\mathcal{F}_{ij}$ is defined as the intersection of a hyperplane with the interior of the simplex $\mathcal{F}$, and consequently, $\mathcal{F}_{ij}$ has volume zero. Since there is a finite number of preferences, the set $\cup_i \cup_j \mathcal{F}_{ij}$ also measures zero. Finally, notice that the set of all collections for which P_S is not unique is contained in $\cup_i \cup_j \mathcal{F}_{ij}$ and, hence, also measures zero. QED

Proposition 7 considers all possible collections of observations, and one may wonder whether the result rests on the domain assumptions. To illustrate, consider the simple case in which we have a finite number of data points, one for each menu of alternatives. Then, obviously, the measure of all collections of observations for which P_S is not unique is no longer zero. However, as the number of alternatives grows, this measure can also be proved to go to zero.

Appendix C

Computational Considerations

Computational considerations are common in the application of the various inconsistency indices provided by the literature. Importantly, Dean and Martin (2012) establish that the problem studied by Houtman and Maks (1985) is equivalent to a well-known problem in the computer science literature, namely, the minimum set covering problem. Smeulders et al. (2012) relate Varian's and Houtman-Maks's indices to the independent set problem. Thus, one can draw from a wide range of algorithms developed by the operations research literature to solve these potential problems in the computation of the desired index.

Exactly the same strategy can be adopted for the swaps index. Consider another well-known problem in the computer science literature: the linear ordering problem (LOP). The LOP has been related to a variety of problems, including some of an economic nature, a particular example being the triangularization of input-output matrices for examining the hierarchical structures of the productive sectors in an economy (see Korte and Oberhofer 1970; Fukui 1986). Formally, the LOP problem over the set of vertices Y, and directed weighted edges connecting all vertices x and y in Y with cost c_{xy}, involves finding the linear order over the set of vertices Y that minimizes the total aggregated cost. That is, if we denote by Π the set of all mappings from Y to $\{1, 2, \ldots, |Y|\}$, the LOP involves solving $\arg\min_{\pi\in\Pi}\sum_{\pi(x)<\pi(y)} c_{xy}$. As the following result shows, the LOP and the problem of computing the swaps preference are equivalent.

PROPOSITION 8.

1. For every $f \in \mathcal{F}$, one can define a LOP with vertices in X, the solution of which provides the swaps preference.
2. For every LOP with vertices in X, one can define an $f \in \mathcal{F}$ in which the swaps preference provides the solution to the LOP.

Proof. For the first part, consider the collection f and define, for every pair of alternatives x and y in X, the weight $c_{xy} = \sum_{(A,y):x\in A} f(A, y)$. It follows that

$$\begin{aligned}\sum_{\pi(x)<\pi(y)} c_{xy} &= \sum_{\pi(x)<\pi(y)}\sum_{(A,y):x\in A} f(A, y)\\ &= \sum_{(A,y)} f(A, y)\ |\ \{x\in A : \pi(x) < \pi(y)\}\ |,\end{aligned}$$

and hence, by solving the LOP, we obtain the swaps preference. To see the second part, consider the LOP given by weights c_{xy}, with $x, y \in X$. Let c be the sum of all weights c_{xy}. Define the collection f such that $f(\{x, y\}, y) = c_{xy}/c$ and 0 otherwise. Since f is defined only over binary problems,

$$\begin{aligned}\sum_{(A,a)} f(A, a)\ |\ \{x\in A : \pi(x) < \pi(a)\}\ | &= \sum_{(\{x,y\},y):\pi(x)<\pi(y)} f(\{x, y\}, y)\\ &= \sum_{\pi(x)<\pi(y)} c_{xy},\end{aligned}$$

as desired. QED

Proposition 8 enables the techniques offered by the literature for the solution of the LOP to be used directly in the computation of the swaps preference. These techniques involve an ample array of algorithms for finding the globally optimal solution.[30] Alternatively, the literature also offers methods, which, while not com-

[30] See, e.g., Grötschel, Jünger, and Reinelt (1984); see also Chaovalitwongse et al. (2011) for a good introduction to the LOP, a review of the relevant algorithmic literature, and the analysis of one such algorithm.

puting the globally optimal solution, are much lighter in computational intensity and provide good approximations.[31]

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Abstract: We develop a nonparametric procedure, called the *lattice method*, for testing the consistency of contingent consumption data with a broad class of models of choice under risk and under uncertainty. Our method allows for risk loving and elation seeking behavior and can be used to calculate, via Afriat's efficiency index, the magnitude of violations from a particular model of choice. We evaluate the performance of different models (including expected utility, disappointment aversion, rank dependent utility, and stochastically monotone utility) in the data collected by Choi, Fisman, Gale, and Kariv (2007), in terms of pass rates, power, and predictive success.

Keywords: expected utility, rank dependent utility, disappointment aversion, generalized axiom of revealed preference, first order stochastic dominance, Afriat efficiency, Bronars power, Selten predictive success

JEL classification numbers: C14, C60, D11, D12, D81

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1. Introduction

This paper is a methodological contribution to the empirical investigation of decision making under risk and under uncertainty. At least since Allais (1953), there has been a large literature developing models of choice under risk or under uncertainty that seek to give a better account of observed behavior than the expected utility (EU) model. An empirical literature that tests the EU and other models on experimental data has also emerged alongside these theoretical developments. These experiments often employ elicitation procedures in which subjects are in effect making repeated choices between two risky or uncertain outcomes; the data obtained in this way consist of a finite number of *binary choices*, which can then be used to partially recover a subject's preference. A more recent strand of experiments employs a different elicitation procedure, which we shall call the *budgetary choice* procedure. In this case subjects are asked to choose a preferred option from a potentially infinite set of alternatives. For example, a subject could be presented with a portfolio problem where she has to allocate her budget between two assets with state-contingent payoffs. An early and influential experiment of this type, the data from which we analyze in this paper, is Choi, Fisman, Gale, and Kariv (2007).[1] Other examples include Loomes (1991), Gneezy and Potters (1997), Bayer *et al.* (2013), Choi *et al.* (2014), Ahn *et al.* (2014), Hey and Pace (2014), Cappelen *et al.* (2015), and Halevy, Persitz, and Zrill (2016).

The main contribution of this paper is to develop an *empirical test* that could be used to analyze data collected from portfolio decisions; it is applicable to experimental data where a budgetary choice elicitation procedure is employed and also to suitable non-experimental or field data. Our test allows us to determine whether a data set is consistent with the EU model or some of its generalizations, without making parametric assumptions on the Bernoulli index or other features of the model. It is worth noting that models of decision making over time (such as the discounted utility model), as well as models of decision making involving both time and risk, are formally very similar to the EU model and its generalizations. Budgetary choice procedures are increasingly used in experiments for studying these types of models,[2] and budgetary choices involving time preferences may occur naturally in the field as well; it

[1] See this paper also for an account of the advantages of such an approach.

[2] See Andreoni and Sprenger (2012) and, for a comprehensive list of papers employing such procedures to study time preferences, Imai and Camerer (2016).

is clear that the tests developed in this paper could be used with little or no modification in these contexts.

1.1 Testing EU and other models on a finite lattice

A feature of the budgetary choice procedure, and a reason why it is sometimes favored over binary choice procedures, is that instead of requiring the subject to pick one alternative or another, it allows her to calibrate her response and pick something 'in between.' But this feature is also the crucial reason why a new empirical tool is needed to analyze nonparametrically data collected from a budgetary procedure, whereas no such method is necessary for binary choices. Indeed, suppose we make a finite number of observations, where at observation t an agent chooses a lottery that gives a monetary payoff x_s^t in state s over one that gives y_s^t in state s (for $s = 1, 2, \ldots, \bar{s}$), where the probability of state s is known to be $\pi_s > 0$. Imagine that we would like to test if this data set is consistent with the EU model. Ignoring the issue of errors for the time being, checking for consistency with the EU model simply involves finding a strictly increasing Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}$ such that $\sum_{s=1}^{\bar{s}} \pi_s u(x_s^t) \geqslant \sum_{s=1}^{\bar{s}} \pi_s u(y_s^t)$ holds at every observation t. This amounts to solving a finite set of linear inequalities, and it is computationally straightforward to ascertain if a solution exists. However, it is clear that this method is no longer applicable when choices are instead made from a classical budget sets at every observation t, since even a single observed choice from a budget set reveals an infinite set of binary preferences.

The empirical test that we develop for solving this problem is very simple, and it is worth giving a short explanation here. Consider a data set with three observations and two states, as depicted in Figure 1. The subject chooses the contingent consumption bundle $(2, 5)$ from budget set B^1, $(6, 1)$ from B^2, and $(4, 3)$ from B^3. Assuming that the probability of state s is commonly known to be π_s, consistency with the EU model would require the existence of a strictly increasing Bernoulli function u such that $\pi_1 u(2) + \pi_2 u(5) \geqslant \pi_1 u(x) + \pi_2 u(y)$ for all (x, y) in B^1, and similarly at the other two observations.

In our main methodological result (Theorem 1), we show that this data set can be rationalized by the EU model if it can be rationalized on an appropriately modified consumption set. Specifically, let $\mathcal{X}$ be the set of consumption levels that are observed to have been chosen at some observation and in some state, plus zero; in this example $\mathcal{X} = \{0, 1, 2, 3, 4, 5, 6\}$.

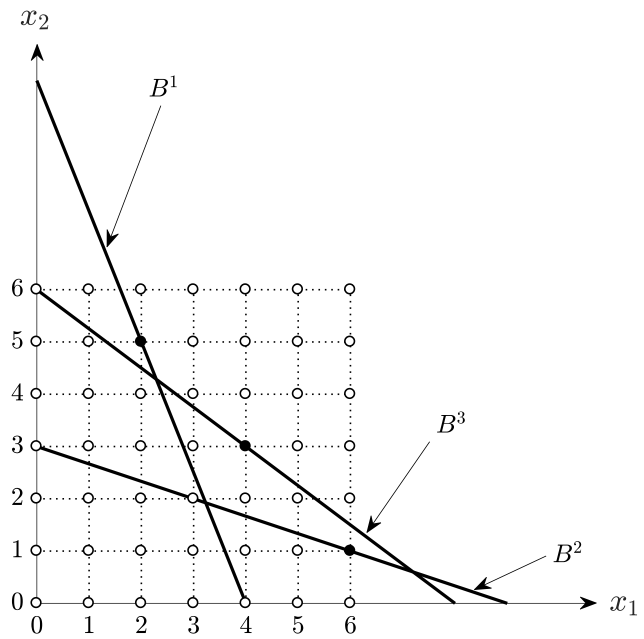


Figure 1: Constructing the finite lattice

Then for the data set to be EU-rationalizable, it is *sufficient* (and obviously necessary) for it to be EU-rationalizable on the reduced consumption set $\mathcal{X}^2$, i.e., there is an increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}$ such that the expected utility of $(2, 5)$ is greater than any other bundle in $B^1 \cap \mathcal{X}^2$, and so forth. The set $\mathcal{X}^2$ is a finite lattice, depicted by the open circles in Figure 1. Therefore, checking for EU-rationalizability involves checking if there is a solution to a *finite* set of linear inequalities, a problem which is computationally feasible.[3]

This lattice method turns out to be very flexible: it can be used not just to check for EU-rationalizability, but also for consistency with other models of choice under risk (such as the rank dependent utility (RDU) model (Quiggin, 1982)) and under uncertainty (such as the maxmin expected utility model (Gilboa and Schmeidler, 1989)). The basic idea is always to convert an infinite collection of revealed preference pairs into a finite number involving only bundles on a finite lattice. Note also that the method *does not require linear budget sets*: it works for any type of constraint set, so long as it is compact.

1.2 Empirical implementation and findings

We implement our empirical method on a data set obtained from the well known portfolio

[3] This example is EU-rationalizable on $\mathcal{X}^2$ and thus EU-rationalizable. One solution is $\bar{u}(0) = 0$, $\bar{u}(1) = 1$, $\bar{u}(2) = 4$, $\bar{u}(3) = 6$, $\bar{u}(4) = 7$, $\bar{u}(5) = 8$, and $\bar{u}(6) = 9$.

choice experiment in Choi, Fisman, Gale, and Kariv (2007), in order to demonstrate that the lattice method works at a practical level, and also for its own sake. In this experiment, each subject was asked to purchase Arrow-Debreu securities under different budget constraints. There were two states of the world, and it was commonly known that states occurred either symmetrically (each with probability 1/2) or asymmetrically (one with probability 1/3 and the other with probability 2/3). In their analysis, Choi *et al.* (2007) first checked whether a subject's observations were consistent with the maximization of a locally nonsatiated utility function by performing the familiar GARP test (prescribed by Afriat's (1967) Theorem). Those subjects who passed or came sufficiently close to passing GARP were then fitted to a parametric version of the disappointment aversion (DA) model (Gul, 1991), which is a special case of the RDU model when there are two states of the world.

The lattice method developed in this paper makes it possible for us to evaluate other models of choice under risk (beyond basic utility maximization) using a completely nonparametric approach. We test whether a subject's choices are consistent with the EU, DA, and RDU models by applying the lattice method. We also test for consistency with the maximization of a utility function that is *stochastically monotone*, in the sense that if a bundle dominates another with respect to first order stochastic dominance, then it must have higher utility. The EU, DA, and RDU models are all special cases of the SMU model, which is in turn more stringent than basic utility maximization.

With 50 observations collected on each subject, it is unsurprising that hardly any subject would be exactly rationalizable by even the the most permissive model of utility maximization. It is possible to quantify a data set's departure from a particular notion of rationality using the *critical cost efficiency index* (Afriat (1972, 1973)); this index is widely used in the empirical revealed preference literature, including Choi *et al.* (2007).[4] This index runs from 1 to 0, with the index equal to 1 if the data set passes the test exactly. We adopt the same measure of rationality in this paper. Given the data obtained from a *particular subject*, and for each of the models that we consider, it is possible to calculate this index; in the case of the EU, DA, and RDU models, this calculation again relies upon the lattice method.

[4] For a recent paper that implements a variant of this index and discusses its merits, see Halevy, Persitz, and Zrill (2016).

In comparing the performance of different models, it is necessary to go beyond comparing (approximate) pass rates since a very permissive model will have a very high pass rate but also little restrictive and predictive power. A standard way of measuring power in the empirical revealed preference literature is to estimate the probability of a randomly generated data set failing the test for a given model (Bronars, 1987); a model has high power if this probability is high. When one is investigating nested models, it is also natural to examine *relative power*: for example, if we randomly select a data set which passes GARP, what is the probability that it is also consistent with the EU model? Even though the power of different models of choice under risk has been investigated in other contexts, we provide the first systematic investigation of this issue in the context of budgetary choice data.

So models can be compared in at least two dimensions: their pass rates and their power. One way of combining these into a single index is to evaluate the difference between the pass rate and the model's *precision*, which is the probability of a random data set passing the test for that model (in other words, 1 minus the power). Selten (1991) provides an axiomatization of this index and calls it the *index of predictive success*. We evaluate the performance of different models according to this index.

The following is a brief summary of our empirical findings:[5]

- At a cost efficiency threshold of 0.9, more than 80% of subjects pass GARP and are therefore consistent with the maximization of a locally nonsatiated utility function.
- Among this group of subjects more than half are rationalizable by the EU model.
- The SMU and RDU models explain a sizable proportion of subjects whose behavior is not captured by the EU model. This is not true of the DA model, even though it is in principle a more permissive model than EU.
- If we randomly generate a data set which passes GARP, the probability of it being consistent with the RDU model, and hence the more stringent DA and EU models, is effectively zero. In other words, the power of these models is close to perfect, even among subjects who are consistent with utility maximization.
- After conditioning on passing GARP, the SMU and RDU models have the highest

[5] These findings are broadly consistent with results from earlier studies (see Section 4 for details).

indices of predictive success. These models perform well because they capture significantly more of the population than the EU model, without sacrificing power.

1.3 Relationship with the revealed preference literature

Our paper is related to the revealed preference literature originating from Afriat's (1967) Theorem, which characterizes consumer demand observations that are consistent with the maximization of a locally nonsatiated utility function (see also Diewert (1973) and Varian (1982)). Afriat's Theorem has *theoretical* significance in the sense that its intuitive behavioral characterization of basic rationalizability (through GARP) provides a justification for utility maximization in the consumer demand context, but it also provides a viable *empirical method* for testing rationalizability, which is why it has given rise to a large empirical literature.

A natural follow up to Afriat's contribution is to characterize those data sets which are rationalizable by more specialized utility functions. Among these papers are those which characterize observations of contingent consumption demand that are consistent with the EU model and (in more recent papers) some of its generalizations; these include Varian (1983a, 1983b, 1988), Green and Srivastava (1986), Diewert (2012), Bayer *et al.* (2013), Echenique and Saito (2015), Chambers, Liu, and Martinez (2016), and Chambers, Echenique, and Saito (2016).[6] The principal difference between our results and this literature is that we do not rely on the methods of convex optimization; this means, in particular, that we do not require (or guarantee) the concavity of the Bernoulli function, and our results are applicable to data sets with general constraint sets rather than just linear budget sets. For reasons which we make clear later in the paper, the fact that we allow for nonlinear constraint sets means that our method can also be used to calculate Afriat's efficiency index.

It is worth mentioning that not all revealed preference results (involving budgetary observations) that flow from Afriat's Theorem have the feature of providing both theoretical insight and an empirical method. There are papers where the emphasis is on providing a characterization that offers theoretical insight; in other cases the emphasis is on providing an empirically viable method of model testing.[7] Our main methodological result (Theorem

[6] There is also a closely related literature on recovering expected utility from asset or contingent consumption demand *functions*, where, in effect the data set is assumed to be infinite (see, for example, Dybvig and Polemarchakis (1981) and Kubler, Selden, and Wei (2014)).

[7] For example, Chambers, Liu, and Martinez (2016) provides a characterization of the former type, and

1) says that, for a broad class of models, to check for rationalizability it suffices to check for rationalizabilty *as if* the subject's consumption space is some finite lattice (constructed from the data). By itself, the result does not furnish us with any theoretical motivation for one model or another; its principal value is in providing us with an empirical tool.

1.4 Organization of the paper

Section 2 provides a description of the lattice method and explains how it can be used to test the EU, DA, and RDU models in a budgetary choice environment. Further applications of the lattice method, including to models of decision making under uncertainty, can be found in the Online Appendix. In Section 3 we explain Afriat's efficiency index and how the lattice method can be used to calculate this index. The empirical application to the portfolio choice data collected by Choi, Fisman, Gale, and Kariv (2007) can be found in Section 4.

2. The lattice method

We assume that there is a finite set of states, denoted by $S = \{1, 2, \ldots, \bar{s}\}$. The contingent consumption space is $\mathbb{R}^{\bar{s}}_+$; for a typical consumption bundle $\mathbf{x} \in \mathbb{R}^{\bar{s}}_+$, the sth entry, x_s, specifies the consumption level in state s.[8] There are T observations in the data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$; by this we mean that the agent is observed choosing the bundle $\mathbf{x}^t$ from $B^t \subset \mathbb{R}^{\bar{s}}_+$. We assume that B^t is compact and that $\mathbf{x}^t \in \partial B^t$, where ∂B^t denotes the *upper boundary* of B^t.[9] The most important example of B^t is the classical linear budget set under complete markets, i.e.,

$$B^t = \{\mathbf{x} \in \mathbb{R}^{\bar{s}}_+ : \mathbf{p}^t \cdot \mathbf{x} \leqslant \mathbf{p}^t \cdot \mathbf{x}^t\}, \tag{1}$$

with $\mathbf{p}^t \gg 0$ denoting the vector of state prices. In this case, we may also write the data set as $\mathcal{O} = \{(\mathbf{x}^t, \mathbf{p}^t)\}_{t=1}^T$. The experiment conducted by Choi *et al.* (2007), the data from which we analyze in Section 4, involves subjects choosing from linear budget sets.

they point out this distinction clearly in the introduction to their paper.

[8] Our results do depend on the realization in each state being one-dimensional (which can be interpreted as a monetary payoff, but not a bundle of goods). This case is the one most often considered in applications and experiments and is also the assumption in a number of recent papers, including Kubler, Selden, and Wei (2014), Echenique and Saito (2015), and Chambers, Echenique, and Saito (2016). The papers by Varian (1983a, 1983b), Green and Srivastava (1986), Bayer *et al.* (2013), and Chambers, Liu, and Martinez (2016) allow for multi-dimensional realizations but (like the three aforementioned papers) they also require the convexity of the agent's preference over contingent consumption and linear budget sets.

[9] An element $\mathbf{y} \in B^t$ is in ∂B^t if there is no $\mathbf{x} \in B^t$ such that $\mathbf{x} > \mathbf{y}$. (For the vectors $\mathbf{x}, \mathbf{y} \in \mathbb{R}^{\bar{s}}$, we write $\mathbf{x} \geqslant \mathbf{y}$ if $x_s \geqslant y_s$ for all s, and $\mathbf{x} > \mathbf{y}$ if $\mathbf{x} \geqslant \mathbf{y}$ and $\mathbf{x} \neq \mathbf{y}$. If $x_s > y_s$ for all s, we write $\mathbf{x} \gg \mathbf{y}$.) For example, if $B^t = \{(x, y) \in \mathbb{R}^2_+ : (x, y) \leqslant (1, 1)\}$, then $(1, 1) \in \partial B^t$ but $(1, 1/2) \notin \partial B^t$.

Bear in mind, however, that our formulation only requires B^t to be compact and, in particular, it does not have to be a linear budget set. A crucial application requiring B^t to be nonlinear is found in Section 3, where we define approximate rationalizability. Another natural example of a nonlinear budget set is when a subject chooses contingent consumption through a portfolio of securities in an incomplete market; in this case, the budget set will be compact so long as the security prices do not admit arbitrage.[10]

Let $\{\phi(\cdot, t)\}_{t=1}^T$ be a collection of functions, where $\phi(\cdot, t) : \mathbb{R}_+^{\bar{s}} \to \mathbb{R}$ is continuous and strictly increasing.[11] The data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is said to be *rationalizable by* $\{\phi(\cdot, t)\}_{t=1}^T$ if there exists a continuous and strictly increasing function $u : \mathbb{R}_+ \to \mathbb{R}_+$, which we shall refer to as the *Bernoulli function*, such that

$$\phi(\mathbf{u}(\mathbf{x}^t), t) \geqslant \phi(\mathbf{u}(\mathbf{x}), t) \text{ for all } \mathbf{x} \in B^t, \tag{2}$$

where $\mathbf{u}(\mathbf{x}) = (u(x_1), u(x_2), \ldots, u(x_{\bar{s}}))$. In other words, $\mathbf{x}^t$ maximizes $\phi(\mathbf{u}(\mathbf{x}), t)$ in B^t. It is natural to require u to be strictly increasing since we typically interpret its argument to be money. The requirements on u guarantee that $\phi(\mathbf{u}(\cdot), t)$ is continuous and strictly increasing in $\mathbf{x}$. Note that continuity is an important property because it guarantees that the agent's utility maximization problem always has a solution on a compact constraint set.[12]

Expected utility. This model clearly falls within the framework we have set up. Indeed, suppose that both the observer and the agent know that the probability of state s at observation t is $\pi_s^t > 0$. If the agent is maximizing expected utility (EU),

$$\phi(u_1, u_2, \ldots, u_{\bar{s}}, t) = \sum_{s=1}^{\bar{s}} \pi_s^t u_s, \tag{3}$$

and (2) requires that

$$\sum_{s=1}^{\bar{s}} \pi_s^t u(x_s^t) \geqslant \sum_{s=1}^{\bar{s}} \pi_s^t u(x_s) \text{ for all } \mathbf{x} \in B^t, \tag{4}$$

[10] Indeed, there is $\mathbf{p}^t \gg 0$ such that $B^t = \{\mathbf{x} \in \mathbb{R}_+^{\bar{s}} : \mathbf{p}^t \cdot \mathbf{x} \leqslant \mathbf{p}^t \cdot \mathbf{x}^t\} \cap \{Z + \boldsymbol{\omega}\}$, where Z is the span of assets available to the agent and $\boldsymbol{\omega}$ is the agent's endowment of contingent consumption. Both B^t and $\mathbf{x}^t$ will be known to the observer, if he knows the asset prices, the agent's holding of securities, the asset payoffs in every state, and the agent's endowment of contingent consumption $\boldsymbol{\omega}$.

[11] By strictly increasing, we mean that $\phi(\mathbf{z}, t) > \phi(\mathbf{z}', t)$ if $\mathbf{z} > \mathbf{z}'$.

[12] The existence of a solution is obviously important if we are to make out-of-sample predictions. More fundamentally, a hypothesis that an agent is choosing a utility-maximizing bundle implicitly assumes that the utility function is such that an optimum exists for a reasonably broad class of constraint sets.

i.e., the expected utility of $\mathbf{x}^t$ is greater than that of any other bundle in B^t. When there exists a Bernoulli function u such that (4) holds, we say that the data set is *EU-rationalizable with the probability weights* $\{\boldsymbol{\pi}^t\}_{t=1}^T$, where $\boldsymbol{\pi}^t = (\pi_1^t, \pi_2^t, \ldots, \pi_{\bar{s}}^t)$.

If $\mathcal{O}$ is rationalizable by $\{\phi(\cdot, t)\}_{t=1}^T$, then since the objective function $\phi(\mathbf{u}(\cdot), t)$ is strictly increasing in $\mathbf{x}$, the rationalizability condition (2) could be strengthened to

$$\phi(\mathbf{u}(\mathbf{x}^t), t) \geqslant \phi(\mathbf{u}(\mathbf{x}), t) \text{ for all } \mathbf{x} \in \underline{B}^t, \tag{5}$$

where $\underline{B}^t$ is the *downward extension of* B^t, i.e.,

$$\underline{B}^t = \{\mathbf{y} \in \mathbb{R}_+^{\bar{s}} : \mathbf{y} \leqslant \mathbf{x} \text{ for some } \mathbf{x} \in B^t\}.$$

Furthermore, the inequality in (5) is strict whenever $\mathbf{x} \in \underline{B}^t \backslash \partial \underline{B}^t$ (where $\partial \underline{B}^t$ refers to the upper boundary of $\underline{B}^t$). We define $\mathcal{X} = \{x' \in \mathbb{R}_+ : x' = x_s^t \text{ for some } t,\, s\} \cup \{0\}$; besides zero, $\mathcal{X}$ contains those levels of consumption that are chosen at some observation and in some state. Since the data set is finite, so is $\mathcal{X}$. Given $\mathcal{X}$, we may construct $\mathcal{L} = \mathcal{X}^{\bar{s}}$, which consists of a finite grid of points in $\mathbb{R}_+^{\bar{s}}$; in formal terms, $\mathcal{L}$ is a finite lattice. Let $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ be the restriction of the Bernoulli function u to $\mathcal{X}$. Given $\mathcal{O}$, the following must hold:

$$\phi(\bar{\mathbf{u}}(\mathbf{x}^t), t) \geqslant \phi(\bar{\mathbf{u}}(\mathbf{x}), t) \text{ for all } \mathbf{x} \in \underline{B}^t \cap \mathcal{L} \text{ and} \tag{6}$$

$$\phi(\bar{\mathbf{u}}(\mathbf{x}^t), t) > \phi(\bar{\mathbf{u}}(\mathbf{x}), t) \text{ for all } \mathbf{x} \in \left(\underline{B}^t \backslash \partial \underline{B}^t\right) \cap \mathcal{L}, \tag{7}$$

where $\bar{\mathbf{u}}(\mathbf{x}) = (\bar{u}(x_1), \bar{u}(x_2), \ldots, \bar{u}(x_{\bar{s}}))$. Our main theorem says the converse is also true.[13]

Theorem 1. *Suppose that for some data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ and collection of continuous and strictly increasing functions $\{\phi(\cdot, t)\}_{t=1}^T$, there is a strictly increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ that satisfies conditions (6) and (7). Then there is a Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ that extends $\bar{u}$ and guarantees the rationalizability of $\mathcal{O}$ by $\{\phi(\cdot, t)\}_{t=1}^T$.*[14]

[13] Note that $\underline{B}^t$ cannot be replaced with B^t in (6) and (7). For example, suppose there are two observations, where $\mathbf{x}^1 = (1, 0)$ is chosen from $B^1 = \{(x_1, x_2) \in \mathbb{R}_+^2 : 2x_1 + x_2 = 2\}$ and $\mathbf{x}^2 = (0, 1)$ is chosen from $B^2 = \{(x_1, x_2) \in \mathbb{R}_+^2 : x_1 + 2x_2 = 2\}$. This pair of observations cannot be rationalized by any increasing utility function (even though the 'budget sets' are just lines) and, in particular, cannot be rationalized in the sense of Theorem 1 (with ϕ constant across t). However, since $\mathcal{L} = \{(0, 0), (0, 1), (1, 0), (1, 1)\}$, $B^1 \cap \mathcal{L} = \{(1, 0)\}$ and $B^2 \cap \mathcal{L} = \{(0, 1)\}$, so conditions (6) and (7) are vacuous. On the other hand $\left(\underline{B}^1 \backslash \partial \underline{B}^1\right) \cap \mathcal{L}$ contains $(0, 1)$ and $\left(\underline{B}^2 \backslash \partial \underline{B}^2\right) \cap \mathcal{L}$ contains $(1, 0)\}$, so (7) requires $\phi(\bar{\mathbf{u}}(\mathbf{x}^1)) > \phi(\bar{\mathbf{u}}(\mathbf{x}^2))$ and $\phi(\bar{\mathbf{u}}(\mathbf{x}^1)) < \phi(\bar{\mathbf{u}}(\mathbf{x}^2))$, which plainly cannot happen. This allows us to conclude, correctly, that the data set is not rationalizable.

[14] The increasing assumptions on ϕ and $\bar{u}$ ensure that we may confine ourselves to checking (6) and (7) for undominated elements of $\underline{B}^t \cap \mathcal{L}$, i.e., $\mathbf{x} \in \underline{B}^t \cap \mathcal{L}$ such that there does not exist $\mathbf{x}' \in \underline{B}^t \cap \mathcal{L}$ with $\mathbf{x} < \mathbf{x}'$.

What Theorem 1 achieves is *domain reduction*: checking the rationalizability of $\mathcal{O}$ is equivalent to checking rationalizability in the case where the agent's consumption space is considered to be $\mathcal{L}$ rather than $\mathbb{R}^{\bar{s}}_{+}$, which (crucially) reduces the rationality requirements to a *finite* number of inequalities, each involving the observed choice and an alternative (see (6) and (7)), and with the Bernoulli function defined on $\mathcal{X}$ rather than $\mathbb{R}_+$.

The intuition for Theorem 1 ought to be strong. Given $\bar{u}$ satisfying (6) and (7), we can define the step function $\hat{u} : \mathbb{R}_+ \to \mathbb{R}_+$ where $\hat{u}(r) = \bar{u}([r])$, with $[r]$ being the largest element of $\mathcal{X}$ weakly lower than r, i.e., $[r] = \max\{r' \in \mathcal{X} : r' \leqslant r\}$. Notice that $\phi(\hat{\mathbf{u}}(\mathbf{x}^t), t) = \phi(\bar{\mathbf{u}}(\mathbf{x}^t), t)$ and, for any $\mathbf{x} \in \underline{B}^t$, $\phi(\hat{\mathbf{u}}(\mathbf{x}), t) = \phi(\bar{\mathbf{u}}([\mathbf{x}]), t)$, where $[\mathbf{x}] = ([x_1], [x_2], \ldots, [x_{\bar{s}}])$ in $\underline{B}^t \cap \mathcal{L}$. Clearly, if $\bar{u}$ obeys (6) and (7) then $\mathcal{O}$ is rationalized by $\{\phi(\cdot, t)\}_{t=1}^T$ and $\hat{u}$ (in the sense that (2) holds). This falls short of the claim in the theorem only because $\hat{u}$ is neither continuous nor strictly increasing; the proof in the Appendix shows how one could in fact construct a Bernoulli function with these additional properties.

2.1 Testing the expected utility model

Theorem 1 provides us with a very convenient way of testing EU-rationalizability. The theorem tell us that $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is EU-rationalizable with the probability weights $\{\boldsymbol{\pi}^t\}_{t=1}^T$ if and only if there is a collection of real numbers $\{\bar{u}(r)\}_{r \in \mathcal{X}}$ such that

$$0 \leqslant \bar{u}(r') < \bar{u}(r) \text{ whenever } r' < r, \tag{8}$$

and the inequalities (6) and (7) hold, where $\phi(\cdot, t)$ is defined by (3). This is a linear program and it is both formally *solvable* (in the sense that there is an algorithm that can decide within a known number of steps whether or not there is a solution to this set of linear inequalities) and also computationally feasible.

At this point it is worth emphasizing that requiring a data set to be EU-rationalizable is certainly more stringent than simply requiring it to be rationalizable by a locally nonsatiated utility function. Indeed, while a data set with a single observation $(\mathbf{x}^t, \mathbf{p}^t)$ must be consistent with the maximization of a strictly increasing (and hence locally nonsatiated) utility function, even a single observation can be incompatible with the EU model.

Example 1. Suppose that there are two equiprobable states of the world, and at the price vector $\mathbf{p}^t = (p_1^t, p_2^t)$ such that $p_1^t > p_2^t$, the agent purchases a bundle $\mathbf{x}^t$ such that

$x_1^t > x_2^t$. We claim that this is not EU-rationalizable, or in other words, the agent cannot buy strictly more of the more expensive good. Indeed, such an observation is not compatible with the maximization of any symmetric and strictly increasing utility function on $\mathbb{R}^2_+$: with symmetry, the bundle (y_1, y_2), where $y_1 = x_2^t$ and $y_2 = x_1^t$, is strictly cheaper than $\mathbf{x}^t$ but gives the same utility, so $\mathbf{x}^t$ is not optimal. Such an observation will also fail the lattice test, since (y_1, y_2) is in $\mathcal{L} \cap (\underline{B}^t \backslash \partial \underline{B}^t)$ but the condition (7) is not satisfied. $\square$

On the other hand, our test is strictly less stringent than a test of EU-rationalizability that also requires the Bernoulli function to be concave (such as Green and Srivastava (1986)); imposing concavity on the Bernoulli function has observable implications over and above those which flow simply from the EU model, as the following example demonstrates.

Example 2. Suppose an agent maximizes expected utility and has the Bernoulli function $u(y) = (y-4)^3$, which is strictly concave for $y < 4$ and strictly convex otherwise.[15] There are two states of the world, which occur with equal probability. At $\mathbf{p}^t = (1, 3/2)$ and with wealth 1, the agent chooses $x_1 \in [0,1]$ to maximize $f(x_1) = (x_1 - 4)^3 + [2(1-x_1)/3 - 4]^3$. Over this range, the Bernoulli function is strictly concave and so is f; one could check that $f'(1) < 0$ so that there is unique interior solution which we denote $\mathbf{x}^t$ (see Figure 2).[16] At the prices $\mathbf{p}^{t'} = (1,1)$ with wealth equal to 64, the agent chooses $x_1 \in [0, 64]$ to maximize $g(x_1) = (x_1 - 4)^3 + (60 - x_1)^3$. It is straightforward to check that g is strictly convex on $[0, 64]$ and it is thus maximized at the two end points $(0, 64)$ and $(64, 0)$.

Now consider a data set consisting of two observations: the bundle $\mathbf{x}^t$ chosen at $\mathbf{p}^t = (1, 3/2)$ and $\mathbf{x}^{t'} = (64, 0)$ chosen at $\mathbf{p}^{t'} = (1,1)$. This data set is EU-rationalizable and it will pass the lattice test, but it cannot be rationalized by a concave Bernoulli function. Indeed,

$$u(x_1^t) + u(x_2^t) \geqslant u(1) + u(0) \tag{9}$$

since $(1,0)$ is affordable to the agent when $\mathbf{x}^t$ is chosen. If we further assume that u is concave, $u(1) - u(x_1^t) \geqslant u(64) - u(x_1^t + 63)$; substituting this into (9), we obtain $u(x_1^t + 63) + u(x_2^t) \geqslant u(64) + u(0)$. The bundle $((x_1^t + 63), x_2^t)$ is strictly cheaper than $\mathbf{x}^{t'} = (64, 0)$ at $\mathbf{p}^{t'}$ (see Figure 2), so $\mathbf{x}^{t'}$ cannot be optimal. To conclude, while $\mathcal{O} = \{(\mathbf{p}^t, \mathbf{x}^t), (\mathbf{p}^{t'}, \mathbf{x}^{t'})\}$ is indeed

[15] A Bernoulli function with a concave region followed by a convex region is used by Friedman and Savage (1948, Figure 2) to explain why an agent can simultaneously buy insurance and accept risky gambles.

[16] Solving the (quadratic) first order condition gives $\mathbf{x}^t \approx (0.83, 0.11)$.

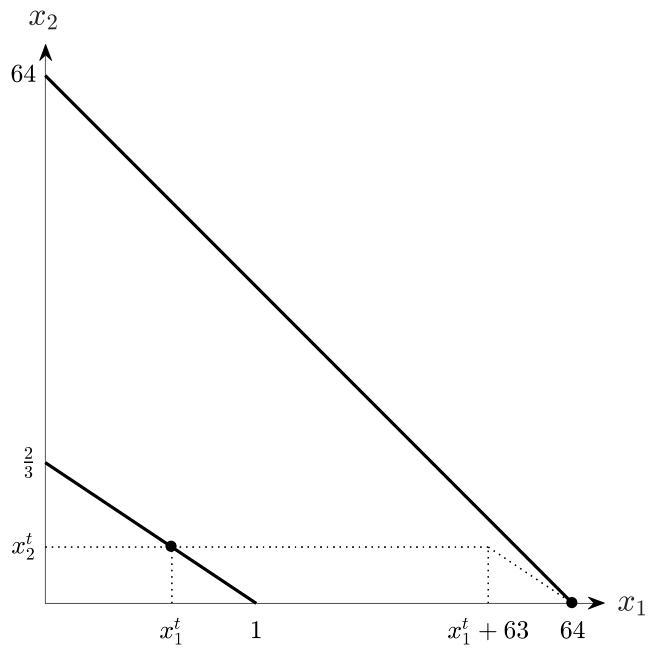


Figure 2: EU-rationalizable but not concave EU-rationalizable data set

EU-rationalizable, it is not EU-rationalizable with a concave Bernoulli function. $\square$

We have shown that a data set is EU-rationalizable if and only if it is EU-rationalizable on $\mathcal{L}$ and the latter is in turn equivalent to the existence of a function $\bar{u}$ obeying conditions (6), (7), and (8) (with $\phi(\mathbf{u}, t) = \sum_{s=1}^{\bar{s}} \pi_s^t u_s$). Conditions (6) and (7) generate a finite list of preference pairs between some chosen bundle $\mathbf{x}^t$ and another bundle $\mathbf{x}$ in $\underline{B}^t \cap \mathcal{L}$ or $\left(\underline{B}^t \backslash \partial \underline{B}^t\right) \cap \mathcal{L}$. Condition (8) can also be reformulated as saying that the bundle $(r, r, \ldots, r)$ is strictly preferred to $(r', r', \ldots, r')$ whenever $r > r'$, for r, $r' \in \mathcal{X}$. We gather these together in a list $\{(\mathbf{a}^j, \mathbf{b}^j)\}_{j=1}^M$, where for all $j \leqslant N$ (with $N < M$), the bundle $\mathbf{a}^j$ is weakly preferred to $\mathbf{b}^j$ (so the pairs are drawn from (6)) and for $j > N$, $\mathbf{a}^j$ is strictly preferred to $\mathbf{b}^j$ (so the pairs are drawn from (7) and (8)). Each bundle $\mathbf{a}^j$ can be written in its lottery form $\hat{\mathbf{a}}^j$, where $\hat{\mathbf{a}}^j$ is the vector with $|\mathcal{X}|$ entries, with the ith entry giving the probability of ith ranked number in $\mathcal{X}$; similarly, $\mathbf{b}^j$ can be written in its lottery form $\hat{\mathbf{b}}^j$. For example, in the example given in the introduction, $\mathcal{X} = \{0, 1, 2, 3, 4, 5, 6\}$ and the two states are equiprobable, so the bundle $(2, 5)$ chosen from B^1 has the lottery form $(0, 0, 1/2, 0, 0, 1/2, 0)$.

We know from Fishburn (1975) that the list $\{(\mathbf{a}^j, \mathbf{b}^j)\}_{j=1}^M$ is rationalizable by EU (i.e., there is $\bar{u}$ that solves (6), (7), and (8) with $\phi(\mathbf{u}, t) = \sum_{s=1}^{\bar{s}} \pi_s^t u_s$) if and only if there does not exist λ^j with $\sum_{j=1}^{M} \lambda^j = 1$, $\lambda^j \geqslant 0$ for all j, and $\lambda^j > 0$ for some $j > N$, such that

$$\sum_{j=1}^{M} \lambda^j \hat{\mathbf{a}}^j = \sum_{j=1}^{M} \lambda^j \hat{\mathbf{b}}^j. \tag{10}$$

This condition is very intuitive: assuming that the agent has a preference over lotteries, the independence axiom says that the lottery $\sum_{j=1}^{M} \lambda^j \hat{\mathbf{a}}^j$ must be strictly preferred to $\sum_{j=1}^{M} \lambda^j \hat{\mathbf{b}}^j$, and therefore (10) is excluded.[17] Put another way, a violation of Fishburn's condition must imply a violation of the independence axiom.

To summarize, we have shown that *a data set* $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ *is EU-rationalizable with probability weights* $\{\boldsymbol{\pi}^t\}_{t=1}^T$ *if and only if it is EU-rationalizable with probability weights* $\{\boldsymbol{\pi}^t\}_{t=1}^T$ *on the domain* $\mathcal{L}$ *and this in turn holds if and only if the preference pairs on* $\mathcal{L}$ *(as revealed by the data) do not contain a contradiction of the independence axiom of the form (10).* Example 3, to be explained later in the paper, gives an example of a data set which violates Fishburn's condition on $\mathcal{L}$ and is therefore not EU-rationalizable.

2.2 Other applications of the lattice method

So far, we have considered tests of EU-rationalizability in the case where the probability of each state is known to both the agent and the observer. The testing procedure extends to the case where no objective probabilities can be attached to each state. A data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is *rationalizable by subjective expected utility* (SEU) if there exist probability weights $\boldsymbol{\pi} = (\pi_1, \pi_2, \ldots, \pi_{\bar{s}}) \gg 0$, with $\sum_{s=1}^{\bar{s}} \pi_s = 1$, and a Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ such that, for all $t = 1, 2, \ldots, T$,

$$\sum_{s=1}^{\bar{s}} \pi_s u(x_s^t) \geqslant \sum_{s=1}^{\bar{s}} \pi_s u(x_s) \text{ for all } \mathbf{x} \in B^t.$$

In this case, ϕ is independent of t and instead of being fixed, it is required to belong to the family of functions Φ_{SEU} such that $\phi \in \Phi_{SEU}$ if $\phi(\mathbf{u}) = \sum_{s=1}^{\bar{s}} \pi_s u_s$ for some $\boldsymbol{\pi} \gg 0$. By Theorem 1, the data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ can be rationalized by some $\phi \in \Phi_{SEU}$ if *there is a strictly increasing* $\bar{u}$ *such that (6) and (7) holds* and it is clear that these conditions are also

[17] To be precise, suppose that the agent has a preference over lotteries with prizes in $\mathcal{X}$. The independence axiom says that if lottery $\hat{\mathbf{a}}$ is preferred (strictly preferred) to $\hat{\mathbf{b}}$, then $\gamma\hat{\mathbf{a}} + (1-\gamma)\hat{\mathbf{c}}$ is preferred (strictly preferred) to $\gamma\hat{\mathbf{b}} + (1-\gamma)\hat{\mathbf{c}}$, where $\hat{\mathbf{c}}$ is another lottery and $\gamma \in [0, 1]$. Repeated application of this property and the transitivity of the preference will guarantee that $\sum_{j=1}^{M} \lambda^j \hat{\mathbf{a}}^j$ is strictly preferred to $\sum_{j=1}^{M} \lambda^j \hat{\mathbf{b}}^j$.

necessary. These conditions form a system of bilinear inequalities with unknowns $\{\pi_s\}_{s=1}^{\bar{s}}$ and $\{\bar{u}(r)\}_{r\in\mathcal{X}}$.

For many of the standard models of decision making under risk or uncertainty, the rationalizability problem has a structure similar to that of SEU in the sense that rationalizability by a particular model involves finding a Bernoulli function u and a function ϕ belonging to some family Φ that together rationalize the data, and this problem can in turn be transformed (via Theorem 1) into a problem of solving a system of bilinear inequalities. In the Online Appendix, we show how tests for various models, including choice acclimating personal equilibrium (Köszegi and Rabin, 2007), maxmin expected utility (Gilboa and Schmeidler, 1989), and variational preferences (Maccheroni, Marinacci, and Rustichini, 2006), can be devised using Theorem 1.

Even though solving a bilinear problem may be computationally intensive, the Tarski-Seidenberg Theorem tells us that this problem is decidable, in the sense that there is a known algorithm that can determine in a finite number of steps whether or not a solution exists. Nonlinear tests are not new to the revealed preference literature; for example, they appear in tests of weak separability (Varian, 1983a), in tests of maxmin expected utility and other models of ambiguity (Bayer *et al.*, 2013), and in tests of Walrasian general equilibrium (Brown and Matzkin, 1996). Solving such problems can be computationally demanding, but some cases can be computationally straightforward because of certain special features and/or when the number of observations is small. In the case of the tests that we develop, they simplify dramatically and are implementable in practice when there are only two states (though they remain nonlinear). The two-state case, while special, is very common in applied theoretical settings and laboratory experiments. For example, to implement the SEU test, simply condition on the probability of state 1 (and hence on the probability of state 2), and then perform a linear test to check whether there is a collection of real numbers $\{\bar{u}(r)\}_{r\in\mathcal{X}}$ solving (6), (7), and (8) (with $\phi(\cdot, t)$ is defined by (3)). If not, choose another probability for state 1, implement, and repeat (if necessary). Even a uniform grid search of up to two decimal places on the probability of state 1 will lead to no more than 99 linear tests, which can be implemented with little difficulty.[18]

[18] While we have not found it necessary to use them in our implementation in this paper, there are solvers available for mixed integer nonlinear programs (for example, as surveyed in Bussieck and Vigerske (2010))

Rank dependence and disappointment aversion. The rank dependent utility (RDU) model (Quiggin, 1982) is a prominent model of choice under risk. In Section 4, we report the findings of a test of this model, so we explain it here in greater detail. Let $\pi_s > 0$ be the objective probability of state s.[19] Given a contingent consumption bundle $\mathbf{x}$, we can rank the entries of $\mathbf{x}$ from the smallest to the largest, with ties broken by the rank of the state. We denote by $r(\mathbf{x}, s)$, the rank of x_s in $\mathbf{x}$. For example, if there are five states and $\mathbf{x} = (1, 4, 4, 3, 5)$, we have $r(\mathbf{x}, 1) = 1$, $r(\mathbf{x}, 2) = 3$, $r(\mathbf{x}, 3) = 4$, $r(\mathbf{x}, 4) = 2$, and $r(\mathbf{x}, 5) = 5$. A rank dependent expected utility function gives to the bundle $\mathbf{x}$ the utility $V(\mathbf{x}) = \sum_{s=1}^{\bar{s}} \delta(\mathbf{x}, s) u(x_s)$ where $u : \mathbb{R}_+ \to \mathbb{R}$ is a Bernoulli function,

$$\delta(\mathbf{x}, s) = g\left(\textstyle\sum_{\{s' : r(\mathbf{x}, s') \leqslant r(\mathbf{x}, s)\}} \pi_{s'}\right) - g\left(\textstyle\sum_{\{s' : r(\mathbf{x}, s') < r(\mathbf{x}, s)\}} \pi_{s'}\right),$$

and $g : [0, 1] \to \mathbb{R}$ is a continuous and strictly increasing function. (If $\{s' : r(\mathbf{x}, s') < r(\mathbf{x}, s)\}$ is empty, we let $g\left(\sum_{\{s' : r(\mathbf{x}, s') < r(\mathbf{x}, s)\}} \pi_{s'}\right) = g(0)$.) The function g distorts the cumulative distribution of the bundle $\mathbf{x}$, so that an agent maximizing rank dependent utility can behave as though the probability he attaches to a state depends on the relative attractiveness of the outcome in that state. Since u is strictly increasing, $\delta(\mathbf{x}, s) = \delta(\mathbf{u}(\mathbf{x}), s)$ and therefore $V(\mathbf{x}) = \phi(\mathbf{u}(\mathbf{x}))$, where for any vector $\mathbf{u} = (u_1, u_2, \ldots, u_{\bar{s}})$,

$$\phi(\mathbf{u}) = \sum_{s=1}^{\bar{s}} \delta(\mathbf{u}, s) u_s.$$

Note that the function ϕ is continuous and strictly increasing in $\mathbf{u}$. So V has the form assumed in Theorem 1, and we can use that result to devise a test for RDU-rationalizability.

We discuss a multiple-state version of the RDU test in the Appendix; at this point it suffices to explain the two-state case, which is the one relevant to the implementation in Section 4. Let $\rho_s = g(\pi_s)$ be the distorted value of π_s (the true probability of state s, for $s = 1, 2$). Then $\phi(u_1, u_2) = \rho_1 u_1 + (1 - \rho_1) u_2$ if $u_1 \leqslant u_2$ and $\phi(u_1, u_2) = (1 - \rho_2) u_1 + \rho_2 u_2$ if $u_1 > u_2$; by Theorem 1, a sufficient (and obviously necessary) condition for a data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ to be RDU-rationalizable is for there to be a solution to (6) and (7), with this formula for ϕ. This test involves solving a set of inequalities that are bilinear in

that are potentially useful for implementing bilinear tests more generally.

[19] To keep the notation light, we confine ourselves to the case where $\boldsymbol{\pi}$ does not vary across observations. There is no conceptual difficulty in allowing for this.

the unknowns $\{\bar{u}(r)\}_{r\in\mathcal{X}}$ and $\{\rho_1, \rho_2\}$. In our implementation, we simply let ρ_1 and ρ_2 take different values on a very fine grid in $[0,1]^2$, subject to $\rho_1 \leqslant \rho_2$ (if and only if $\pi_1 \leqslant \pi_2$) and (for each case) perform the corresponding linear test.

It is worth emphasizing at this point that for certain values of ρ_1 and ρ_2, the function ϕ is clearly not concave or even quasiconcave, and therefore we cannot guarantee the quasiconcavity of the agent's utility over contingent consumption, *even if* we restrict ourselves to concave Bernoulli functions. While the lattice method still works in these cases, it is not possible to formulate a test for rationalizability that allows for non-quasiconcave utility functions using concave optimization methods (such as those cited in Section 1.3) because the first order conditions are no longer *sufficient* for optimality.

We also implement a lattice test of Gul's (1991) model of disappointment aversion (DA). When there are two states, the DA model is a special case of the RDU model with a further restriction on ρ_1 and ρ_2. Specifically, there is some $\beta \in (-1, \infty)$ such that, for $s = 1, 2$,

$$\rho_s = \frac{(1+\beta)\pi_s}{1+\pi_s\beta}. \tag{11}$$

Note that this restriction has bite only if $\rho_1 \neq \rho_2$, so in fact the RDU and DA models are identical when $\pi_1 = \pi_2$. If $\beta = 0$, the agent simply maximizes expected utility. If $\beta > 0$, we have $\rho_s > \pi_s$, so the agent attaches a probability on s that is higher than the objective probability when s is the less favorable state; in this case, the agent is said to be *disappointment averse*. If $\beta < 0$, then $\rho_s < \pi_s$, and the agent is said to be *elation seeking*; this is an instance where the function ϕ is not quasiconcave. As in the RDU model, we test the DA model by letting β take on different values and performing the associated linear test.

While it is well known that the RDU and EU models lead to different predictions, it not immediately clear that they are observationally distinct in the context of observations drawn from linear budgets. We end this section with an example of a data set that is RDU-rationalizable but not EU-rationalizable.

Example 3. Suppose the data set consists of three observations $(\mathbf{x}^t, \mathbf{p}^t)$, for $t = 1, 2, 3$, where $\mathbf{p}^1 = (1, q)$, $\mathbf{x}^1 = (a, a)$; $\mathbf{p}^2 = (1, 1/q)$, $\mathbf{x}^2 = (b, b)$; and $\mathbf{p}^3 = (1, (1/q^2) + \epsilon)$, $\mathbf{x}^3 = (a + (a-b)/q, b + (b-a)q)$, with $q > 1$ and $a < b$, and $\epsilon > 0$ is a small number. The three observations are depicted Figure 3, where $c = a + (a-b)/q$ and $d = b + (b-a)q$.

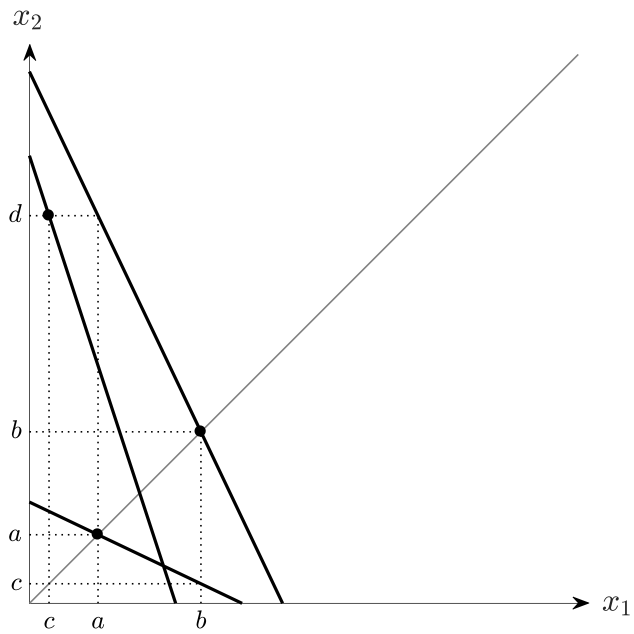


Figure 3: RDU-rationalizable but not EU-rationalizable data set

We claim that these observations are not EU-rationalizable if the two states are equiprobable. Suppose that they are, for some Bernoulli function u. Then the first observation tells us that $2u(a) \geqslant u(b) + u(c)$, since (b, c) is available when (a, a) is chosen. Similarly, from the second observation, we know that $2u(b) \geqslant u(a) + u(d)$. Together this gives

$$u(b) - u(d) \geqslant u(a) - u(b) \geqslant u(c) - u(a),$$

from which we obtain $u(a) + u(b) \geqslant u(c) + u(d)$. On the other hand, it is straightforward to check that, with $\epsilon > 0$, the bundle (a, b) is strictly cheaper than (c, d) at $\mathbf{p}^3$, which leads to a contradiction since (c, d) is chosen over (a, b) at the third observation.[20]

We claim that these observations are RDU-rationalizable; in fact, they can be rationalized with a smooth and concave Bernoulli function. Suppose $V(x_1, x_2) = \rho u(x_1) + (1-\rho)u(x_2)$ when $x_1 \leqslant x_2$ and $V(x_1, x_2) = (1-\rho)u(x_1) + \rho u(x_2)$ when $x_1 > x_2$, with $\rho = q/(q+1)$. Since $\rho > 1/2$, the agent displays disappointment aversion. So long as u is strictly concave, the agent's utility is maximized at $x_1 = x_2$ whenever $p_1 = 1$ and $p_2 \in [1/q, q]$. So V rationalizes the first two observations. To justify the third, it suffices to find u such that $u' > 0$ and

[20] Equivalently, note that there is a violation of Fishburn's condition. The bundle/lottery (a, a) is preferred to (b, c), (b, b) is preferred to (a, d), and (c, d) is *strictly* preferred to (a, b). However, the compound lottery where (a, a), (b, b) and (c, d) each occur with probability 1/3 is stochastically equivalent to the compound lottery where (b, c), (a, d) and (a, b) each occur with probability 1/3.

$u'' < 0$ satisfying the first order condition

$$\frac{\rho u'(c)}{(1-\rho)u'(d)} = q\frac{u'(c)}{u'(d)} = \frac{p_1^3}{p_2^3} = \frac{q^2}{1+q^2\epsilon}.$$

If ϵ is sufficiently small, this is possible since the price ratio p_1^3/p_2^3 is greater than q.[21] $\square$

3. Goodness of fit

The revealed preference tests presented in the previous section are 'sharp', in the sense that a data set either passes the test for a given model or it fails. This either/or feature of the tests is not particular to our results but is true of all classical revealed preference tests, including Afriat's. It would, of course, be desirable to develop a way of measuring *the extent* to which a certain class of utility functions succeeds or fails in rationalizing a data set, and the most common approach adopted in the revealed preference literature to address this issue was developed by Afriat (1972, 1973) and Varian (1990).[22, 23] We now give an account of this approach and explain why implementing it in our setting is possible (or at least no more difficult than implementing the exact tests).

Suppose that the observer collects a data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$; following the earlier literature, we focus attention on the case where B^t is a classical linear budget set given by (1). For any number $e^t \in [0,1]$, we define

$$B^t(e^t) = \{\mathbf{x} \in \mathbb{R}_+^{\bar{s}} : \mathbf{p}^t \cdot \mathbf{x} \leqslant e^t \mathbf{p}^t \cdot \mathbf{x}^t\} \cup \{\mathbf{x}^t\}. \tag{12}$$

Clearly $B^t(e^t)$ is smaller than B^t and shrinks with the value of e^t. Let $\mathcal{U}$ be a collection of utility functions defined on $\mathbb{R}_+^{\bar{s}}$ belonging to a given family; for example, $\mathcal{U}$ could be the family of locally nonsatiated utility functions (which was the family considered by Afriat (1972, 1973) and Varian (1990) in their work). We define the set $E(\mathcal{U})$ in the following manner: a vector $\mathbf{e} = (e^1, e^2, \ldots, e^T)$ is in $E(\mathcal{U})$ if there is some function $U \in \mathcal{U}$ that rationalizes the

[21] If it were smaller than q, this would not be possible since the concavity of u requires $u'(c) \geqslant u'(d)$.

[22] For examples where Afriat-Varian type indices are used to measure a model's fit, see Mattei (2000), Harbaugh, Krause, and Berry (2001), Andreoni and Miller (2002), Choi *et al.* (2007, 2014), Beatty and Crawford (2011), and Halevy, Persitz, and Zrill (2016), and Pastor-Bernier, Plott, and Schultz (2017). Echenique, Lee, and Shum (2011) develops and applies a related index called the money pump index.

[23] For an account of why such measures may be more suitable then other measures of goodness-of-fit, such as the sum of squared errors between observed and predicted demands, see Varian (1990) and Halevy, Persitz, and Zrill (2016).

modified data set $\mathcal{O}(\mathbf{e}) = \{(\mathbf{x}^t, B^t(e^t))\}_{t=1}^T$, i.e., $U(\mathbf{x}^t) \geqslant U(\mathbf{x})$ for all $\mathbf{x} \in B^t(e^t)$. Clearly, the data set $\mathcal{O}$ is rationalizable by a utility function in $\mathcal{U}$ if and only if the unit vector $(1, 1, \ldots, 1)$ is in $E(\mathcal{U})$. We also know that $E(\mathcal{U})$ must be nonempty since it contains the vector $\mathbf{0}$, and it is clear that if $\mathbf{e} \in E(\mathcal{U})$ then $\mathbf{e}' \in E(\mathcal{U})$, where $\mathbf{e}' < \mathbf{e}$. The closeness of the set $E(\mathcal{U})$ to the unit vector is a measure of how well the utility functions in $\mathcal{U}$ can explain the data. Afriat (1972, 1973) suggests measuring this distance with the supnorm, so the distance between $\mathbf{e}$ and 1 is $D_A(\mathbf{e}) = 1 - \min_{1 \leqslant t \leqslant T} \{e^t\}$, while Varian (1990) suggests that we choose the square of the Euclidean distance, i.e., $D_V(\mathbf{e}) = \sum_{t=1}^T (1 - e^t)^2$.

Measuring distance by the supnorm has the advantage that it is computationally straightforward, and it is also the measure most commonly used in the empirical revealed preference literature, so this is the approach that we adopt in our implementation (see Section 4). Note that $D_A(\mathbf{e}) = D_A(\tilde{\mathbf{e}})$ where $\tilde{\mathbf{e}}$ is the vector with identical entries equal to $\min\{e^1, e^2, \ldots, e^T\}$, where $\mathbf{e} = (e^1, e^2, \ldots, e^T)$. Since $\tilde{\mathbf{e}} \leqslant \mathbf{e}$, we obtain $\tilde{\mathbf{e}} \in E(\mathcal{U})$ whenever $\mathbf{e} \in E(\mathcal{U})$. Therefore, $\min_{\mathbf{e} \in E(\mathcal{U})} D_A(\mathbf{e}) = \min_{\mathbf{e} \in \widetilde{E}(\mathcal{U})} D_A(\mathbf{e})$, where $\widetilde{E}(\mathcal{U}) = \{\mathbf{e} \in E(\mathcal{U}) : e^t = e^{t'} \text{ for any } t, t'\}$, i.e., in searching for $\mathbf{e} \in E(\mathcal{U})$ that minimizes the supnorm distance from $(1, 1, \ldots, 1)$, we can focus our attention on the set $\widetilde{E}(\mathcal{U})$, which consists of those vectors in $E(\mathcal{U})$ that shrink each observed budget set by the same proportion. Given a data set $\mathcal{O} = \{(\mathbf{x}^t, \mathbf{p}^t)\}_{t=1}^T$, Afriat refers to $\sup\{e : (e, e, \ldots, e) \in E(\mathcal{U})\}$ as the *critical cost efficiency index*; we say that $\mathcal{O}$ is *rationalizable in $\mathcal{U}$ at the efficiency index/threshold* e' if $(e', e', \ldots, e') \in E(\mathcal{U})$.

Suppose that for a given data set, the critical cost efficiency index is 0.95. In that case, while we cannot guarantee that $\mathbf{x}^t$ is optimal in the true budget set, we know that there is some utility function in $\mathcal{U}$ for which $\mathbf{x}^t$ is optimal in $B^t(0.95)$ at every observation t. With this utility function, there could be bundles in B^t which the subject prefers to $\mathbf{x}^t$, but choosing such a bundle (instead of $\mathbf{x}^t$) will not lead to savings of more than 5%. Furthermore, this number is tight in the following sense: given any $\epsilon > 0$, then for every utility function in $\mathcal{U}$, there is at least one observation t where the subject could indeed have saved $(5 - \epsilon)$% of her expenditure.[24] We can interpret this index as a characteristic of the subject and, specifically, a measure of her bounded rationality; the bounded rationality could have arisen because she is simply incapable of better decision making, or it could be that she has consciously or

[24]Formally, for every $U \in \mathcal{U}$ there is t such that $\max\{U(\mathbf{x}) : \mathbf{p}^t \cdot \mathbf{x} \leqslant \mathbf{p}^t \cdot \mathbf{x}^t(0.95 + 0.01\epsilon)\} > U(\mathbf{x}^t)$.

otherwise judged that it is not, from a broader perspective, rational for her to expend the mental powers needed for exactly rational portfolio decisions.

Calculating the efficiency index (or, more generally, an index based on the Euclidean metric) will require checking whether a particular vector $\mathbf{e} = (e^1, e^2, \ldots, e^T)$ is in $E(\mathcal{U})$, i.e., whether $\mathcal{O}(\mathbf{e}) = \{(\mathbf{x}^t, B^t(e^t))\}_{t=1}^T$ is rationalizable by a member of $\mathcal{U}$. When $\mathcal{U}$ is the family of all locally nonsatiated utility functions, Afriat (1972, 1973) provides a necessary and sufficient condition for the rationalizability of $\mathcal{O}(\mathbf{e}) = \{(\mathbf{x}^t, B^t(e^t))\}_{t=1}^T$ (which we describe in greater detail in the Online Appendix).

More generally, the calculation of the efficiency index will hinge on whether there is a suitable test for the rationalizability of $\mathcal{O}(\mathbf{e}) = \{(\mathbf{x}^t, B^t(e^t))\}_{t=1}^T$ by members of $\mathcal{U}$. Even if a test of the rationalizability of $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ by members of $\mathcal{U}$ is available, this test may rely on the convexity or linearity of the budget sets B^t; in this case, extending the test so as to check for the rationalizability of $\mathcal{O}(\mathbf{e}) = \{(\mathbf{x}^t, B^t(e^t))\}_{t=1}^T$ is not straightforward since the modified budget sets $B^t(e^t)$ are clearly nonconvex. Crucially, this is *not* the case with the lattice method, which is applicable even for nonconvex constraint sets, so long as they are compact. Thus extending our testing procedure to measure goodness of fit in the form of the efficiency index involves no additional difficulties.

3.1 Approximate smooth rationalizability

While Theorem 1 guarantees that there is a Bernoulli function u that extends $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ and rationalizes the data when the required conditions are satisfied, the Bernoulli function is not necessarily smooth (though it is continuous and strictly increasing by definition). Of course, the smoothness of u is commonly assumed in applications of expected utility and related models and its implications can appear to be stark. For example, suppose that it is commonly known that states 1 and 2 occur with equal probability and we observe the agent choosing $(1, 1)$ at a price vector (p_1, p_2), with $p_1 \neq p_2$. This observation is incompatible with a smooth EU model; indeed, given that the two states are equiprobable, the slope of the indifference curve at $(1, 1)$ must equal -1 and thus it will not be tangential to the budget line and will not be a local optimum. On the other hand, it is trivial to check that this observation is EU-rationalizable in our sense. In fact, one could even find a *concave*

Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ for which $(1, 1)$ maximizes expected utility. (Such a u will, of course, have a kink at 1.)

These two facts can be reconciled by noticing that, even though this observation cannot be exactly rationalized by a smooth Bernoulli function, it is in fact possible to find a smooth function that comes arbitrarily close to rationalizing it. Indeed, given any strictly increasing and continuous function u defined on a compact interval of $\mathbb{R}_+$, there is a strictly increasing and smooth function $\tilde{u}$ that is uniformly and arbitrarily close to u on that interval. As such, if a Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ rationalizes $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ by $\{\phi(\cdot, t)\}_{t=1}^T$, then for any efficiency threshold $e \in (0, 1)$, there is a smooth Bernoulli function $\tilde{u} : \mathbb{R}_+ \to \mathbb{R}_+$ that rationalizes $\mathcal{O}' = \{(\mathbf{x}^t, B^t(e))\}_{t=1}^T$ by $\{\phi(\cdot, t)\}_{t=1}^T$. In other words, if a data set is rationalizable by some Bernoulli function, then it can also be rationalized by a smooth Bernoulli function, for any efficiency threshold arbitrarily close to 1. In this sense, imposing a smoothness requirement on the Bernoulli function does not radically alter a model's ability to explain a given data set.

4. Implementation

We examine the data collected from the well known portfolio choice experiment in Choi, Fisman, Gale, and Kariv (2007). The experiment was performed on 93 undergraduate subjects at the University of California, Berkeley. Every subject was asked to make consumption choices on 50 decision problems under risk. The subject divided her budget between two Arrow-Debreu securities, with each security paying one token if the corresponding state was realized, and zero otherwise. In a symmetric treatment applied to 47 subjects, each state of the world occurred with probability 1/2, and in a (balanced) asymmetric treatment applied to 46 subjects, the probabilities of the states were 1/3 and 2/3. These probabilities were objectively known. Lastly, income was normalized to one, and the state prices were chosen at random and varied across subjects.

In their analysis, Choi *et al.* (2007) first tested whether each subject could have been maximizing a locally nonsatiated utility function by performing a GARP test (or, strictly speaking, an extended version of the GARP test that characterizes rationalizability at a given efficiency threshold). Those subjects who passed GARP at a sufficiently high efficiency threshold were then fitted individually to a two-parameter version of the disappointment aversion model of Gul (1991). The lattice method developed in this paper makes it possible for us to re-analyze the same data using purely revealed preference techniques, without appealing to any parametric assumptions. In this section we evaluate different models of decision making with these newly developed tests according to three criteria: (1) the ability of the model to explain the observed data; (2) the precision of the model's predictions (in various senses that we shall define); and (3) an index combining (1) and (2).

We consider five nested models in our empirical analysis. The most stringent of these is the expected utility (EU) model, followed by the disappointment aversion (DA) and rank dependent utility (RDU) models. (Recall from Section 2, however, that the RDU and DA models are identical when the two states are equiprobable.) We test these three models using the lattice method developed in Sections 2 and 3. In addition we test for basic rationalizability, i.e., consistency with locally nonsatiated utility maximization. A more stringent criterion is rationalizability by a stochastically monotone utility (SMU) function; this is a utility function that gives strictly higher utility to the bundle $\mathbf{x}$ compared to $\mathbf{y}$ whenever $\mathbf{x}$ first order stochatically dominates $\mathbf{y}$ (with respect to the objective probabilities attached to each state) and gives them the same utility whenever they are stochastically equivalent.

A test of consistency with a stochastically monotone utility function (at a given efficiency threshold) was recently developed by Nishimura, Ok, and Quah (2017); this test has features similar to GARP and we shall refer to it as F-GARP (see the Online Appendix for details). In the Choi *et al.* (2007) experiment, there are just two states. In this case it is straightforward to check that when $\pi_1 = \pi_2 = 1/2$, a utility function is stochastically monotone if and only if it is strictly increasing and symmetric, and when $\pi_2 > \pi_1$, a utility function U is stochastically monotone if and only if it is strictly increasing and $U(a, b) > U(b, a)$ whenever $b > a$. Notice that the RDU, DA, and EU models all obey this property; in fact, it is well known that these three models respect first order stochastic dominance, even when there are more than two states of the world.

4.1 Exact pass rates and efficiency indices

We first test all five models on the Choi *et al.* (2007) data, and the results from these exact tests are displayed in Table 1, where each cell contains a pass rate. Across 50 decision

$\pi_1 = 1/2$		$\pi_1 \neq 1/2$		Total	
GARP	12/47 (26%)	GARP	4/46 (9%)	GARP	16/93 (17%)
F-GARP	1/47 (2%)	F-GARP	3/46 (7%)	F-GARP	4/93 (4%)
RDU/DA	1/47 (2%)	RDU	2/46 (4%)	RDU	3/93 (3%)
		DA	1/46 (2%)	DA	2/93 (2%)
EU	1/47 (2%)	EU	1/46 (2%)	EU	2/93 (2%)

Table 1: Pass rates

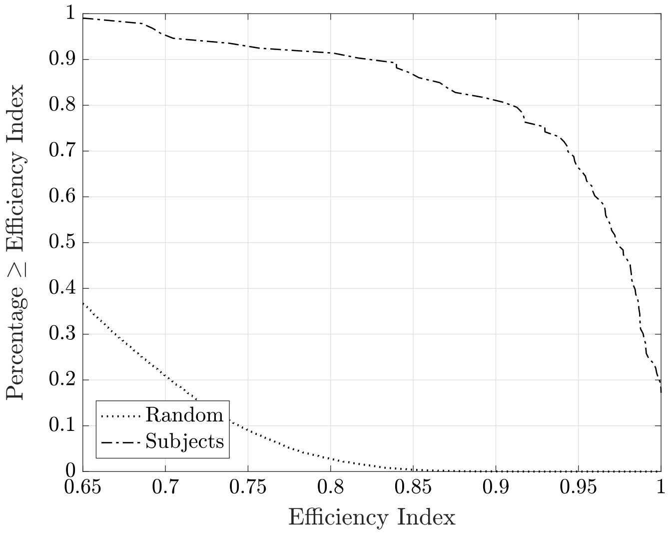
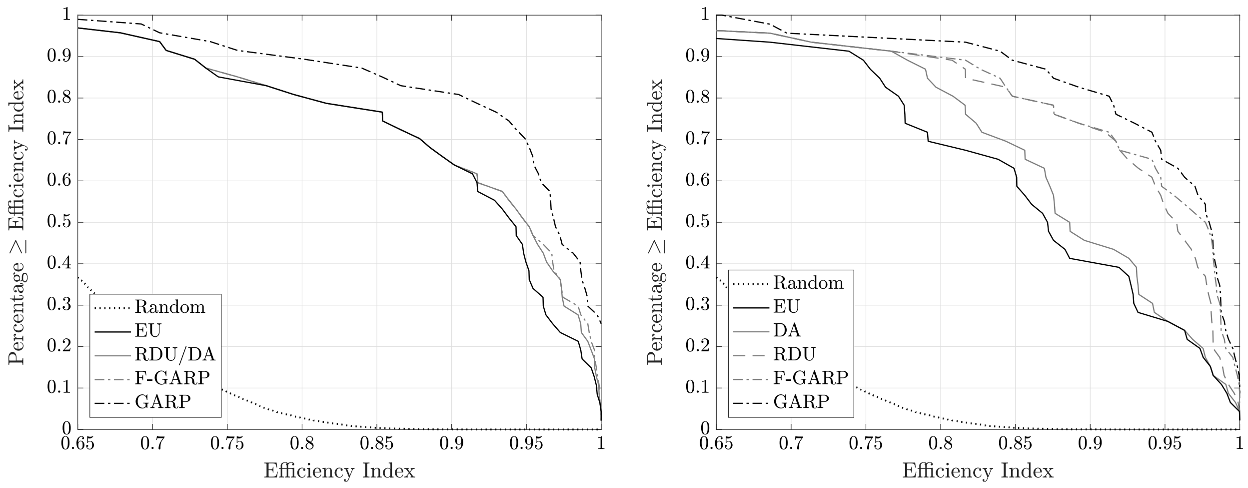


Figure 4: Distribution of efficiency indices for utility maximization

problems, 16 out of 93 subjects obey GARP and are therefore consistent with basic utility maximization; subjects in the symmetric treatment perform distinctly better than those in the asymmetric treatment. Of the 16 subjects who pass GARP, only 4 pass F-GARP; still fewer subjects are rationalizable by the RDU, DA, and EU models.

Given that we observe 50 decisions for every subject, it may not be intuitively surprising that so many subjects should have violated GARP (let alone more stringent conditions). We next investigate the efficiency thresholds at which subjects pass the different tests. First, we calculate the efficiency index at which each of the 93 subjects passes GARP; this empirical distribution is depicted in Figure 4. (Note that this figure is essentially a replication of Figure 4 in Choi *et al.* (2007).) We see that more than 80% of subjects have an efficiency index above 0.9, and more than 90% have an index above 0.8. A first glance at these results suggest that the data are largely compatible with the locally nonsatiated utility model.

To better understand what the observed distribution of efficiency indices says about the



(a) $\pi_1 = 1/2$ (b) $\pi \neq 1/2$

Figure 5: Distributions of efficiency indices

success or failure of a particular model to explain the data collected, it is useful to see what distribution of efficiency indices will arise if we postulate an alternative form of behavior. We adopt an approach first suggested by Bronars (1987) that simulates random uniform consumption, i.e., which posits that consumers are choosing randomly uniformly from their budget frontiers. The Bronars (1987) approach has become common practice in the revealed preference literature as a way of assessing the *power* or *precision* of revealed preference tests. We follow exactly the procedure of Choi *et al.* (2007) and generate a random sample of 25,000 simulated subjects. Each simulated subject chooses randomly uniformly from 50 budget lines that are selected in the same random fashion as in the experimental setting. The dotted curve in Figure 4 corresponds to the distribution of efficiency indices for the simulated subjects. The experimental and simulated distributions are starkly different. For example, while 80% of subjects have an efficiency index of 0.9 or higher, the chance of a randomly generated data set passing GARP at an efficiency index of 0.9 is negligible. In other words, even though the locally nonsatiated utility model could accommodate much of the choice behavior observed in the experiment, it is also precise enough to exclude behavior that is simply randomly generated, which lends support to basic utility maximization as an accurate and discriminating model of choice among contingent consumption bundles.

Going *beyond* Choi *et al.* (2007), we then calculate the distributions of efficiency indices associated with the SMU, RDU, DA, and EU models among the 93 subjects. These distributions are shown in Figures 5a and 5b, which correspond to the symmetric and asymmetric treatments, respectively. Since all of these models are more stringent than basic utility maximization, one would expect their efficiency indices to be lower, and they are. Nonetheless, at an efficiency threshold of 0.9, around half of all subjects are consistent with the EU model, with the proportion distinctly higher under the symmetric treatment. In the symmetric case, the performance of the EU, RDU/DA,[25] and SMU models are very close; in fact, their efficiency index distributions are almost indistinguishable. In the asymmetric case, the distinctions between models are sharper. The RDU and SMU models appear to perform considerably better than the EU and DA models, with their distributions of efficiency indices close to the distribution for the locally nonsatiated utility model. We have not depicted any efficiency index distributions for the SMU, RDU, DA, or EU models using randomly generated data, but plainly these will be even lower than for basic utility maximization and therefore very different from the distributions for the experimental subjects.

4.2 Pass rates

In order to compare the pass rates for different models more closely, we now concentrate on their performance at the 0.9 and 0.95 efficiency thresholds. These efficiency levels seem like reasonable standards that one might set in order to consider whether a model is consistent with the data; exact rationalizability is too stringent and anything less than 0.9 may be too permissive. The pass rates at these thresholds are presented in Table 2, where the models are arranged according to their generality, with the most permissive at the top. Assuming that the experimental subjects are a random sample drawn from a larger population of decision makers, we can use the sample pass rate for a model to estimate its expected population pass rate; these confidence intervals can be calculated *exactly* using the Clopper-Pearson procedure and are displayed in the Online Appendix.

We see from Table 2 that, at the 0.9 threshold, around 80% of all subjects pass GARP. Among this group, about half in turn display behavior that is consistent with the EU model (and, in fact, significantly more than half under the symmetric treatment). There is some evidence that the RDU model explains a significant number of subjects not captured by the EU model. In particular, in the asymmetric case, almost 90% of subjects who pass

[25] Recall that in the symmetric case, the RDU and DA models are identical.

$\pi_1 = 1/2$			$\pi_1 \neq 1/2$		
	$e = 0.90$	$e = 0.95$		$e = 0.90$	$e = 0.95$
GARP	38/47 (81%)	32/47 (68%)	GARP	37/46 (80%)	29/46 (63%)
F-GARP	30/47 (64%)	23/47 (49%)	F-GARP	33/46 (72%)	26/46 (57%)
RDU/DA	30/47 (64%)	23/47 (49%)	RDU	33/46 (72%)	24/46 (52%)
			DA	20/46 (43%)	12/46 (26%)
EU	30/47 (64%)	18/47 (38%)	EU	18/46 (39%)	12/46 (26%)

Table 2: Pass rates by efficiency level

GARP at the 0.9 threshold are also consistent with the RDU model, which appears to be a significantly better performance than for the EU model. Indeed, there is very little room for the RDU model to perform better, since it manages to accommodate almost every subject who passes F-GARP at the same threshold. On the other hand, the DA model, which lies strictly between the RDU and EU models under the asymmetric treatment, does not perform significantly better than the EU model.[26]

We should be more precise on what we mean by a model performing 'significantly better' than another. We do not simply mean that the difference between the true (population) pass rates is distinct from zero; such a statistical claim is not meaningful when two models are nested.[27] We adopt a more stringent notion of 'significant' difference: we test the null hypothesis that the difference in expected pass rates between model A and model B *is equal to 5%*, against an alternative hypothesis that this difference is greater than 5%; since model B is nested within A, we are checking whether the additional data sets which are *accommodated by model A but not B* significantly exceeds 5%. The findings of these tests are reported in Table 3. For example, at the 0.95 threshold under the symmetric treatment, Table 2 tells us that the sample proportion of subjects who *pass the test for RDU/DA but fail the test for EU* is 5/47; this gives a p-value of 0.085, which is not statistically significant, i.e., we fail to reject the null hypothesis of a 5% difference at the 0.05 significance level.

The performance of the RDU model under the asymmetric treatment is quite different.

[26] While the contexts and methods are very different, the relatively poor performance of the DA model has been noted in some other studies, for example, Hey and Orme (1994) and Barseghyan *et al.* (2013).

[27] Suppose model A contains model B. Denoting the expected pass rates of model A (B) by μ_A (μ_B), the null hypothesis that $\mu_A = \mu_B$ is rejected if there is one data set which passes A but not B. Indeed, given that B is a special case of A, we are effectively testing the proportion of data sets which pass A and fail B; we conclude that the proportion of data sets *of this type* is nonzero as long as one such data set is observed.

$\pi_1 = 1/2$					$\pi_1 \neq 1/2$					
		GARP	F-GARP	RDU/DA			GARP	F-GARP	RDU	DA
$e = 0.95$	F-GARP	*0.000*	·	·	$e = 0.95$	F-GARP	0.406	·	·	·
	RDU/DA	*0.000*	1.000	·		RDU	0.079	0.677	·	·
						DA	*0.000*	*0.000*	*0.000*	·
	EU	*0.000*	0.085	0.085		EU	*0.000*	*0.000*	*0.000*	1.000
		GARP	F-GARP	RDU/DA			GARP	F-GARP	RDU	DA
$e = 0.90$	F-GARP	*0.002*	·	·	$e = 0.90$	F-GARP	0.197	·	·	·
	RDU/DA	*0.002*	1.000	·		RDU	0.197	1.000	·	·
						DA	*0.000*	*0.000*	*0.000*	·
	EU	*0.002*	1.000	1.000		EU	*0.000*	*0.000*	*0.000*	0.677

Note: Each cell contains a p-value.

Table 3: Pairwise 5%-differences in pass rates

First, while the stochastically monotone utility model is theoretically more general, its pass rate is not significantly higher than 5% of that for the RDU model (at both efficiency thresholds). On the other hand, the pass rate for the RDU model compared to the EU model *does* significantly exceed 5%. Another way of saying the same thing is that if we are to form a 90% confidence interval on the expected proportion of subjects who are RDU-rationalizable but not EU-rationalizable, the lower bound of that interval exceeds 5%. What is that lower bound? At the 0.9 and 0.95 efficiency thresholds it is, respectively, 21% and 16%, which is sizeable by any reckoning. The RDU model generalizes the EU model by permitting a distortion of the objective probabilities; the Online Appendix contains more information on the type of distortions which are consistent with the data.

There is a large empirical literature that evaluates the performance of different models of choice under risk using experimental or field data and our results appear to be broadly in line with the findings obtained in earlier studies, even though the very different empirical methods employed make formal comparisons difficult. In particular, other papers have also found that the RDU model performs well (see, for example, Bruhin, Fehr-Duda, and Epper (2010) and Barseghyan *et al.* (2013) and their references). We find that the EU model captures a significant portion of subjects, though by no means everyone, which is broadly consistent with the fairly common finding that the EU model puts in a respectable performance (see, for example, Hey and Orme (1994)). The pass rate that we report for the EU model is higher than that in some other papers (for example, Bruhin, Fehr-Duda, and Epper (2010) reports a pass rate of 20% for the EU model), but it is worth bearing in mind that our formulation of the EU model is about as permissive as it could get. We require the Bernoulli function to be increasing in money, but it is estimated nonparametrically and with no curvature assumptions (such as concavity), so we have given the EU model the greatest possible scope to capture a subject's behavior.

4.3 Power

To examine more closely the power of different models, we adopt and then adapt the approach first suggested by Bronars (1987). We first generate data sets where on each budget set, the bundle is randomly chosen based on a uniform distribution on the budget frontier. The *power* of the model (or its *Bronars power*) is then given by the probability of such a data set being inconsistent with (say) utility maximization, which is synonymous with it failing GARP. As we have already pointed out in Section 4.1, when the data set consists of uniform bundles on 50 randomly chosen budget sets, the Bronars power is approximately 1 at both the 0.9 and 0.95 efficiency thresholds; in other words, the probability of such a randomly generated data set passing GARP at either threshold is vanishingly small. Obviously, the Bronars power of the other models we consider is also roughly equal to 1, since all of them imply locally nonsatiated preferences.

When we consider a model that is theoretically more stringent than basic utility maximization, it is natural to investigate the power of the model *in the context of observed behavior that is already consistent with GARP*. In other words, we would like to know the sharpness of the model's predictions relative to basic utility maximization. For example, to check the relative power of the EU model in this sense, we randomly generate a large number of data sets that pass GARP at a given efficiency threshold, and then test if they obey EU at the same threshold. (See the Online Appendix for details.) Since the EU, DA, and RDU models are consistent with stochastic monotonicity, it is also natural to investigate the power of these models, *relative to the SMU model*; this would give us a sense of how stringent are the restrictions imposed by (say) the EU model, over and above those imposed by F-GARP.

Table 4 presents the power of the different models, conditional on passing GARP. The most obvious and important feature in this table is the ubiquity of numbers close to 1: even

$\pi_1 = 1/2$			$\pi_1 \neq 1/2$		
	$e = 0.90$	$e = 0.95$		$e = 0.90$	$e = 0.95$
F-GARP	1.00	1.00	F-GARP	0.88	0.92
RDU/DA	1.00	1.00	RDU	1.00	1.00
			DA	1.00	1.00
EU	1.00	1.00	EU	1.00	1.00

Table 4: Power (conditional on GARP)

$\pi_1 = 1/2$			$\pi_1 \neq 1/2$		
	$e = 0.90$	$e = 0.95$		$e = 0.90$	$e = 0.95$
RDU/DA	0.75	0.99	RDU	0.99	1.00
			DA	1.00	1.00
EU	0.87	0.99	EU	1.00	1.00

Table 5: Power (conditional on F-GARP)

after conditioning on passing GARP, all of the models remain very precise. For example, the probability of a data set which obeys GARP also passing the EU test at the 0.9 or 0.95 threshold is effectively zero. The only partial exception is for the SMU model in the asymmetric case, where the power is around 90%.

Table 5 tells us that all the models remain very precise relative to the SMU model when the treatment is asymmetric. But in the symmetric case, the relative power of the RDU/DA and EU models is noticeably lower than 1; for example, a quarter of all subjects who pass F-GARP at the 0.9 threshold are also consistent with the RDU/DA model. A possible reason for the loss of relative power in the symmetric case is that stochastic monotonicity itself is very restrictive in this context since it is synonymous with an increasing *and symmetric* utility function. However, we should emphasize that the relative power of the RDU, DA, and EU models remains high.

4.4 Predictive success

The *index of predictive success* proposed by Selten (1991) (or the Selten index, for short) combines pass rates and power into a single measure. This index is defined as the difference between the pass rate and the size of the set of predicted outcomes (the imprecision), with the latter typically measured by a uniform measure on all outcomes. Selten provides an axiomatic foundation for this index. The index varies between 1 and -1. It is close to 1 when the pass rate is close to 1 and the imprecision is close to zero; in other words, even though the model is very precise in its predictions (i.e., has a high power), the data collected are very often consistent with the model. On the other hand, an index close to -1 occurs when the pass rate is close to zero even though the model is very imprecise (i.e., has a low power). An index above zero indicates that the model has some predictive success. Our use of the Selten index to evaluate different models is not novel. In the context of consumer demand (which formally is very similar to ours), it has been used by Beatty and Crawford (2011); it has also been used by Harless and Camerer (1994) to compare the performance of different models of choice under risk.

As we have emphasized, with 50 observations on every subject, the locally nonsatiated utility model has a power that is almost indistinguishable from 1 at the 0.9 and 0.95 efficiency thresholds. The same is true of course of all the other models, since they are more restrictive than basic utility maximization. In other words, all the models have an imprecision of zero, so that the Selten indices for the different models are effectively given by their pass rates, as displayed in Table 2. Note also that because the different models are nested within one another, any observed differences (from zero) in the pass rates/Selten indices in Table 2 are all statistically significant. One conclusion to be drawn from this table is that the best model, as evaluated by the Selten index, is the locally nonsatiated utility model. This observation may be simple but it is not without interest: while a great deal of academic discussion is often focussed on comparing different models that have been tailor-made for decision making under risk, we should *not* take it for granted that such models are necessarily better than basic utility maximization in explaining choice behavior. In environments where state payoffs vary while state probabilities are fixed, one should not exclude the possibility that the locally nonsatiated utility model does a better job in explaining the data, even after accounting for its relative lack of specificity.[28]

[28] This observation is not an argument against the value of models of choice under risk, such as the SMU, RDU, and all other models considered here. In particular, these other models provide a theory of choice across *all* lotteries, allowing even for comparisons between lotteries where the same outcomes occur with different probabilities. In environments where agents are making choices among lotteries of this type, all the other models are still potentially applicable, but it is not clear how one could naturally generalize the locally nonsatiated utility model to accommodate such a context.

<table>
<tr><td colspan="3">$\pi_1 = 1/2$</td><td colspan="3">$\pi_1 \neq 1/2$</td></tr>
<tr><td></td><td>$e = 0.90$</td><td>$e = 0.95$</td><td></td><td>$e = 0.90$</td><td>$e = 0.95$</td></tr>
<tr><td>F-GARP</td><td>0.79</td><td>0.72</td><td>F-GARP</td><td>0.77</td><td>0.81</td></tr>
<tr><td rowspan="2">RDU/DA</td><td rowspan="2">0.79</td><td rowspan="2">0.72</td><td>RDU</td><td>0.89</td><td>0.83</td></tr>
<tr><td>DA</td><td>0.54</td><td>0.41</td></tr>
<tr><td>EU</td><td>0.79</td><td>0.56</td><td>EU</td><td>0.49</td><td>0.41</td></tr>
</table>

Table 6: Predictive success (conditional on GARP)

Our next objective is to investigate *conditional predictive success.* We first turn to the case where we condition on basic rationalizability. The conditional pass rate of each model can be calculated from Table 2. A model's imprecision is simply 1 minus the Bronars power (conditional on GARP) and this is supplied in Table 4. The Selten indices, constructed by taking the difference between the conditional pass rate and the conditional imprecision, are displayed in Table 6. For the symmetric treatment, all of the models have a conditional power of approximately 1 (see Table 4), so the Selten indices are nearly completely determined by the conditional pass rates and, as such, the differences between the SMU, RDU/DA, and EU models are not large. For the asymmetric treatment, the best performing model is RDU; this is driven by two factors: its pass rate is higher than all models except SMU and its power is higher than SMU. In the Online Appendix, we show that this difference between RDU model and the more restrictive DA and EU models are statistically significant.

Lastly, we investigate the predictive success of the EU, DA, and RDU models when we condition on passing F-GARP. The Selten indices displayed in Table 7 are obtained by taking the difference between the conditional pass rates (constructed from Table 2) and conditional power (from Table 5). Focussing firstly on the symmetric treatment, an interesting phenomenon is that, according to the Selten index, the EU model is now better than the RDU/DA model at the 0.9 efficiency threshold; this is entirely driven by the greater power of the EU model in this context. That said, the difference between the indices is not large and it is also reversed at the 0.95 threshold. For the asymmetric treatment, we notice that the RDU model performs well relative to the other models, because its pass rate is high *and* because the model continues to have high power, even after conditioning on passing F-GARP; we show in the Online Appendix that this difference is statistically significant.

$\pi_1 = 1/2$			$\pi_1 \neq 1/2$		
	$e = 0.90$	$e = 0.95$		$e = 0.90$	$e = 0.95$
RDU/DA	0.75	0.99	RDU	0.99	0.92
			DA	0.61	0.46
EU	0.87	0.78	EU	0.55	0.46

Table 7: Predictive success (conditional on F-GARP)

Appendix

The proof of Theorem 1 uses the following lemma.

Lemma 1. *Let $\{C^t\}_{t=1}^T$ be a finite collection of constraint sets in $\mathbb{R}_+^{\bar{s}}$ that are compact and downward closed (i.e., if $\mathbf{x} \in C^t$ then so is $\mathbf{y} \in \mathbb{R}_+^{\bar{s}}$ such that $\mathbf{y} < \mathbf{x}$) and let the functions $\{\phi(\cdot, t)\}_{t=1}^T$ be continuous and increasing in all dimensions. Suppose that there is a finite set $\mathcal{X}$ of $\mathbb{R}_+$, a strictly increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}_+$, and $\{M^t\}_{t=1}^T$ such that the following holds:*

$$M^t \geqslant \phi(\bar{\mathbf{u}}(\mathbf{x}), t) \ \textit{ for all } \mathbf{x} \in C^t \cap \mathcal{L} \textit{ and} \tag{13}$$

$$M^t > \phi(\bar{\mathbf{u}}(\mathbf{x}), t) \ \textit{ for all } \mathbf{x} \in (C^t \backslash \partial C^t) \cap \mathcal{L}, \tag{14}$$

where $\mathcal{L} = \mathcal{X}^{\bar{s}}$ and $\bar{\mathbf{u}}(\mathbf{x}) = (\bar{u}(x_1), \bar{u}(x_2), \ldots, \bar{u}(x_{\bar{s}}))$. Then there is a Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ that extends $\bar{u}$ such that

$$M^t \geqslant \phi(\mathbf{u}(\mathbf{x}), t) \ \textit{ for all } \mathbf{x} \in C^t \textit{ and} \tag{15}$$

$$\textit{if } \mathbf{x} \in C^t \textit{ and } M^t = \phi(\mathbf{u}(\mathbf{x}), t), \textit{ then } \mathbf{x} \in \partial C^t \cap \mathcal{L} \textit{ and } M^t = \phi(\bar{\mathbf{u}}(\mathbf{x}), t). \tag{16}$$

Remark: The property (16) needs some explanation. Conditions (13) and (14) allow for the possibility that $M^t = \phi(\bar{\mathbf{u}}(\mathbf{x}'), t)$ for some $\mathbf{x}' \in \partial C^t \cap \mathcal{L}$; we denote the set of points in $\partial C^t \cap \mathcal{L}$ with this property by X'. Clearly any extension u will preserve this property, i.e., $M^t = \phi(\mathbf{u}(\mathbf{x}'), t)$ for all $\mathbf{x}' \in X'$. Property (16) says that we can choose u such that for all $\mathbf{x} \in C^t \backslash X'$, we have $M^t > \phi(\mathbf{u}(\mathbf{x}), t)$.

Proof: We shall prove this result by induction on the dimension of the space containing the constraint sets. It is trivial to check that the claim is true if $\bar{s} = 1$. In this case, $\mathcal{L}$ consists of a finite set of points on $\mathbb{R}_+$ and each C^t is a closed interval with 0 as its minimum. Now let us suppose that the claim holds for $\bar{s} = m$ and we shall prove it for $\bar{s} = m + 1$. If, for each t, there is a strictly increasing and continuous utility function $u^t : \mathbb{R}_+ \to \mathbb{R}_+$ extending $\bar{u}$ such that (15) and (16) hold, then the the same conditions will hold for the increasing and continuous function $u = \min_t u^t$. So we can focus our attention on constructing u^t for a single constraint set C^t.

Suppose $\mathcal{X} = \{0, r^1, r^2, r^3, \ldots, r^I\}$, with $r^0 = 0 < r^i < r^{i+1}$, for $i = 1, 2, \ldots, I-1$. Let $\bar{r} = \max\{r \in \mathbb{R}_+ : (r, 0, 0, \ldots, 0) \in C^t\}$ and suppose that $(r^i, 0, 0, \ldots, 0) \in C^t$ if and only if $i \leqslant N$ (for some $N \leqslant I$). Consider the collection of sets of the form $D^i = \{\mathbf{y} \in \mathbb{R}^m_+ : (r^i, \mathbf{y}) \in C^t\}$ (for $i = 1, 2, \ldots, N$); this is a finite collection of compact and downward closed sets in $\mathbb{R}^m_+$. By the induction hypothesis applied to $\{D^i\}_{i=1}^N$, with $\{\phi(\bar{u}(r^i), \cdot, t)\}_{i=1}^N$ as the collection of functions, there is a strictly increasing function $u^* : \mathbb{R}_+ \to \mathbb{R}_+$ extending $\bar{u}$ such that

$$M^t \geqslant \phi(\bar{u}(r^i), \mathbf{u}^*(\mathbf{y}), t) \text{ for all } (r^i, \mathbf{y}) \in C^t \text{ and} \tag{17}$$

$$\text{if } (r^i, \mathbf{y}) \in C^t \text{ and } M^t = \phi(\bar{u}(r^i), \mathbf{u}^*(\mathbf{y}), t), \text{ then } (r^i, \mathbf{y}) \in \partial C^t \cap \mathcal{L} \text{ and } M^t = \phi(\bar{\mathbf{u}}(r^i, \mathbf{y}), t). \tag{18}$$

For each $r \in [0, \bar{r}]$, define

$$U(r) = \{u \leqslant u^*(r) : \max\{\phi(u, \mathbf{u}^*(\mathbf{y}), t) : (r, \mathbf{y}) \in C^t\} \leqslant M^t\}.$$

This set is nonempty; indeed $\bar{u}(r^k) = u^*(r^k) \in U(r)$, where r^k is the largest element in $\mathcal{X}$ that is weakly smaller than r. This is because, if $(r, \mathbf{y}) \in C^t$ then so is $(r^k, \mathbf{y})$, and (17) guarantees that $\phi(\bar{u}(r^k), \mathbf{u}^*(\mathbf{y}), t) \leqslant M^t$. The downward closedness of C^t and the fact that u^* is increasing also guarantees that $U(r) \subseteq U(r')$ whenever $r < r'$. Now define $\tilde{u}(r) = \sup U(r)$; the function $\tilde{u}$ has a number of significant properties. (i) *For $r \in \mathcal{X}$, $\tilde{u}(r) = u^*(r) = \bar{u}(r)$* (by the induction hypothesis). (ii) *$\tilde{u}$ is a nondecreasing function* since U is nondecreasing. (iii) *$\tilde{u}(r) > \bar{u}(r^k)$ if $r > r^k$, where r^k is largest element in $\mathcal{X}$ smaller than r.* Indeed, because C^t is compact and ϕ continuous, $\phi(\tilde{u}(r), \mathbf{u}^*(\mathbf{y}), t) \leqslant M^t$ for all $(r, \mathbf{y}) \in C^t$. By way of contradiction, suppose $\tilde{u}(r) = \bar{u}(r^k)$ and hence $\tilde{u}(r) < u^*(r)$. It follows from the definition of $\tilde{u}(r)$ that, for any sequence u_n, with $\tilde{u}(r) < u_n < u^*(r)$ and $\lim_{n\to\infty} u_n = \tilde{u}(r)$, there is $(r, \mathbf{y}_n) \in C^t$ such that $\phi(u_n, \mathbf{u}^*(\mathbf{y}_n), t) > M^t$. Since C^t is compact, we may assume with no loss of generality that $\mathbf{y}_n \to \hat{\mathbf{y}}$ and $(r, \hat{\mathbf{y}}) \in C^t$, from which we obtain $\phi(\tilde{u}(r), \mathbf{u}^*(\hat{\mathbf{y}}), t) = M^t$. Since C^t is downward closed, $(r^k, \hat{\mathbf{y}}) \in C^t$ and, since $\bar{u}(r^k) = u^*(r^k)$, we have $\phi(\mathbf{u}^*(r^k, \hat{\mathbf{y}}), t) = M^t$. This can only occur if $(r^k, \hat{\mathbf{y}}) \in \partial C^t \cap \mathcal{L}$ (because of (18)), but it is clear that $(r^k, \hat{\mathbf{y}}) \notin \partial C^t$ since $(r^k, \hat{\mathbf{y}}) < (r, \hat{\mathbf{y}})$. (iv) *If $r_n < r^i$ for all n and $r_n \to r^i \in \mathcal{X}$, then $\tilde{u}(r_n) \to u^*(r^i)$.* Suppose to the contrary, that the limit is $\hat{u} < u^*(r^i) = \bar{u}(r^i)$. Since u^* is continuous, we can assume, without loss of generality, that $\tilde{u}(r_n) < u^*(r_n)$. By the compactness of C^t, the continuity of ϕ, and the definition of $\tilde{u}$, there is $(r_n, \mathbf{y}_n) \in C^t$ such that $\phi(\tilde{u}(r_n), \mathbf{u}^*(\mathbf{y}_n), t) = M^t$. This leads to $\phi(\hat{u}, \mathbf{u}^*(\mathbf{y}'), t) = M^t$, where $\mathbf{y}'$ is an accumulation point of $\mathbf{y}_n$ and $(r^i, \mathbf{y}') \in C^t$. But since ϕ is strictly increasing, we obtain $\phi(u^*(r^i), \mathbf{u}^*(\mathbf{y}'), t) > M^t$, which contradicts (17).

Given the properties of $\tilde{u}$, we can find a continuous and strictly increasing function u^t such that u^t extends $\bar{u}$, i.e., $u^t(r) = \bar{u}(r)$ for $r \in \mathcal{X}$, $u^t(r) < u^*(r)$ for all $r \in \mathbb{R}_+ \backslash \mathcal{X}$ and $u^t(r) < \tilde{u}(r) \leqslant u^*(r)$ for all $r \in [0, \bar{r}] \backslash \mathcal{X}$. (In fact we can choose u^t to be smooth everywhere except possibly on $\mathcal{X}$.) We claim that (15) and (16) are satisfied for C^t. To see this, note that for $r \in \mathcal{X}$ and $(r, \mathbf{y}) \in C^t$, the induction hypothesis guarantees that (17) and (18) hold and they will continue to hold if u^* is replaced by u^t. In the case where $r \notin \mathcal{X}$ and $(r, \mathbf{y}) \in C^t$, since $u^t(r) < \tilde{u}(r)$ and ϕ is increasing, we obtain $M^t > \phi(\mathbf{u}^t(r, \mathbf{y}), t)$. **QED**

Proof of Theorem 1: This follows immediately from Lemma 1 if we set $C^t = \underline{B}^t$, and $M^t = \phi(\bar{\mathbf{u}}(\mathbf{x}^t), t)$. If $\bar{u}$ obeys conditions (6) and (7) then it obeys conditions (13) and (14). The rationalizability of $\mathcal{O}$ by $\{\phi(\cdot, t)\}_{t \in T}$ then follows from (15). **QED**

Description of the RDU-rationalizability test for multiple states: Suppose that $\pi_s > 0$ is the objective probability of state s. To develop a necessary and sufficient test for RDU-rationalizability, we first define $\Gamma = \{\sum_{s \in S} \pi_s : S \subseteq \{1, 2, \ldots, \bar{s}\}\}$, i.e., Γ is a finite subset of $[0, 1]$ that includes both 0 and 1 (corresponding to S equal to the empty set and the whole set, respectively). Suppose *there are strictly increasing functions $\bar{g} : \Gamma \to \mathbb{R}$ and $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ such that (6) and (7) are satisfied, with $\phi(\mathbf{u}) = \sum_{s=1}^{\bar{s}} \delta(\mathbf{u}, s) u_s$ and*

$$\delta(\mathbf{u}, s) = \bar{g}\left(\textstyle\sum_{\{s' : r(\mathbf{u}, s') \leqslant r(\mathbf{u}, s)\}} \pi_{s'}\right) - \bar{g}\left(\textstyle\sum_{\{s' : r(\mathbf{u}, s') < r(\mathbf{u}, s)\}} \pi_{s'}\right).$$

By Theorem 1, this guarantees that $\mathcal{O}$ is RDU-rationalizable, with $g : [0, 1] \to \mathbb{R}$ chosen to be any strictly increasing extension of $\bar{g}$. This test involves finding a solution to a set of inequalities that are bilinear in the unknowns $\{\bar{g}(\gamma)\}_{\gamma \in \Gamma}$ and $\{\bar{u}(r)\}_{r \in \mathcal{X}}$. It is also clear that these conditions are necessary for RDU-rationalizability since they will be satisfied if we simply let $\bar{g}$ and $\bar{u}$ be the restrictions of g and u to Γ and $\mathcal{X}$ respectively. **QED**

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Online Appendix

to

Revealed preferences over risk and uncertainty

Matthew Polisson, John K.-H. Quah, and Ludovic Renou

A1. Introduction

This Online Appendix consists of six parts. In Section A2, we discuss further applications of the lattice method which are not covered in the Main Text. In particular, we cover the choice acclimating personal equilibrium (CPE) model (Köszegi and Rabin, 2007), the maxmin expected utility (MEU) model (Gilboa and Schmeidler, 1989), the variational preference (VP) model (Maccheroni, Marinacci, and Rustichini, 2006), and a model with budget-dependent reference points. The other sections provide additional information on the empirical implementation in the main paper.

A3. Description of GARP and F-GARP Tests

A4. Confidence intervals on pass rates

A5. Bronars power calculation

A6. Probability distortions in the RDU model

A7. Statistical analysis of Selten index differences

A2. Further applications of the lattice method

Theorem 1 in the Main Text can also be used to test the rationalizability of many other models of choice under risk and under uncertainty. Formally, this involves finding a Bernoulli function u and a function ϕ belonging to some family Φ (corresponding to the particular model at hand) which together rationalize the data. In the subjective expected utility (SEU) case that was discussed in the Main Text, the lattice test involves solving a system of inequalities that are bilinear in the utility levels $\{\bar{u}(r)\}_{r\in\mathcal{X}}$ and the subjective probabilities $\{\pi_s\}_{s=1}^{\bar{s}}$. Such a formulation seems natural enough in the SEU case; what is worth remarking (and perhaps not obvious *a priori*) is that the *same* pattern holds across many of the common models of choice under risk and under uncertainty: they can be tested by solving a system of inequalities that are bilinear in $\{\bar{u}(r)\}_{r\in\mathcal{X}}$ and a finite set of variables specific to the particular model in question. It is known that bilinear systems are decidable, in the sense that there is an algorithm that can determine in a finite number of steps whether or not a solution exists. In the Main Text, we have already explained how the expected utility (EU), disappointment aversion (DA), and rank dependent utility (RDU) models can be tested using the lattice method. In this section we further illustrate the flexibility of the lattice method by applying it to several prominent models of decision making under risk or uncertainty.

A2.1 Choice acclimating personal equilibrium

The choice acclimating personal equilibrium (CPE) model (Köszegi and Rabin, 2007) (with a piecewise linear gain-loss function) specifies utility as $V(\mathbf{x}) = \phi(\mathbf{u}(\mathbf{x}), \boldsymbol{\pi})$, where

$$\phi((u_1, u_2, \ldots, u_{\bar{s}}), \boldsymbol{\pi}) = \sum_{s=1}^{\bar{s}} \pi_s u_s + \frac{1}{2}(1-\lambda) \sum_{r,s=1}^{\bar{s}} \pi_r \pi_s |u_r - u_s|, \tag{A.1}$$

$\boldsymbol{\pi} = \{\pi_s\}_{s=1}^{\bar{s}}$ are the objective probabilities, and $\lambda \in [0, 2]$ is the coefficient of loss aversion.[1] We say that a data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^{T}$ is CPE-rationalizable with the probability weights $\boldsymbol{\pi} = (\pi_1, \pi_2, \ldots, \pi_{\bar{s}}) \gg 0$ if there is ϕ in the collection Φ_{CPE} of functions of the form (A.1), and a Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ such that, for each t, $\phi(\mathbf{u}(\mathbf{x}^t), \boldsymbol{\pi}^t) \geqslant \phi(\mathbf{u}(\mathbf{x}), \boldsymbol{\pi}^t)$ for all $\mathbf{x} \in B^t$. Applying Theorem 1 in the Main Text, $\mathcal{O}$ *is CPE-rationalizable if and only if there is* $\lambda \in [0, 2]$ *and a strictly increasing function* $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ *that solve (6) and (7) in the Main Text.* It is notable that, irrespective of the number of states, this test is linear in the remaining variables for any given value of λ. Thus it is relatively straightforward to implement via a collection of linear tests (running over different values of $\lambda \in [0, 2]$).

A2.2 Maxmin expected utility

[1] Our presentation of CPE follows Masatlioglu and Raymond (2016). The restriction of λ to $[0, 2]$ guarantees that V respects first order stochastic dominance but allows for loss-loving behavior (see Masatlioglu and Raymond (2016)).

We again consider a setting where no objective probabilities can be attached to each state. An agent with maxmin expected utility (MEU), first presented by Gilboa and Schmeidler (1989), evaluates each bundle $\mathbf{x} \in \mathbb{R}_+^{\bar{s}}$ using the formula $V(\mathbf{x}) = \phi(\mathbf{u}(\mathbf{x}))$, where

$$\phi(\mathbf{u}) = \min_{\boldsymbol{\pi} \in \Pi} \left\{ \sum_{s=1}^{\bar{s}} \pi_s u_s \right\}, \tag{A.2}$$

where $\Pi \subset \Delta_{++} = \{\boldsymbol{\pi} \in \mathbb{R}_{++}^{\bar{s}} : \sum_{s=1}^{\bar{s}} \pi_s = 1\}$ is nonempty, compact in $\mathbb{R}^{\bar{s}}$, and convex. (Π can be interpreted as a set of probability weights.) Given these restrictions on Π, the minimization problem in (A.2) always has a solution and ϕ is strictly increasing.

A data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is said to be MEU-rationalizable if there is a function ϕ in the collection Φ_{MEU} of functions of the form (A.2), and a Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ such that, for each t, $\phi(\mathbf{u}(\mathbf{x}^t), \boldsymbol{\pi}^t) \geqslant \phi(\mathbf{u}(\mathbf{x}), \boldsymbol{\pi}^t)$ for all $\mathbf{x} \in B^t$. By Theorem 1 in the Main Text, this holds if and only if there exist Π and $\bar{u}$ that solve (6), (7), and (8) in the Main Text. We claim that this requirement can be reformulated in terms of the solvability of a set of bilinear inequalities.

This is easy to see for the two-state case where we may assume, without loss of generality, that there is π_1^* and $\pi_1^{**} \in (0,1)$ such that $\Pi = \{(\pi_1, 1-\pi_1) : \pi_1^* \leqslant \pi_1 \leqslant \pi_1^{**}\}$. Then it is clear that $\phi(u_1, u_2) = \pi_1^* u_1 + (1-\pi_1^*)u_2$ if $u_1 \geqslant u_2$ and $\phi(u_1, u_2) = \pi_1^{**} u_1 + (1-\pi_1^{**})u_2$ if $u_1 < u_2$. Consequently, for any $(x_1, x_2) \in \mathcal{L}$, we have $V(x_1, x_2) = \pi_1^* \bar{u}(x_1) + (1-\pi_1^*)\bar{u}(x_2)$ if $x_1 \geqslant x_2$ and $V(x_1, x_2) = \pi_1^{**} \bar{u}(x_1) + (1-\pi_1^{**})\bar{u}(x_2)$ if $x_1 < x_2$ and this is independent of the precise choice of $\bar{u}$. Therefore, *$\mathcal{O}$ is MEU-rationalizable if and only if we can find π_1^* and π_1^{**} in $(0,1)$, with $\pi_1^* \leqslant \pi_1^{**}$, and an increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ that solve (6) and (7) in the Main Text.* The requirement takes the form of a system of bilinear inequalities that are linear in $\{\bar{u}(r)\}_{r \in \mathcal{X}}$ after conditioning on π_1^* and π_1^{**}.

The result below covers the general case. The test involves solving a system of bilinear inequalities in the variables $\bar{\pi}_s(\mathbf{x})$ (for all s and $\mathbf{x} \in \mathcal{L}$) and $\bar{u}(r)$ (for all $r \in \mathcal{X}$). Note that $\bar{\boldsymbol{\pi}}(\mathbf{x}) = (\bar{\pi}_1(\mathbf{x}), \bar{\pi}_2(\mathbf{x}), \ldots, \bar{\pi}_{\bar{s}}(\mathbf{x}))$ is used to construct the set of priors Π (in (A.2)) and that $\bar{\boldsymbol{\pi}}(\mathbf{x})$ is the distribution in Π that minimizes the expected utility of the bundle $\mathbf{x}$ (see (A.6)).

Proposition A.1. *A data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is MEU-rationalizable if and only if there is a function $\bar{\boldsymbol{\pi}} : \mathcal{L} \to \Delta_{++}$ and a strictly increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ such that*

$$\bar{\boldsymbol{\pi}}(\mathbf{x}^t) \cdot \bar{\mathbf{u}}(\mathbf{x}^t) \geqslant \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) \ \textit{ for all } \mathbf{x} \in \mathcal{L} \cap \underline{B}^t, \tag{A.3}$$

$$\bar{\boldsymbol{\pi}}(\mathbf{x}^t) \cdot \bar{\mathbf{u}}(\mathbf{x}^t) > \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) \;\; \textit{for all } \mathbf{x} \in \mathcal{L} \cap (\underline{B}^t \backslash \partial \underline{B}^t), \textit{ and} \tag{A.4}$$

$$\bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) \leqslant \bar{\boldsymbol{\pi}}(\mathbf{x}') \cdot \bar{\mathbf{u}}(\mathbf{x}) \;\; \textit{for all } (\mathbf{x}, \mathbf{x}') \in \mathcal{L} \times \mathcal{L}. \tag{A.5}$$

If these conditions hold, $\mathcal{O}$ admits an MEU-rationalization where Π (in (A.2)) is the convex hull of $\{\bar{\boldsymbol{\pi}}(\mathbf{x})\}_{\mathbf{x} \in \mathcal{L}}$, the Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}$ extends $\bar{u}$, and

$$V(\mathbf{x}) = \min_{\boldsymbol{\pi} \in \Pi} \{\boldsymbol{\pi} \cdot \bar{\mathbf{u}}(\mathbf{x})\} = \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) \;\; \textit{for all } \mathbf{x} \in \mathcal{L}. \tag{A.6}$$

Proof: Suppose that $\mathcal{O}$ is rationalizable by ϕ as defined by (A.2). For any $\mathbf{x}$ in the finite lattice $\mathcal{L}$, let $\bar{\boldsymbol{\pi}}(\mathbf{x})$ be an element in $\arg\min_{\boldsymbol{\pi} \in \Pi} \boldsymbol{\pi} \cdot \mathbf{u}(\mathbf{x})$ and let $\bar{u}$ be the restriction of u to $\mathcal{X}$. Then it is clear that the conditions (A.3)–(A.5) hold.

Conversely, suppose that there is a function $\bar{\boldsymbol{\pi}}$ and a strictly increasing function $\bar{u}$ obeying the conditions (A.3)–(A.5). Define Π as the convex hull of $\{\bar{\boldsymbol{\pi}}(\mathbf{x}) : \mathbf{x} \in \mathcal{L}\}$; Π is a nonempty and convex subset of Δ_{++} and it is compact in $\mathbb{R}^{\bar{s}}$ since $\mathcal{L}$ is finite. Suppose that there exists $\mathbf{x} \in \mathcal{L}$ and $\boldsymbol{\pi} \in \Pi$ such that $\boldsymbol{\pi} \cdot \bar{\mathbf{u}}(\mathbf{x}) < \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x})$. Since $\boldsymbol{\pi}$ is a convex combination of elements in $\{\bar{\boldsymbol{\pi}}(\mathbf{x}) : \mathbf{x} \in \mathcal{L}\}$, there must exist $\mathbf{x}' \in \mathcal{L}$ such that $\bar{\boldsymbol{\pi}}(\mathbf{x}') \cdot \bar{\mathbf{u}}(\mathbf{x}) < \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x})$, which contradicts (A.5). We conclude that $\bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) = \min_{\boldsymbol{\pi} \in \Pi} \boldsymbol{\pi} \cdot \bar{\mathbf{u}}(\mathbf{x})$ for all $\mathbf{x} \in \mathcal{L}$. We define $\phi : \mathbb{R}^{\bar{s}}_+ \to \mathbb{R}$ by $\phi(\mathbf{u}) = \min_{\boldsymbol{\pi} \in \Pi} \boldsymbol{\pi} \cdot \mathbf{u}$. Then the conditions (A.3) and (A.4) are just versions of (6) and (7) in the Main Text, and so Theorem 1 in the Main Text guarantees that there is Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ extending $\bar{u}$ such that $\mathcal{O}$ is rationalizable by $V(\mathbf{x}) = \phi(\mathbf{u}(\mathbf{x}))$. **QED**

A2.3 Variational preferences

A popular model of decision making under uncertainty which generalizes maxmin expected utility is variational preferences (VP), introduced by Maccheroni, Marinacci, and Rustichini (2006). In this model, a bundle $\mathbf{x} \in \mathbb{R}^{\bar{s}}_+$ has utility $V(\mathbf{x}) = \phi(\mathbf{u}(\mathbf{x}))$, where

$$\phi(\mathbf{u}) = \min_{\boldsymbol{\pi} \in \Delta_{++}} \{\boldsymbol{\pi} \cdot \mathbf{u} + c(\boldsymbol{\pi})\} \tag{A.7}$$

and $c : \Delta_{++} \to \mathbb{R}_+$ is a continuous and convex function with the following boundary condition: for any sequence $\boldsymbol{\pi}^n \in \Delta_{++}$ tending to $\tilde{\boldsymbol{\pi}}$, with $\tilde{\pi}_s = 0$ for some s, we obtain $c(\boldsymbol{\pi}^n) \to \infty$. This boundary condition, together with the continuity of c, guarantee that there is $\boldsymbol{\pi}^* \in \Delta_{++}$

that solves the minimization problem in (A.7).[2] Therefore, ϕ is well-defined and strictly increasing.

We say that $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is VP-rationalizable if there is a function ϕ in the collection Φ_{VP} of functions of the form (A.7), and a Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}_+$ such that, for each t, $\phi(\mathbf{u}(\mathbf{x}^t), \boldsymbol{\pi}^t) \geqslant \phi(\mathbf{u}(\mathbf{x}), \boldsymbol{\pi}^t)$ for all $\mathbf{x} \in B^t$. By Theorem 1 in the Main Text, this holds if and only if there exists a function $c : \Delta_{++} \to \mathbb{R}_+$ that is continuous, convex, and has the boundary property, and an increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ that together solve (6) and (7) in the Main Text, with ϕ defined by (A.7). The following result is a reformulation of this characterization that has a similar flavor to Proposition A.1; crucially, the necessary and sufficient conditions on $\mathcal{O}$ are formulated as a finite set of bilinear inequalities.

Proposition A.2. *A data set $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is VP-rationalizable if and only if there is a function $\bar{\boldsymbol{\pi}} : \mathcal{L} \to \Delta_{++}$, a function $\bar{c} : \mathcal{L} \to \mathbb{R}_+$, and a strictly increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ such that*

$$\bar{\boldsymbol{\pi}}(\mathbf{x}^t) \cdot \bar{\mathbf{u}}(\mathbf{x}^t) + \bar{c}(\mathbf{x}^t) \geqslant \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x}) \text{ for all } \mathbf{x} \in \mathcal{L} \cap \underline{B}^t, \tag{A.8}$$

$$\bar{\boldsymbol{\pi}}(\mathbf{x}^t) \cdot \bar{\mathbf{u}}(\mathbf{x}^t) + \bar{c}(\mathbf{x}^t) > \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x}) \text{ for all } \mathbf{x} \in \mathcal{L} \cap (\underline{B}^t \backslash \partial \underline{B}^t), \text{ and} \tag{A.9}$$

$$\bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x}) \leqslant \bar{\boldsymbol{\pi}}(\mathbf{x}') \cdot \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x}') \text{ for all } (\mathbf{x}, \mathbf{x}') \in \mathcal{L} \times \mathcal{L}. \tag{A.10}$$

If these conditions hold, then $\mathcal{O}$ can be rationalized by a variational preference V, with ϕ given by (A.7), such that the following holds:

(i) *$c : \Delta_{++} \to \mathbb{R}_+$ satisfies $c(\bar{\boldsymbol{\pi}}(\mathbf{x})) = \bar{c}(\mathbf{x})$ for all $\mathbf{x} \in \mathcal{L}$;*

(ii) *the Bernoulli function $u : \mathbb{R}_+ \to \mathbb{R}$ satisfies $\bar{u}(r) = u(r)$ for all $r \in \mathcal{X}$; and*

(iii) *$\bar{\boldsymbol{\pi}}(\mathbf{x}) \in \arg\min_{\boldsymbol{\pi} \in \Delta_{++}} \{\boldsymbol{\pi} \cdot \mathbf{u}(\mathbf{x}) + c(\boldsymbol{\pi})\}$, leading to $V(\mathbf{x}) = \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x})$, for all $\mathbf{x} \in \mathcal{L}$.*

Proof: Suppose $\mathcal{O}$ is rationalizable by ϕ as defined by (A.7). Let $\bar{u}$ be the restriction of u to $\mathcal{X}$. For any $\mathbf{x}$ in $\mathcal{L}$, let $\bar{\boldsymbol{\pi}}(\mathbf{x})$ be an element in $\arg\min_{\boldsymbol{\pi} \in \Delta_{++}} \{\boldsymbol{\pi} \cdot \mathbf{u}(\mathbf{x}) + c(\boldsymbol{\pi})\}$, and let $\bar{c}(\mathbf{x}) = c(\bar{\boldsymbol{\pi}}(\mathbf{x}))$. Then it is clear that the conditions (A.8)–(A.10) hold.

[2] Indeed, pick any $\tilde{\boldsymbol{\pi}} \in \Delta_{++}$ and define $S = \{\boldsymbol{\pi} \in \Delta_{++} : \boldsymbol{\pi} \cdot \mathbf{u} + c(\boldsymbol{\pi}) \leqslant \tilde{\boldsymbol{\pi}} \cdot \mathbf{u} + c(\tilde{\boldsymbol{\pi}})\}$. The boundary condition and continuity of c guarantee that S is compact in $\mathbb{R}^{\bar{s}}$ and hence $\arg\min_{\boldsymbol{\pi} \in S} \{\boldsymbol{\pi} \cdot \mathbf{u} + c(\boldsymbol{\pi})\} = \arg\min_{\boldsymbol{\pi} \in \Delta_{++}} \{\boldsymbol{\pi} \cdot \mathbf{u} + c(\boldsymbol{\pi})\}$ is nonempty.

Conversely, suppose that there is a strictly increasing function $\bar{u}$ and functions $\bar{\boldsymbol{\pi}}$ and $\bar{c}$ obeying conditions (A.8)–(A.10). For every $\boldsymbol{\pi} \in \Delta_{++}$, define $\tilde{c}(\boldsymbol{\pi}) = \max_{\mathbf{x} \in \mathcal{L}}\{\bar{c}(\mathbf{x}) - (\boldsymbol{\pi} - \bar{\boldsymbol{\pi}}(\mathbf{x})) \cdot \bar{\mathbf{u}}(\mathbf{x})\}$. It follows from (A.10) that $\bar{c}(\mathbf{x}') \geqslant \bar{c}(\mathbf{x}) - (\bar{\boldsymbol{\pi}}(\mathbf{x}') - \bar{\boldsymbol{\pi}}(\mathbf{x})) \cdot \bar{\mathbf{u}}(\mathbf{x})$ for all $\mathbf{x} \in \mathcal{L}$. Therefore, $\tilde{c}(\bar{\boldsymbol{\pi}}(\mathbf{x}')) = \bar{c}(\mathbf{x}')$ for any $\mathbf{x}' \in \mathcal{L}$. The function $\tilde{c}$ is convex and continuous but it need not obey the boundary condition. However, we know there is a function c defined on Δ_{++} that is convex, continuous, obeys the boundary condition, with $c(\boldsymbol{\pi}) \geqslant \tilde{c}(\boldsymbol{\pi})$ for all $\boldsymbol{\pi} \in \Delta_{++}$ and $c(\boldsymbol{\pi}) = \tilde{c}(\boldsymbol{\pi})$ for $\boldsymbol{\pi} \in \{\bar{\boldsymbol{\pi}}(\mathbf{x}) : \mathbf{x} \in \mathcal{L}\}$. We claim that, with c so defined, $\min_{\boldsymbol{\pi} \in \Delta_{++}}\{\boldsymbol{\pi} \cdot \bar{\mathbf{u}}(\mathbf{x}) + c(\boldsymbol{\pi})\} = \boldsymbol{\pi}(\mathbf{x}) \cdot \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x})$ for all $\mathbf{x} \in \mathcal{L}$. Indeed, for any $\boldsymbol{\pi} \in \Delta_{++}$,

$$\boldsymbol{\pi} \cdot \bar{\mathbf{u}}(\mathbf{x}) + c(\boldsymbol{\pi}) \geqslant \boldsymbol{\pi} \cdot \bar{\mathbf{u}}(\mathbf{x}) + \tilde{c}(\boldsymbol{\pi}) \geqslant \boldsymbol{\pi} \cdot \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x}) - (\boldsymbol{\pi} - \bar{\boldsymbol{\pi}}(\mathbf{x})) \cdot \bar{\mathbf{u}}(\mathbf{x}) = \bar{\mathbf{u}}(\mathbf{x}) + \bar{c}(\mathbf{x}).$$

On the other hand, $\bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \mathbf{u}(\mathbf{x}) + c(\bar{\boldsymbol{\pi}}(\mathbf{x})) = \bar{\boldsymbol{\pi}}(\mathbf{x}) \cdot \mathbf{u}(\mathbf{x}) + \bar{c}(\mathbf{x})$, which establishes the claim. We define $\phi : \mathbb{R}^{\bar{s}}_{+} \to \mathbb{R}$ by (A.7); then (A.8) and (A.9) are just versions of (6) and (7) in the Main Text, and so Theorem 1 in the Main Text guarantees that there is a Bernoulli function $u : \mathbb{R}_{+} \to \mathbb{R}_{+}$ extending $\bar{u}$ such that $\mathcal{O}$ is rationalizable by $V(\mathbf{x}) = \phi(\mathbf{u}(\mathbf{x}))$. **QED**

A2.4 Models with budget-dependent reference points

So far in our discussion we have assumed that the agent has a preference over different contingent outcomes, without being too specific as to what actually constitutes an outcome in the agent's mind. On the other hand, models such as prospect theory have often emphasized the impact of reference points, and *changing* reference points, on decision making. Some of these phenomena can be easily accommodated within our framework.

For example, imagine an experiment in which subjects are asked to choose from a constraint set of state contingent monetary prizes. Assuming that there are $\bar{s}$ states and that the subject never suffers a loss, we can represent each prize by a vector $\mathbf{x} \in \mathbb{R}^{\bar{s}}_{+}$. The subject is observed to choose $\mathbf{x}^t$ from $B^t \subset \mathbb{R}^{\bar{s}}_{+}$, so the data set is $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^{T}$. The standard way of thinking about the subject's behavior is to assume his choice from B^t is governed by a preference defined on the prizes, which implies that the situation where he never receives a prize (formally the vector 0) is the subject's constant reference point. But a researcher may well be interested in whether the subject has a different reference point or multiple reference points that vary with the budget (and perhaps manipulable by the researcher). Most obviously, suppose that the subject has an endowment point $\boldsymbol{\omega}^t \in \mathbb{R}^{\bar{s}}_{+}$ and a classical budget set

$B^t = \{\mathbf{x} \in \mathbb{R}^{\bar{s}}_+ : \mathbf{p}^t \cdot \mathbf{x} \leqslant \mathbf{p}^t \cdot \boldsymbol{\omega}^t\}$. In this case, a possible hypothesis is that the subject will evaluate different bundles in B^t based on a utility function defined on the deviation from the endowment; in other words, the endowment is the subject's reference point. Another possible reference point is that bundle in B^t which gives the same payoff in every state.

Whatever it may be, suppose the researcher has a hypothesis about the possible reference point at observation t, which we shall denote by $\mathbf{e}^t \in \mathbb{R}^{\bar{s}}_+$, and that the subject chooses according to some utility function $V : [-K, \infty)^{\bar{s}} \to \mathbb{R}_+$ where $K > 0$ is sufficiently large so that $[-K, \infty)^{\bar{s}} \subset \mathbb{R}^{\bar{s}}$ contains all the possible reference point-dependent outcomes in the data, i.e., the set $\bigcup_{t=1}^{T} \tilde{B}^t$, where

$$\tilde{B}^t = \{\mathbf{x}' \in \mathbb{R}^{\bar{s}} : \ \mathbf{x}' = \mathbf{x} - \mathbf{e}^t \text{ for some } \mathbf{x} \in B^t\}.$$

Let $\{\phi(\cdot, t)\}_{t=1}^T$ be a collection of functions, where $\phi(\cdot, t) : [-K, \infty)^{\bar{s}} \to \mathbb{R}$ is increasing in all of its arguments. We say that $\mathcal{O} = \{(\mathbf{x}^t, B^t)\}_{t=1}^T$ is *rationalizable by* $\{\phi(\cdot, t)\}_{t=1}^T$ *and the reference points* $\{\mathbf{e}^t\}_{t=1}^T$ if there exists a Bernoulli function $u : [-K, \infty) \to \mathbb{R}_+$ such that $\phi(\mathbf{u}(\mathbf{x}^t - \mathbf{e}^t), t) \geqslant \phi(\mathbf{u}(\mathbf{x} - \mathbf{e}^t), t)$ for all $\mathbf{x} \in B^t$. This is formally equivalent to saying that the modified data set $\mathcal{O}' = \{(\mathbf{x}^t - \mathbf{e}^t, \tilde{B}^t)\}_{t=1}^T$ is rationalizable by $\{\phi(\cdot, t)\}_{t=1}^T$. Applying Theorem 1 in the Main Text, rationalizability holds if and only if there is a strictly increasing function $\bar{u} : \mathcal{X} \to \mathbb{R}_+$ that obeys (6) and (7) in the Main Text, where

$$\mathcal{X} = \{r \in \mathbb{R} : r = x^t_s - e^t_s \text{ for some } t,\ s\} \cup \{-K\}.$$

Therefore, we may test whether $\mathcal{O}$ is rationalizable by expected utility, or by any of the models described so far, in conjunction with budget dependent reference points. Note that a test of rank dependent utility in this context is sufficiently flexible to accommodate phenomena emphasized by cumulative prospect theory (see Tversky and Kahneman (1992)), such as a Bernoulli function $u : [-K, \infty) \to \mathbb{R}$ that is S-shaped (and hence nonconcave) around 0 and probabilities distorted by a weighting function.

A3. Description of GARP and F-GARP tests

In addition to the expected utility (EU), disappointment aversion (DA), and rank dependent utility (RDU) models which we implement in the Main Text, there are other more basic notions of rationalizabiity that we also test. In this section, we describe these models and their revealed preference tests.

A3.1 Locally nonsatiated utility

The locally nonsatiated utility model is the most permissive of the models that we consider in the sense that all others are special cases. A utility function $U : \mathbb{R}^{\bar{s}}_{+} \to \mathbb{R}$ is *locally nonsatiated* if at every open neighborhood N of $\mathbf{x} \in \mathbb{R}^{\bar{s}}_{+}$, there is $\mathbf{y} \in N$ such that $U(\mathbf{y}) > U(\mathbf{x})$. We say that a data set $\mathcal{O} = \{(\mathbf{x}^t, \mathbf{p}^t)\}_{t=1}^{T}$ is *rationalizable* if it can be rationalized by a continuous and locally nonsatiated utility function.

Afriat's (1967) Theorem tells us that $\mathcal{O}$ is rationalizable if and only if it obeys a consistency condition known as the *generalized axiom of revealed preference* (GARP).[3] Afriat (1972, 1973) also shows that there is natural generalization of GARP that characterizes rationalizability at some efficiency index e, which we now describe. Let $\mathcal{D} = \{\mathbf{x}^t : t = 1, 2, \ldots, T\}$; in other words, $\mathcal{D}$ consists of those bundles that have been observed somewhere in the data set. For bundles $\mathbf{x}^t$ and $\mathbf{x}^{t'}$ in $\mathcal{D}$, $\mathbf{x}^t$ is said to be *revealed preferred to* $\mathbf{x}^{t'}$ at the efficiency index (or threshold) e (we denote this by $\mathbf{x}^t \succcurlyeq^*_e \mathbf{x}^{t'}$) if $\mathbf{x}^{t'} \in B^t(e)$, where $B^t(e)$ is given by (12) in the Main Text;[4] $\mathbf{x}^t$ is said to be *strictly revealed preferred to* $\mathbf{x}^{t'}$ (and we denote this by $\mathbf{x}^t \succ^*_e \mathbf{x}^{t'}$) if $\mathbf{x}^{t'} \in B^t(e)$ and $\mathbf{p}^t \cdot \mathbf{x}^{t'} < e\, \mathbf{p}^t \cdot \mathbf{x}^t$. $\mathcal{O}$ is rationalizable at the efficiency index e if and only if, whenever there are observations $(\mathbf{p}^{t_i}, \mathbf{x}^{t_i})$ (for $i = 1, 2, \ldots, n$) in $\mathcal{O}$ satisfying

$$\mathbf{x}^{t_1} \succcurlyeq^*_e \mathbf{x}^{t_2},\ \mathbf{x}^{t_2} \succcurlyeq^*_e \mathbf{x}^{t_3},\ \ldots\ ,\mathbf{x}^{t_{n-1}} \succcurlyeq^*_e \mathbf{x}^{t_n}, \text{ and } \mathbf{x}^{t_n} \succcurlyeq^*_e \mathbf{x}^{t_1}, \tag{A.11}$$

then we cannot replace $\succcurlyeq^*_e$ with $\succ^*_e$ anywhere in this chain; in other words, while there can be revealed preference cycles in $\mathcal{O}$, they cannot contain a strict revealed preference. This property is a generalization of GARP, which is the special case where $e = 1$. Checking for this property is computationally undemanding: the (strict) revealed preference relations on $\mathcal{D}$ can be easily constructed; once this has been established, we can apply Warshall's

[3] This term and its acronym were coined by Varian (1982), who also provides a proof of Afriat's Theorem. To be specific, the theorem says that GARP is necessary whenever the data set is rationalizable by a locally nonsatiated utility function (continuity is not needed); conversely, when a data set obeys GARP, then it is rationalizable by a continuous, strictly increasing, and concave utility function.

[4] Our terminology differs a little from the standard, which refers to $\succcurlyeq^*_e$ as the *direct revealed preference* relation and uses *revealed preference* to refer to the transitive closure of this relation. Since our exposition avoids any discussion of the transitive closure, we have adopted the simpler terminology here.

algorithm to compute the transitive closure of the revealed preference relations and then check for the absence of cycles containing strict revealed preferences.

A3.2 Stochastically monotone utility

For $\mathbf{x}$ and $\mathbf{y}$ in $\mathbb{R}^{\bar{s}}_+$, we write $\mathbf{x} \geqslant_{FSD} \mathbf{y}$ if $\mathbf{x}$ first order stochastically dominates $\mathbf{y}$ (given the payoffs and the objectively known probabilities) and write $\mathbf{x} >_{FSD} \mathbf{y}$ if $\mathbf{x} \geqslant_{FSD} \mathbf{y}$ and the two distributions are distinct. One way of sharpening the locally nonsatiated utility model is to require that the utility function $U : \mathbb{R}^{\bar{s}}_+ \to \mathbb{R}$ is *stochastically monotone*. By this we mean that $U(\mathbf{x}) > (\geqslant) \, U(\mathbf{y})$ whenever $\mathbf{x} >_{FSD} \mathbf{y}$ ($\mathbf{x} \geqslant_{FSD} \mathbf{y}$). Note that the RDU, DA, and EU models all obey this property.

In the Choi *et al.* (2007) experiment, there are two states; it is straightforward to check that when $\pi_1 = \pi_2 = 1/2$, a utility function is stochastically monotone if and only if it is strictly increasing and symmetric; when $\pi_2 > \pi_1$, a utility function U is stochastically monotone if and only if it is strictly increasing and $U(a, b) > U(b, a)$ whenever $b > a$.

A data set $\mathcal{O} = \{(\mathbf{x}^t, \mathbf{p}^t)\}_{t=1}^T$ is said to be rationalizable by the stochastically monotone utility (SMU) model if there is a continuous and stochastically monotone utility function U that rationalizes the observations. Since a utility function U that is stochastically monotone will be strictly increasing, it is also locally nonsatiated. Hence any SMU-rationalizable data set is also rationalizable by utility maximization but the converse is not true. Indeed, the single observation given in Example 1 passes GARP trivially, but it cannot be rationalized by any symmetric and strictly increasing utility function.

Nishimura, Ok, and Quah (2017) have recently developed a test for rationalizability by the SMU model. The test can be thought of as a version of GARP, but with suitably modified revealed preference relations. We say that the bundle $\mathbf{x}^t$ *is SMU-revealed preferred to* $\mathbf{x}^{t'}$ at the efficiency threshold e (for $\mathbf{x}^t$ and $\mathbf{x}^{t'}$ in $\mathcal{D}$) if there is a bundle $\mathbf{y}$ such that $\mathbf{y} \in B^t(e)$ and $\mathbf{y} \geqslant_{FSD} \mathbf{x}^{t'}$; this revealed preference is *strict* if $\mathbf{y}$ can be chosen to satisfy $\mathbf{y} >_{FSD} \mathbf{x}^t$. Nishimura, Ok, and Quah (2017) show that a data set is rationalizable by the SMU model at a threshold e if and only if it does not admit SMU-revealed preference cycles (such as (A.11)) containing strict SMU-revealed preferences; we call the latter property F-GARP (at the efficiency threshold e), where 'F' stands for first order stochastic dominance. Clearly this result is analogous to the characterization for basic rationalizability, except that the revealed preferences are defined differently. With two states, the SMU-revealed preference relations are easily obtained, and therefore checking F-GARP is also easy to implement.

A4. Confidence intervals on pass rates

$\pi_1 = 1/2$			$\pi_1 \neq 1/2$		
	$e = 0.90$	$e = 0.95$		$e = 0.90$	$e = 0.95$
GARP	0.81 $[0.67, 0.91]$	0.68 $[0.53, 0.81]$	GARP	0.80 $[0.66, 0.91]$	0.63 $[0.48, 0.77]$
F-GARP	0.64 $[0.49, 0.77]$	0.49 $[0.34, 0.64]$	F-GARP	0.72 $[0.57, 0.84]$	0.57 $[0.41, 0.71]$
RDU/DA	0.64 $[0.49, 0.77]$	0.49 $[0.34, 0.64]$	RDU	0.72 $[0.57, 0.84]$	0.52 $[0.37, 0.67]$
			DA	0.43 $[0.29, 0.59]$	0.26 $[0.14, 0.41]$
EU	0.64 $[0.49, 0.77]$	0.38 $[0.25, 0.54]$	EU	0.39 $[0.25, 0.55]$	0.26 $[0.14, 0.41]$

Note: Each cell contains a pass rate and *exact* 95% confidence interval (in square brackets), where the latter is obtained using the Clopper-Pearson procedure.

Table A.1: Pass rates and 95% confidence intervals

$\pi_1 = 1/2$			$\pi_1 \neq 1/2$		
	$e = 0.90$	$e = 0.95$		$e = 0.90$	$e = 0.95$
F-GARP	0.79 $[0.63, 0.90]$	0.72 $[0.53, 0.86]$	F-GARP	0.89 $[0.75, 0.97]$	0.90 $[0.73, 0.98]$
RDU/DA	0.79 $[0.63, 0.90]$	0.72 $[0.53, 0.86]$	RDU	0.89 $[0.75, 0.97]$	0.83 $[0.64, 0.94]$
			DA	0.54 $[0.37, 0.71]$	0.41 $[0.24, 0.61]$
EU	0.79 $[0.63, 0.90]$	0.56 $[0.38, 0.74]$	EU	0.49 $[0.32, 0.66]$	0.41 $[0.24, 0.61]$

Note: Each cell contains a pass rate and *exact* 95% confidence interval (in square brackets), where the latter is obtained using the Clopper-Pearson procedure.

Table A.2: Pass rates and 95% confidence intervals (conditional on GARP)

A5. Bronars power calculation

In order to calculate the Bronars (1987) power, we need to generate random data sets. As we described in the Main Text, this involves first producing budget sets in the same random fashion as in the Choi *et al.* (2007) experiment itself, and then randomly selecting bundles from these budget sets. In the case where we are interested in power unconditionally, our algorithm simply selects bundles randomly uniformly from the frontiers of these budget sets. While the unconditional procedure needs little explanation, the method for calculating conditional power, i.e., power conditional on passing the generalized axiom of revealed preference (GARP) or F-GARP, requires further explanation.

The process of generating a random data set obeying GARP (F-GARP) *at a given efficiency threshold* is as follows. First, we generate 50 budget sets as in Choi *et al.* (2007). Next, we select a budget line and randomly (uniformly) choose a bundle on that line. Then we select a second budget line and randomly choose a bundle from that part of the line which guarantees that this observation, along with the first, obeys GARP (F-GARP) at the given efficiency threshold. A third budget line is then selected and a bundle randomly chosen from that part of the line so that all three observations together obey GARP (F-GARP). Note that such a bundle *must exist*; indeed, the demand (on the third budget line) arising from any locally nonsatiated (stochastically monotone) utility function rationalizing the first two observations will have this property. We then choose a fourth budget line and a bundle on that line randomly so that the first four observations obey GARP (F-GARP), and so on. We generate 30,000 data sets (with 50 observations each) which pass (GARP) F-GARP at each of the two efficiency thresholds (0.9 and 0.95) in this manner,[5] before subjecting each data set to a test for a given model. By the Azuma-Hoeffding inequality, in order to be $100(1-\delta)$ percent confident that the sample pass rate resulting from a simulation is within ϵ of the true probability of passing the test, we require at least $N = (1/2\epsilon^2)\log(2/\delta)$ samples; with 30,000 samples, we can be 99.5 percent sure that our estimate of the Bronars power is within 0.01 of the true value.

A6. Probability distortions in the RDU model

The RDU model generalizes the EU model by permitting a distortion of the objective probabilities. With two states, the probability of the less favorable state is distorted to be $g(\pi)$ when π is the true probability. In the asymmetric treatment of Choi *et al.* (2007) analyzed in the Main Text, π is either 1/3 or 2/3. It turns out that, at the 0.9 threshold, all of the 15 subjects who fail EU but pass RDU continue to do so if we restrict $g(2/3) \in [0.55, 0.75]$ and $g(1/3) \in [0.25, 0.45]$. (Note that g may differ across subjects.) At the 0.95 threshold, the same restrictions on the distorted probabilities capture 11 of the 12 subjects who pass RDU and fail EU. So it seems that those who pass RDU do so with fairly modest distortions of the true probabilities.

[5] So there are four distinct collections of data sets, with each collection containing 30,000 data sets.

Furthermore, there is some evidence that subjects deflate the probability of the less favorable state when it is objectively 2/3 and inflate the probability when it is 1/3, so that the cumulative probability weighting function has the shape favored by cumulative prospect theory. Indeed, if we restrict ourselves to choosing $g(2/3) \in [0.55, 2/3]$ and $g(1/3) \in [1/3, 0.45]$, we still manage to capture every subject who passes the RDU test at the 0.9 threshold and all but two who pass at the 0.95 threshold. On the other hand, the mirror restriction performs very badly: if we insist on choosing $g(2/3) \in [2/3, 0.75]$ and $g(1/3) \in [0.25, 1/3]$, the RDU model captures *no subject* at either efficiency threshold other than those who are already EU-rationalizable. (Note that for any subject who passes RDU, there will typically be more than one set of distorted probabilities at which the subject is rationalizable.)

We know from Table 2 in the Main Text that, for the symmetric treatment the pass rates of the EU and RDU/DA models differ only at the efficiency threshold 0.95, where 5 subjects pass RDU/DA but fail EU. All 5 subjects pass the RDU test for some $g(1/2) < 0.5$, which is consistent with disappointment aversion, and 4 of them pass with values of $g(1/2)$ chosen from the interval $[0.45, 0.5)$.

A7. Statistical analysis of Selten index differences

A statistical analysis of the differences in the Selten indices conditional on GARP is provided in Table A.3. (Table A.4 provides the same analysis after conditioning on F-GARP.) Each entry in the table gives the p-value of the test of null hypothesis that the two models have the same Selten index, with the alternative hypothesis that they do not. To illustrate how this test is carried out, consider a test of the equality of the Selten indices between the EU and SMU models, under the asymmetric treatment and at the efficiency threshold 0.9. As shown in Table 4, the SMU model has a power of 0.88 and the EU model has a power of 1; we take this as given. The null hypothesis that the Selten indices are equal is equivalent to the hypothesis that $\mu_{SMU} - \mu_{EU} = 1 - 0.88 = 0.12$, where μ_{EU} and μ_{SMU} are the expected conditional pass rates for the EU and SMU models. The alternative hypothesis is $\mu_{SMU} - \mu_{EU} \neq 0.12$. The sample estimate of $\mu_{SMU} - \mu_{EU}$ has a binomial distribution and its realized value is $(33 - 18)/37$; according to Table A.3, the probability of obtaining this sample estimate or something more extreme if $\mu_{SMU} - \mu_{EU} = 0.12$ is effectively zero.

	$\pi_1 = 1/2$				$\pi_1 \neq 1/2$			
$e = 0.95$		F-GARP	RDU/DA	$e = 0.95$		F-GARP	RDU	DA
	RDU/DA	1.000	·		RDU	0.432	·	·
					DA	*0.000*	*0.000*	·
	EU	*0.000*	*0.000*		EU	*0.000*	*0.000*	1.000
$e = 0.90$		F-GARP	RDU/DA	$e = 0.90$		F-GARP	RDU	DA
	RDU/DA	1.000	·		RDU	*0.005*	·	·
					DA	*0.002*	*0.000*	·
	EU	1.000	1.000		EU	*0.000*	*0.000*	*0.000*
Note: Each cell contains a p-value, with values below 0.05 italicized.								

Table A.3: Pairwise differences in predictive success
(conditional on GARP)

	$\pi_1 = 1/2$				$\pi_1 \neq 1/2$	
$e = 0.95$		RDU/DA	$e = 0.95$		RDU	DA
				DA	*0.000*	·
	EU	*0.000*		EU	*0.000*	1.000
$e = 0.90$		RDU/DA	$e = 0.90$		RDU	DA
				DA	*0.000*	·
	EU	*0.003*		EU	*0.000*	*0.000*
Note: Each cell contains a p-value.						

Table A.4: Pairwise differences in predictive success
(conditional on F-GARP)

Notice that, in the asymmetric case, the differences in Selten indices between the RDU model and the more restrictive DA and EU models are all statistically significant. This is true after conditioning on GARP, and also after conditioning on F-GARP (see Tables A.3 and A.4 respectively).

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SAMUELSONIAN ECONOMICS

AND THE TWENTY-FIRST CENTURY

EDITED BY

Michael Szenberg, Lall Ramrattan, & Aron A. Gottesman

FOREWORD BY *Kenneth Arrow*

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Samuelsonian Economics and the Twenty-First Century

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Edited by
Michael Szenberg
Lall Ramrattan
Aron A. Gottesman

Foreword by Kenneth J. Arrow

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B'H

Dedicated to the memory of my sister, Esther;
to the memory of my parents, Henoch and Sara
to my children Naomi and Avi and
their spouses Marc and Tova;
to my grandchildren
Elki, Batya, Chanoch, Devorah, Ephraim, Ayala, and Jacob;
and to my wife, Miriam
-M.S.

To my wife, Noreena;
to my children Devi, Shanti, Hari, and Rani;
and to my grandchildren Soham and Lakshmi
-L.R.

To my wife Ronit, with adoration, and to my children
Libby, Yakov, Raphael, and Tzipora
-A.A.G.

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For us, being fully aware of Faraday's dictum "work finish publish", editing the volume has been done for pleasure and, in Samuel Taylor Coleridge's words, "the gratification of knowing."

The simplest things are often the most complicated to understand fully.

It is better to have a model with inexact foundations that gives you a good grip to handle reality than to wait for better foundations or to continue to use a model with good foundations that is not usefully relevant to explain the phenomena that we have to explain.

Paul A. Samuelson

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Foreword

Samuelsonian Economics and the 21st Century

Kenneth J. Arrow

I first encountered the work of Paul Samuelson during my graduate studies (1940–42) when my interests were changing from statistics and mathematics to economics. My mentor, Harold Hotelling, who was President of the Econometric Society, informed me that I had to join the society (at a student rate), and so I started reading *Econometrica* (Those early issues have a remarkably high fraction of papers that still resonate.) There I encountered several papers by Paul, particularly those defining and applying the concept of stability of economic equilibrium.

I was largely an autodidact when it came to economics, and, of course, having the skills and inclinations I did, economics to me meant economic theory using the tools of mathematics. Like Paul, I had profited greatly by happening on J. R. Hicks's *Value and Capital*. This work gave, like no other since Walras, an overview of the economic system, including, most importantly, its time dimension. For me, at least, it provided a map of the economic system into which individual issues would find their place. But it did not provide many specific results. My teacher, Hotelling, was primarily interested in statistics. His few papers on economics did deal deeply with a very limited range of special issues (the economic meaning of depreciation, nonrenewable resources, and spatial competition, most notably). Curiously, his own course dealt only with the theories of the consumer and the firm, the former one of several rediscoveries of Slutzky's ignored classic. (Considering its place of publication, an Italian actuarial journal, it is surprising that it was ever found before the modern technologies of retrieval.)

Paul's work combined breadth and intensity. On the one hand, his structures were grounded in a very wide knowledge of the nature of mathematical systems used to describe natural phenomena. On the other, he studied individual questions in economics, sometimes at a very detailed level.

The first underlay his pioneering and piercingly clear statement of the meaning of stability of competitive (or other) equilibrium in a world of many commodities and the relativity of the definition to a dynamic system of adjustment. The second was exemplified by his lengthy analysis of the meaning of constancy of marginal utility of income or, perhaps more usefully, by the multiplier-accelerator model.

The publication of *Foundations of Economic Analysis*, probably the most famous economics dissertation ever written, in 1948 was eagerly seized on by the growing community of young mathematical economists. I still have my well-worn copy; the much more elegantly printed expanded edition doesn't have the same resonance. But in fact it was only the beginning of Paul's contributions. The volume to which this is a Foreword contains detailed studies of the work of his long and stunningly productive career (the five volumes of *The Collected Scientific Papers of Paul A. Samuelson* contains 388 entries, but they carry us only until 1986), and there is no point duplicating them here.

In a review article (1967) on the first two volumes of *The Collected Scientific Papers*, I listed six categories of analyses which I thought to be his most vital contributions: (1) consumption theory, especially the revealed preference approach and the accompanying work on index numbers; (2) capital theory, both the pioneering work on intertemporal optimization and, even more novel and influential, the overlapping-generations model, which has been so stimulating to empirical work; (3) non-substitution theorems, both static and intertemporal; (4) factor-price equalization or, perhaps better put, the determination of factor prices by commodity prices, also a great inspiration for empirical studies; (5) stability analysis, especially of competitive equilibrium, which was followed by a great flowering of literature; and (6) formal analogies between dynamic systems and economic models. (Paul's knowledge of mathematics as applied to physical problems is clearly deep and acquired early; it has informed a significant body of his work throughout his career.) His subsequent work has added at least one more category of the same extraordinary level; the probabilistic analysis of securities prices and, more generally, reactions to uncertainty under repeated trials. The observations that there was little predictability of securities price changes worked powerfully on Paul's analytically-sharpened imagination. It led through a series of steps to a theory of warrant prices, which, in turn, inspired Fischer Black, Robert Merton, and Myron Scholes to the complete rethinking (or perhaps, first clear thinking) of the evolution of the prices of securities and their derivatives and, with that, a move to practical application unprecedented in the history of economics. Related to this was his challenge to naïve applications of the law of large numbers to willingness to undertake risks; it is by no means necessarily true that one should be more willing to take risks if one has many future opportunities to take the same risk. This line of study, in my judgment, is as important as Paul's other accomplishments.

There are a few remarks one can make about the character of Paul's writings. One is its pedagogical character. He frequently gives the feeling that he is concerned with explaining a point clear to him to a somewhat backward student. He remarks somewhere that his ideas were usually clarified by arguments with his teachers, so I assume they are still his target student audience.

Another outstanding characteristic of Paul's work is his strong and scholarly interest in the history of economic thought. This attitude shows up not merely in the many papers devoted explicitly to analysis of the doctrines of earlier economists, but even in many papers on new propositions in economics. Indeed, sometimes one has the feeling that earlier economists are among those in need of Paul's pedagogy, as he carefully explains their errors. But the pedagogy works very well for those of us listening in on the lecture.

In particular, Paul has devoted much attention, in fact fifteen of the *Collected Scientific Papers*, to the economics of Karl Marx, in particular, to the "transformation problem." This does seem a bit excessive. It is certainly not based on admiration for Marx as an economist. Perhaps it is another reflection of Paul's pedagogical ambitions; he has to repeat and expand his expositions because the audience has proved so obdurate.

Finally, I mention one interesting characteristic not only of Paul but of most, though not all, of the economic theorists in the second half of the twentieth century: the minimal influence of John Maynard Keynes on his and their work. Paul has repeatedly paid tribute to Keynes: "John Maynard Keynes was scientifically the greatest economist of this century. Only Adam Smith and Léon Walras can be mentioned in the same breath with him." Yet I look in vain in Paul's work for any serious studies of income analysis along Keynesian lines (the multiplier-accelerator model is the best that I can find, and Paul has ascribed the basic idea to Alvin Hansen). While, of course, others, like James Tobin, did construct rich models in which quantities played a central role in equilibrating savings and investment, I think on the whole the evidence is that Keynesian theory, for better or worse, is not a model rich in the kind of implications that competitive equilibrium theory has led to in the hands of a master like Paul. (Anyone brought up, as Paul and I were, in the Great Depression, may wonder whether analytic richness is a decisive criterion.) Indeed, the theoretically most interesting studies that emerged subsequently are those designed to criticize Keynesian conclusions, the new classical economics and the rational expectations theories (about which Paul has some kind words), as pursued by, among others, Robert Barro, Robert Lucas, Edmund Phelps, and Thomas Sargent.

Since I still have the floor, let me conclude by expressing my gratitude and that of the economics profession and of the world to which we have done a little good for all that Paul has created. Modern economics is inconceivable without his accomplishments.

Preface

The underlying notion in this volume is to spotlight, critically assess, and illuminate Paul A. Samuelson's extraordinarily voluminous, diverse, and groundbreaking contributions that encompass the entire field of economics through the lens of most eminent scholars. All this in honor of his ninetieth birthday celebrated on May 15, 2005 in Fairmont Hotel in Boston in the company of hundreds of scholars and their spouses. Samuelson, the first American economist to win the Nobel prize in 1970, the foremost voice in the second half of the twentieth century economics, set himself the task of creating a new way of presenting economics making it possible for it to be cast all in mathematical terms. He thus single-handedly transformed the discipline. What does it mean to transform a discipline? Wolpe inquires "What does it mean to follow the path of one's teacher?" A Hasidic story that Wolpe tells enlightens us. "Two students were disciples of the same rabbi. Years later they ran into each other. One had developed his own interpretations with their own nuances; he had even developed some of his own practices. The other had slavishly followed every word the teacher had spoken. The meticulous follower was angry with his less punctilious colleague. "How could you do this?" he asked. "How could you violate the way of our teacher?" The other responded, "Actually, I followed his way better than you. For he grew up and left his teacher. Now I have grown up and left mine."[1] In other words, distinguished teachers breed distinguished students who blaze new trails.

Sir Hans Krebs reports on a chart exhibited in the Munich Museum of Science and Technology summarizing the teacher–student genealogy of the Nobel laureates descended from Justus von Liebig, the founder of organic chemistry. The chart contains sixty individuals, all with important discoveries, and includes over thirty Nobel Prize winners.[2] A similar genealogy is unfolding in economics. As of now, five of Samuelson's students, George Akerlof, Thomas Schelling, Lawrence Klein, Robert Merton, and Joseph Stiglitz won the Nobel. The last three acted as his graduate research assistants. Samuelson's Harvard teacher, Wassily Leontief, received the Nobel in 1973.

Thomas Schelling tells us, "In 1946 I was enrolled in Joseph Schumpeter's 'advanced theory' course at Harvard. Half way through the semester Schumpeter asked whether Mr. Schelling was in attendance. I raised my hand to identify myself, and Schumpeter embarrassed me by suggesting I correct him whenever he made a mistake. Everybody looked at me, as perplexed as I was. I later visited him at his office to find out what it was all about. He said that my response to an exam question, a few weeks before, had been so professional and so superior to anybody else's that he figured he could count on me to keep him straight. I couldn't tell whether he was serious. Then he asked me how I'd learned so much about dynamic stability conditions. I answered that I had read Samuelson's *Foundations*. He said, 'Oh.' "[3]

Adrian Leverkuhn in Thomas Mann's *Doctor Faustus* declares, "There is basically one problem in the world and it is this: how do you break through? How do you get into the open?" Paul Samuelson not only broke through with singular virtuosity but he is being in Henry James' words "someone upon whom nothing is lost." Paul Samuelson serves as the secret and open inspiration for the generations of economists who follow him. He continues to weave new strands in economics without let up. As Frederico Fellini said, "If you do what you were born to do, you will never grow old."

Notes

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3. Personal correspondence, 2004, as previously quoted in Szenberg, M., A.A. Gottesman, and L. Ramrattan, Paul A. Samuelson: On Being an Economist. New York: Jorge Pinto Books, 2005.

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Ten Ways to Know Paul A. Samuelson*

> I do not know what I may appear to the world, but to myself
> I seem to have been only like a boy playing on the seashore,
> and diverting myself in now and then finding a prettier shell
> or a smoother pebble than ordinary whilst the great ocean
> of truth lay all undiscovered about me.
>
> —Isaac Newton

In his lectures, Paul often opens with anecdotes that serve as a light introduction for the substantive analysis that follows. In the spirit of his lectures, my talk will be a warm-up for the main event. I will present selected vignettes that portray Paul's personality and character with, I hope, insight and humor. To quote Nigel Rees: "An anecdote can often say more about a person than pages of biography" (1999, p. ix). An historian once noted that in time the legacy of any individual can be distilled into succinct sound bytes. Think of Presidents Washington, Lincoln, and Franklin D. Roosevelt.

10. Paul Samuelson is a Great Maestro

While Paul describes himself as having "an important role in the symphony orchestra," we see him as the conductor for the economists of the second half of the twentieth century. A first-rate university is neither made by brilliant students nor by brilliant teachers alone, but by the cheerful and fruitful interaction between the two. Paul's visible hands, gifted mind, and heart succeeded in not only attracting exceptional teachers and students to MIT, but in orchestrating a superbly-tuned ensemble which takes true interest in one another.

A Version of this talk was delivered by Michael Szenberg at the birthday celebration for Paul A. Samuelson on May 15, 2005 in Boston Massachusetts.

*Reprinted with permission from *Economics of Education Review* (Spring 2006).

A conductor's wife once asked Alexander Kipnis, the Russian basso, "What is it about Toscanini? What is it he does that my husband cannot do? Does he do something with his hands? Or with his eyes? Does he conduct faster? Or slower?" And Kipnis answered by quoting Gurnemanz's reply to Parsifal's query: "Who is the Grail?" The answer was: "That may not be told, but if you are chosen for it, you will not fail to know" (Sachs, 1991, p. 159).

In the world of music, it is a rarity to find a person who is both a gifted composer and a top conductor. So it is in economics as well. Paul is that rarity. When Paul is writing, the sun is always out. His writing—ever eloquent, ever stirring—is done with the kind of verve that one seldom finds today.

9. Paul Samuelson Lives a Balanced Life

There is a widely exaggerated and stereotyped notion shared by many that superior scientists can neither live a balanced life nor be paragons of virtue. Consider the words of William Butler Yeats, the poet: "The intellect of man is forced to choose perfection of the life or of the work," or those of David Hull: "The behavior that appears to be the most improper actually facilitates the manifest goals of science. . . . As it turns out, the least productive scientists tend to behave the most admirably, while those who make the greatest contributions just as frequently behave the most deplorably" (1988, p. 32). In other words, aggressiveness and selfishness are associated with superior performance by scientists. But my experiences and observations of eminent economists do not support these assertions. In Paul's case, not only does he know how to maintain a balance between scholarship, family, and play, but he exhibits a high degree of humanity and kindness. The term *mentsch* aptly describes him.

8. Paul Samuelson Knows How to Disagree Agreeably

The Houses of Friedman and Samuelson disagreed on both methodology and policy. Nevertheless, the intellectual battles never encroached upon their personal respect for and friendship to each other. In fact, Milton told me of how Paul influenced him in a long telephone conversation to accept the offer to write a column for *Newsweek*.

In ancient Israel there were two houses of learning, Hillel and Shamai, which fiercely disagreed with each other on most issues. We are told that, although they opposed each other, they respected each other; and their children married one another (*Yevamot*, Mishna 4). For my analogy to be complete, the grandchildren of Samuelson must marry the grandchildren of Friedman.

7. Paul Samuelson is Politically Savvy

Theodore Schultz, then chairman of the Economics Department at the University of Chicago, sought Samuelson as a counterbalance to the school's laissez faire philosophy. Schultz's argument to Paul was enticing: "We'll have two leading minds of different philosophical bent—you and Milton Friedman—and that will be fruitful." Paul tells us that he verbally accepted the offer initially, but changed his mind twenty four hours later, fearing that the position would force him to counterbalance Friedman by adopting leftist opinions that he didn't fully agree with.[1] Samuelson clearly defined himself as a centrist, rather than an advocate of a right- or left-wing philosophy. Also, he resisted requests by former Presidents John F. Kennedy and Lyndon B. Johnson to join the Council of Economic Advisers. As he said, "in the long-run the economic scholar works for the only coin worth having—our own applause" (Samuelson, 1962). Also, by distancing himself from politics, Paul can call the "shots as they really appear to be."

In 1952, Albert Einstein graciously declined the presidency of Israel. He later remarked "equations are more important to me because politics are for the present, but an equation is something for eternity" (Gelb, 1999, p. 323).

6. Paul Samuelson is Piercingly Witty

Einstein had this to say about fame: "Yesterday idolized, today hated and spat upon, tomorrow forgotten, and the day after tomorrow promoted to sainthood. The only salvation is a sense of humor" (Gelb, 1999, p. 322). Alan Brown tells us that when Paul was asked how many children he had, he responded: "First we got one, then we got two, then we got three, then we got scared."[2] Avinash Dixit recalls Paul's humorous description of Joan Robinson's visit to the United States: "She was taken in a sealed train from coast to coast—from Paul Baran to Paul Sweezy."[3] Jagdish Bhagwati relates a story about Paul's encounter with the British economist, Lord Thomas

Balogh. Paul "once traveled from Heathrow airport to a party in Oxford; he walked up to Balogh and said: 'Tommy, I have just been reading the *Financial Times* and I find that someone has signed your name to a terrible article; you must do something about it!' "[4] Indeed, Paul's capacity for irreverence and wit is true to the John Maynard Keynes maxim: "Words ought to be a little wild, for they are the assault of thoughts on the unthinking" (June 1933, p. 761).

5. Paul Samuelson is Human

Paul has no problem with wealth, yet he advocates positions that work against his personal economic interests. He notes that, while advocating the closing of tax loopholes, he has no difficulty taking advantage of those that remain. Fundamentally, Paul represents a middle path, as expressed in the ancient dictum that "men with vision walk in the middle."[5]

Paul once remarked that the only fault of a certain person was that he had a loose string on his coat. One of the most famous loose strings on Paul's own coat has to do with his 1944 prediction in the *New Republic* that foretold of greater unemployment following the end of the Second World War, a prediction that subsequently was proven incorrect. This is reminiscent of Nathan Milstein, the famous violinist, who was approached by an admirer and asked to play a false note, only to prove his humanity.

4. Paul Samuelson is Unique

In ancient times, sages argued whether vast knowledge and erudition take precedence over brilliant depth and sharp dialectics. The argument was inconclusive and ended in a draw. Archilochus, the pre-Socratic philosopher, expressed this enigmatically: "The fox knows many things, but the hedgehog knows one big thing." There are economists who are hedgehogs, who search for scientific insights by turning their critical lens toward a few selected areas. Others, being more fox-like, drive their talents into many directions. Paul has reached immortal stature in the history of economics by being both a hedgehog and a fox. He possesses a genius that covers in its depth and breadth many areas within economics. Will Baumol describes him as a jack of all trades and a master of every one.[6] It is rare, indeed, to find conversation, correspondence, and scholarship so well blended in one person.

In contrast to the natural sciences, where Isaac Newton and Albert Einstein made their major contributions, most economics masterpieces were written when the authors were middle aged. Adam Smith, Karl Marx, John Maynard Keynes, and Milton Friedman come to mind. However, Paul started much earlier, in his twenties; and, even now, his new articles influence the fields of economics and finance.

Furthermore, not only is he a master of economics, his vast knowledge is far-reaching. For example, János Kornai tells us "He *knows* history. If he had a Hungarian sitting at his side at the dinner table, he would quote easily names of politicians or novelists of the Austrian–Hungarian empire of the late nineteenth century. He also *understands the significance* of the history of a country. This is a rare quality at a time when the education of economists has become excessively technical."[7]

Similarly, Bengt Holmstrom recalls a dinner at his house for a group of young faculty members, which Paul attended as well. "Between meals I arranged a light, informal trivia competition. Had answers been counted, he would have won hands down. He even knew the third president—of Finland—a question I threw in as a joke."[8]

With so much encyclopedic knowledge at his disposal, there is one challenge left for Paul: to appear on *Jeopardy* or *Who Wants to be a Millionaire?* His appearance, however, would probably bankrupt the American television network (American Broadcasting Company) ABC.

3. Paul Samuelson is a Mentor

When Paul's *Foundations* was published in the 1940s, readers experienced a kind of revelation that created a sensation. This brings to mind the post-First World War public's similar reaction to Marcel Proust's *Remembrance of Things Past*. Critics compared Proust's prolific writings to those of Homer, Dante, and Shakespeare. But, what a difference in Proust's and Paul's dispositions!

The attitude and the embrace by Paul of younger scholars lead to the nurturing and development of great minds, many of whom are seated in this hall. It is revealing to contrast his actions with those of Marcel Proust. Whenever Proust was asked to evaluate a manuscript, he always enclosed the following letter: "Divine work. It is a work of genius. I would not change a word. I take my hat off for you. All the best, Marcel Proust."[9] The novelist wrote the same laudatory note to all potential writers who contacted him. When confronted about what he was doing, Proust said that he did not have time to read the submitted material because it interfered with his writing. By telling young authors that their work was that of a genius, he made sure that they would not return their revised papers to him with changes. Interestingly, when Proust first wrote *Remembrance*, his monumental seven-volume tome, no one would publish it. In fact, he had to use his own funds to publish the first volume. Yet, this experience did nothing to fill Proust's heart with empathy for young writers or to enhance his reverence for the human spirit. Proust's behavior, though amusing and seemingly innocent, illustrates the gross impediments the turn of the century classical author was willing to place before fledgling writers and thereby violate an important moral principle, "Before the blind do not put a stumbling block" (Leviticus 19:14). Paul would find such a deceitful act abhorrent.

Perry Mehrling of Columbia University told me a characteristic story of Paul. "[Paul] mentioned that he had heard about a piece I had written on Irving Fisher. I have no idea how he heard about it, but I offered to send him a copy and within a few days I got back a letter. [Paul] read the paper and wanted to set down his own interpretation, but then he closes the letter with a remarkable line that I treasure: 'Do disregard my heresies and follow your own star.' "[10] It is remarkable that you, Paul, extended the same type of support and nurturing to me and other economists who occupy the back benches of the academy.

2. Paul Samuelson is a Pioneer

André Gide remarked that "I will maintain that an artist needs this: a special world of which he alone has the key" (1948, p. 77). In the world of the physical sciences, it was Isaac Newton who used mathematics to unravel the mystery of the universe. So did Paul in economics by moving economic methodology from Marshall's diagrammatic presentations to the present-day quantitative approaches. He single-handedly and fruitfully rewrote the theory of many branches of economics. Among his greatest contributions is his neoclassical synthesis. As such, he broadened the discipline, deepened it, and opened the doors to others. As Paul said to the students present at the Nobel banquet, "You are the posterity we work for. I can assure you that we are bestowing on you the most glorious gift of all—plenty of difficult problems still unsolved."[11] As a pioneer, Paul adhered to Ralph Waldo Emerson's tenet that one should not "follow where the path may lead, [but] go, instead, where there is no path and leave a trail." To rephrase what Cicero wrote of Socrates, Paul called down modern economics from the skies and implanted it in the universities throughout the world (Gelb, 1999, p. 15).

1. Paul Samuelson Continues to Contribute

What is amazing about Paul is that his life's work continues even today. What trumpet player Clark Terry stated of Duke Ellington applies equally well to Paul; "He wants life and music to be always in a state of becoming. He doesn't even like to write definitive endings to a piece. He'd often ask us to come up with ideas for closings, but when he'd settle on one of them, he'd keep fooling with it. He always likes to make the end of a song sound as if it's still going somewhere" (Hentoff, 2004, p. xix).

We are drawn to thinkers, musicians, and scientists who are in a constant state of becoming. When Pablo Casals, the famous cellist, was asked why he continued to practice four hours a day at the age of ninety-three, he said, "Because I think I can still make some progress" (Szenberg, 1998, p. 17). Similarly, Michelangelo frequently used the phrase *Ancora Imparo*, I am still learning, as he continued in his old age to perfect his masterpieces.

Because there is this tension between striving for perfection and never truly reaching it, no scientist or artist is ever satisfied or ever stops working. We owe to Martha Graham the following central insight on the subject: "There is no satisfaction whatever at any time. There is only a queer, divine dissatisfaction; a blessed unrest that keeps us marching and makes us more alive than the others."[12]

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Notes

1. Interview, July 1, 2004.
2. Personal Correspondence, 2004.
3. Personal Correspondence, 2004.
4. Personal Correspondence, 2004.
5. Tosefta: *Baba Kama*, 2.12.
6. In his unpublished essay, "Generalists' Generalissimo".
7. Personal Correspondence, 2004.
8. Personal Correspondence, 2004.
9. *Maariv*, "Literary Supplement," weekend edition in the 1990s.
10. Personal Correspondence, 2004.
11. Paul A. Samuelson, Nobel Banquet Speech, December 10, 1970; available at http://nobelprize.org/economics/laureates/1970/samuelson-speech.html
12. From a letter to Agnes de Mille, undated.

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Introduction: The Significance of Paul A. Samuelson in the Twenty-First Century

The contributors to this volume had unambiguous foundations upon which to build, due to Samuelson's use of mathematics as a language, "physics as the science for economics to imitate" (Hayek, 1992, p. 5), and reality over theory as a paradigm. Samuelson's early and later writings are consistent and somewhat invariant, and in harmony with Adam Smith's maximizing individual in society. Samuelson asked: "What is it that the scientist finds useful in being able to relate a positive description of behavior to the solution of a maximizing problem? That is what a good deal of my own early work was about. From the time of my first papers on 'Revealed preference' . . . through the completion of *Foundations of Economic Analysis*, I found this a fascinating subject . . . my positive descriptive relations could be interpreted as the necessary and sufficient conditions of a well-defined maximum problem" (Samuelson, 1972, p. 3).

Samuelson developed broad frameworks such as the neoclassical synthesis, a mixed economy, and the surrogate production function, which provided practitioners with a vision for research. His contributions to economics are rich, complex, heavy with facts, consequential, and relevant to the ordinary economics of life. Because of the quality of Samuelson's output and methods, the contributors to this volume see a near complete success for his theories in the twenty-first century.

Many of the contributors have defended Samuelson's work elsewhere. Now they have gathered to appraise the relevance of his work in the twenty-first century. Robert Solow explicitly states that Samuelson's Overlapping Generations Model (OLG) had slipped through the cracks in a previous work, which he now seeks to remedy in this volume. Luigi Pasinetti demonstrates elements of similarity between Samuelson's and Piero Sraffa's writings to explain why they were friends and not enemies, as would be expected. Geoff Harcourt discusses Samuelson's repeated and vexed interest in Karl Marx's approaches, and identifies an "Aha!" moment about how Samuelson treats the transformation problem.

Appraising Samuelson: Units of Appraisal

As "Archimedes' lever is useless without a fulcrum to rest it on, and . . . angels need the point of a needle to dance upon" (Samuelson, 1978, p. 790), so too do we need a template against which to appraise the significance of Samuelson's writings for twenty-first century economists. Samuelson provided some insight when he argued that in order to appraise Alfred Marshall's originality we must take into consideration economists such as John Stuart Mill and Antoine-Augustin Cournot, whose contributions Marshall knew well (Samuelson, 1972, p. 22). If we were to sample the writings that Samuelson knows well, then mathematicians such as Henri Poincare, Frank Ramsey, and John von Neumann, scientists such as Albert Einstein, James Clerk Maxwell, and Henri-Louis Le Chatelier, philosophers such as Ernst Mach, Karl Popper, and Thomas Kuhn, and economists such as Adam Smith, David Ricardo, Karl Marx, and John Maynard Keynes would be included.

In awarding the Nobel Prize to Samuelson in 1970, the Nobel committee identified works that were worthy of appraisal. They were "*scientific work through which he has developed static and dynamic economic theory and actively contributed to raising the level of analysis in economic science*." These include Samuelson's novel view that "under free trade both parties are better off than under no trade at all, but are not necessarily in the optimum position" (Samuelson, 1966, p. 779); his Le Chatelier principle that explains how an economic system that is in equilibrium will react to a perturbation; his Samuelson–Bergson utility function that measures welfare gains; and his factor price equalization theory, which as John Hicks pointed out, "if there is a removal, not only of the obstacles to free trade, but also of the obstacles of factor movement . . . the two economies then become virtually one economy" (Hicks, 1983, p. 235). Those enumerated novelties are in addition to his neoclassical synthesis, revealed preference, multiplier-accelerator, and surrogate production function models. These novel contributions have already withstood the rigorous tests of coherency, consistency, falsification, and pragmatism, and have become objective and valid scientific achievements because they are open for revision and criticism. For over half a century, Samuelson's contributions have held up well with much cross-fertilization from other areas. In sum, as Kenneth Arrow puts it, "Samuelson is one of the greatest economic theorists of all time" (Arrow, 1967, p. 735). He should, therefore, be appraised as such.

An appraisal is different from a mere description. It appraises why a theory is superior and does not bother about how to construct an even more superior theory (Latsis, 1980, p. 3). Samuelson's hypotheses fit in this appraisal schema because they are scientific. Karl Popper draws a separating line between the "empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical systems' on the other," and called this separation "the *problem of demarcation*" (Popper, 1968, p. 35). The fact that mathematics is included in the nonscience side should not be perceived as having a negative implication for economics and Samuelson's work. T. W. Hutchison cites Popper as saying that "The success of mathematical economics shows that one social science at least has gone through its Newtonian revolution." He adds that "surely such an outstanding post-Newtonian salient would deserve the closest analysis and appraisal from philosophers of science" (Hutchison, 1980, p. 187). Popper fuses the idea of "corroboration" with the idea of "appraisal" when he notes that "corroboration can only be expressed as an appraisal" (Popper, 1968, p. 265). Mark Blaug states that "By the degree of corroboration of a theory, I mean a concise report evaluating the state (at a certain time *t*) of the critical discussion of a theory, with respect to the way it solves its problems; its degree of testability; the severity of the test it has undergone; and the way it stood up to these tests. Corroboration (or degree of corroboration) is thus an evaluating *report of past performance*" (Blaug, 1983, p. 26).

A "corroborative appraisal" establishes a fundamental relationship between accepted basic statements and the hypothesis (Popper, 1968, p. 266). Hypotheses "are 'provisional conjectures' (or something of the sort); and this view too, can only be expressed by way of an appraisal of these hypotheses" (Popper, 1968, p. 265). "From a new idea . . . conclusions are drawn by means of logical deduction. These conclusions are then compared with one another and with other relevant statements, so as to find what logical relations (such as equivalence, derivability, compatibility, or incompatibility) exist between them" (Popper, 1968, p. 32). Therefore, Popper's demarcation criteria are the "standards for appraising competing scientific hypotheses in terms of their degrees of verisimilitude" (Blaug, 1983, p. 10). Hypotheses must be subjected to a severe test and found compatible, that is, not falsified. The testing procedure is fourfold: (1) A test of the internal consistency of the system by comparing conclusions among themselves, (2) A test of the logical form of the theory to see if it is empirical or tautological, (3) A comparison of the theory with others to see whether it truly is a "scientific advance should it survive our various tests," and (4) A test of the theory "by way of empirical applications of the conclusions which can be derived from it" (Blaug, 1983, pp. 32–34). The overall implication is that if Samuelson's theories are incompatible, we can regard them as falsified. If they are compatible, then we might give them some degree of "positive corroboration," but that will depend on the "severity of the various tests" that the hypothesis has passed (Blaug, 1983, pp. 266–267). A positive degree of corroboration is like a nested function: Positive corroboration = f [severity of test = g (degree of testability = h {simplicity of the hypothesis})].

Although Samuelson has some grand unifying scientific theories such as the neoclassical synthesis, he has also made particular scientific contributions to many subdisciplines in economics that need to be appraised as well. Fortunately, appraisal methodology comes in "units" as well as in bundles of "units." Imre Lakatos, a student of Popper, called units of appraisal in science a "research program" or a series of connected theories, rather than a single theory (Lakatos and Musgrave, 1977). A single theory can be falsified when only one instance of refutation appears. However, a research program is not easily falsified. It has theories in its "hard core" that practitioners are not willing to abandon, and theories in its "protective belt" that practitioners are interested in improving. The Duhem–Quine hypothesis also embraces units of appraisal. It holds that the incompatibility of one consequence does not falsify all of the antecedents (Quine, 1990, pp. 13–14). For example, if one finds that the paradox of thrift hypothesis is incompatible, one does not have to give up the neoclassical synthesis. For Samuelson, such units of appraisal are exemplified in the roles that mathematics and facts play in economics. Building on Pareto's idea that mathematics represents complexly interacting and independent phenomena, Samuelson adds that "after mathematical notions have performed the function of reminding us that everything depends upon everything else, they may not add very much more—unless some special hypothesis can be made about the facts" (Samuelson, 1966, p. 1758).

Besides the above broad methodological perspectives, Samuelson can be appraised as a "craftsman" using his personal knowledge to improve economic science. A "personal appraisal" holds that "in every act of knowing there enters a passionate contribution of the person knowing what is being known" (Polanyi, 1958, p. viii). As Jerome R. Ravetz (1979, p. 75) frames the problem, we are interested in how "objective scientific knowledge can result from the intensely personal and fallible endeavor of creative scientific inquiry." On the craftsman's side of appraisal, Samuelson has certainly demonstrated high "morale" defined as "any positive and energetic attitude toward a goal" (Bateson and Mead, 1941, p. 206). The contributors of this book are living proof of successful "morale transfer," and good "morale resonance" among colleagues and students with Samuelson's craftsmanship. But besides craftsmanship and good morale, Samuelson can be appraised from the perspective of "universally valid appraisals" (Polanyi, 1958, p. 22), "systems of appraisals" (Polanyi, 1958, p. 43), and "appraisal of order" (Polanyi, 1958, p. 36), the criteria of which include "(1) a correct satisfaction of normal standards, (2) a mistaken satisfaction of normal standards, and (3) action or perception satisfying subjective, illusory standards" (Polanyi, 1958, p. 363). The neoclassical syntheses and intergenerational models are objective and valid scientific achievements because they are open for revision and criticism from everyone, and have had significant and verified progress thus far.

Samuelson should further be appraised for his adoption of successful epistemological viewpoints in his scientific approach to economics. The big "M" approach of Rod Cross places Samuelson's works beside those of Mach. Samuelson's method is also consistent with the epistemological approach of Poincare and Einstein. Poincare would build up science from lower level hypotheses, such as Galileo Galilei's one-dimensional to two-dimensional motions of falling bodies, ending up with Isaac Newton's laws. Einstein would make inference statements from axioms, but in moving from axiom to inference statements one has a clear link with the empirical world of data and experiments, including mental (*Gedanken*) experiments (Miller, 1984, pp. 39–46). Such research programs are geared to find novel facts. "Einstein's program . . . made the stunning prediction that if one measures the distance between two stars in the night and if one measures the distance between them during the day (when they are visible during an eclipse of the sun), the two measurements will be different" (Lakatos, 1980, p. 5). As Popper indicates, "stars close to the sun would look as if they had moved a little away from the sun, and from one another" (Popper, 1963, p. 37). This is indeed a novel prediction of Einstein's program. For Poincare, "facts outstrip us, and we can never overtake them; while the scientist is discovering one fact, millions and millions are produced in every cubic inch of his body" (Poincare, 1908, p. 16). We must therefore select facts. "The most interesting facts are those which can be used several times, those which have a chance of recurring" (Poincare, 1908, p. 17). Recurring facts are "simple" such as the stars, the atoms, and the cell. In short, simple facts lie "in the two extremes, in the infinitely great and in the infinitely small" (Poincare, 1908, 18–19). One is reminded of Samuelson's Simple Mathematics of Income Determination, Surrogate Production Function, and simple 2×2 trade models. To sum up, in Samuelson's own words, "we theorists like to work with extreme polar cases, what is the natural model to formulate so as to give strongest emphasis to external effect?" (Samuelson, 1966, p. 1235).

On the empirical side, Samuelson is primarily appraised for his study of the mixed economy. "The mixed economy is not a very definite concept. I have purposely left it vague, in part because that is its intrinsic nature and in part because increased precision should come at the end rather than at the beginning of extensive research" (Samuelson, 1966, p. 632). Further, "A mixed economy in a society where people are by custom tolerant of differences in opinion, may provide greater personal freedom and security of expression than does a purer price economy where people are less tolerant" (Samuelson, 1972, p. 628). Even though "History oscillates, backtracks, and spirals" (Samuelson, 1972, p. 612), in the end the mixed economy emerges because it is based on reality. We know that Samuelson nourishes his foresight with strong doses of the reality paradigm, for he said that "I would take aid from the Devil if that would help crack the puzzle of economic reality" (Samuelson, 1986, p. 873), and that "it is better to have a model with inexact foundations that gives you a good grip to handle reality than to wait for better foundations or to continue to use a model with good foundations that is not usefully relevant to explain the phenomena that we have to explain" (Samuelson, 1986, p. 295). In short, "A good economist has good judgment about economic reality" (Samuelson, 1972, p. 775).

On the theoretical side, Samuelson is also most appraised for how his theories explain reality. For his theories to survive in the twenty-first century, they must solve and explain problems and anomalies in a normal scientific way. Normal science will require us to: (1) unearth economic values; (2) compare theories T_n with T_{n-1} to see which one performs better; and (3) overthrow or re-specify degenerating theories.[1]

Samuelson's neoclassical synthesis can be used to appraise different theories to see which is better. Take the demand versus supply-side theories of the 1970s and 1980s for example. The demand-side theory was well tested in the 1970s against the stagflation problem, and some economists charged that it was weakening. Other Keynesian ideas such as the paradox of thrift were eliminated from some of the newer editions of Samuelson's own book, *Economics*. Some major principle textbooks dropped the Keynesian IS/LM and the Keynesian-cross diagrams. However, the hard-core elements of the demand-side system such as the mixed economy, wage rigidity, and interest inelasticity remain intact, and its future seems assured: "the underlying framework for a new period of creative consensus in economic thought . . . will be a newly appraised balance between the public and private sectors in which the role of the former is considerably elevated over its earlier status" (Heilbroner and Milberg, 1995, p. 119). Newer policy problems such as targeted interest rate policies and monetary policies within the EMU saw the need to keep the Keynesian modified paradigm within the Mundell–Fleming BOP/IS/LM model, and its implied Keynesian-cross framework. This example underscores the fact that the Keynesian paradigm has a strong heuristic power to suggest ways of solving a wide variety of problems; that it is theoretically progressive in that it will yield more testable content in T_n than in T_{n-1}; and that it is empirically progressive, for at least some of its additional testable consequences can be confirmed.

As an appraisal of Samuelson's ability to re-specify a weakening theory, let us consider his improvement of the Heckscher–Ohlin theory. As a criterion for a better theory, one may be content to accept von Mises' "Science and Value" concept, "a situation in which a given amount of capital and labor was able to produce a definite quantity of material economic goods 'better' than a situation in which the same amount could produce only a smaller quantity" (von Mises, 1960, p. 36). But Samuelson would not settle there: "What Samuelson did was graft Ohlin's trade theory and the problems connected with its rigorous articulation and generalization onto the mainline research tradition concerned with the conditions governing the existence, uniqueness and stability of general competitive equilibrium" (De Marchi, 1976, pp. 112–113).

To sum up, we have been very restrictive in our appraisal of Samuelson's work for the twenty-first century, limiting our examples to only the mixed economy and policy combinations. The contributors to this volume are more impressive in their predictions and explanations of their topics, which include ideas, theories, and facts.

Samuelson Appraised Through His Own Methodology

In at least two articles, Samuelson uses the word "appraisal" explicitly in the title. In one, "Economics of Futures Contracts on Basic Macroeconomic Indexes: An Economist's Appraisal" (Samuelson, 1986, p. 557), he reaches the conclusion that "Economists' theories will be better when they can perceive what the *expected rate of inflation* really is.... What is infinitely more important, the players in the economic game... will be better off when the exchange institutions evolve that help to signal risks, reduce them, and allocate the residual irreducible risks optimally"

(Samuelson, 1986, p. 558). In the other, "2 Nobel Laureates' Theories on Trade: An Appraisal" (Samuelson, 1986, p. 831), where, after his appraisal of the contributions of J. E. Meade and Bert Ohlin, he reaches the conclusion that "Each man has demonstrated that those who are best at pure science are often outstanding policy advisers and public servants" (Samuelson, 1986, p. 831).

One can surmise that Samuelson uses the term "appraisal" broadly in accordance with the methodologies described above. It could include his way of appraising two competing paradigms, such as when he writes: "The two paradigms seem to tell different stories. Which is the relevant story, the correct one for the case of competitive capitalism? There can be no doubt as to the answer. The bourgeois paradigm of Smith, Ricardo, Piero Sraffa, and Leon Walras correctly predicts that the new invention will displace the old technique under ruthless competition" (Samuelson, 1986, p. 365). It could also mean finding necessary and sufficient conditions, and logics to appraise a theory: "Here I provide necessary and sufficient conditions for price invariance in the presence of exponentially depreciating durable capital goods. The result is a surprisingly simple criterion: price ratios are invariant to interest rate changes if and only if all industries have . . . the same capital-to-labor ratio" (Samuelson, 1986, p. 375).[2]

Fritz Machlup distinguishes three streams of Samuelson's methodology: (1) theories, antecedents, and consequents that must be mutually implicative, and identical in meaning; (2) strong simple cases like the 2×2 trade model that bring out elements of truth in a complex theory; and (3) methods as advocated in his *Foundations of Economic Analysis* that emphasize the derivation of "operationally meaningful" theorems (Samuelson, 1972, p. 758). We look briefly at these three streams below.

Mutually Implicative Theories

Regarding mutually implicative theories, Samuelson wrote that "after mathematical notions have performed the function of reminding us that everything depends upon everything else, they may not add very much more—unless some special hypothesis can be made about the facts" (Samuelson, 1966, p. 1758). Again, "mathematics is neither a necessary nor a sufficient condition for a fruitful career in economic theory" (Samuelson, 1966, p. 1760). Furthermore, "Marshall in his own way also rather pooh-poohed the use of mathematics. But he regarded it as a way of arriving at the truths, but not as a good way of communicating such truths" (Samuelson, 1966, p. 1755).

2 × 2 Trade Theory

Nowhere is the process of scientific appraisal more clearly demonstrated than in the trade theory of economics. Trade theory, from mercantilism to Adam Smith's absolute advantage, and from Ricardo's comparative advantage to Heckscher–Ohlin's factor proportion–intensity assumptions, have been now standardized in a 2 × 2 form (two goods and two factors). Samuelson's place in this research is secured when he demonstrates that "Both the classical and Ohlin versions of the explanation of trade may thus be viewed as adaptations of a common general equilibrium framework" (De Marchi, 1976, p. 112). Samuelson has shown that "the assumptions sufficient to yield factor price equalization also suffice . . . to yield the Heckscher-Ohlin theorem" (De Marchi, 1976, p. 112). In scientific parlance, Samuelson has shifted the research in trade theory from "before Samuelson" to "after Samuelson." This shift has made it possible for "rigorous articulation and generalization on the mainline research tradition concerned with the conditions governing the existence, uniqueness, and stability of general competitive equilibrium" (De Marchi, 1976, p. 113). Therefore, under the idea of appraisal that asks "Is theory A better than theory B?" (Latsis, 1976, p. 3) it is fair to say that Samuelson's research on the 2 × 2 trade theory is poised for scientific conjectures and refutations in the twenty-first century.

Operationalism

A byproduct of Einstein's Special Theory of Relativity is that it allows us to define a term by the physical operations the agent performs in order to observe the object. Any concept corresponds with a set of operations. Samuelson seeks "the derivation of operationally meaningful theorems" in economics (Samuelson, 1947, p. 3). One aim of meaningful theorems is that we can ascertain their truth values, the likelihood that they will occur in reality, and that we can attempt to verify them (Machlup, 1978, pp. 165–166). Another view is that through these operational theorems, we may not "verify" but "infer" the object.

According to Machlup's argument, operationalism purges a theory of its assumptions, and therefore of its theoretical and mathematical framework. Friedman's F-twist theory emphasizes the independence of assumptions from prediction. Others, such as Bateson and Mead (1941, p. 55), think that "whenever we start insisting too hard upon 'operationalism' or symbolic logic or any other of these very essential systems of tramlines, we lose something of the ability to think new thoughts. And equally, of course, whenever we rebel against the sterile rigidity of formal thought and exposition and let our ideas run wild, we likewise lose. As I see it, the advances in scientific thought come from a *combination of loose and strict thinking*, and this combination is the most precious tool of science."

To cut through the hurdles of the various brandings of his methodology, Samuelson reminds us that he is a truth seeker. On the one hand he argues for reality as the paradigm, but on the other hand he holds that "observations are not merely seen or sensed but rather often are perceived in gestalt patterns that impose themselves on the data and even distort those data" (Samuelson, 1993, p. 244). Here we have the interplay between theory and fact. We know that his basic paradigm of facts is not based on dialectic but on the cumulative method.[3] As Samuelson puts it, "in the language of [Thomas] Kuhn, knowledge in economics accumulates, and paradigms can be commensurable only if the 'black' in one paradigm can be considered as the 'white' in another paradigm" (Samuelson, 1993, p. 244).

Analysis of Samuelson's Specific Contributions

In this section, we look at some of Samuelson's specific contributions to highly specialized topics in economics. The idea is to underscore the essence of the contributions in relation to the respective authors' appraisal as to why the material will be relevant for the twenty-first century.

Overlapping Generation Models

The first section of this book contains three chapters:

- "Overlapping Generations," Robert M. Solow
- "Paul Samuelson's Amazing Intergenerational Transfer," Laurence J. Kotlikoff
- "Social Security, the Government Budget, and National Savings," Peter Diamond

Samuelson introduced the overlapping generations model (OLG) to "develop the equilibrium conditions for a rational consumer's lifetime consumption-saving pattern" (Samuelson, 1958, p. 104). Since that time, the OLG has become a strong rival theory to the Arrow–Debreu general equilibrium model, extending theoretical and empirical research to areas that previously could not be reached.

Solow's description of the original Samuelson model invites the distinction between theoretical and practical uses of the model. Essentially, OLG has an overlapping generation structure, which operates in infinite time. Although no generation can step into the same time twice, a person in one generation, G^t, can trade with a person in another generation, G^{t-1}, G^{t-2}, etc., where a "household born at *t* will be said to be of generation *t*, or simply G^t" (Hahn and Solow, 1995, p. 13). For example, a person in a three-period model that wants to save for retirement in the next period can lend to a person in the first period. This is essential because the model deals with generations which are defined in terms of age, and people have different tastes and preferences over different age cohorts.

A similar model is found in the appendix of Maurice Allais' book, *Économie et Intérêt* (1947). Both Solow and Malinvaud (1987) essentially agree by implication. They agree that time is infinite in both cases, but Allais uses two periods and Samuelson uses three periods. In Allais' model, the presence of Government is as a wealth holder and/or a debtor, say of land and financial assets, following a variety of hypotheses (Malinvaud, 1987, p. 104). According to Kotlikoff, in Samuelson's model, "government can redistribute across generations." The interest rate in Allais' model "is not fully determined by the individual's preference for the present and by technical feasibilities" (Malinvaud, 1987, p. 104), and, according to Samuelson, it can have a biological dimension that results when a correlation between prices and population changes leads to equality between the interest rate and population growth rate.

The interest rate as a bridge between current and postponed consumption appears as a time cohort model. To turn this into a real OLG model, we have to layer it with people in different age cohorts. Using the didactics of Samuelson's three-period model, the middle generation may want to lend excess current income (savings) to the younger generation, but not to the old, for when the middle generation becomes old, it wants to consume its savings plus earned interest. Therefore, consumption, c, and savings, s, cannot exceed wealth, w, that is, $c + s \leq w$. Solow's chapter continues in this line of symbolic generalization, originating in his joint paper with Frank Hahn, to illustrate market imperfections (Hahn and Solow, 1995, p. 13).

Kotlikoff's work has given rise to a new term in the expansion and articulation of the OLG model, particularly in generational accounting. Both Kotlikoff and Diamond take up current and future concerns of the Social Security problem, a good indication of the relevance of the model for the twenty-first century.

Expectation, Uncertainty and Public Goods

The second section of this book contains two articles:

- Prospective Shifts, Speculative Swings: "Macro" for the 21st Century in the Tradition Championed by Paul Samuelson, Edmund S. Phelps.
- "Paul Samuelson and Global Public Goods," William D. Nordhaus

Edmund Phelps appraises Samuelson from the point of view of expectation and uncertainty. Although his multiplier-accelerator contribution to Keynesian modeling became famous, his interest in macroeconomics ran deep and spanned the perspectives of theorists before and after Keynes. Ever interested in real-life economies, Samuelson was alert to the occasional sea change in the American economy. In the 1970s, he invented the term "stagflation" to stand for the ratcheting down to slower growth and increased unemployment amidst no lessening of inflation. Evidently parametric shifts were occurring, from the world price of energy to the exhaustion of the stock of unused ideas. Comparing this history with the stationary rational-expectations models becoming fashionable in the 1980s, Samuelson concluded that "[a]s a description of what happens in the real world and as a tool for intermediate-run macro predictions, the Lucas-Sargent-Barro model is a poor tool" (Samuelson 1986, Vol. V, 294).

Samuelson's concepts pertaining to a business economy – its orientation toward the future, the uncertainty of future prospect and the non-stationarity of its demographic and technological environment – were tools, Phelps argues, that Samuelson used and are tools we need in this century for an understanding of the secular shifts and big swings in the American economy. Samuelson does not see rational expectations as taking root in an enterprise economy. Samuelson, Phelps acknowledges, was "a pioneer of rational expectations theory" and willing to postulate rational expectations for the purpose of explaining differences in share prices across categories over a given period; and the "micro efficiency" he sees in those markets, if he is right, may justify the postulate for that purpose. Yet Samuelson finds "macro inefficiency" in the index of share prices. Thus rational expectations appear not to describe the ups and downs of the values entrepreneurs in aggregate put on new projects (and the values that retails investors place on the shares financing those projects).

Phelps, supporting Samuelson's views, points out that an enterprising economy is driven by the "visions" and "fears" of entrepreneurs, financiers and speculators. The postulate of rational expectations, Phelps argues, is simply inapplicable to the expectations of entrepreneurs peering into the unknown. Furthermore, an outsider in the public has no way of forming rational expectations of the expectations of the entrepreneurs: Their knowledge is importantly "personal knowledge," which the public cannot access. And the public infer from its data available what the entrepreneurs must be expecting, for to suppose that their expectations could be the subject of unbiased estimates by the public would be to imply that entrepreneurs have no special knowledge and play no special role.

This piece recalls Keynes's point of view. In its essence, Samuelson's brand of expectation and uncertainty subscribes to Chapter 12 of Keynes's *The General Theory*. Keynes exposed a disequilibrium paradigm, in contrast to the rational expectation models that exposed an equilibrium paradigm.

Following Phelps' presentation, we need to understand the effects of the "visions and fears" excluded by the now-standard paradigm theory. Phelps examines the effects of expectations regarding three future events – future "debt bombs," future productivity surges, and war prospects. An economy continually pinged by such expectations is "never 'vibrating' up and down its saddle path"(Ibid.). Samuelsonian economics of the 21st century will emphasize that "macroeconomics must incorporate future prospects if it is to capture the big swings in economic activities." It will preserve the perspective that the rational-expectations movement excluded.

The distinction between public and private goods follows from concerns about market efficiency concepts. Nordhaus' piece fits in with the appraisal of polar cases of fact from an epistemological appraisal point of view. A public good, "for which the cost of extending the service to an additional person is zero," is "a polar case of an externality," says Nordhaus. Externality, nonrivalry, and nonexcludability are budding research programs, which will be of great concern for the twenty-first century because "private markets generally do not guarantee efficient outcomes." The "stock externalities" concern for public goods, particularly with regard to nuclear energy and greenhouse gases that have a firm hold in the twenty-first century policy concerns, and have implications for the course that international laws will take.

Revealed Preference and Consumer Behavior

There are two chapters in the third section of this book:

- "Revealed Preference," Hal R. Varian
- "Samuelson's 'Dr Jekyll and Mrs Jekyll' Problem: A Difficulty in the Concept of the Consumer," Robert A. Pollak

The explosion of research on revealed preference still builds on the foundations which Samuelson constructed. One foundation is the weak axiom of consumer behavior, another is the strong axiom, and a third is the fundamental theorem. While these axioms still support the model for empirical work, the empirical work now relates the strong axiom to Afriat's "cyclical consistency" criterion. Varian found that testing consistency with utility maximization requires a general axiom as well. The three axioms performed well, that is, "aggregate consumption data easily satisfied the revealed preference conditions." Besides consistency, Varian investigates the form, forecasting, and recoverability criteria. He concludes that the strong axiom is necessary and sufficient for utility maximization, and rich in empirical content.

Samuelson foresaw all the possible progressive birth signs of the revealed preference model when he initially proposed it. First, he gave it a twin feature (which he later integrated into a more single condition): (1) a single-value function on prices and income, subject to a budget constraint, and (2) homogeneity of order zero so as to make consumer behavior independent of the units of measurement of prices. Consider two batches or vectors of goods, ψ and ψ', with their respective price vectors, p and p', and denote their inner product by $[\psi p]$, and $[\psi' p']$, respectively. Now, we can observe the following: (3) "If this cost $[\psi' p]$ is less than or equal to the actual expenditure in the first period when the first batch of goods $[\psi p]$ was actually bought, then it means that the individual could have purchased the second batch of goods with the price and income of the first situation, but did not choose to do so. That is, the first batch (ψ) was selected instead of (v')" (Samuelson, 1966, p. 7).

A third proposition deals with consistency. "If an individual selects batch one over batch two, he does not at the same time select two over one" (Samuelson, 1966, p. 7). In a later note, Samuelson compacts the first two propositions with the third; "Postulates 1 and 2 are already implied in postulate 3, and hence may be omitted" (Samuelson, 1966, p. 13).

At its inception, Samuelson had the foresight that the revealed preference model had some virtue: "even within the framework of the ordinary utility- and indifference-curve assumptions, it is believed to be possible to derive already known theorems quickly, and also to suggest new sets of conditions. Furthermore... the transitions from individual to market demand functions are considerably expedited" (Samuelson, 1966, p. 23). But the revealed preference theory matured into an even more powerful rival research paradigm. Samuelson wrote: "I suddenly realized that we could dispense with almost all notions of utility; starting from a few logical axioms of demand consistency; I could derive the whole of the valid utility analysis as corollaries" (Samuelson, 1966, p. 90). The corollaries followed from axioms of consumer behavior noted above, which Varian has cast in more up-to-date symbolic form. But the way Samuelson stated them shows that he could be as eloquent in prose as in mathematics. The axioms as originally stated are as follows:

> Weak axiom: If at the price and income of situation A you could have bought the goods actually bought at a different point B and if you actually chose not to, then A is defined to be "revealed to be better than" B. The basic postulate is that B is never to reveal itself to be also "better than" A (Samuelson, 1966, p. 90).

> Strong axiom: If A reveals itself to be "better than" B, and if B reveals itself to be "better than" C, and if C reveals itself to be "better than" D, etc . . . , then I extend the definition of "revealed preference" and say that A can be defined to be "revealed to be better than" Z, the last in the chain. In such cases it is postulated that Z must never also be revealed to be better than A (Samuelson, 1966, pp. 90–91).

Samuelson then elevated the revealed preference theory to the empirical domain: "consumption theory does definitely have some refutable empirical implications" (Samuelson, 1966, p. 106), or we can "score the theory of revealed preference" (Samuelson, 1966, p. 106). Samuelson required a benchmark to allow refutation/scoring, for which he postulated this fundamental theorem: "Any good (simple or composite) that is known always to increase in demand when money income alone rises must definitely shrink in demand when its price alone rises" (Samuelson, 1966, p. 107). He then proceeded "to show that within the framework of the narrowest version of revealed preference the important fundamental theorem, stated above, can be directly demonstrated (a) in commonsense words, (b) in geometrical argument, (c) by general analytic proof" (Samuelson, 1966, p. 108).

How good a rival theory is the revealed preference theory? As Hildenbrand puts it, "Instead of deriving demand in a given wealth-price situation from the preferences, considered as the primitive concept, one can take the demand function (correspondence) directly as the primitive concept. If the demand function *f* reveals a certain 'consistency' of choices . . . one can show that there exists a preference relation . . . which will give rise to the demand function *f*" (Hildenbrand, 1974, p. 95).

Pollak's chapter describes a rich number of cases in which Samuelson distinguishes between the individual and the family as the consuming agent. On the theoretical side, he considers a Bergson–Samuelson type of social welfare utility function, with implications for Arrow's impossibility theorem. On the application side, he features Becker's "rotten kid" model problem, holding out the possibility of a solution with a family member as a possible dictator.[4]

Marxism

There is a single chapter in this section on Marx:

- "Paul Samuelson on Karl Marx: Were the Sacrificed Games of Tennis Worth It?" Geoff Harcourt

Is Samuelson a Marxist? The late Adolph Lowe said in one of his lectures that if Samuelson would say that Marx was right, then standard/orthodox economics would collapse. In Samuelson's words, "John Maynard Keynes was scientifically the greatest economist of this century. Only Adam Smith and Leon Walras can be mentioned in the same breath with him. Karl Marx can be mentioned in the same breath with Mohammed and Jesus, but it is of scientific scholarship that I speak and not of political movements and ideology" (Samuelson, 1986, p. 262). Again, Samuelson wrote: "I regard Marx as a scholar deserving of analysis on his objective merits and without regard to the deification or denigration meted out to him in various regions and ideologies." "I appraised Marx as a mathematical economist, . . . hailing Marx's most original contribution in *Capital*'s Volume II *Tableaus of Simple and Expanded Reproduction*. Marx's critics have missed this achievement, while at the same time his partisans have been praising his sterile paradigm" (Samuelson, 1986, p. 263). Also, "I follow Marx's portrait with Table II, which is what he and his followers wrongly think makes him great" (Samuelson, 1986, p. 273).

Harcourt challenges us to look at Samuelson's ideas from an eraser's point of view. When we look, we find Samuelson saying: "Contemplate two alternative and discordant systems. Write down one. Now transform by taking an eraser and rubbing it out. Then fill in the other one. *Voila*! You have completed your transformation algorithm" (Samuelson, 1972, p. 277). Here is a question and Samuelson's answer on this idea applied to Marx's transformation problem: "*The 'algorithmic transformation' from the 'value' model to the 'price' model (or vice versa), is truly a process of rejection of the former and replacement by the latter?* Here is my true crime. I pointed out the blunt truth. And this has been construed as an attack on Marx, covert or explicit" (Samuelson, 1978, p. 284). Naturally then, if Samuelson's idea is appraised as the truth, it shall prevail.

Stability

There is a single chapter in this section on Stability:

- "Paul Samuelson and the Stability of General Equilibrium," Franklin Fisher

Franklin Fisher wishes to attract attention to the topic of stability in the twenty-first century, for it has not yet reached a "satisfactory conclusion." Why is stability important? Not because it attracted the attention of Walras and Marshall (Walker, 1983, pp. 276–277). Stability is about "the determination of equilibrium values of given variables (unknowns) under postulated conditions (functional relationships) with various data (parameters) being specified" (Samuelson, 1966, p. 539). When rummaging into the equations of a theory, be it simple supply and demand equations, one always wants to know its stability properties, for if no more than equilibrium can be found, then "the economist would be truly vulnerable to the gibe that he is only a parrot taught to say 'supply and demand'" (Samuelson, 1966, p. 539). There would always be a need for stability analysis, even of a comparative static nature, to unearth the predictive power of models for scientific appraisals. Samuelson likened stability behavior to the soul and mind of business. "Since this competitive industry's comparative-statics can be shown to behave as if the industry had a soul and an integrated mind, expediency urges us to pretend it has" (Samuelson, 1986, p. 103). Those are, therefore, reasons to expect that this model will attract attention in the twenty-first century.

The sequel of statics versus dynamics also deserves unique treatment. Samuelson wrote: "For comparative-statics analysis to yield fruitful results we must first develop a theory of dynamics" (Samuelson, 1947, pp. 262–263). "Statics concerns with the simultaneous and instantaneous or timeless determination of economic variables by mutually interdependent relations . . . It is the essence of dynamics that economic variables at different points of time are functionally related . . . functional relationships between economic variables and their rates of change, their 'velocities,' 'accelerations,' or higher 'derivatives of derivatives'" (Samuelson, 1966, p. 354). There is a "formal dependence between comparative statics and dynamics . . . the *Correspondence Principle*" (Samuelson, 1966, p. 565). But there is a "two-way nature: not only can the investigation of the dynamic stability of a system yield fruitful theorems in static analysis, but also known properties of a (comparative) static system can be utilized to derive information concerning the dynamic properties of a system" (Samuelson, 1966, p. 565). "The nature of dynamic processes can best be appreciated from a study of concrete examples" (Samuelson, 1966, p. 593), and "in the field of pure theory, the important problem of the stability of equilibrium is wholly a question of dynamics. For it involves the question of how a system behaves after it has been disturbed into a disequilibrium state" (Samuelson, 1966, p. 613). How then do we involve facts and reality? George Feiwel (1982, p. 7) has a simple answer: "The growth of general equilibrium has given increased focus to static concepts." No theory can be more static than Keynesian. "All sciences have the common task of describing and summarizing empirical reality. Economics is no exception" (Samuelson, 1966, p. 1756) and "no a priori empirical truths can exist in any field. If a theory has a priori irrefutable truth, it must be empty of empirical content" (Samuelson, 1966, p. 1757). Stability concerns are here to stay. All models of reality call upon them to assess their compatibility. Samuelson has grounded stability in dynamics, which is promising for the twenty-first century economics.

Keynes and Post-Keynesians

There are three chapters in the sixth section of this book:

- "Paul Samuelson and Piero Sraffa—Two Prodigious Minds at the Opposite Poles," Luigi Pasinetti
- "Paul Samuelson as a 'Keynesian' Economist," Lawrence R. Klein
- "Samuelson and the Keynesian/Post-Keynesian Revolution," Paul Davidson

Luigi Pasinetti appraises the source of Samuelson's interest in Piero Sraffa's work. He finds that the two authors were attracted to each other because their works are of "equally foundational character." Samuelson's *Foundations of Economic Analysis* (1947) is on the side of maximization under constraints, with the use of mathematics as a language, while Sraffa's *Production of Commodities by Means of Commodities* (1960) tries to weed out ambiguities in solving analytical problems, starting with a prelude and advancing to a more constructive stage. Although their approaches are different, "they reached the same analytical conclusion, though with different nuances, accentuation of details, or shades of emphasis" as it is illustrated by the debate on reswitching.

Where Samuelson and Sraffa differ is in methodology. Samuelson defends the "exchange paradigm," while Sraffa defends the "production paradigm." This disagreement is evident in Sraffa's work (1960), which Samuelson sees as a defense of Ricardo's labor theory of value. Pasinetti appraises Samuelson's labor theory view along with two other propositions, reaching a somewhat different conclusion in answer to the question, "Why has Samuelson been so interested in Sraffa?" Pasinetti argues that Samuelson's stated desire "of formulating a general theory of economic theories . . . , would seem to imply the absorption and inclusion also of Sraffa."

Klein's chapter "Paul Samuelson as a 'Keynesian' Economist" traces the development of Keynesian economics in the United States from the early days. Although Keynesian economics came into the United States in fragments, Samuelson views it as a unifying principle. "What made Keynes different . . . was the fact that . . . He tackled the whole thing in one brilliant analytic formulation and provided economists with a new way of looking at how the entire gross national product is determined and how wages and prices and the rate of unemployment are determined along with it" (Samuelson, 1986, p. 280).

One of Samuelson's first attempts in Keynesian economics is the simple mathematical formulation: "$Y = C + I$, and $C = C(Y)$, $I = \hat{I}$" (Samuelson, 1966, pp. 1197–1219). Klein traces the development of "Paul Samuelson as a 'Keynesian' Economist," and also appraises the econometric foundation of Keynesian economics. Paul Davidson considers a different stream of post-Keynesian economics. Samuelson writes: "In . . . contrast to monetarism is the mainstream of modern economics today which . . . I shall call 'Post-Keynesian Economics.' Modern economics, as represented by men like James Tobin, Franco Modigliani, and myself, basically believes that changes in the money supply engineered by Federal Reserve policy have important effects upon the level of money, Gross National Product (GNP) and, depending upon the state of slackness in the employment market, upon real output and the price level" (Samuelson, 1978, p. 765). He then gives three post-Keynesian propositions "Even when the money supply is held constant" (1) Changes in thriftiness and the marginal propensity to consume can affect output, prices, and production; (2) An exogenous burst in I or investor's instinctive behavior has a systematic effect on GNP; and (3) An increase in public expenditure, or a cut in the tax rate has a systematic effect on GNP (Samuelson, 1978, p. 765). Davidson appraises the many versions of Keynes—Old Neoclassical, New Keynesians, Old Classical or New Classical Theorists—as adopting Keynesian general theory as their basic framework. As long as those research programs last, Samuelson's input into Keynesian economics will be followed.

International Economics and Finance

There are three chapters in this section of the book:

- "Paul Samuelson and International Trade Theory Over Eight Decades," Avinash Dixit

- "Paul Samuelson's Contributions to International Economics," Kenneth Rogoff
- "Protection and Real Wages: The Stolper–Samuelson Theorem," Rachel McCulloch

Avinash Dixit provides an appraisal of Samuelson's work on trade theory. He explains the scientific importance of starting with the Ricardian 2×2 model, and the current literature on comparative advantage. Samuelson rests this model on the shoulders of giants, whether we want to explain gains from trade by swapping bananas for steel, or from the modern theoretical points of view.

Samuelson has picked up this model without any rigorous proof and has given it many "operational" assumptions so that one can test—or falsify—its predictive or explanatory powers. He provides this intuitive proof: "Anybody can see that the tropics are capable of producing bananas while the temperate regions cannot produce bananas but can produce steel. Thus there would be profitable interchange between the cold, northern region, and the tropical region. That's the theory of comparative advantage" (Samuelson, 1986, p. 52).

In modern times this cannot be improved too much from the intuitive side. Dixit and Norman (1980, p. 5), for instance, argue that "the concepts involved are imprecise," and therefore "it is far from trivial to establish them rigorously." More advanced proof led to the welfare gain concept that Samuelson advanced from the classics, and anchored squarely on the First and Second Welfare theorems. With these theories, we can now model trade to assess who gains, for instance, in the formation of a Free Trade Agreement (FTA). This kind of research has only just begun.

Rogoff's chapter, which addresses the contemporary policy debate, appraises Samuelson's trade contributions such as the Stolper–Samuelson and factor–prices equalization theorems as "vital in today's globalization debate." The idea that gains from trade can be modeled through side-payment, that Samuelson added intuitive understanding and easy testing of the Stolper–Samuelson theory, and that his simple "iceberg-cost" metaphor helped our understanding of transit cost and friction in trade, are very active in the modern scientific and development views of modern trade theory. Rogoff's chapter spans a wide range of thought, from the "iceberg-cost" concepts to a Ricardian "continuum of goods" trade model. In between are financial analysis models such as the Harrod–Balassa–Samuelson theorem that the exchange rate increases faster for growing countries, which has led to the development of the Heston–Summer database for world comparison of income and prices. In between is his contribution on the "Transfer Problem," which at that time proved Keynes correct in his prediction of the cost of Germany's postwar reparations, and is still used today to assess the effects of a trade deficit on trade.

McCulloch appraises the Stolper and Samuelson (1941) theorem under varying conditions and assumptions. The model predicts that a country will export commodities produced by its abundant factors, and will import commodities produced by its scarce factors. The consequence is that trade will lower the real wages of the scarce factors.

In the current milieu of free trade, factors can move from import competing to export industries. In that movement, the factors including labor may lose real income, independent of taste and spending patterns. Using Jones' reformulation of the theorem, McCulloch demonstrates that when the relative prices for labor-intensive goods fall, real wages will decline in that sector, and the returns to other factors will increase, resulting in a redistribution effect.

The model had been robust in its predictions. When the number of goods and factors are increased, at least one factor is likely to gain or lose. The model is relevant for modern policies such as the Trade Adjustment Assistance (TAA) and unemployment insurance programs that assist distressed industries, where compensation can make a difference for individuals or firms that are hurt from free trade. The model is also relevant in the political arena where owners of factors vote or lobby. It has shown strength in explaining patterns of protection across countries. We see that the model had a long life in explaining distributive shares of factors, an area that will undoubtedly occupy social scientists in the twenty-first century.

Finance and Portfolio Theory

There are three chapters in this section:

- "Samuelson and the Factor Bias of Technology Change," Joseph Stiglitz
- "Samuelson and Investment for the Long Run," Harry M. Markowitz
- "Paul Samuelson and Financial Economics," Robert C. Merton

Stiglitz's chapter appraises a simple model Samuelson enunciated over 40 years ago on the liberalization of the capital markets. The model finds a home in the globalization of modern capital markets, indicating Samuelson's anticipation of free trade ahead of its time. The story is that unlike the situation of OLG where capital markets without liberalization but with technology shock will transfer over to another generation, with liberalization the shock may be dissipated in the current generation.

Stiglitz appraises the capital liberating model first from the traditional equilibrium points of view, and then from his new paradigm of disequilibria or market imperfection points of view. In the equilibrium version, some ambiguity exists as to how technological progress would augment capital or labor. Kaldor's stylized facts approach had assumed away the problem. The standard Harrod–Domar model did not include the effect of technological change, and when it was added, the disequilibria between exogeneous labor, and adjusted warranted growth rate becomes clear. Solow's modification did improve the analysis by making capital and effective labor grow at the same rate, but at the price of diminishing the concept of a job.

Stiglitz's appraisal of the capital liberating model now points out that wage adjustments with given capital and technology can support only maximum employment, and not necessarily lead to full employment. He provides two versions. A fixed coefficient version that extends Samuelson's paradigm to include wage effect through technology and capital accumulation on employment, and a version based on agency theory or efficiency wage theory where the wage rate must be adequate to induce labor to work. By integrating efficiency wage theory into the capital liberalizing model, Stiglitz has shown that Samuelson's model will be significant for future research in explaining dynamic economic problems.

Markowitz appraises a debate with Samuelson regarding which criteria the long-run investor should maximize in their portfolio. Markowitz provides the example of receiving either 6 percent per year with certainty or a lottery with an equal chance of 200 percent gain or 100 percent loss each year. The expected value returns criteria yields a return of 0.5. The expected log of 1 plus the returns is negative infinity ($-\infty$). Therefore, the investor would choose the certain prospect.

Markowitz considers whether the long run investor should follow the arithmetic mean or the log arithmetic (geometric mean) criteria in maximizing its portfolio. Markowitz makes the case for the log model. Samuelson argues that "It is a mistake to think that, just because a w^{**} decision ends up with almost-certain probability to be better than a w^{*} decision, this implies that w^{**} must yield a better expected value of utility"

(Samuelson, 1986, p. 246). He complemented this argument with an intransitive rule of odds: "You may well pick A(N) from A(N) and B(N), and pick B(N) from B(N) and C(N), and yet still pick C(N) from A(N) and C(N)" (Samuelson, 1986, p. 554). Nevertheless, the Markowitz chapter continues to present the case for the log model.

Merton's appraisal identifies essential and substantive gems from Samuelson's original contributions to financial economics. He selects financial facts that form a synthesis, implying that some conflicts have been resolved about them. Merton found that much of Samuelson's contributions, which he had appraised 25 years ago, are even more significant today.

Merton's appraisal places models of time and uncertainty in household allocation of resources at the center of the Samuelson contribution. He assesses Samuelson's contributions to the areas of efficient market theory and risk analysis, portfolio selection, and option and warrant pricing. Samuelson found that efficient markets do not allocate resources the way casinos do. Rather, asset prices vary randomly around an optimal path that can be discerned mathematically. The theory links space (spot price) with time (current future prices) in order to forge a solution (current futures prices = future spot prices), where spot prices are determined by optimal control theory. Can a George Soros, for example, provide superior performance? The answer depends on whether he can explain variation around expected returns. Operationally, that depends on the amount of information he has. The ranking of information produces strong, semi-strong, and weak versions of the efficiency hypothesis. Technically speaking, the matter of superior performance requires explaining the difference between a random variable and its conditional expectation, a difference with martingale properties. Researchers today are testing for these properties, and will continue to do so in the future.

Prior to Samuelson's works, options pricing centered mainly on European options, defined as options exercised on their expiration date. Samuelson introduced the American option that can be exercised before that date, and he considered longer-term horizons as well. For long-held or perpetual options, he discovered that the option would sell for more than the value of the stock. To correct that anomaly, he introduced log values that eliminated negative terms in stock pricing calculations. In short, Samuelson's work in warrants and option pricing provide a bridge between early and later option pricing models, thanks to his insights on hedging and mathematical analysis that were incorporated into subsequent theories.

Merton further appraises Samuelson's financial contribution for some other long-run issues. Samuelson introduced very specialized utility functions to address age-dependent and risk issues relating to, for instance, the notion that stocks are not risky in the long run. The argument for that proposition was based on some empirical fact on returns over 15- or 20-years horizon, and the argument that people become more conservative with age. Samuelson reached the conclusion that stocks are risky in short, intermediate, and long runs, and therefore, he leans toward the rejection of that hypothesis. Not heeding Samuelson's advice, retirement investment during the 2000–2002 period has experienced much loss.

Samuelson's Relevance

There are three chapters in this section:

- "Multipliers and the LeChatelier Principle," Paul Milgrom
- "The Surprising Ubiquity of the Samuelson Configuration: Paul Samuelson and the Natural Sciences," James B. Cooper and Thomas Russell
- "Paul Samuelson's Mach," Rod Cross

Milgrom appraises Samuelson's LeChatelier principle of how the market responds to a change in parameters of demand and supply curves. He uses examples in demand theory, economic policy, and empirical research to illustrate the principle. He also stresses the flexibility of the principle to adapt to changing assumptions. We notice changes from the optimizing agents to equilibrium systems whose primary use is to "provide a foundation for understanding multipliers."

Milgrom evaluates how the principle performed when it was confronted with local optimization problems as in production function settings, and in positive feedback systems as in gaming situations. The principle is found progressive in that it is able to capitalize on symmetric relations among substitutes and complements. In that regard, the principle has extended research into multiplier analysis, a research area that would continue into the twenty-first century.

Cooper and Russell appraise one aspect of how Samuelson adopted the methods of physics for economics. Samuelson is considered a leader in adopting the concepts of optimization with constraints to economics.

Cooper and Russell grasped the source of this principle in the "little used" and "amazingly obscure" writings of the physicist James Clerk Maxwell.

Cooper and Russell's appraisal delves deeply into the areas of physics and mathematics. "Classical thermodynamics is the subject that deals with such interactions of mechanics and 'temperatures' . . . my formulation . . . is idiosyncratic in that the formal relations that are important in analytical economics have motivated my choice of physical axioms and the order of their introduction." In thermodynamics, Samuelson "utilize[s] the area of a closed curve in the (pressure, volume) plane." In economic analysis, we use the terms price and quantity to replace pressure and volume, respectively. In such a diagram, equivalent areas between level curves can be interpreted as solutions to the maximization of profit subject to input constraints problems. From the area of classical mechanics, Samuelson borrowed "the law of conservation of (mechanical) energy for a (frictionless) system." A frictionless pendulum has the greatest kinetic energy (squared velocity) at the bottom of its swing, when its potential energy is minimal. The sum of a conservative system's kinetic and potential energies is constant along any motion, conserving the initial value of that sum (Samuelson, 1986, pp. 231–232). This conservation model has been a workhorse in modern economics in optimization problem, and shows no sign of weakening in the twenty-first century.

Cross appraises Samuelson's methodology through the work of the physicist, psychologist, and philosopher Ernst Mach, categorizing such thoughts with a big "M". The range of thought of Mach is from sensory observation to phenomenology, including other disciplines such as psychology to the extent that such disciplinary thoughts are in harmony with the stability of the concept.

Cross appraises Samuelson from the Heraclitus dictum to the effect that one cannot step into the same river twice because of changes that have taken place. The river has essential properties that do not change, and substantive properties that do change from an Aristotelian viewpoint. For Samuelson, "Science, even inexact science is public knowledge, reproducible for analysis by everyone" (Samuelson, 1986, p. 564). Cross locates that reproducibility in the "action and reactions" of the elemental qualities of a river, or relationship among economic variables. When the qualities such as *a, b, c* satisfy a functional form such as $F(a,b,c\ldots) = 0$, then they come together at that point, forming a state between appearance and disappearance. States have recall properties that allow association of past observations with current observations, enabling scientific discovery to take place. Cross appraises how Samuelson meandered among the thoughts of his teachers, colleagues, theoretical and empirical concepts to place his final allegiance on facts as the pivot on which to gauge scientific economic theories.

Cross ventures that Samuelson's methodology arises from the consideration of many views of Science. Faced with a blurred distinction between facts and theory that was highlighted by W. V. Quine, and the argument that economics deals with the world of social phenomena, Samuelson maintains a firm foot in reality, letting the facts tell their story. Samuelson also acknowledges how new thoughts such as Thomas Kuhn's paradigm, regarding the notions of cumulative knowledge, and incommensurability brings to the theory of science. He accepted that facts are numerous and therefore must be carefully sampled for their economy. But his goal remains that we should be able to tell the "how" and the "why" of things and phenomena of the economic world.

Samuelson's relevance starts with his curiosity for finding simple facts. He wrote that "The simplest things are often the most complicated to understand fully." He suggests an approach that treats simple things with: "(1) A literary discussion; (2) A mathematical treatment, and finally; (3) A history of the subject" (Samuelson, 1978, p. 3). Even in the area of mathematics, which has the reputation of being precise, he reminds us that "When mathematicians, like Debreu, speak of a competitive equilibrium, they do not insist that it is to be the only one but merely that it be self-warranting in the sense of satisfying all the conditions of the problem" (Samuelson, 1978, p. 143). In the same vein, he writes that "If the solution is simple, the assumptions must be heroic . . . Assumptions would not be heroic if they could be easily taken for granted as being exactly applicable" (Samuelson, 1978, p. 150). In one sense, we can think of the term "generalist" as a polymath—that is, one who has an umbrella of concepts under which to predicate particular concepts. From this point of view, Samuelson reaches for infinity as the limit. His search for the truth is far reaching. It is worth repeating his famous quote: "I would take aid from the Devil if that would help crack the puzzle of economic reality" (Samuelson, 1986, p. 873). Again, he is willing to look at the recesses of the subconscious to find the truth if need be: "We are eternally grateful to Henri Poincare for his detailed exposition of the role that the subconscious plays in the discovery of mathematical theories: how one wrestles consciously and unsuccessfully with a theorem, then puts it aside, as if out of mind, but apparently not really out of mind; for suddenly . . . the successful solution arrives" (Samuelson, 1978, p. 846). From Samuelson's generalist point of view, the truth can be approached from anywhere. "A mathematical theoretical Walras–Debreu system would find a full-employment equilibrium path even if it started out from initial conditions like those of 1933" (Samuelson, 1978, p. 915).[5] To appraise Samuelson as a generalist is to put him in the heart of research for the twenty-first century. In the twentieth century, the physical scientists could tell what to do and where to go if some object was hurled at you, needing only Newtonian laws. As we enter the new millennium, we are bombarded with more uncertainty in science. Just think of such phenomena as global warming, parallel universes, antimatter, and spin theory.

Samuelson's "generalist" approach seems to be right at home with the general view of the physical sciences.[6] In his *Foundations of Economic Analysis* (1947), Samuelson looked for similarities among the various areas of economics. He then proceeded as a "generalist" to bind them together. This is in the time-honored canonist approach to scientific discovery extolled by Bacon and Mill, of which we list the First Cannon according to Mill: "*If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree is the cause (or effect) of the given phenomenon*" [Italics original] (Mill, 1970, p. 255). To do that for economics, Samuelson prescribed the tools of a precise language, which he found in mathematics. He also prescribed and invigorated concepts such as maximization, minimization, equilibrium, efficiency, stability, and multiplier. With these generalized concepts came new ideas in the areas of cost, production, consumer demand, revealed preference, and trade in their static and dynamic points of view.

Samuelson is explicit and conscious about his position as a generalist, for he notes "I can claim that in talking about modern economics I am talking about me. My finger has been in every pie. I once claimed to be the last generalist in economics" (Samuelson, 1986, p. 800). Without proper appreciation of Samuelson's "generalist" perspectives, many analysts who have attempted to appraise Samuelson's work, not necessarily for the twenty-first century, have not been able to contain their analysis to the scientific domain, but rather were led into directions dictated by their own partiality. For instance, in reviewing the contribution to utility theory, Hayek was led to the view that "refinements suggested by P. A. Samuelson are hardly in the Austrian tradition" (Hayek, 1992, p. 54). But even Hayek too sought the general, not reasoned, approach, which he found in the theory of evolution. How democratic socialism can evolve into totalitarianism is the thesis of Hayek's *Road to Serfdom* (1945). Even though the Austrians are apt to argue that Samuelson has adopted the wrong method, "establishing physics as the science for economics to imitate," (Hayek, 1992, p. 5) their sympathizer, Robbins (1970, pp. 40–41), had this to say of Samuelson's methods: "It is difficult to argue that . . . the comprehensive treatise of Dorfman, Samuelson and Solow have not deepened our insights in many directions . . . in the raison d'etre of the price mechanism as something inherent in any maximization process within the restraints of different degrees of scarcity."

Conclusion

In this introduction, we reviewed the significance of Samuelson in the twenty-first century using the scientific criteria of "unit" and "units of appraisal." Our appraisal suggests continued success for Samuelson's specific and general studies for the twenty-first century. His work on trade is finding applications in the modern global economy. While his writings on the neoclassical synthesis were put to severe tests (and some parts shaken) during the 1970s and 1980s, they have an increasing role to play in the modern global economy. A mixed economy and public versus private goods with their associated externalities are very much twenty-first century concepts. Samuelson's theoretical contributions, when given operational meaning, are increasingly being confronted with facts and reality, and are performing well. Overall, the appraisals place a high value on Samuelson's vast output from a scientific point of view, which supports their endurance in the years ahead.

Notes

1. To appraise Samuelson, consider his preference for mixing fiscal and monetary policies. Samuelson wrote: "Now, I will very briefly summarize my view on the subject. The late C.O. Hardy said, 'Fiscal policy really has not independent importance. It is just a complicated way of getting the banking system to create some extra money. It is like burning the house in order to roast a pig' " (Samuelson, 1972, p. 552). This story falls in with Dewey's *Propositions of Appraisal*, where an "examination of these appraisals discloses that they have to do with things as they sustain to each other the relation of *means to ends or consequences*" (Dewey, 1939, p. 23). One interpretation of this "means to ends" model is that one should skip fiscal policy and get to where the action is—monetary policy. This will be an instance of "the end justifies the means"

(Dewey, 1939, p. 41). Samuelson thinks, "this view is profoundly wrong . . . that the mixture of fiscal policy and monetary policy we actually use was absolutely crucial in this and other regards" (Samuelson, 1972, p. 552). Such policy mix has already made its way into the current economic thinking that argues for monetary targets when the IS curve shifts about, or fixed interest rate policy if the LM curve shifts about (Dornbusch, 2004, p. 426).

2. Samuelson appears to substitute the term "audit" for "appraisal" at times: "What does an audit show for these opposite-line claims? Does it confirm any hope to explain the trends toward deterioration of the double factorial terms of trade of the Third World vis-à-vis the affluent nations by means of the concept of unequal exchange?" (Samuelson, 1986, p. 477). Also, he used the synonym "analyze" from time to time: "Here in this brief investigation I hope to analyze what the effects are on the welfare of different regions of a great burgeoning of productivity in the Pacific Basin" (Samuelson, 1986, p. 484), or: "I shall be analyzing the merits and demerits of protection" (Samuelson, 1986, p. 493), and the synonym "evaluate" as well: "To evaluate the question of how different the classical paradigm was from today's mainstream economics, it is worth sketching briefly the consequences of replacing *f* (Min [L,K]) by smooth constant-returns-to-scale technology" (Samuelson, 1986, pp. 606–607).
3. Note that Popper (1962) says that to appraise is not to accumulate knowledge but to replace one paradigm with another.
4. The longevity of Samuelson's model is assured in the twenty-first century and beyond. For as long as marriage and divorce will be around, the need for utility function inside and outside of family relationships will be needed to assess benefits and losses. The paper also appraises the pitfall of Samuelson's model, indicating the pros and cons of various empirical applications that are currently being carried out.
5. This should not be taken to imply that economics is all about common sense. Samuelson was clear about this matter when he wrote that "While it is true that few with advanced training in economics can be trusted to use common-sense economics, fewer still, and maybe no one, without advanced training in economics, can be trusted to use common-sense economics . . . experience does show that the best economic policy-makers have spent years studying economics and doing scientific research . . . Common sense, and folklore generally, lack empirical content . . . I would liken common sense to the hands of a watch, hands so short that they lie in every direction; lying in every direction, the hands cannot point in any direction and such a watch can tell us the correct time only after we have already learned it elsewhere" (Samuelson, 1962, pp. 16–17).
6. We see this as compatible with Adam Smith's intention when he set out to do for the social sciences what Newton did for the physical sciences, and predict that the foundation that Samuelson laid for the twenty-first century is a progressive one.

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Part I

Analysis of Samuelson's Specific Contributions

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1

Overlapping Generations

Robert M. Solow

A little over 20 years ago, Cary Brown and I edited a book with the title *Paul Samuelson and Modern Economic Theory* (1983). Kenneth Arrow, Robert C. Merton, and I are the only carryovers from that book to this. My contribution to the first volume was a chapter on the Modern Theory of Capital. Before the book was sent to the printer, but after it was no longer possible to make any real changes, I realized that Samuelson's invention in 1958 of the overlapping-generations model had fallen through the cracks. It was not included in my chapter, nor did the authors of other plausibly appropriate chapters take account of it. Already in 1983, and even more so now, a much elaborated overlapping-generations (OLG) model was and is one of the standard vehicles for studying questions of intertemporal equilibrium. It occupies all of chapter 3 of the Blanchard–Fischer *Lectures on Macroeconomics* (1989) and recurs throughout that excellent compendium of "What Every Graduate Student Should Know". So I will use this opportunity to sketch its origin in Samuelson's work.

The basic article is "An Exact Consumption-Loan Model of Interest with or without the Social Contrivance of Money" (1958). In fact, though Samuelson did not know this, Maurice Allais had already formulated a fairly straightforward overlapping-generations model in an appendix to his *Économie et Intérêt* (1947). Samuelson's use of the model, however, is quite different from that of Allais. In any case the subsequent literature had its source in Samuelson's paper, not Allais's appendix with its misfortune to be written in French. The scope of the model has broadened considerably during the 50 years since Samuelson's paper. (An excellent modern survey is the article by John Geanakoplos in the *Palgrave Dictionary*.)

For his particular purposes, Samuelson outlines a model in which identical agents are born, live for three periods, and then disappear. Most OLG models get along with two-period lives; allowing for more than three gets laborious, though numerical computation is always possible even with many-period lives. Samuelson's project of a pure consumption-loan model *with perishable goods* needs three periods. (The perishability assumption is a way of ruling out any determination of interest rates via the productivity of investment.) People work in the first two periods of their lives, and produce one unit of perishable output each time; everyone retires and produces nothing in period three. Suppose I am in my middle period, looking to make provision for my old age in period three, when I will have no earned income. I must lend part of my current income to someone who will be able to pay me back, with interest, next period. Older people obviously won't do; they will not be around when I am retired. Nor can I deal with my contemporaries who are in exactly my position. So I will have to contract with younger people who will have earnings next period. With two-period lives, there would be no such people; with three or more, there are.

The inhabitants have identical well-behaved utility functions over three-period-long consumption sequences, with no presumptions about time preference. Let R_t, R_{t+1}, . . . be the successive discount factors, effectively the price of a unit of good at $t + 1$ ($t + 2$) in terms of the same good at t ($t + 1$), etc. Thus $R = (1 + i)^{-1}$. It is elementary to deduce the demand functions $C_k(R_t,R_{t+1})$ ($k = 1,2,3$), with the usual properties, for a person new-born at t; with incomes of 1,1,0, the budget constraint is $C_1 + R_tC_2 + R_tR_{t+1}C_3 = 1 + 1R_t$. It is then trivial to change notation to the savings functions $S_k(R_t, R_{t+1})$.

Suppose B_t people are born at t, so at time t the population consists of B_t first-period people, B_{t-1} second-period people and B_{t-2} third-period (retired) people. Since there is no possibility of investment, the goods-market-clearing equilibrium condition is that

$$B_tS_1(R_t,R_{t+1})+B_{t-1}S_2(R_{t-1},R_t)+B_{t-2}S_3(R_{t-2},R_{t-1})=0$$

In principle there are infinitely many such equations, one for each t, to determine the infinite sequence of discount factors or interest rates. Even starting at t, with R_{t-1} and R_{t-2} predetermined, so to speak, we still have R_t and R_{t+1} as unknowns; adding the next equation introduces the next unknown.

If the population is stationary ($B_t = B$ for every t), it is easily seen that one solution is $R_t = 1$, that is, a zero interest rate in every period, because then the sequence of equilibrium conditions reduces to the sequence of budget constraints, which are always satisfied. There can, however, be other solutions. This possibility of multiple equilibria enriches or plagues the long history of OLG models, depending on your point of view. Here it is already. I will say a little more about this property later.

There is a quick and, at least at first, rather surprising generalization of this result: Samuelson calls it the "biological theory of interest." Let the number of births (and therefore the population) grow geometrically through all time: $B_t = (1 + g)B_{t-1}$. Then one can look for a solution in which R is constant. There is one such, $R = (1 + g)^{-1}$, or $i = g$: so one possible equilibrium interest rate is the population growth rate itself.

Samuelson finds this result odd. He goes on to ask what was, at least for him, a characteristic question. This is a competitive equilibrium with no externalities: what is it "trying" to optimize? What planner's problem is "the market" solving? The answer emerges straightforwardly. If you imagine this economy being run always by an immortal dynastic family, with no beginning and no end, playing no favorites, the natural objective would be to maximize the lifetime utility ($U(C_1,C_2,C_3)$) shared by every member of the family throughout—doubly—infinite time.

Indeed it is easy to see that the steady-state equilibrium corresponding to the biological rate of interest has just this property. There is a fly in this particular ointment, however. Samuelson shows that if the economy starts anywhere outside the biological equilibrium, even nearby, and evolves by equating supply and demand period by period, it will not move toward the biological steady-state equilibrium. This sort of instability is another endemic characteristic of OLG models, as the later literature documents in many different contexts.

Abba Lerner would have none of this, by the way. In a comment on the 1958 article—to which Samuelson replied politely but firmly—Lerner argued that the welfare problem should be formulated differently. Take the stationary case for simplicity. In any period t there are three (or $3N$) people alive, one (or N) at each age. One of them is retired, so total consumption available is two. A benevolent planner would distribute the consumption to equalize marginal utility for each living person. Under the sort of strong symmetric, time-neutral assumptions of the Samuelson article, each person would get exactly 2/3 units of the good to consume. When the population is growing geometrically, the calculation is only slightly more complicated. In any case, the period by period social optimum as defined by Lerner is not the dynastic optimum previously defined and described. Samuelson was not about to deny Lerner the right to define social welfare according to his lights; but he was not inclined to abandon his own definition.

One could pursue this line of thought further. Indeed Samuelson surfaced some interesting questions about the doubly infinite nature of time in the model. For instance, what happens if this world will (might) come to an end at some specified time? Obviously some debts will not get settled. That is very like asking about the fate of the first or the last generational cohort in a pay-as-you-go social security scheme. I think it is much more enlightening to observe that these welfare-economic issues that seemed to attract the immediate interest of Samuelson, Lerner, W. J. Meckling, and other commentators turned out not to be the enduring value of the OLG model. The model itself could be fleshed out into a flexible vehicle for intertemporal general equilibrium theory, and proved to be capable of many different applications. (The study of social security systems was one of them.) In 1958, no one foresaw the future scope and power of Samuelson's little invention. It has to be said, however, that Samuelson understood at the very beginning one important implication of the OLG set-up, to which I have already alluded in passing. The determination of equilibrium interest rates is inherently forward-looking, of course; but the OLG model makes it very clear that every time you tack on another time period, and thus another equation, you also pick up another unknown. This is not trivial.

I will conclude by saying a little about the later development of the OLG model. But first it will be useful to pick out one further common characteristic of those models that emerged in the original paper, and in Samuelson's reply to an early comment by Meckling. Again I will stick to the stationary case to avoid notation. I will also take it for granted that in this case we are looking for an equilibrium with a constant interest rate and discount factor. So $S_1(R,R)$, $S_2(R,R)$, $S_3(R,R)$ are the savings functions for first, second, and third-period agents.

Since numbers are the same in all three age groups, and since perishability and non-satiation imply that all income is consumed, equilibrium requires that aggregate net saving vanish at each time. Thus $S_1(R,R) + S_2(R,R) + S_3(R,R) = 0$. And the intertemporal budget constraint for each agent says $S_1(R,R) + RS_2(R,R) + R^2S_3(R,R) = 0$. These two equations imply that $(1\text{-}R)S_1(R,R) = (1 - R)RS_3(R,R)$. We already knew that $R = 1$ satisfies this last equation. But we are alerted to the fact that there may be other (constant) solutions, in particular any R that satisfies $S_1(R,R) = RS_3(R,R)$. (And that is only for constant solutions.) There is no guarantee that other solutions exist, but they may. If they do, clearly S_3 $(= 0-C_3)$ is negative; and so is S_1 $(= 1 - C_1)$, meaning that the middle-aged are lending to the young now, looking for repayment next period. The point is that the ubiquity of multiple equilibria was present and seen to be present at the birth of the OLG model. When the model grew up, multiplicity played an even larger role.

With his usual acute historical sense, Samuelson had invented the pure consumption-loan model to test Böhm-Bawerk's idea that time preference would be needed to produce a positive rate of interest. (It turned out to be wrong.) It was only necessary to introduce production and durable assets to convert the OLG model into a neat, transparent, tractable vehicle for intertemporal general-equilibrium analysis at various levels of aggregation.

The power of the idea was established clearly with Peter Diamond's "National Debt in a Neoclassical Growth Model" (1965). The basic model was equipped with a stock of real capital, and with a paper asset to serve as an alternative way of holding wealth. The OLG set-up became established as one of the workhorse models for macroeconomics. It provided, among other things, a way to escape the temptation induced by use of the Ramsey model to formulate any long-run equilibrium process in a way that guarantees nice properties without further thought. As already indicated, the Blanchard–Fischer text exhibits the wide range of macroeconomic applications that followed after Diamond's work. I feel an urge to mention that the first chapter of Hahn and Solow's universally unread *Critical Essay* (1995) uses an OLG model with money to show that "perfect" nominal wage flexibility, while guaranteeing full employment by definition, is likely to lead to clearly pathological fluctuations in output, induced by necessary variations in the real interest rate.

Earlier on came the discovery by Jean-Michel Grandmont (1985) that a well-posed real competitive OLG model had the capacity to generate continuing fluctuations, indeed a large variety of fluctuating trajectories. These are not the business cycles of the older literature (or of reality), but they exemplify the capacity of the OLG model to uncover unsuspected possibilities in otherwise well-behaved economies.

The last such line of research I want to mention harks back to Samuelson's observation, in his reply to Meckling, that: "To know today's interest rate we have to know tomorrow's interest rate, because that helps determine today's saving on the part of the young. Inductively, then, today's interest rate is determined simultaneously with—and not prior to—*all* subsequent interest rates." A formulation like that points irresistibly to the idea of an equilibrium trajectory as a self-validating prophecy: a consistent pattern of expectations which, if held, will induce just the decisions that will make the expectations come true.

Further work along these lines was soon forthcoming, as for instance with Costas Azariadis's paper "Self-Fulfilling Prophecies" (1981). This path led to the work by David Cass and Karl Shell (1983) and by Roger Farmer (1993), among many others, on what has come to be called "sunspots." A fundamentally irrelevant process (like the waxing and waning of sunspots) might come to serve as a way of coordinating expectations. If everybody believes that sunspots determine economic outcomes in a certain way, is it possible that their resulting actions will confirm—and therefore strengthen—that belief? And if sunspots, why not other irrelevant processes as well? The answer is that many such equilibrium trajectories can exist, in some cases a continuum of them. The deep point here is not to detect or classify or interpret sunspot equilibria. What is really interesting is the demonstration that a fairly plain vanilla intertemporal model can easily fall into self-consistent modes of behavior that bear no relation to "fundamentals." Who would have expected that?

Thus this innocent little device of Samuelson's has been developed into a serious and quite general modeling strategy that uncovers equilibrium possibilities not to be found in standard Walrasian formulations. What is there about the OLG model that does this? Is it the overlapping structure, or the infinity of time, or something else? Apparently it is all of those characteristics together, as Geanakoplos remarks at the end of his compact survey. Probably it is a strategic mistake to worry about the essence of the OLG model. The sensible course is to forget about essences and study how the model behaves.

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2

Paul Samuelson's Amazing Intergenerational Transfer

Laurence J. Kotlikoff

I am deeply honored to participate in this forum celebrating Paul Samuelson's ongoing contributions to economics. Paul's work has profoundly influenced, irrevocably altered, and dramatically improved economic analysis in virtually all areas of economics. A prime example is the field of generational policy, which focuses on the extent and means by which governments redistribute across generations. Paul's masterpiece—"An Exact Consumption-Loan Model of Interest with and without the Social Contrivance of Money"—is *the* seminal article in this field and permeates virtually all postwar research on the issue. The paper's insights and messages have particular salience today given what many view as the grave demographic/fiscal threat facing the developed world.

Like all of Paul's writings, this paper is a literary gem with copious references to our intellectual forefathers. Bentham, Mills, Engels, Myrdal, Kant, Robertson, Böhm-Bawerk, Harrod, Fisher, Landry, Hobbes, Rousseau, and others make an appearance. But the paper's real appeal is the theoretical vistas it provides. Here in one fell swoop we learn that competitive economies can be Pareto inefficient (dynamically inefficient), that altruism can promote survival, that constitutions and social norms can have economic determinants, that dynamic economies can have an infinite number of equilibria, that biology can determine interest and inflation rates, that financial markets can be highly volatile, if not unstable, that monetary and fiscal policy can be isomorphic, that fiscal policy can be

I thank Herakles Polemarcharkis and Karl Shell for extensive and extremely valuable comments.

endogenous, that the same economic policy can be labeled a zillion different ways, and that there is an economic limit to expropriating the young.

The paper and Paul's *Journal of Political Economy* subsequent exchange (Samuelson, 1960) with W. H. Meckling (1960) provide a winding road through this splendid garden of issues and ideas. Just when you think you've come to the end, there's another twist presenting an even more striking view. Paul clearly delights in story telling, knows how to keep his reader in suspense, and waits until the last minute to pull his paper oblongs out of the hat.

A simple rendition of Paul's story takes place on a very hot island with very tall cocoa trees, which only the young can climb. At the top of the trees grow the only source of sustenance—Hershey chocolate bars.[1] The young climb the trees, harvest the bars, and eat them immediately for they won't keep in the heat. The earth-bound elderly grovel for chocolate, but to no avail. Their elevated kids see no quo for their quid, have no *Up* in their *Uc*, and experience no qualm in watching their parents starve.

This unfortunate state of competition continues year after year until some enterprising generation of oldsters offers to swap chocolate for pink sea shells that have washed up on shore. The young could not care less about sea shells, but they make the swap in order to have shells with which to swap when old.

Voila! The economy moves from brutish to blissful. People no longer starve when old, everyone is better off, and the young and old celebrate the economy's Pareto improvement by washing each other with melted chocolate.

Now what determines the price level—the rate at which shells swap for chocolate? The answer is expectations. The price today depends on what people think it will be tomorrow. But what it will be tomorrow depends on what tomorrow's people think it will be the day after, and so on. Nothing in the economic environment pins down these expectations, so nothing limits the number of paths the price level and, thus, the economy can take. In particular, nothing says the price level will change in line with population growth or, if this does occur, when such a steady state will arise. Moreover, since the public's expectations of future prices determine the course of chocolate transfers, such transfers, which might be termed fiscal policy, are endogenous.

Since the change in the price level determines the rate at which one can swap consumption today for consumption tomorrow, the dynamics of the price level are also those of the implicit interest rate. When prices move in line with population growth, the interest rate equals the population growth rate (the biological rate). But when they do not, the interest rate can go its merry way, including fluctuating wildly. So our little chocolate paradise can have lots of what some would describe as "financial instability" along any given equilibrium path or, indeed, across paths, if the economy jumps equilibria.[2]

Regardless of what path the economy takes—what equilibrium prevails—birds (nonedible ones) perched in the cocoa trees will take notice. They will no longer hear the moans of starving geezers or watch the young pelt the old with candy wrappers. But while the birds will all agree about the amount of chocolate being passed from the young to the old each period, they will vehemently disagree as to the policy in place. Some birds will claim that monetary policy is at work and that the shells are money. Others will see a pay-as-you-go social security system in which the chocolate handed over when young constitutes a tax and the shells simply represent bookkeeping for one's future claim to chocolate social security benefits. Yet others will claim the shells are bonds that are purchased when young and sold when old. And there will even be some birds who will claim that the shells are irrelevant—just a shell game, if you will—and that the chocolate eaters must have drawn up a constitution forcing each generation of young to make transfers to the old.

After the birds spend several centuries arguing and forming societies called the Monetarists, the Socialists, the Keynesians, and the Strict Constructionists, a young bird named Paul points out that the argument is not about economics, but about language. This stops the fight for a full nanosecond, after which it proceeds apace.

Like this island, our society contains a lot of bird brains, many located in Washington, who constantly mistake linguistics for economics. They would do well to read Paul's article.

We economists, in contrast, have read and reread Paul's article and incorporated it fully into our teaching and research. In this respect, Paul's paper is the gift that keeps on giving—not a chocolate bar, but a cocoa tree! This is plain to see in Edmund Phelp's (1961, 1965) work on the golden rule and dynamic efficiency, Peter Diamond's (1965) analysis of debt in an OLG model, Martin Feldstein's (1974) work on pay-as-you-go social security, Robert Barro's (1974) work on Ricardian Equivalence, Karl Shell's (1971, 1977) work on the economics of infinity and sunspots, David Cass and Karl Shell's (1983, 1989) work on sunspots, Costas Azariadis (1981) and Roger Farmer's (1993) work on sunspots, Yves Balasko and Karl Shell's (1980, 1981a,b) detailed investigation of the OG model, Glenn Loury's (1981) work on intra-family human capital transfers, Jean-Michel Grandmont's

(1985) work on "temporary equilibrium," Richard Benveniste and David Cass' (1986) work on optimal stationary equilibria, the work of Andy Abel *et al*. (1989) on dynamic efficiency under uncertainty, Michael Woodford's (1990) work on the convergence of rational expectations equilibrium under adaptive learning, the recent work by Rochon and Polemarchakis (2005) distinguishing money and debt in OLG models, and in the work of literally thousands of others. Indeed, since 1988 alone Paul's paper has been cited 638 times in published articles and books!

For my part, I encountered Paul's paper in several courses in graduate school and was immediately intrigued by the issues it raised, although I was not able to clearly sort them out in my mind or fully grasp the lessons being taught. (I am still doing that.) But Paul's article, its offshoots, and the strong influence of my thesis advisor, Marty Feldstein, got me completely hooked on generational economics.

Let me connect my own research in this field to Paul's paper. I do so not to put my work on the same plane. It is in a much lower dimensional manifold. I do so to suggest the reach of Paul's intellectual transfer.

One of the key issues raised by Paul's paper is the role of intergenerational altruism in society. In considering the possibility that kids might let their parents starve, Paul was inviting economists to find out if that was really the case. And, by extension, he was inviting us to study whether parents would let their kids starve.

Starvation is, of course, an extreme outcome that arises only in unusual circumstances. So we need to test altruism in our everyday world in which both parents and kids have access to their own chocolate. The simple way to test intergenerational altruism is to see whether parents and kids share resources when it comes to consumption. Consumption sharing means that the consumption levels of those doing the sharing should move together. If parents get more (less) income, both their consumption and that of their kids should rise (fall). If kids get more (less) income, the same thing should happen. Stated differently, the ratio of the kids' consumption to that of the parents should be independent of the ratio of the kids' resources to that of the parents. Moreover, a dollar taken from a child and given to a parent should lead to a dollar increase in the parent's transfer to the child. Finally, when altruism is operational, other family decisions, like living together, should depend on the sum, not the division, of resources.

Over the years I and a variety of coauthors have conducted a number of studies to test these propositions using cross section, cohort, and time series data.[3] We have even tested altruism within extended families in which parents are actively making transfers to their children. The tests have all strongly rejected intergenerational altruism. Thus, Paul's assumption of selfish behavior is, regrettably, on the mark. In particular, when it comes to consumption, there is no evidence that cohorts share resources very much, that extended family members share resources very much, or that nuclear families share resources very much. And there is no evidence that parents whose incomes rise substantially compensate their kids whose incomes fall. This last finding holds even for parents who are actively making transfers to their children!

Paul's paper also raises questions about the range of social compacts that can be sustained and the manner of their enforcement. In Kotlikoff, Persson, and Swensson (1988) my coauthors and I pointed out that the old can sell the young more than simple covenants to support the elderly. They can also sell the young other economic laws, including a law prohibiting capital levies. Such a law can prevent second best taxation from degenerating into third best taxation due to the time inconsistency problem noted by Fischer (1980). The young can make their payment in the form of tax contributions to finance public goods for the old or transfer payments to the old.

Kotlikoff, Persson, and Swensson (1988) also explore ways of enforcing the sale of social contracts. In particular, we showed that if setting up new social contracts involves transactions costs, it will be easier to sustain existing social contracts. The first generation that sets up such a contract is forced to pay the setup costs, but has an offsetting advantage in not having to pay for the law. Subsequent generations find it cheaper to buy the law and then resell it (effectively rent the law) than incurring the setup costs from scratch. Our paper also showed that laws could be sold (laws are assets) even in the absence of transactions costs if generations adopt strategies under which they only purchase laws that have never been broken.

A third research area that intersects with Paul's paper and Diamond's (1965) own diamond involves trying to understand how interest and wage rates evolve in dynamic general equilibrium. My work with Alan Auerbach and others in this area adds capital accumulation, variable labor supply, demographics, multiple periods, and a variety of fiscal policies to Paul's framework.[4] These additions plus the assumption of CES production and CES intertemporal preferences admit unique and dynamically efficient equilibria.

My graduate student, Javier Hamann (1992), showed that adding money to this model as well as nominal government liabilities permits one to calculate a unique path for the price level. Here we see the price level endogenously determining real fiscal policy, just as in Paul's paper, Sargent and Wallace's examination of monetarist arithmetic, and Woodford's (1994) fiscal theory of the price level. The fact that nicely behaved, dynamically efficient, neoclassical models can have unique equilibria is reassuring given Paul's concern, raised in response to Meckling (Meckling, 1960; Samuelson, 1960), that "a perpetual competitive system seems to be an indeterminate one." But, as Nakajima and Polemarcharkis' (2005) work suggests, indeterminacy may yet rear its ugly head if the monetary-fiscal authorities adjust real fiscal policy in response to changes in prices and interest rates, where such changes are governed by rational, but otherwise freely determined expectations.

A fourth connection between my work and Paul's paper involves generational accounting. The impetus for generational accounting derives from Paul's insight that the same policy can be "run" in different ways. But since the math makes no distinction between one way and the other, any one way can be called the other. So Paul's point is really that a given policy can be labeled different ways; that is, whether we call a policy one thing or another is a matter of language, not economics.

This labeling problem is not specific to Paul's model, as shown in Kotlikoff (2003) and Green and Kotlikoff (2006). The problem is generic to any neoclassical model with rational agents. This fact calls into question essentially all conventional analysis of fiscal affairs given that such analysis is predicated on deficit accounting. Paul's point, writ large, is that governments are free to choose fiscal labels so as to report any time-path of deficits or surpluses independent of the actual policy they are running. We could well call this Samuelson's Relativity Theory. It shows that each observer's reference point (his/her choice of labels) alters the perception of economic policy, as conventionally measured, but not the reality of what the policy actually is or what it is doing to the economy.[5]

Generational accounting, when properly conducted, does not suffer from this labeling problem. Its assessment of the fiscal burden facing future generations is the same regardless of the government's nomenclature. So are its measures of changes in the fiscal burdens facing current and generations arising from policy changes. This is not surprising since generational accounting is trying to answer an economic question rather than engage in mindless measurement.

Given the nature of the fiscal/demographic problems facing the developed world, we no longer have the luxury of relying on inherently uninformative indicators of nations' fiscal conditions. Generational accounting, while far from perfect, is, at this point, a necessity, not an option. In the case of the United States, generational accounting indicates that if current adults do not step up to the plate, young and future generations will face lifetime net tax rates that are twice those of current adults. Attempting to foist such a burden on the next generation is not only immoral; it is also economically infeasible. There is a limit to fiscal child abuse, and the United States and other developed countries are, in my view, rapidly approaching that limit. Corroborating evidence on this score comes from Gokhale and Smetters's (2005) measure of the fiscal gap, which is closely related to the measure of the collective fiscal burden facing future generations discussed in Auerbach, Kotlikoff, and Gokhale (1991).[6] The fiscal gap, which is also a label-free measure, compares the present value of all projected future US government expenditures, including official debt service, with the present value of all projected future government receipts. Gokhale and Smetters's estimate of the fiscal gap for 2005 is $65.9 trillion or 8.5 percent of the present value of GDP. To put this figure in perspective, note that 2004 federal personal and corporate income taxes totaled 8.6 percent of 2004 GDP.

What alternative policies could be taken to eliminate the US fiscal gap? One is to immediately and permanently double person and corporate income taxes.[7] A second option is to immediately and permanently cut all Social Security and Medicare benefits by two thirds. A third alternative is to cut federal discretionary spending immediately and permanently; but even eliminating all such spending would leave us significantly short of the needed $65.9 trillion.

The Gokhale and Smetters's estimates are updates of the fiscal gap accounting they did while working at the US Treasury under former Treasury Secretary Paul O'Neill.[8] Their analysis relies exclusively on government projections or extensions of such projections. These projections are quite optimistic with respect to future demographics and growth in Medicare and Medicaid spending per beneficiary.

Notwithstanding the magnitude of the fiscal gap and the downward bias in its measurement, some prominent economists[9] see the short- and medium-term projected deficits as manageable and assume the long run will take care of itself. This, to be kind, is misguided. Paul's relativity theory tells us that we can choose labels to arrive at whatever deficit, tax, and transfer projections we want. Hence, there is no legitimate way to consider the short-term apart from the long-term. Only the infinite horizon measures calculated in the generational accounting and fiscal gap analyses are label-free and, thus, well defined.[10]

The fiscal/demographic optimists might well respond that the government's real borrowing rate is low relative to the economy's growth rate and that, given this fact, taking from the young and giving to the old could continue to work well for decades, if not centuries. These modern-day Ponzis should re-read the Abel *et al*. (1989) study, which shows that comparing the government's borrowing rate and the economy's growth rate is not appropriate in a setting in which both growth rates and rates of return to capital are uncertain. They should also think about how Paul's model works if the government finds itself taking ever more chocolate from the young in order to satisfy promises made to the old. At some point, the young run out of chocolate to hand over. At that point, it is the young, not the old, who are starving.

Kotlikoff and Burns (2004) raise the alarm about excessive transfers from the young to the old. In particular, we suggest that the US economy could go critical once financial markets recognize the implications of the magnitude of the fiscal gap for US money creation and inflation. But what if there is no financial meltdown to awaken the country to the limits to fiscal child abuse? How will these limits be reached?

As mentioned, in Paul's model the limit hits when all the chocolate is taken from the young and handed to the old. If we add capital to Paul's model, the limit is reached when the young are using all of their after-tax savings to purchase government bonds and, thus, are unable to accumulate physical capital. At this point, the game is over since production requires capital as well as labor. From a general equilibrium perspective, things would get pretty dicey well before this limit was hit. The reason is that wages depend on capital, so every period the capital stock falls, wages fall as well, which reduces what the young have to save.

This simple model suggests that we need to look at net national saving to understand if we are eating up our capital stock or would be doing so were foreigners to stop investing in the United States. In this regard, last year's net national saving rate is quite telling. It was only 2.2 percent of national income! With the exception of 2003's 2.1 percent rate, this is the lowest US rate of net national saving in 45 years. By way of comparison, note that the US net national saving rate averaged 13.0 percent in the 1960s, 10.3 percent in the 1970s, 7.6 percent in the 1980s, 5.6 percent in the 1990s, and 3.8 percent since 2000.

The decline in the rate of net national saving in the United States reflects the ever growing rate of household consumption. Government (federal, state, and local) consumption as a share of national income was 17.6 percent in 2004, which is lower than the average rate observed in each of the last four decades. For example, the government consumption rate was 19.5 percent in the 1970s. In contrast, the ratio of household consumption to the difference between national income and government consumption—what I call the household consumption rate—is now at a postwar high of 97.3 percent. This rate averaged 84.0 percent in the 1960s, 87.2 percent in the 1970s, 90.6 percent in the 1980s, 93.1 percent in the 1990s, and 95.5 since 2000.

As shown in Gokhale, Kotlikoff, and Sabelhaus (1996), the dramatic increase in the household consumption rate can be traced to higher levels of consumption of the elderly. My extrapolation from that study is that the per capita consumption of middle-aged retirees relative to that of middle-aged workers has doubled since 1960. The reason is simple. The government is taking ever more chocolate from young and, implicitly, future generations and giving it to the old, increasingly in the form of medical goods and services.

The huge US current account deficit is, of course, reflective of our country's low saving rate. Last year foreigners invested 3 dollars in the United States for every dollar Americans invested here![11]

While it may be hard to believe, the ever rising transfers to the elderly could shortly lead the US net national saving rate to go negative. At this point, we'll be eating up our national wealth.[12] Ignoring government assets (could we really sell the White House?) and assets held by nonprofits, my guestimate of current national wealth is $35 trillion. Were the United States saving rate to hit, say, −5 percent, and who is to say it will not given the trend and the pending retirement of the baby boomers, we would be eating up close to $2 trillion a year of national wealth. At that rate we would have only 15 or so years before the country was out of wealth and also out of income from that wealth. At that point we would have only our wages to finance our consumption. And US labor income is significantly less than US consumption. So we would, indeed, reach a limit to our intergenerational profligacy. Consumption would have to fall. In Herb Stein's words, "Something that can't go on has to stop." The problem with Stein's aphorism is that it fails to clarify that when things that cannot go on finally stop, they may stop at a very bad place and stay there forever. The bottom line here is that Paul's model identifies not just how intergenerational transfers can help the old, but also how they can hurt the young. As we all continue to study, learn from, and build upon his absolutely brilliant analysis, we, unfortunately, need to keep this concern front and center.

Notes

1. Karl Shell appears to be the first to use the chocolate metaphor for the good in Paul's model.
2. Paul's concern (Samuelson, 1960), raised in response to Meckling (1960), that "a perpetual competitive system seems to be an indeterminate one" depends, as most recently shown by Rochon and Polemarchakis (2005), on whether the economy in question is dynamically efficient.
3. See Kotlikoff (2001).
4. See, for example, Auerbach and Kotlikoff (1987).
5. The term "relativity theory" seems apt since this situation is akin to Einstein's revelation that different observers of the same physical reality will describe it differently depending on their relative speed.
6. The fiscal gap is the present value net tax burden on future generations that is calculated in Auerbach, Kotlikoff, and Gokhale (1991) less the present value of net taxes future generations would pay were they to face the same lifetime net tax rates as current generations.
7. This abstracts from tax evasion and tax distortions as well as "Laffer curve" effects.
8. Gokhale and Smetters (2003) is, in fact, the Treasury study commissioned by Treasury Secretary Paul O'Neill. It was published through the American Enterprise Institute rather than the Treasury because the Treasury censured the study within a few days of O'Neill's being fired.
9. See, for example, Porter (2005).
10. As a public service, the government should present alternative official deficit time series (past and projected) based on alternative labeling conventions. Some of these series would show the deficit soaring; others would show it heading south at exponential rates; still others would place and keep it at zero. Economists yearning to support their theories of how deficits connect to interest rates, saving rates, or other economic variables would surely find a series to meet their needs. Politicians dying for a balanced budget could die in peace. Others who crave red or black ink would shout for joy. And the public would finally see that the deficit has no clothes.
11. In 2004, the US rate of net domestic investment (measured relative to national income) was 8.7 percent. The US net national saving rate was 2.2 percent. The 6.5 percent difference represents the current account deficit as a share of US national income.
12. For example, over the past four years Medicare benefits per beneficiary grew sixteen times faster than real wages per worker.

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3

Social Security, the Government Budget, and National Savings

Peter Diamond

3.1 Introduction

Paul Samuelson has had an enormous impact on the public economics of the twentieth century. In models with heterogeneity in the population, Pareto improvements are not possible from policies that are restricted to even vaguely resemble realism. Thus, the Bergson–Samuelson social welfare function plays a key role in the evaluation of alternative policies. Since this key contribution was discussed by Kenneth Arrow in the 1983 Festschrift for Paul (Brown and Solow, 1983), I will merely say that I foresee no diminution in the importance of this contribution in the twenty-first century.[1] Similarly, the formulation of public goods by Paul was discussed by Richard Musgrave in the same volume and I foresee no diminution in its importance either.[2] Alas, the use of fiscal policy as part of the stabilization of an economy has moved out of contemporary public economics (and apparently macroeconomics as well). Thus his writings, celebrated by Tobin in the Festschrift, must await a revival in this key topic for a future impact.[3] As explained in the introduction to the 1983 Festschrift, Paul's development of the overlapping generations (OLG) model and applications to social security fell between the cracks of chapter assignments for that volume. Thus it is fitting that I provide an example of the use of that extraordinarily fruitful model in analysis of a social security question for this volume.[4] This list of Paul's direct contributions to public economics

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leaves out Paul's enormous impact through his writings on both individual choice and equilibrium, which have affected all of economic theory, but that would take me astray.

But first, I want to say a few words about how twenty-first century public economics may differ from that of the twentieth, and how that might affect the Samuelsonian legacy. When I look at the recent research trends that I expect to continue, two developments stand out. One is rapid growth of interest in behavioral economics, while the other is the use of computers for calculations of far more complex examples, both deterministic and stochastic, than could have been contemplated before. Computerized examples rely on theory in the same way that simpler examples do. And while some bottom up interactive simulations might make little use of the economic theory that Paul has helped to build, overwhelmingly, to date, the calculations and simulations have relied on the same basic theory. I do not expect that to change.

There are two complementary developments in theoretical behavioral economics. One is the development of new models of individual choice that rely heavily on empirical input from psychology. The second is the development of equilibrium models that make use of simplified models of behavior that are informed by both empirical findings on behavior and the first strand of new individual behavioral modeling. Thus these models are not consistent with the standard model. In Social Security, such modeling has gone on for a considerable time. In particular there are a number of analyses employing the OLG model while assuming nonstandard behavior in savings (and sometimes in labor supply as well). These still rely on the Samuelsonian OLG model as before—changing the model of individual choice does not remove the legacy. This model is an example of such modeling, offered as a tribute to Paul.

3.2 Social Security

In the 1983 Social Security reform, Congress chose to build a substantial trust fund, with principal and interest both to be used for later benefits. That is, Congress chose payroll tax rates higher than pay-as-you-go levels while the baby-boomers were in the labor force in order to have payroll tax rates lower than pay-as-you-go while the baby-boomers were retired. The impact on national capital of these higher payroll taxes, with the implied trust fund buildup, has been controversial. The impact depends on the response of the rest of the government budget as well as the responses of individuals to these government actions.[5]

In the absence of an empirically supported, widely accepted connection between Social Security and non-Social Security budgets, research has naturally considered the implications of alternative ways of modeling this connection. In particular, Elmendorf and Liebman (2000) analyzed the impact of Social Security savings on national savings under different assumptions as to the response of the rest of the budget to a Social Security surplus. Implicitly, they assumed a representative taxpayer and so did not distinguish between a payroll tax increase and an income tax cut that might be induced by the payroll tax increase. Yet the distribution of payroll and income tax burdens by income level are very different and propensities to save by income level are also very different. The top quintile in earners in 1995 paid 71 percent of the individual income tax and 37 percent of the Social Security payroll tax.[6]

The 1983 legislation can be viewed as a commitment to finance the additional debt in the Trust Fund out of future income taxes insofar as it resulted in offsetting expenditure increases or tax decreases. That is, pre-funding through the payroll tax should be seen as a commitment to workers, whether the government and the country save more overall or not. If tax changes are proportional to taxes and if income tax changes fully offset payroll tax changes (a balanced unified budget on the margin), then the legislation could be viewed in part (roughly one-third say, reflecting the differences in shares of the two taxes paid by low earners) as a transfer from current payroll tax payers to current income tax payers with an exactly offsetting (in present discounted value terms) future transfer from income tax payers to payroll tax payers. I believe that very little of the trust fund buildup of the 1980s and early 1990s resulted in offsetting budgetary changes.[7] Since this belief is not held by all analysts, it is helpful to consider the model with a parametric level of offset.

This note contributes to evaluating the impact of the 1983 reform on national savings by considering a one-period rise in the payroll tax to permanently increase the trust fund, with the increased interest income used to finance a decrease in all future payroll tax rates. Since future social security budgets are balanced, it is assumed that the income tax rate decreases to partially offset the social security surplus in the first period and thereafter the income tax rate rises by enough to pay the increase in the interest owing on the national debt. That is, after the initial period it is assumed that both the Social Security budget and the unified budget are always balanced. The impact of these government actions on national capital is solved for the end of the initial period and for the asymptotic steady state (reached after all those alive in the initial period have died since the production technology is assumed to be linear). Capital is larger at the end of the initial period. Whether it is higher or lower in the asymptotic steady state depends on the fraction of the payroll tax revenue change that is offset by the income tax. A simple calculation suggests that the break-even point for long-run capital is with an 80 percent offset. In a setting of a temporary trust fund buildup, eventually the only effects are those of the income tax increase, thereby lowering national capital, before the model eventually returns to the same long-run capital as would have happened without the temporary trust fund buildup.[8]

These effects are derived in a two-types model (as in Diamond and Geanakoplos, 2003), where one group, called workers, does no savings while the other group, called savers, are standard life-cycle optimizers.[9] For convenience the savers are assumed to plan constant consumption as they would with a discount rate equal to the interest rate. Reflecting the empirical observation that those with higher earnings have higher propensities to save (Dynan, Skinner, and Zeldes, 2004; Saez, 2002), and the patterns of payroll and income tax liabilities by earnings level, the model assumes that workers pay only payroll taxes and savers pay only income taxes. Thus the model should be interpreted in terms of the differences between the two types of taxes at the margin in response to a payroll tax excess over the level needed for pay-as-you-go.

The logic behind the effects in the initial period in both settings is clear. While the initial payroll tax increase comes fully out of worker consumption, the initial income tax cut is partially saved. This savings comes from a forward-looking spreading of a one-period income increase over consumption in all the remaining periods of life and from the assumed awareness that future income tax rates will be increased. While solved in a specific model, this result would follow more generally when payroll taxpayers have a lower propensity to save out of payroll taxes than income taxpayers do out of the income tax, as is plausible.

The model structure is presented in Section 3.3. Analysis of a permanent trust fund buildup is in Section 3.4, with concluding remarks in Section 3.5.

3.3 Model Structure

There is great diversity in earnings, savings propensities, and in the ratios of payroll to income taxes paid. The starting place for this model is the diversity in savings. Assume two-types, so that in each cohort there are n "workers" who do no saving whatsoever and N "savers" who are standard life-cycle savers. We use lower-case letters to refer to workers and upper-case letters to refer to savers. For notational simplicity, assume no population growth. Given the positive correlation between savings propensities and earnings (Dynan, Skinner, and Zeldes, 2000; Saez, 2002), the ratio of payroll taxes to income taxes is higher for workers than savers. For simplicity, we model this by assuming that the workers pay no income taxes at all—a simplification which calls for interpreting the model relative to tax differences. While most savers are also covered by Social Security, for notational simplicity we assume that they are not. Again, this calls for interpretation in terms of the difference between types of taxes paid.

The workers rely on social security for retirement consumption, while the savers do their own retirement savings. In recognition of the tax advantages of retirement savings, we model the income tax as falling only on the earnings of savers. We also assume that labor is inelastically supplied—with work for L periods (length of career) and retirement for $D-L$. We assume that careers are longer than retirements, $L > D-L$. We do not consider differences in career length or life expectancy between the two types.

For simplicity, assume a linear technology, with each worker earning w per period, each saver earning w per period, and capital earning a gross return $R(=1+r > 1)$.[10] With constant payroll taxes, t, each worker would consume $w(1-t)$ while working and a social security benefit of b while retired. For simplicity, assume that savers equalize the consumption each period over their entire lives or their remaining lives when there is a policy change. This would follow from the standard model if the savers have additive lifetime utility functions with the same period utility functions in each period and a utility discount rate that equals the interest rate. With savers choosing the same level of consumption, C, in each of the D periods of life, the present discounted value (PDV) of period consumption for a saver newly entering the labor force is equal to the PDV of L periods of net-of-tax earnings, $W(1-T)$. With unanticipated changes in income taxes, T, we will have to pay attention to the timing of tax changes.

The social security system is partially funded, with a fund of size F. With equally sized cohorts, the Social Security budget constraint if the fund is held constant is

$$n(D - L)\, b = nLtw + rF \tag{3.1}$$

That is, benefits of b are paid to each of the $n(D-L)$ retirees alive in each period. Financing comes from payroll tax revenues and the interest on the trust fund.

For simplicity, we assume no government expenditures other than interest on the outstanding public debt, denoted G. If the debt is constant, per period non-Social Security budget balance implies

$$NLTW = rG \tag{3.2}$$

That is, the interest on the total debt outstanding is paid from the income tax on the earnings of the NL savers in the labor force in each period. Thus the income tax on savers finances the non-Social Security budget while the payroll tax on workers finances the Social Security budget. The public debt held by the savers is $G-F$, the rest of their savings being in physical capital.

3.4 Permanent Fund Increase

Assume that the government increases the payroll tax rate by Δt for one period, using the revenue to permanently increase the trust fund, with the additional interest earnings used to lower the payroll tax rate thereafter. Assume that the government decreases the income tax in the initial period by an amount chosen to offset the fraction α ($0 \leq \alpha \leq 1$) of the additional payroll tax revenue, with no changes in either public consumption or government investment.

$$\alpha nLw\Delta t = -NLW\Delta T \tag{3.3}$$

We assume unified budget balance in all later periods. That is, we are assuming that the deviations from budget balance for the non-Social Security budget are $-\alpha$ times the deviations in the Social Security budget. The analysis would be different if the non-Social Security budget responded to the social security payroll tax revenue less benefit payments, thereby ignoring the interest on the trust fund. Initially this policy change decreases the debt held by the public by $(1-\alpha)$ times the increase in the trust fund. That is, G increases by α times the increase in F. The trust fund increases in the initial period by $nLw\Delta t$. Thereafter, neither the trust fund nor the debt held by the public make further changes.

With benefits and cohort size unchanged, the payroll tax rate can be reduced because of the interest on the increased revenue from the initial tax increase. Thus, the payroll tax rate after the initial period, t', satisfies

$$t' = t-r\Delta t \tag{3.4}$$

Similarly, the income tax rate thereafter, T', is increased to pay the increase in interest from the increase in the public debt

$$T' = T - r\Delta T = T + \alpha r \frac{nw}{NW} \Delta t \tag{3.5}$$

That is, there is an intertemporal trade between payroll taxpayers and income taxpayers, which is balanced in PDV. This also involves changes in the timing of tax payments by each agent and redistribution across cohorts of each type.

The central question is what happens to the time shape of national capital.

3.4.1 National Savings in the Initial Period

To analyze the impact of the changes in payroll and income taxes on national savings in the initial period, we can examine the changes in consumption of workers and savers. In the period of the initial tax change, the aggregate consumption of workers falls by their tax increase: $nLw\Delta t$. In all later periods, the aggregate consumption of workers is higher by $rnLw\Delta t$. This is equal to the return on the increase in the trust fund.[11] Thus, if the trust fund increase were fully an increase in national savings, there would be no impact on national savings after the initial period as the increase in consumption by workers would match the increase in national income. That is, national capital would increase in the initial period and remain at the higher level thereafter. This would be the case in this model if the government did not alter the income tax ($\alpha = 0$). But we have assumed that the income tax may change, so we must examine the response of savers to the income tax changes, which is more complicated.

The change in income tax in the initial period for a saver who is still working is $W\Delta T$, equal to $-\alpha nw\Delta t/N$. Thereafter there is a tax change of $-rW\Delta T$, equal to $\alpha rnw\Delta t/N$ in each of the remaining $L-z-1$ periods until retirement for a saver of age z in the initial period. In PDV terms, the tax change for an age z saver is $(\alpha nw\Delta t/N)(-1+r\sum_{s=z+1}^{L} R^{z-s})$. With a discount rate equal to the interest rate, each saver preserves equal consumption in each remaining period of life. With an unexpected change in taxes starting at age z, the change in consumption each remaining period of life that preserves equality of consumption for the rest of life is

$$\Delta C_z \sum_{s=z}^{D} R^{z-s} = (\alpha nw\Delta t/N)\left(1 - r\sum_{s=z+1}^{D} R^{z-s}\right) \tag{3.6}$$

The change in consumption in the initial period is less than the tax cut for two reasons—anticipation of future tax increases and the spreading of consumption over the rest of life.

Summing over working savers, we get an initial consumption increase for savers of

$$\Delta C = N\sum_{z=1}^{L}\Delta C_z = \alpha nw\Delta t\sum_{z=1}^{L}\frac{1-r\sum_{s=z+1}^{L}R^{z-s}}{\sum_{s=z}^{D}R^{z-s}} \tag{3.7}$$

The change in national savings, ΔNS, in the initial period is equal to minus the change in aggregate consumption:

$$\Delta NS = nw\Delta t\left(L-\alpha\sum_{z=1}^{L}\frac{1-r\sum_{s=z+1}^{L}R^{z-s}}{\sum_{s=z}^{D}R^{z-s}}\right)$$

$$= nw\Delta t\sum_{z=1}^{L}\left(1-\frac{\alpha}{\sum_{s=z}^{D}R^{z-s}}+\alpha r\frac{\sum_{s=z+1}^{L}R^{z-s}}{\sum_{s=z}^{D}R^{z-s}}\right)>0 \tag{3.8}$$

If savers were to consume all of their income tax cut in the initial period and the tax cut balanced the unified budget ($\alpha = 1$), there would be no increase in national savings. But they do not consume all of the income tax cut. They save part of their initial tax cut to finance later consumption and part of it to finance higher tax payments in their remaining working years. Thus national savings increase in the initial period even if $\alpha = 1$. Insofar as α is less than one, there is a further increase in savings in the initial period.

3.4.2 National Capital in the Steady State

In later years the pattern changes. Once the savers who received the initial tax cut have all died off, all savers have the same net earnings and so the same consumption and we are in a steady state. To examine the impact on steady-state capital, we can consider the impact on consumption since in a steady state with no growth, aggregate consumption equals aggregate output. With a linear technology, the change in output is equal to the interest rate times the change in capital. For convenience we now switch from discrete time to continuous time.

In aggregate, workers have consumption which is higher by the amount of their tax decrease: $rnLw\Delta t$. For savers we need to consider lifetime planning, which determines the constant level of consumption that they choose. We need to calculate how their consumption changes in response to an income tax rate increase of $\alpha rnLw\Delta t/NW$.

The lifetime budget constraint for a saver is

$$C'\int_0^D e^{-rs}ds = W(1-T')\int_0^L e^{-rs}ds \tag{3.9}$$

or

$$C' = W(1-T')\frac{1-e^{-rL}}{1-e^{-rD}} \tag{3.10}$$

The change in aggregate consumption by savers is $ND\Delta C'(=-W\Delta T'((1-e^{-rL})/(1-e^{-rD})))$, which equals $-D((1-e^{-rL})/(1-e^{-rD}))\alpha rnw\Delta t$.

Adding the two pieces, aggregate consumption changes by $rnLw\Delta t - D((1-e^{-rL})/(1-e^{-rD}))\alpha rnw\Delta t$, which equals $rnw\Delta t(L - \alpha D((1-e^{-rL})/(1-e^{-rD})))$. Thus aggregate capital changes by $nw\Delta t(\mathrm{L} - \alpha D((1-e^{-rL})/(1-e^{-rD})))$.

Note that if D were equal to L and α equal to 1, this expression would be zero. Moreover, the expression $D/(1-e^{-rD})$ is increasing in D, implying that the expression is everywhere negative for $\alpha = 1$ and $D > L$. Thus if $\alpha = 1$, capital is decreased in the steady state. The result follows from the same logic as above, run in reverse. Workers have lower taxes, all of which flows into higher consumption. Savers have higher taxes, but the induced consumption decrease is spread over their entire lives. With positive interest, there is a smaller consumption decrease for savers than the increase in consumption of workers.

Thus national capital increases in the steady state if $\alpha = 0$ and decreases in the steady state if $\alpha = 1$. For intermediate values of α we get an increase or decrease depending on whether α is below or above a critical value, denoted α^*. To find this critical value, we set the change in aggregate capital to zero:

$$\alpha^* = \frac{L(1-e^{-rD})}{D(1-e^{-rL})} \tag{3.11}$$

For example, with $L = 40$, $D = 60$, and $r = 0.03$, we have $\alpha^* = 0.8$. That is, long-run capital is increased if the income tax cut uses up no more than 80 percent of the revenue raised by the payroll tax increase.

3.5 Concluding Remarks

The distributions of income and social security taxes are very different. Assuming proportional cuts in income taxes in response to a surplus generated by the payroll tax, there are winners and losers from the two tax changes. Also, there are future winners and losers insofar as current tax changes result in future tax changes. It was convenient to model the economy with savers paying only the income tax and workers paying only the payroll tax. In order to interpret the results, we need to consider the net change in taxes for each group that comes about from the policy change. We have no simple way of distinguishing savers from workers, but approximating this by assuming that top quintile of individuals in terms of family income are savers and the rest are workers, roughly one-third of the payroll tax change can be modeled as above (see footnote 7).[12] For the other two-thirds, an offsetting income tax change results in no net effect.

In the formal model, legislated decisions about income taxes and expenditures are influenced by the deficit but not the level of debt. This simplification is missing an effect that is plausible (at least for high levels of debt) and has been found by Bohn (1998). While the debt level influences the deficit level through interest expenditures, it is plausible that there is also a direct influence. (Also missing in the model is any effect of debt levels on government interest rates.)

Given my view that very little of the Social Security surplus showed up in changes in the rest of the budget during the 1980s and 1990s, I think that most of the surplus has represented an increase in national savings. It is not clear what impact the surplus has had on the Bush tax cuts—the first was sold as not touching the Social Security surplus and the second happened despite touching the surplus. Taking a view that the political process was more responsive to this change in the unified budget balance than I believe to be the case, one still has a short run increase in savings, while in different years after the baby-boomers retire and the accumulated trust fund is used to help finance their benefits, there are increases and then decreases in capital.

The development of the OLG model and its application to analyzing social security represents an achievement of Paul Samuelson that will influence at least the next century.

Notes

1. Arrow wrote: "The analysis of concepts that lie so close to the roots of the social essence of humanity can never be definitive, but certainly the formulation of Bergson and Samuelson profoundly affected the direction of all future thinking, at least by economists." Page 15.
2. Musgrave wrote: "Never have three pages had so great an impact on the theory of public finance." Page 141.
3. Tobin wrote: "In this appreciation of Paul Samuelson as macroeconomist I shall concentrate on his contributions to the methodology and substance of macro-model building and to the positive and normative theory of stabilization, with emphasis on fiscal policy. This was Samuelson's own emphasis in his first twenty-five years, both in his pathbreaking early papers on multiplier statics and dynamics and in his crystallization of the neoclassical synthesis after the second world war." Page 191.
4. Samuelson's role in the revival and extension of Ramsey pricing was not so central, but that topic will last as long as there are linear taxes. And his analysis of tax deductibility of economic depreciation (1964) while a big help in my analysis of adjusting income taxes for inflation (1975), has not generated much response that I am aware of. And his introduction of the LeChatelier principle into economics (1947) helped my optimal tax paper with Mirrlees.
5. I ignore any possible impact on employer-provided pensions.
6. In 1995, the individuals and families in the top quintile of people in cash income have incomes above $71,510 (CBO, 1998). These are estimated to pay 71 percent of the individual income tax and 41 percent of social insurance taxes (Table 5). (They also pay 66 percent of the corporate income tax.) Social insurance taxes include the uncapped Medicare tax, and perhaps the unemployment insurance tax, as well as the capped Social Security tax. Ignoring unemployment insurance and using SSA data (2002) to convert the percentage of total payroll taxes into the percentage of Social Security payroll taxes (since almost all of Medicare taxes due to earnings above the Social Security taxable maximum of $61,200 are paid by the top quintile in cash incomes, we calculate as if all of it were), we estimate that those in the top quintile pay 37 percent of the relevant payroll tax. Thus approximately one-third of exactly offsetting income and payroll tax changes would be a redistribution between the top quintile and the other four quintiles.
7. For an interpretation of the historic record, see Diamond and Orszag (2004, pp. 47–54).
8. A longer working paper version of this paper includes examination of a temporary trust fund buildup, to follow more closely the plan for addressing the retirement of the baby-boomers.
9. Two-types models can have very different results than representative agent models. For example, see Diamond and Geanakoplos (2003) on diversifying Social Security assets and Saez (2000) on taxing interest income. The contrast

between types is stark and overstates the differences between them—many people doing little saving may have a small response to a tax change rather than zero and many people doing considerable savings may not be so responsive to future taxes as is assumed in the model.
10. Without this linearity assumption we would need to track the changes in wages and interest rates and their impact on taxes and savings.
11. The lack of growth of the labor force simplifies the calculation.
12. This assumes that the income tax change is proportional to average taxes.

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4

Prospective Shifts, Speculative Swings: "Macro" for the Twenty-First Century in the Tradition Championed by Paul Samuelson

Edmund S. Phelps

> The markets have predicted nine of the last five recessions.
>
> Paul A. Samuelson

In recent decades many of the best minds in macroeconomics have been dedicated to the development of dynamic, recursive models portraying the market economy as a stationary stochastic process in rational-expectations equilibrium. Prototype models include Lucas (1972) and Lucas and Prescott (1971, 1974). Such is the influence of these powerful models that central bankers boast that their monetary policies incorporate the considerations highlighted by these models. I myself, in close or loose collaboration with Columbia colleagues, injected staggered wages and staggered markups into just such models.[1] The pride and enthusiasm the innovators and developers take in this project is understandable. I for one place some heuristic value in these models for some purposes, so I have no intention to dismiss them. Yet some of us dissent from those who say that with the perfection of these models the task of macroeconomics is becoming fully achieved. Paul Samuelson's is a long dissenting voice, as I will document below.

Models I built in the 1960s introduced "expectations" about the current or incipient general wage level as a determinant of current or incipient employment or, if it could not jump, its rate of change.[2] The models implied that the unemployment rate would never stay far from some medium-term "natural rate" determined by nonmonetary mechanisms and forces, since wage expectations begin adjusting to a large and long-lived error. In the back of my mind, the demand increase under study was caused by some discrete and idiosyncratic event of a permanent nature; for example, the demand for money was shifted down by some innovation in banking that initially was not widely understood. After the event and its impacts, wage-level expectations (and thus the wage level itself) did not react by enough immediately to drive the true "expected value" of employment to its natural level since there was no knowledge of the event's permanence.[3] There was no "class" of disturbances in which the event could be placed about which there was statistical knowledge.[4]

The rational-expectations revolution utterly transformed these expectational models. Abstracting from imperfect knowledge about the economy's structure and the structure's shifts, it took the expectational setup but equated expectations of wages (and prices) to expected values. "Macro" became the study of the vibrations (to use Robert Hall's coinage) of a statistical equilibrium induced by stochastic "shocks" of known statistical properties and of a stationary sort. The idiosyncratic forces that in 1960s thinking were behind large demand changes were replaced by a random disturbance term that is the sum of myriad small and independent forces. But the loss of historical concreteness is not the main cost of the revolution. If it were it might well be regarded as offset by the benefit of the findings generated.[5]

There are two difficulties with rational expectations. One is that, even on favorable terrain, rational expectations lack microfoundations. The meaning of rational expectations goes far beyond correct expectations in a still, tranquil setting: it means correct expectations in every state of the economy that turns up. So an economy's ability to acquire rational expectations must be demonstrated. Unfortunately, there seem to be obstacles to actors' convergence to a model's rational-expectations solution in stationary stochastic settings.[6]

The trouble with rational expectations that this chapter addresses is that they cannot be applied to the expectations that are at the core of enterprise economies: the fresh visions and fears that entrepreneurs and financiers have about the evolving future—novel forces in the future and factors affecting the future consequences of innovative actions in the present. (The visions and fears of future entrepreneurs and financiers are also absent.) The attractiveness of the dynamic recursive RE—of restricting attention to objective and observable forces, which rational might imaginably apply to—led the model builders to turn their backs to a whole category of forces to which rational expectations would not appear to apply.[7] This shortcoming deprives recursive dynamic models from giving any meaningful explanation of the big swings of economic history.

Excluding expectations about the future has operated to reorient macroeconomics 180 degrees from what it had been over most of the twentieth century. From Spiethoff and Cassel to Schumpeter, Knight, and Keynes and on to Samuelson, the major figures in macroeconomics all thought that the big business swings were triggered by new expectations by entrepreneurs about the future profits to be earned from new investments. This orientation had been inspired by studies of the German School connecting surges in investment activity to commercial prospects opened up by technological and navigational discoveries.

Paul Samuelson almost single-handedly synthesized and transmitted to the next generation this macroeconomics tradition in all its breadth. The first tenet of these figures from Spiethoff onward was a lesson that Samuelson never tired of emphasizing—the centrality of the future. The importance of the future was driven home for me when I was a young student, as it doubtless was for many other readers, by Samuelson (1948) with one of those lyrical images he so often summoned up in making a major point: "An outside observer would be struck with the fact that . . . almost everyone is doing work of a preparatory nature, with final consumption a distant goal." (Samuelson, 1958, p. 46.) If expectations about the future drive the directions of economic activity, which was Samuelson's context, it is hardly surprising that they also drive the level of activity, as Cassel and Keynes argued in their different models, the one nonmonetary, the other monetary.[8]

The next tenet of this macroeconomics tradition that Samuelson passed down is that future developments are subject to radical uncertainty, which was basic to the thinking of Knight and Keynes. Samuelson as an undergraduate at the University of Chicago had the opportunity to learn about Knightian uncertainty from Knight himself. In a meditation on the problem of inference in a changing world (1963), Samuelson does not presuppose that the current structure of equations describing the system is a known structure; it is unknown and the problem of the economic actors is to decide how much weight to give new evidence in revising their guesses or estimates of the structure.

Samuelson added that, as he saw it, the probabiltity distributions of the basic forces driving the economy are nonstationary, contrary to the rational expectations models. Of course, the presence of nonstationarity, such as random walk behavior, must further limit the possibilities of forecasting and further widen the errors from using the past to forecast the future.

Owing to this complex of factors, Samuelson concluded, market expectations about whole *classes* of prices and earnings, namely, asset prices in the future and thus asset prices in the present and earnings in the future, cannot be supposed to predicted by some model's rational expectations, since they are not predictable. Although Samuelson was a pioneer of rational expectations theory and appreciated the rational calculation that went into the relative price of, say, Oracle shares relative to the price of SAP shares or IBM shares, he did not regard the broad market indexes of share prices as an outcome of rational expectations (Samuelson, 1998). Samuelson's famous quip that "the markets have predicted nine of the last five recessions" could be interpreted to mean simply that the stock market is a sideshow without consequences for investment and employment activity. But it can just as well be interpreted to mean primarily that markets are skittish and take fright more often than they would under rational expectations, if such foresight were possible.

Thus Samuelson for decades virtually stood alone preserving and illuminating the perspective that the rational-expectations movement excluded.

The consequences of this exclusion have been striking. Real business cycle theory has not illuminated the US boom in the late 1920s, the record-length Great Depression in the United States in the 1930s, the gathering slump in the United States in the 1970s, Europe's deep slump in the 1980s, the still high unemployment on the Continent, and the investment boom in several economies in the late 1990s.[9] In contrast, models oriented to prospects of future parameter shifts have been able to make some sense of the 1920s boom in share prices, investment and employment in the United States, the huge swelling of unemployment in Europe and Japan from the mid-1970s well into the 1980s, and the great investment boom in the second half of the 1990s in several economies.

Some recent papers of mine are all about a category of influences on economic activity that might be called *future prospects*.[10] In this view, at any moment there may occur a *new* future prospect—either a qualitatively new development or a purely quantitative change in some future prospect. This concept is not exactly the "animal spirits" in Keynes (1936). A "prospect" will refer to *expectations* of some *future event*, or *state*, and "prospects" will refer to the set of such future events or states. In general, a prospect has an influence—the more so the less uncertain it is—on the willingness of existing firms and start-up firms to invest and the willingness of investment bankers and the stock market to finance them; thus a prospect or a change in its uncertainty may lift or depress business "spirits." (Possibly Keynes thought such "spirits" depended on so many prospective future events and states that it would be unworkable to hope to solve for spirits as a function of prospects.) Neither does attention to future prospects mean a return to the "optimism" and "pessimism" dwelled on by A. C. Pigou (1927, Chap. VII). His thesis was that the response of investments to a class of future prospects exceeded what "rational" calculation would suggest ("errors in optimism"). The recent papers I cited analyze the effects of future prospects that it is "rational" for investments of various kinds to respond to.

Part I below takes up three future events and shows that, in the model used, the prospect of each has an impact through the capital market channel on the course of economic activity. In each case I point to *topical* or *historical* evidence bearing out such an impact on economic activity. Part II looks at share prices as a proxy for the shadow values of the business assets to get a sense of the *statistical* importance of future prospects in general—of speculation about the future—relative to the importance of unexpected developments actually observed, that is, shocks. Part III concludes that considering future prospects helps us to understand the big swings noted above. It also suggests that uncertainty about future prospects strengthens the modernist views that wage and price levels are not correctly forecast.

Let me remark here that much of the analysis below is devoted to the benchmark case in which, when the new expectations about the future arrive, the economy's participants are supposed to work out the correct-expectations path to that new future—as if they possessed rational expectations over the rest of the future. In that respect the analysis resembles rational-expectations methodology. But in other respects it is different. The new expectations are not a regime—one of many regimes that are already incorporated into some grand model and have known probabilities of switching on and switching off. In general, the new expectations refer to a future that has never been imagined before. The picture is one of an open system of ever richer possibilities rather than the closed system of rational-expectations equilibrium.

4.1 Future "Debt Bombs," Productivity Surges and Wars

In the category of future prospects perhaps the oldest topic among economists is the prospect—for simplicity, the newly arisen prospect—of a delayed-fuse "debt bomb," as I have dubbed it—a "time bomb" of exploding public debt, such as the present enactment of a tax cut to become effective at a future date and with a sunset provision soon thereafter. (Thus there is some small interval over which there is a big government deficit.) Another topic in this category is the sudden expectation of a future step-increase in productivity at some specific date. A third topic, which I bring up here, is the expectation of the start of a war or of the end of a war. Maybe a terrorist attack would be a more modern interpretation. In all these cases I will discuss—very informally—some piece of historical evidence.

I will *not* allow for differences of opinion about the size or the timing of the prospective events; where a probability is introduced, it is a subjective probability held by all. This restriction may block dynamics of interest in some cases.[11] Yet it does sometimes happen that a conventional view is virtually universal.

4.1.1 Future "Debt Bombs" and Pension Overhang

The literature on the present expectation of a future fiscal shock goes back quite far. In the 1980s Keynesian treatments were offered by Olivier Blanchard (1981) and William Branson (1986). They obtain the proposition that enactment of an explosion of transfer payments or of temporary tax cuts in the neighborhood of some future date $t1$ may be a depressant for real asset prices at that time and, if so, the public's grasp of that prospect will have repercussions for the level of real asset prices in the present. However, the proposition undoubtedly antedates Keynesian modeling. And since many "future shocks" are several years off, few readers can be satisfied with an argument resting on the money wage/price stickiness of a Keynesian model.[12] A few years ago, however, Hian Teck Hoon and Phelps (2001) extended a structuralist model of the closed economy model, this one with a customer market, to show that the sudden prospect of a temporary future tax cut or temporary future transfer payment, if built at once into the expectations of firms, causes immediately—thus, ahead of the event—an anticipatory drop in the shadow price they attach to a unit of the business asset—with contractionary consequences for employment.

It will be easier to argue such a proposition from a structuralist model that is less rich, namely, the *turnover-training model* (Hoon and Phelps, 1992). Take the closed economy case. The increase in the public debt around some future date $t1$ can be seen to have two contemporary effects: first, to rebalance the budget after the splash of debt issues, it will then be necessary to service the increased debt by an increase in tax rates: either tax rates on *wage income*, which will have deleterious effects on employees' quit rates and thus raise business costs, or on business income, which will directly reduce after-tax profits; second, it will force an elevation of real interest rates at that time, provided we exclude the Ricardo–Ramsey–Barro case where government debt is not net wealth. Both of these impacts will cause the shadow price of the business asset—the shadow price of an extra job-ready employee—to be lower at that time and beyond than it otherwise would have been, evaluated at the original, or reference, level of employment. By a standard inductive argument it follows that the shadow price at the present moment $t0$ is also depressed below what it was; in fact, we do not need such an inductive argument, since the integral giving the present shadow price involves increased interest rates after $t1$ and decreased gross profits (or quasi-rents) after $t1$, so the impact on the value of the integral from $t0$ is unambiguously negative.

A beautiful observation by Hian Teck is that the short-term real interest rate will actually drop at $t0$, since consumption will jump up and thereupon be steadily falling, thus possibly causing the long real rate of interest required by savers to decrease at first before rising toward its elevated future level. Hence, the argument of skeptics that the specter of bulging future deficits cannot be contractionary, otherwise we would observe an elevation of real interest rates, is unsatisfactory because, theoretically, the contractionary effect does not imply and does not require any such elevation of interest rates—only a drop in the shadow value attached to the business asset.

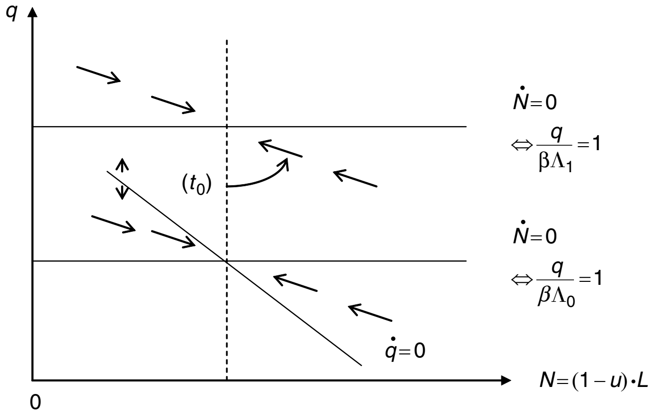
In the small *open-economy* version of the turnover-training model, in which output is sold at unchanging terms in the world product market, the public debt's net wealth impact on *future real interest rates* will not be operative, since domestic interest rates are given by overseas real interest rates, which the country is too small to affect. Yet the increase in public debt at $t1$, most clearly if it results from a tax cut on wage incomes, has the effect of making workers richer (at the expense of future generations of tax payers). And this extra net wealth may increase employment costs by worsening employees' quit rates, shirking and unreliability.[13]

Some cross-section evidence. There is evidence of the empirical significance of such future fiscal prospects for the present level of economic activity. Investors in many countries have come to recognize a huge looming overhang of pension liabilities in relation to present projections of GDP and tax revenues, owing either to the government's having overestimated the growth of tax revenue when they were setting benefit levels or to having shrunk from raising tax rates by the amount that was necessary for intertemporal budget balance in view of the bulge of baby boomers soon to retire. Allison Schrager, a doctoral candidate at Columbia, has regressed the average price-dividend ratio (and soon the price-earnings ratio) on the projected pension benefits to GDP ratio alongside standard explanatory variables for a cross-section of OECD economies. The results show a statistically significant coefficient of the right (negative) sign on the pension variable. If right, the result means that prospect of delayed increases in public debt and of paper wealth from pension entitlements do indeed impact the capital market, just as the theory implies. There is also ample evidence that a decrease of share prices has, in turn, contractionary consequences for the level of economic activity as measured by unemployment and participation rates.

4.1.2 Future Productivity Surges

To analyze the sudden expectation of a future step-increase, or lift, to productivity, we can revert to the turnover-training model, which is so convenient. A simple analysis is provided in two recent papers (Fitoussi *et al.*, 2000; Phelps and Zoega, 2001).[14] The basic proposition there is illustrated by a phase diagram. Here I have simplified the model further by replacing the premise of rising marginal hiring costs with the premise of constant hiring costs. As a result the locus of points at which the stock of employees is constant at a firm is horizontal. (If employment is increased, the quit rate is increased as a result, with the consequence that there must be an equal increase in the hire rate; but since the derivative of the hire rate with respect to the shadow price of the employee is infinite in the constant-costs case, no increase in the shadow price is required to maintain a steady state at the increased employment level.) The phase diagram in Figure 4.1 shows that the shadow price jumps up, causing employment to grow until *t*1, at which point the path of the system must have just reached the new saddle path; from that point, the system follows the new saddle path, proceeding toward the new rest point. The equations of the dynamic system are in the aforementioned papers.

To gain the essential insight, we need only consider an integral expression giving the value of the shadow price at the present time, *t*0. The step-increase in the prospective future rents on the business asset—the employees—unambiguously increases the value of the integral, evaluated at the initial employment path. And, again, any such increase in the shadow price of the employee, unaccompanied by any increase in the opportunity cost of training additional employees, unambiguously stimulates a sharp increase in hiring, which pulls up employment.



$$\Lambda = \begin{cases} \Lambda_0, & t_0 \leq t < t_1 \\ \Lambda_1, & t \geq t_1 \end{cases}$$

Figure 4.1 Anticipatory effects of the prospect and the realization of a future step-increase of total factor productivity

Some historical evidence. How can we adduce evidence that the investment boom of the late 1990s in the US economy and several others did rest, at least to an important degree, on newfound expectations of a lift to productivity on the horizon? And, similarly, how can we test the thought that the great investment boom of the 1920s was likewise driven by expectations of rapid productivity growth over the future? Perhaps we can never obtain strong enough evidence to satisfy all skeptics. However, for me at any rate, it is important circumstantial evidence supporting that interpretation of the 1990s boom that productivity growth has in fact been startlingly rapid in the four years beginning in 2000 and appears to be slowing down only very gradually. With productivity growth so rapid in those years, it is easy for me to believe that many managers in industry had information in the second half of the 1990s leading them to expect a very substantial lift to productivity and hence to investment returns in the next several years.

Now consider the 1920s boom. The parallels of that boom with the 1990s boom have led several of us to dig out the Kendrick data on productivity growth in that bygone era. I was stunned to see in Commerce Department's *Long Term Economic Growth 1860–1965* that productivity lifted off like a rocket during the 1930s.[15] Alex Field's calculation in a paper a few months later showed that growth rates of total factor productivity were unprecedented between 1929 and 1941.

I alluded to the productivity gains in the 1930s and, to date, in the 2000s in a couple of pieces in the financial press where I argued that Alan Greenspan was mistaken to think that the recent spate of productivity gains translates into high employment: if the productivity gains were already anticipated in the mid- and late 1990s and were precisely the inspiration for the wave of new investments at that time, then the realization of these gains will not occasion another wave of investment; the realized gains are "how booms end, not how they begin."[16] Employment as a ratio to active-age population in this decade too is still a bit subdued, though it has recovered most of the lost ground by now. The question, now resolved, was only whether other forces may stall that march back to full normalcy, as happened in 1937–38 when the clouds of war derailed the recovery, leaving employment depressed for the rest of the decade.

4.1.3 War Prospects

The essence of my thesis is as follows. At the present time, the value, to be denoted $q(0)$, that a firm's manager would put on having an additional functioning employee is a probability mixture of the value of that employee in the scenario in which war breaks out, weighted by the subjective probability of war, π, and the value in the scenario in which war does not break out, weighted by the probability the war does not break out, $1 - \pi$. The war scenario gives a lower value, since the manager anticipates that there will be an increased tax burden on the firm's profits or sales or both in the event of the war. The conclusion that can be deduced is that any *small increase* in the subjective probability of war *lowers* the value of the probability mixture—the so called "expected value" of the two integrals (the one the no-war integral, the other the war integral). The argument for that conclusion involves the point that the firm's reactions in the event of war do not have to be factored into the result, since small adjustments by the firm in its hiring rate will not have a first-order effect on the value of the integral, the hiring rates having been in the neighborhood of their q-maximizing levels to begin with.

Another proposition that is obvious at least to economists is that, with the passage of time, the date at which the war is feared to break out draws *nearer*—unless $t1$ is pushed back one day (or more!) for every day that goes by after $t0$. So the present discounted value of having an extra employee, which means discounting back to the current time t, not to the initial time, $t0$, is *falling*, since the *losses from the war* in the event it occurs are getting nearer, hence *discounted less heavily*.

Once a war has broken out, the passage of time is the firm's friend: The date at which the war is hoped to *end* draws nearer unless that date, $t2$, say, is pushed back one day or more for every day that the war goes on after $t1$. Here the *gains* from the war's end are being discounted less heavily as the end of the war nears.

Some evidence. If I am not mistaken, then, this analysis leads to the proposition that the prospect of war ahead causes a drop in the shadow prices put on business assets. In almost any theory, there will be, in reflection of that drop, a sympathetic drop in share prices too. And if, during a war, the prospective time left to go before the war's end keeps shrinking as expected or even faster than expected, these shadow prices—and share prices too—will tend to be recovering. Is this what happens, at least in normal cases? Certainly the evidence in the years leading up to the Second World War bear out this story. Painting with a broad brush, I would say, going largely by my recollection of the data, that share prices fell and fixed investment expenditures as a share of GDP fell in the United States from 1937 to 1941. The same was true, I remember from a look at the data years ago, in the Netherlands over the late 1930s. Then, during the war years 1942–45, the stock market in the United States was strongly rising—in a recovery mode.

I would add that the real prices of shares did not recover fully to their lofty levels of 1936 and 1937 for quite some time—not until the last years of the 1960s, if I remember correctly. I would say by way of explanation—entirely in the spirit of my thesis that future prospects are important—that the cloud of the cold war came over the US economy by 1948, blocking any chance of a full recovery. With the Korean War of the early 1950s this tension broke out into open conflict.

4.1.4 Drawing Conclusions

If these future prospects and possibly others not treated here are empirically important, we can conclude that real-life economies with an active commercial character are almost *never* "vibrating" up and down their saddle path. They are almost always *off* their saddle path. Somewhat surprisingly, the trajectory of shadow value of the employee in the above model jumps off the saddle path in spite of the simplifying postulate of constant (rather than increasing) marginal hiring costs.

The pressing question now is whether changes in future prospects are pronounced enough from one year to the next or from one decade or era to the next to generate a generally important—and typically fluctuating—discrepancy from the saddle path. I would like in the second part of this chapter to tackle that question.

4.2 Evidence of the Mutability of Future Prospects

A long-time theme of mine regarding fluctuations is that most of the national economies of the past few centuries are *mutable*—especially the more capitalist economies and those highly interdependent with the capitalist world.[17] I mean that a capitalist economy is always changing qualitatively and often lastingly. So the description of a theoretical economy given by a stochastic steady-state model does not really fit this sort of economy. Maybe some macrostatistics will pass some tests for stationarity but, if so, that may indicate only that it takes a few decades for an economy to transform itself; it doesn't mean that we can use a model estimated on nineteenth century data to obtain the best possible prediction of, say, the rate of technical progress or the long-term natural unemployment rate. Although some of these "parameters" appear to be trendless, they also appear to be capable of shifting perceptibly from one half-century to another. Some theorists speak of regime change or model change, but why not admit that the regime is always evolving, sometimes abruptly, and the model with it? I have to add that I am not exactly sure what it means to speak of the best possible, or true, prediction or the expected value of these things: using what model?

As Part I showed, the shadow value of the business asset is capable of jumping off the saddle path; in fact, the shadow value may never be on the saddle path for a single day of its life except to pass through to the other side on some occasions. (This is true even though I posited constant costs of hiring.) But how much do these shadow prices move in fact? And do their movements, such as they are, match up with shifts (surges) and swings in investment activities of the various kinds—hiring, customer chasing (advertising, cutting markups), plant and office construction, etc.?

4.2.1 Inferential Movements in the Shadow Values

Hian Teck Hoon came across a paper by Casey Mulligan (2002) that examines the part played by public finance distortion in the swings in the American labor supply over the period 1889–1996. For his neoclassical model Mulligan adopts the neoclassical model of labor-leisure choice, with its condition, $\mathrm{MRS}(C, \overline{L} - L) = \nu^h$, where the MRS function gives the marginal rate of substitution (MRS) between consumption and work, or "marginal value of time" in terms of the final good, and is increasing in current consumption C and in hours worked L, hence decreasing in leisure; the right-hand side variable, ν^h, is the after-tax hourly wage rate. The latter is related to the firms' demand wage ν^f and to the proportional tax rate τ on after-tax wage income by $\nu^h \equiv (1 + \tau)^{-1}\nu^f$. Invoking pure competition, he equates ν^f to the marginal product of labor, MPL. The result is $(C, \bar{L} - L) = (1 + \tau)^{-1}$ MPL. The implication is that an increase of τ, in decreasing the right-hand side, operates to decrease hours, given consumption and the value of MPL. Mulligan argues from his empirical exercise that marginal tax rates are well correlated with labor–leisure distortions at low frequencies, but they cannot explain the distortions during the Great Depression, the Second World War and the 1980s: the decade-to-decade fluctuations in consumption, wages, and labor supply do not jibe very well with this competitive equilibrium model.

From the perspective of my structuralist models, the difficulty with this competitive-equilibrium theory—adopted wholesale by the real-business-cycle school in the 1980s—is that it lacks business assets and the possibility of corresponding fluctuations in the shadow values attaching to those assets; as a result, the model is hopelessly myopic. Hian Teck, viewing the matter accordingly, reasoned that to understand the depth of the downturn in the 1930s it might be of crucial help to introduce such shadow prices. From customer market theory, Hian Teck derived a contrasting employment equation: In the Phelps-Winter model, a firm generally profits from the sluggishness of information, for it can "mark up" its price above marginal cost without at once losing all its customers; this transient monopoly power gives value to its current stock of customers. Let m denote the markup $(P - \mathrm{MC})/P$, where P is price and MC is marginal cost. Then it is straightforward to deduce that $1 + m \equiv \Psi$, where the function Ψ makes m inversely related to $\tilde{q}$, the shadow price that firms attach to a customer when taken as a *ratio* to how much output a customer has to be supplied. (That ratio is fully analogous to Tobin's Q ratio.) In this model, the labor-market relation becomes $\mathrm{MRS}(C, \bar{L} - L) = (1 + \tau)^{-1}\,[\Psi(\tilde{q})]^{-1}$ MPL. If we substitute for MPL the parameter $\wedge$ and, in the closed economy case, substitute $\wedge L$ for C in MRS, then L is fully determined. An increase of $\tilde{q}$ pulls up the right-hand side (i.e. it increases the ν^h that firms are willing to offer); since $\mathrm{MRS}(\wedge L, \bar{L} - L)$ is doubly increasing in L, that induces an increase in hours supplied. Thus the *markup* wedge between net pay and labor's marginal value productivity joins the *tax* wedge as a potential factor in the determination of the equilibrium (i.e. correct-expectations) path of employment, here average hours. Sometimes both are needed in an analysis since they move in opposite directions, so the one helps to escape from the other.

My point here, after that lengthy exposition, is that we can *infer* what the 1930s shadow price of customers must have dropped to from the 1920s by solving for the $\tilde{q}$ ratio that solves the equation, given the data and given our "knowledge" of the functions appearing in the equations. We can do that for each decade of the past century, thus obtaining a century of inferred $\tilde{q}$ ratios attaching to the business asset we call the customer.

To do the same with the turnover-training model we can use the equation giving the incentive wage as a function of the unemployment rate and the shadow price of the functioning employee to solve for the shadow price that delivers the correct wage rate, taking account of tax rates. Thus we could calculate decadal levels of the shadow value of the employee.

Since these shadow values of the various business assets have a lot of work to do to reconcile the equation with the observed employment levels in the 1930s, the Second World War years (1941–45) and the 1980s, one can presume that the required shadow values will exhibit quite a lot of fluctuation from decade to decade. This is one piece of circumstantial evidence for believing that future prospects are important. It suggests that, over the past century, the world real interest rate, trend growth rates of national productivity and tax rates have not shown enough variation to be able by themselves to push the shadow values enough to explain the huge swings of the 1930s, the war years, and the 1980s.

4.2.2 What do Share Price Time Series Say?

To obtain another somewhat indirect view of the movements of the shadow prices of business assets, we might do well to examine the time series of stock-price indexes, such as the Standard and Poor 500 index (and its predecessors).

We would like to find evidence that would help to establish (or to dis-establish, as you like) the proposition that share prices are driven by subject understandings of future prospects to an important degree, not just by unexpected developments in the situation and performance of the economy. How to do that? To do that we need to distinguish the actual change of the share price level from the change that was previously expected; then we have to decompose this into the component attributable to surprises in observable things and the component presumably reflecting unanticipated revisions, based possibly on reappraisals of existing information or surprising new information, of the economy's future prospects. The dichotomy is between the unexpected changes in observed levels of present variables and the unexpected changes in the forecast future.

To this end, let Rx denote the logarithmic rate of change of any variable x; and let $F(y)$ denote the expected, or forecast, value of any variable y. In this notation, the familiar Fisher equation applied to the expected proportionate rate of change of the real share price is

$$F(R(p)) = [F(r) - d/p] \tag{4.1}$$

where p denotes the real share-price level, r is the short-term real rate of interest and d/p is the dividend per share as a ratio to price per share (hereafter, the dividend-price ratio). The utility of this equation lies in its implication that the right-hand side can serve as a proxy for what is not directly measurable, namely the expected algebraic real capital gain.

If we subtract the *actual* $R(p)$ from both sides, add to and subtract from the right-hand side the current growth rate of real earnings per share, $R(e)$, and multiply both sides by minus one we get

$$R(p) - F(R(p)) = d/p + R(e) - F(r) + [R(p) - R(e)]. \tag{4.2}$$

This equation is reminiscent of the thesis of John Bogle (2000) that there were in the history of the US stock market a *few* decades in which the dividend yield plus the growth rate of real earnings per share, $d/p + R(e)$, thus the rate of return that would have been earned on stocks if the price-earnings ratio had not changed—neglecting F(r), the right-side of (4.2) if the bracketed expression were zero—was less than the rate of return from the rise of the price-earnings ratio, $R(p) - R(e)$. The 1950s were one such decade, Bogle observes. But there were not many.[18]

For our purposes we want to look not at the returns to shares but rather at the real *price* of shares as a ratio to, say, GDP per share, since that can serve as a crude proxy for the shadow price of the business asset as a ratio to the asset's productivity and this cousin of the "Tobin Q ratio" is a key determinant of the current investment expenditure on the asset. If for the sake of exploration we take that ratio to be proxied satisfactorily by the price-earnings ratio, that approach can be investigated by going back to equation (4.1) and rewriting it to derive

$$R(p) - R(e) = [F(r) - d/p - R(e)] + R(p) - F(R(p)). \tag{4.3}$$

This equation decomposes the *growth rate* of the price-earnings ratio into two components, the explained part and the unexplained part—the latter being the unexplained portion of the growth rate of share prices. The former component, in square brackets, is the *expected* growth rate of real share prices, as stated in (4.1), *net* of the growth rate of real earnings per share. That component is in a sense the *explicable* portion, or—better perhaps—the *determinable* portion, of the growth rate of the price-earnings ratio, since the expected growth rate of share prices is theoretically given by the observed excess of interest rate over observed dividend yield. The lower is the ratio of dividend to price or the higher the expected real interest rate, the higher the expected growth rate of the real share is implied to be. The second component is just the residual portion of the growth rate of real share prices, which is given by the last two terms. (Capital gains on shares based on the *nearing* of unchanging expected future events are theoretically reflected in the first two terms. Unexpected growth in *earnings*, other things equal, also cause unexpected growth in the share price but not unexpected growth of the price-earnings ratio.) This residual component is, in the same sense, the *inexplicable*, or *undeterminable*, part of the growth rate of the price-earnings ratio. As should be clear, the framework here portrays this latter component as driven by *changes* in *future prospects*. It could therefore also be dubbed the *speculative* component of the change in the price-earnings ratio. This part of the change in the price-earning ratio the outsider-modeler and outsider-analyst would have no clue about, especially if future prospects are multidimensional, even infinite-dimensional.

The attached Figure 4.2 plots separately the two components, using 5-year backward growth rates and representing the expected real interest rate by the actual real rate, over the period 1920–2000. It is clear that the residual, which I interpret as speculative, is large, especially in some epochs, for example, the early part of the gathering boom in the second half of the 1990s. That suggests that changes in the future prospect, though heretofore neglected, are indeed a powerful cyclic force driving employment swings.

I would remark that the theory sketched here offers no theoretical reason to believe that the *speculative*, or undeterminable, component, which is the excess of the *actual* over the *expected* growth rate of real share

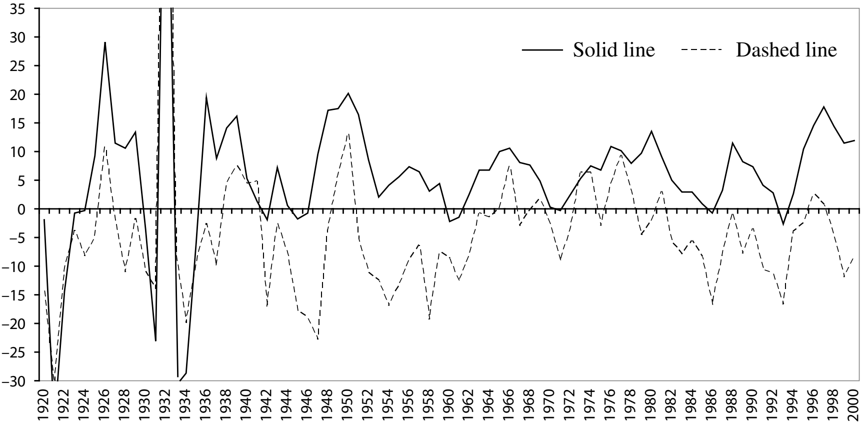


Figure 4.2 The 5-year growth rate of the average price-earnings ratio of standard and poor 500 stocks (solid line) and the part explained by the excess of the real interest rate over the expected growth rate of the average share price net of the average growth rate of earnings per share (dashed line), yearly, 1920–2000.

prices, is the *more* powerful influence on the actual growth rate of the price-earnings ratio and thus of the improvement ahead in business activity. (Theoretically the ups and downs of real interest rates and of the underlying determinants of dividends per share could be the more important force.) It does appear, though, that the episodes of rapid expansion—the last half of the 1920s, the years 1934–37, the early 1950s and the latter half of the 1990s—were more strongly associated with a rise of the *speculative* component (i.e. the difference between the total, charted by the solid line, and the determinate part, charted by the dashed line) than with the determinate component. However, this question is not crucial. It is enough to show that the speculative component is an important driver of the price-earnings ratio, thus of Tobin's *Q*, and hence investment activity and "natural" employment.

4.3 Concluding Remarks

My thesis here is that macroeconomics will flourish in the twenty-first century—it will capture the causes and mechanisms of the big swings in growth and in economic activity—only if it returns to the grand tradition championed by Samuelson. That means, to begin with, the introduction of future prospects into our macro models. Investment, growth, and the (current) natural level of employment are *conditional* on expectations about the future. Incorporating that into our thinking is crucial for our theory of how the economy works, the economic history we write, and the policy advice we give.

At the level of theory, the level of activity is *always* being driven—up or down—by the prospects for the future *in the minds* of entrepreneurs, financiers and holders of stocks. The logic of these propositions is straightforward. In the structuralist models I have been using, firms' rates of investment in employees, customers, plant, and office space are a key force determining the course of economic activity, as measured by employment and unemployment. And future prospects are a driver of these investment decisions. In the models here, which abstract from nontradeable physical capital, Tobin's *Q* is driven by the *expected* future productivity developments, by *expected* future fiscal burdens (and expected future immigration helping to deal with them), and *expectations* about peace in the future; "shocks" affecting *actual* productivity, foreign labor and perhaps actual peace may make little difference and are, in any case, a different story.

The revival of the grand tradition also means the recognition in our models of Knightian uncertainty and the complications that follow from it. Fellner had the idea that uncertainty operates to scale down the probabilities that decision makers attach to the contingencies they know about in recognition of what they know they do not know. Where there is uncertainty there is also the possibility of speculative gyrations. Frydman and Goldberg (2003) have delved into the mechanism of overshooting so common in stock markets and foreign exchange markets.

To conclude: Until macroeconomics opens up to the study of economies driven by new conceptions, new visions, new fears—mostly about the future—and comes to grips with the attending uncertainty our macroeconomic models will not fit very well the world we live in. There will always tend to be individuals willing to step out of the tightly closed models and fashion special-purpose models with which to address current questions about the future. These efforts will go better, though, if a range of future prospects become a normal part of standard models. The big opportunity for progress in macroeconomics now appears to be in that direction.

Notes

1. See Phelps and Taylor (1977), Phelps (1978), Taylor (1979), and Calvo (1983).
2. Phelps (1968, 1969, 1970).

3. As I saw later, such equilibrium expectations would also have required participants to understand that the inferences drawn by others were the same as their own (Phelps, 1983; Woodford, 2003).
4. It was not dreamt of in my philosophy that every event, once the news was out, could be classified as being of one or another type, each of which had a known probability of terminating.
5. In any model-theoretic analysis of a macro question, the *equilibrium* case is an indispensable benchmark, even if nonequilibrium effects seriously alter the equilibrium ones.
6. See Frydman (1982) and Frydman and Phelps (1983).
7. The reason, I suspect, was that while it proved attractive to impute known statistical properties to a visible force that is already upon the economy, it would have put in question the plausibility of the whole rational-expectations project to impute known statistical properties to the entire panoply of uncertain future prospects that entrepreneurs and speculators entertain or invent.
8. My book *Structural Slumps* presents models forging a link between expectations of the future, which influence the expectational shadow value per unit placed on each business asset and the path of the natural unemployment rate. Samuelson in his ninetieth birthday dinner speech recalls studying Gustav Cassel in his first economics course at the University of Chicago.
9. Eclectics who tried to apply these models were driven to supplement them with generally underdeveloped arguments about oil prices in the 1970s, exchange rates in the 1980s, and productivity growth in the 1990s. But without attention to shifts in the climate of future expectations, they were unable to get very far.
10. Fitoussi *et al.* (2000); Phelps and Zoega (2001); Hoon and Phelps (2001, 2004)
11. See Phelps (1983). Roman Frydman and Michael Goldberg in some of their works have modeled an economy containing “bulls” and “bears.”
12. It would be bizarre to apply Keynesian analysis to the pension benefit explosion centered around 2015. Furthermore, as I showed in a talk given in the 1980s at Queens University, Kingston, even when the future prospect is only several years away, if the economy can be projected to reach its medium-run natural rate in a prior year, the present expectation of fiscal stimulus *after* that year cannot cause a recession below the natural level *before* that year.
13. There is another open-economy model in the structuralist family, one based on customer markets at home and abroad (Hoon and Phelps, 2004). Thanks to these customer markets and the non-Ricardian property that public debt is net wealth, the future step-increase of public debt causes domestic real interest rates to be elevated at that time, which crowds out overseas or domestic customers. As a result, customers are worth less in the present, which, if recognized, immediately prompts firms to raise markups and decrease employment. Hian Teck has proved that, on certain conditions, the real exchange rate abruptly *depreciates* at *t*0, then gradually recovers to its long-run purchasing-power-parity level.

14. The ink had hardly dried on the manuscript for the April 2000 Brookings Panel on Economic Activity meeting at Brookings, where the idea was first presented, when another paper emerged, that by Beaudry and Portier (2001), having the same theme. See also the related paper by Steffen Reichold (2002).
15. Phelps (2003b). See also Field (2003) and Phelps (2004b).
16. Phelps (2003a, 2004a). These and two other essays are combined in Phelps (2004b).
17. In *Webster's*: **mutable** adj. **1** prone to change **2** capable of or liable to mutation (from the Latin *mutare* to change).
18. If doing these calculations over again I would prefer earnings per share to dividends per share.

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5

Paul Samuelson and Global Public Goods

A commemorative essay for Paul Samuelson

William D. Nordhaus

It is both easy and hard to write an essay commemorating Paul Samuelson's contributions to economics. Easy, because he has created so much of modern economics that you could write on virtually anything—stabilization policy, economic growth, international trade, welfare economics, or just about any topic that caught your fancy. Hard, because, like Buridan's ass, you could easily procrastinate forever in deciding which of the many treasures of his ideas to draw from.

In the end, I chose to draw from Paul's writings on public goods. In two and one-half pages, he reshaped the way economists and political philosophers think about the distinction between private goods and public goods.[1] Once those concepts are learned, we can never again forget why the allocational questions for bread are fundamentally different from those of lighthouses. I will focus on an important example of this topic, and one that poses particularly thorny practical issues, which is the case of global public goods. A brief intellectual history of the concepts is appended.

What great blessings or scourges have befallen humanity? Consider issues as disparate as greenhouse warming and ozone depletion, the Internet and William Shakespeare, terrorism and money laundering, the discovery of antibiotics and nuclear proliferation. Each is an example of a complex system whose effects are global and resist the control of individuals and even the most powerful governments. These are examples of *global public goods*, which are goods whose impacts are indivisibly spread around the entire globe. These are not new phenomena. However, they are becoming more important in today's world, because of rapid technological change and of the astounding decline in transportation and communication costs.

What makes global public goods different from other economic issues, however, is that there is no workable mechanism for resolving these issues efficiently and effectively. If a terrible storm destroys a significant fraction of America's corn crop, the reaction of prices and farmers will help equilibrate needs and availabilities. If scientists discover the lethal character of lead in the air and soil, the government is likely, eventually and often haltingly, to undertake to raise the necessary resources and regulations to reduce lead in gasoline and paint. But if problems arise for global public goods, such as global warming or nuclear proliferation, there is no market or government mechanism that contains both political means and appropriate incentives to implement an efficient outcome. Markets can work wonders, but they routinely fail to solve the problems caused by global public goods.

This chapter examines four facets of global public goods. I begin with a discussion of the nature of global public goods. I then discuss the stock nature of many global public goods and the consequent involvement of the time dimension. I next discuss why global public goods pose such a difficult decision problem. Finally, I describe how the production technologies may affect the production of global public goods.

5.1 The Character of Global Public Goods

Most of economic life involves voluntary exchange of private goods, like bread or blue jeans. These are commodities consumed by one person and which directly benefit no one else. However, many activities involve spillovers among producers or consumers. A polar case of an externality is a *public good*. Public goods are commodities for which the cost of extending the service to an additional person is zero and for which it is impossible or expensive to exclude individuals from enjoying.

In other words, public goods have the two key properties of nonrivalry and nonexcludability. Nonrivalry denotes that the consumption of the public good by one person does not reduce the quantity available for consumption by another person. Take global positioning systems as an example. These are used for hiking, missile guidance, and determining the distance of a golf ball from a hole. These are public goods because people who find their location are not reducing the value of signals for others. The second feature of a pure public good is nonexcludability. This means that no person can be excluded from benefiting from or being affected by the public good (or can only be excluded at a very high exclusion cost). In the case of smallpox eradication, once smallpox was eradicated, no person could be excluded from the benefits.

The important point about public goods, which was carefully analyzed in Samuelson's 1954 article, is that private markets generally do not guarantee efficient production. In this respect, then, production of public goods such as GPS signals differs from production of bread. Efficient production of public goods requires collective action to overcome the inability of private firms to capture the benefits of a cure for malaria. The inefficiencies are the greatest for global public goods, whose benefits are spread most widely across space and time.

In reality, there are many shades of privateness and publicness; there are "pure" public goods and "impure" public goods. Consumption of bread probably has some public-good qualities from fertilizer use, emissions from the transportation system, and garbage. Similarly, a few public goods are really pure because most public goods have some privateness at different points of space or time. Global public goods are not qualitatively different from other public goods. They are the only ones where the effects spill widely around the world and for a long time to come.

5.2 Stock Externalities and the Time Dimension in Global Public Goods

One of the distinguishing features of most global public goods is that they are generally "stock externalities." This term means that their impact depends upon a stock of a capital-like variable that accumulates over time. For example, the impacts might be functions of pollution concentrations or knowledge, which are augmented by flows of emissions or learning, and which depreciate according to some process such as precipitation or obsolescence.

The stock character is particularly important when depreciation rates are low, as with Plutonium-239, which has a half-life of 24,000 years. In global warming, the impact of greenhouse gases depends upon the concentrations of greenhouse gases in the atmosphere rather than on the current flow of emissions, and the most important gas, carbon dioxide, has an atmospheric residence time with a half-life in the order of a century. Most important global public goods involve some kind of stock—stocks of pollution, piles of radioactive wastes, stocks of knowledge, biological or genetic stocks, reputational stocks in the case of monetary systems, and institutional stocks in the cases of market and democratic systems. Being stock externalities gives global public goods special characteristics. By their nature, stocks accumulate, often very slowly, so that it may be difficult to recognize the symptoms of the associated ailment until it is too late to cure. Moreover, because stocks accumulate slowly, and some depreciate very slowly, stock externalities often have long-lasting consequences and are irreversible or near-irreversible. For example, once the stock of a species has disappeared, it is gone forever (or until science fiction becomes science reality) as a viable biological system. Because of the long time lags, the impacts may fall far in the future, which lends enormous uncertainties to the problem. Our actions today will affect the climate many decades in the future, but who knows where, when, how, or how much? These features of stock public goods make analysis and policy-making more difficult than with transient or flow public goods.

The stock character of global public goods also adds a time dimension to the dilemmas involved in public goods. The nature of the spillover depends upon the depreciation rate of the stock. If the depreciation rate is high, then most of the impacts will occur quickly (as would be the case with flow pollutants). However, when the depreciation rate is low (as in global warming or many radioactive wastes) or even negative (as might be the case of knowledge), then the impacts will occur over many generations as well as many nations, and presumably even for nations that have no current legal existence. Just as children and the unborn cannot vote, so unborn nations cannot represent their interests under international law.

Just as global public goods involve externalities over space, in the case of stock public goods they involve externalities over time. While markets are linked over time through capital markets, there is no similar linkage over time for stock global public goods. No market today accurately reflects the impact of global warming on the possible deterioration of air quality or cross-country skiing a century from now. Appropriate decisions must weigh the intergenerational benefits across time just as in conventional public goods they must weigh the benefits across space. It seems likely that, for stock global public goods with low depreciation rates, we will shortchange unborn generations, and even unborn nations, because their interests will be discounted by under-representation and too-high market or decisional discount rates.

5.3 Global Public Goods, Federalism, and the Westphalian Dilemma

While global public goods raise no new analytical issues, they do encounter a unique political hurdle, which is the Westphalian dilemma. Whenever we encounter a social, economic, or political problem, one of the first questions concerns the level at which the problem should be addressed. We expect households to deal with children's homework assignments and taking out the trash; we expect local or regional governments to organize schools and collect the trash; we expect national governments to defend their borders and manage their currencies.

For the case of global public goods, there exists today no workable market or governmental mechanism that is appropriate for the problems. There is no mechanism by which global citizens can make binding collective decisions to slow global warming, to cure overfishing, to efficiently combat AIDS, to form a world army to combat dangerous tyrants, or to rein in dangerous nuclear technologies.

The decision-making difficulties of global public goods raise what might be called the Westphalian dilemma. National governments have the actual power and legal authority to establish laws and institutions within their territories; this includes the right to internalize externalities within their boundaries and provide for national public goods. Under the governing mechanisms of individual countries, whether they are acts of democratic legislatures or despotic decrees, they can take steps to raise taxes or armies and command their citizens to clean their air and water.

By contrast, under international law as it has evolved in the West and then the world, there is no legal mechanism by which disinterested majorities, or supermajorities short of unanimities, can coerce reluctant free-riding countries into mechanisms that provide for global public goods. Participants of the Treaty of Westphalia recognized in 1648 the *Staatensystem*, or system of sovereign states, each of which was a political sovereign with power to govern its territory. As the system of sovereign states evolved, it led to the current system of international law under which international obligations may be imposed on a sovereign state only with its consent.

Because nations, particularly the United States, are deeply attached to their sovereignty, the Westphalian system leads to severe problems for global public goods. The requirement for unanimity is in reality a recipe for inaction. Particularly where there are strong asymmetries in the costs and benefits (as is the case for nuclear non-proliferation or global warming), the requirement of reaching unanimity means that it is extremely difficult to reach universal and binding international agreements. One answer to the political vacuum is to create international institutions, such as the Intergovernmental Panel on Climate Change or the International Maritime Organization. Such organizations generally work by unanimity, have few provisions that are binding on recalcitrant countries, and in any case apply only to countries which have agreed to participate. Even for life and death issues such as nuclear weapons, if a state like North Korea declines to participate in the Non-Proliferation Treaty, there is no provision for forcing its agreement.

To the extent that global public goods may become more important in the decades ahead, one of our major challenges is to devise mechanisms that overcome the bias toward the status quo and the voluntary nature of current international law in life-threatening issues. To someone who is an outsider to international law, the Westphalian system seems an increasingly dangerous vestige of a different world. Just as economists recognize that consumer sovereignty does not apply to children, criminals, and lunatics, international law must come to grips with the fact that national sovereignty cannot deal with critical global public goods.

5.4 The Production Technology for Global Public Goods

Most discussions of public goods focus on the nonrivalry and nonexcludability in their *use*. A neglected feature is the nature of the technology for *production* of public goods, that is, the technology underlying the production of the indivisible benefits. Most analyses of public goods such as global warming, deforestation, or information tend to view the production of public goods as an "additive" technology, akin to pouring water in a vat or adding houses in the suburbs. In fact, the production technologies of public goods vary considerably, and the kinds of policies or institutions that are necessary for efficient provision of public goods will also differ according to the technology.

5.4.1 Three Production Technologies

Three interesting examples of production technologies for public goods provide quite different outcomes.[2]

1. *Additive technologies*. The conventional case, stemming from the original Samuelson 1954 model, comes where the production of the public good is simply the sum of the contributions of the different producers.

This is exemplified by global warming, where total emissions are equal to the sum of the emissions of different parties. In this case, it makes no difference whether ten units are produced by one country or by ten countries.

2. *Best-shot technologies*. Quite a different situation comes when the outcome is the result of the maximum of the individual contributions. For example, if ten missiles are fired at an incoming warhead, then the success of the effort will be largely determined by the missile that comes closest to the target. Another important example is technological change: If ten researchers are trying to find a cure for malaria, the payoff will generally come from the best outcome.

3. *Weakest-link technologies*. Many cases exhibit a technology where the overall production is only as good as the weakest link in the chain. For example, when different communities are building a dike, the success in holding back the waters will depend upon the minimum strength or height of the different parts. Similar outcomes sometimes occur in protecting the spread among countries of infectious diseases like SARS, combating illegal drugs, or preventing money laundering. Perhaps the most frightening example is nuclear proliferation, where countries or groups can buy or steal nuclear materials and designs from countries with the weakest security protection.

5.4.2 Efficient Provision

We introduce the different cases because they have different implications for efficient provision and for the equilibrium outcome. (This discussion relies on the analysis referred to earlier by Hirshleifer.) In the additive case, efficient provision requires the familiar rule that everyone contribute to the point where private marginal cost equals social marginal benefit.

While the equilibrium condition is unchanged, the outcomes for the other cases are different and even strange. Efficient production for the weakest-link technology would require that all parties contribute equally. Efficient dike building requires that each section have equal height and strength (ignoring water pressure, water flows, and other similar factors). If a virulent influenza or SARS-like illness began to spread, good public health protocols in our highly linked world require that all countries are vigilant in tracking and treating the disease. Similarly, efficient prevention of nuclear proliferation requires stringent minimum standards for all countries possessing the relevant technologies.

The best-shot technology is the opposite of the weakest-link technology. It requires that production be concentrated in the low-cost or most efficient producer. Ignoring uncertainty, production should follow the rule that private marginal cost of production of the low-cost producer should equal social marginal benefit, while production of all other higher-cost producers should be zero. In climate-change policy, this rule would definitely not be appropriate for emissions reductions. However, in other aspects of global warming, specialization might be appropriate. For example, we would expect that the research and development on low-carbon fuels should be concentrated in the most efficient research environments. Similarly, if it were thought that geoengineering approaches to climate change (such as shooting smart particles into the stratosphere) were appropriate, it would be natural that the high-technology countries would undertake this task.

5.4.3 Noncooperative Provision

Similarly, we can inquire into the equilibrium production of global public goods for different production technologies. It is sensible for global public goods, given the Westphalian dilemma, to examine an equilibrium in which different parties (nations) behave in a noncooperative fashion. The additive case would provoke the standard syndrome of free riding and underprovision of the public good, with small and poor countries underproviding more than large and rich countries.

In the weakest-link case, by contrast, we see strong incentives for parties to cooperate and provide for the common defense. Since I will be inundated if I do not keep up my share of the dike, there is little incentive (or possibility) for free riding. Weakest-link technologies, then, are ones where the noncooperative outcome most closely approaches the efficient outcome as long as countries have similar tastes and incomes. With weakest-link technologies, coordination and technological cooperation may be sufficient to produce reasonably efficient outcomes.

The best-shot case poses serious problems. In the case of a single superpower, that country will naturally be the low-cost provider and is likely to end up being the single provider. The equilibrium outcome is likely to be the most inefficient of all three cases. This result occurs because the low-cost provider still equates marginal private cost with marginal private benefit, but other providers drop out and produce nothing. Thus, in the cases of providing security guarantees, GPS systems, or combating international security threats, the United States is clearly the dominant provider, with more than half of defense and intelligence spending. It is likely to remain the sole provider of the public good (if this term is aptly applied here) as long as it remains so dominant.

However, as long as the US decisions adopt a noncooperative strategy, provision of global public goods will be highly inefficient. Particularly when the benefits of action are widely dispersed or perceived as insignificant by the United States (as is apparently the case for technological development of low-carbon fuels to slow climate change or developing effective treatment against African AIDS or malaria), it is likely that there will be serious underinvestment in the global public good.

It is tempting to divide views of global issues into those who see the world largely in terms of the additive technology and those who view events through the lens of the non-linear technologies. Those who see the world in terms of additive public goods would tend to emphasize policies requiring cooperative efforts by all or most nations. There is, in that view, no substitute for finding cooperative Coase-type solutions in which bargaining leads to efficient outcomes. By contrast, those whose worldview is largely shaped by conflict and military doctrines may view the world more as one in which unilateral or imposed solutions are necessary. Action in the best-shot world requires but a single actor, whose role is to govern benevolently while taking into account the aggregate of impacts across all nations. Alas, it is but a small step from the benevolent actor to the nationalistic actor, one who acts unilaterally and concentrates on the benefits to the dominant country, perhaps with a bow to the interests of friends and coalitions of the coerced.

5.5 Conclusion

In this chapter, I have reviewed the fascinating problem involved in managing global public goods. All public goods pose severe challenges, but global public goods are even more daunting, and stock global public goods are the polar case of affecting vast numbers of people for long periods of time. The structure of international law and political power raises enormous obstacles to obtaining the unanimous or near-unanimous consent of sovereign nations to take collective international action. Problems of global public goods will also differ depending upon the production process underlying the public good. The peril of using the incorrect mental model of global public goods is that a proposed solution will lead to little improvement over the status quo. Solving the problems involved in global public goods is an open and fascinating economic question with major consequences for our world.

These thoughts recall for me a time when I was a graduate student at MIT and we were having a bad day in one of Paul's classes on advanced capital theory. His questions—perhaps on E. v. Böhm-Bawerk or I. Fisher—were eliciting no sensible answers. Eventually, he looked out at us and remarked to the effect that we were the returns on his human capital, but he was not sure that he was earning a supernormal return. Fortunately, the returns on his human capital are the ultimate global public good. To paraphrase Jefferson, Paul does not diminish the light of his wisdom and generosity when he passes that light to his students and colleagues; rather it glows larger than ever.

5.6 Historical Notes on Public Goods

The germinal article on public goods is Paul Samuelson (1954), "The pure theory of public expenditure," *The Review of Economics and Statistics*, 36(4), 387–389. He originally called these "collective consumption goods." The first use of the term "public good" in this context in the JSTOR archives of economies journals (at www.jstor.org) appears to be in Paul A. Samuelson (1955), "Diagrammatic exposition of a theory of public expenditure," *The Review of Economics and Statistics*, 37(4), 350–356.

Samuelson referred to earlier writings on the theory of public expenditure of Emil Sax, Knut Wicksell, Erik Lindahl, and Richard Musgrave. Early writers, such as Musgrave, generally used the term "social goods." An early definition of what we today mean as public goods (using the term "social goods") appears in Howard R. Bowen (1943), "The interpretation of voting in the allocation of economic resources," *The Quarterly Journal of Economics*, 58(1), 27–48. The notion of public goods is implicit in the discussion in Richard Musgrave (1939), "The voluntary exchange theory of public economy," *The Quarterly Journal of Economics*, 53(2), 213–237. One of the most influential analyses was in Richard Musgrave (1959), *The Theory of Public Finance*, New York, McGraw-Hill.

The notion of global public goods appeared sometime in the early 1990s in the context of global environmental issues. These were analyzed in my work on global warming, particularly William Nordhaus (1994), *Managing the Global Commons: The Economics of Change*, Cambridge, MA, MIT Press. An excellent early study surveying the area is Todd Sandler (1997), *Global Challenges: An Approach to Environmental, Political, and Economic Problems*, Cambridge, U.K., Cambridge University Press. Many of the issues discussed here were presented in a lecture I delivered, “Global Public Goods and the Problem of Global Warming,” Annual Lecture of The Institut d’Economie Industrielle (IDEI), Toulouse, France, June 14, 1999. The United Nations has a web page devoted to global public goods at www.undp.org/globalpublicgoods/.

Notes

1. Paul Samuelson. (1954). “The pure theory of public expenditure,” *The Review of Economics and Statistics*, 36(4), 387–389.
2. This discussion draws upon Jack Hirshleifer in “From weakest-link to best-shot: the voluntary provision of public goods,” *Public Choice*, 41, 371–386, 1983.

6

Revealed Preference

Hal R. Varian

6.1 Introduction

In January 2005 I conducted a search of JSTOR business and economics journals for the phrase "revealed preference" and found 997 articles. A search of Google scholar returned 3,600 works that contained the same phrase. Surely, revealed preference must count as one of the most influential ideas in economics. At the time of its introduction, it was a major contribution to the pure theory of consumer behavior, and the basic idea has been applied in a number of other areas of economics.

In this chapter I will briefly describe the history of revealed preference, starting with descriptions of the concept in Samuelson's papers. These papers subsequently stimulated a substantial amount of work devoted to refinements and extension of Samuelson's ideas. These theoretical works, in turn, led to a literature on the use of revealed preference analysis for empirical work that is still growing rapidly.

6.2 The Pure Theory of Revealed Preference

Samuelson (1938) contains the first description of the concept he later called "revealed preference." The initial terminology was "selected over."[1] In this paper, Samuelson stated what has since become known as the "Weak Axiom of Revealed Preference" by saying "if an individual selects batch one over batch two, he does not at the same time select two over one." Let us state Samuelson's definitions a bit more formally.

Definition 6.1 (Revealed Preference) *Given some vectors of prices and chosen bundles* (p^t, x^t) *for* $t = 1, \ldots, T$, *we say* x^t *is directly revealed preferred to a bundle* x *(written* $x^t R_D x$*) if* $p^t x^t \geq p^t x$. *We say* x^t *is revealed preferred to* x *(written* $x^t Rx$*) if there is some sequence* $r, s, t, \ldots, u, v$ *such that* $p^r x^r \geq p^r x^s$, $p^s x^s \geq p^s x^t, \ldots, p^u x^u \geq p^u x$. *In this case, we say the relation* R *is the transitive closure of the relation* R_D.

Definition 6.2 (Weak Axiom of Revealed Preference) *If* $x^t R^D x^s$ *then it is not the case that* $x^s R^D x^t$. *Algebraically,* $p^t x^t \geq p^t x^s$ *implies* $p^s x^s < p^s x^t$.

Subsequently, building on the work of Little (1949), Samuelson (1948) sketched out an argument describing how one could use the revealed preference relation to construct a set of indifference curves. This proof was for two goods only, and was primarily graphical. Samuelson recognized that a general proof for multiple goods was necessary, and left this as an open question.

Houthakker (1950) provided the missing proof for the general case. As Samuelson (1950) put it, "He has given us the long-sought test for integrability that can be formed in finite index-number terms, without need to estimate partial derivatives."

Houthakker's contribution was to recognize that one needed to extend the "direct" revealed preference relation to what he called the "indirect" revealed preference relation or, for simplicity, what we call the "revealed preference" relation. Houthakker's condition can be stated as:

Definition 6.3 (Strong Axiom of Revealed Preference (SARP)) *If* $x^t R_x{}^s$ *then it is not the case that* $x^s Rx^t$. *Algebraically, SARP says* $x^t Rx^s$ *implies* $p^s x^s < p^s x^t$.

Afriat (1965) later offered a formal argument that the Strong Axiom and the Weak Axiom were equivalent in two dimensions, providing a rigorous, algebraic foundation for Samuelson's earlier graphical exposition.

Samuelson (1953), stimulated by Hicks (1939), summed up all of consumer theory in what he called the *Fundamental Theorem of Consumption Theory*. "Any good (simple or composite) that is known always to increase in demand when money income alone rises must definitely shrink in demand when its price alone rises." In this paper he lays out a graphical and algebraic description of the Slutsky equation and the restrictions imposed by consumer optimization. Yokoyama (1968) elegantly combined Samuelson's verbal and algebraic treatments of the Slutsky equation and made the connection between the Samuelson and the Hicks approaches explicit.

By 1953, the basic theory of consumer behavior in terms of revealed preference was pretty much in place, though it was not completely rigorous.

Subsequent contributors, such as Newman (1960), Uzawa (1960), and Stigum (1973) added increasing rigor to Houthakker and Samuelson's arguments.

During the same period Richter (1966) recognized that one could dispense with the traditional integrability approach using differential equations and base revealed preference on pure set-theoretic arguments involving the completion of partial orders.

This period culminated in the publication of Chipman *et al.* (1971), which contained a series of chapters that would seem to be the last word on revealed preference. Several years later Sondermann (1982), following Richter (1966)'s analysis, provided a one-paragraph proof of the basic revealed preference result, albeit a proof that used relatively sophisticated mathematics.

6.3 Afriat's Approach

Most of the theoretical work described above starts with a demand function: a complete description of what would be chosen at any possible budget. Afriat (1967) offered quite a different approach to revealed preference theory. He started with a *finite* set of observed prices and choices and asked how to actually construct a utility function that would be consistent with these choices.[2]

The standard approach showed, in principle, how to construct preferences consistent with choices, but the actual preferences were described as limits or as a solution to some set of partial differential equations.

Afriat's approach, by contrast, was truly constructive, offering an explicit algorithm to calculate a utility function consistent with the finite amount of data, whereas the other arguments were just existence proofs. This makes Afriat's approach much more suitable as a basis for empirical analysis. Afriat's approach was so novel that most researchers at the time did not recognize its value. In addition, Afriat's exposition was not entirely transparent. Several years later Diewert (1973) offered a somewhat clearer exposition of Afriat's main results.

6.4 From Theory to Data

During the late 1970s and early 1980s there was considerable interest in estimating aggregate consumer demand functions. Christensen *et al.*

(1975) and Deaton (1983) are two notable examples. In reading this work, it occurred to me that it could be helpful to use revealed preference as a pre-test for this econometric analysis.

After all, the Strong Axiom of Revealed Preference was a necessary and sufficient condition for data to be consistent with utility maximization. If the data satisfied SARP, there would be some utility function consistent with the observations. If the data violated SARP, no such utility function would exist. So why not test those inequalities directly?

I dug into the literature a bit and discovered that Koo (1963) had already thought of doing this, albeit with a somewhat different motivation. However, as Dobell (1965) pointed out, his analysis was not quite correct so there was still something left to be done.

Furthermore I recognized the received theory, using WARP and SARP, was not well-suited for empirical work, since it was built around the assumption of single-valued demand functions. In 1977, during a visit to Berkeley, Andreu Mas-Collel pointed me to Diewert (1973)'s exposition of Afriat's analysis, which seemed to me to be a more promising basis for empirical applications.

Diewert (1973) in turn led to Afriat (1967). I corresponded with Afriat during this period, and he was kind enough to send me a package of his writing on the subject. In his monograph, Afriat (1987) offered the clearest exposition of his work in this area, though, as I discovered, it was not quite explicit enough to be programmed into a computer.

I worked on reformulating Afriat's argument in a way that would be directly amenable to computer analysis. While doing this, I recognized that Afriat's condition of "cyclical consistency" was basically equivalent to Strong Axiom. Of course, in retrospect this had to be true since both cyclical consistency and SARP were necessary and sufficient conditions for utility maximization. Even though the proof was quite straightforward, this was a big help to my understanding since it pulled together the quite different approaches of Afriat and Houthakker.

During 1978–79 I worked on writing a program for empirical revealed preference analysis. The code was written in FORTRAN77 and ran on the University of Michigan MTS operating system on an IBM mainframe. This made it rather unportable, but then again this was before the days of personal computers, so everything was unportable. During 1980–81 I was on leave at Nuffeld College, Oxford and became more and more intrigued by the empirical applications of revealed preference. As I saw it, the main empirical questions could be formulated in the following way.

Given a set of observations of prices and chosen bundles, (p^t, x^t) for $t = 1, \dots, T$, we can ask four basic questions.

Consistency. When is the observed behavior consistent with utility maximization?

Form. When is the observed behavior consistent with maximizing a utility function of particular form?

Recoverability. How can we recover the set of utility functions that are consistent with a given set of choices?

Forecasting. How can we forecast what demand will be at some new budget?

In the rest of the chapter I will review some of the literature concerned with pursuing answers to these four basic questions.

6.5 Consistency

Consistency is, of course, the central focus of the early work on revealed preference. As we have seen, several authors contributed to its solution, including Samuelson, Houthakker, Afriat, and others. The most convenient result for empirical work, as I suggested above, comes from Afriat's approach.

Definition 4 (Generalized Axiom of Revealed Preference) *The data* (p^t, x^t) *satisfy the Generalized Axiom of Revealed Preference (GARP) if* $x^t\ R_x{}^s$ *implies* $p^s x^s \leq p^s x^t$.

GARP, as mentioned above, is equivalent to what Afriat called "cyclical consistency." That the only difference between GARP and SARP is that the strong inequality in SARP becomes a weak inequality in GARP. This allows for multivalued demand functions and "flat" indifference curves, which turns out to be important in empirical work.

Now we can state the main result.

Theorem 1 (Afriat's Theorem) Given some choice data (p^t, x^t) *for* $t = 1, \ldots, T$, the following conditions are equivalent.

1. There exists a nonsatiated utility function $u(x)$ that rationalizes the data in the sense that for all t, $u(x^t) \geq u(x)$ for all x such that $p^t x^t \geq p^t x$.
2. The data satisfy GARP.
3. There is a positive solution (u^t, λ^t) to the set of linear inequalities $u^t \leq u^s + \lambda^s p^s (x^t - x^s)$ for all s, t.
4. There exists a nonsatiated, continuous, monotone, and concave utility function $u(x)$ that rationalizes the data.

This theorem offers two equivalent, testable conditions for the data to be consistent with utility maximization. The first is GARP, which, as we have seen, is a small generalization of Houthakker's SARP. The second condition is whether there is a positive solution to a certain set of linear inequalities. This can easily be checked by linear programming methods. However, from the viewpoint of computational efficiency it is much easier just to check GARP. The only issue is to figure out how to compute the revealed preference relation in an efficient way.

Let us define a matrix m that summarizes the direct revealed preference relation. In this matrix the (s, t) entry is given by $m_{st} = 1$ if $p^t x^t \geq p^t x^s$ and $m_{st} = 0$ otherwise. In order to test GARP, all that is necessary is to compute the transitive closure of the relation summarized by this matrix. What algorithms are appropriate?

Dobell (1965) recognized that this could be accomplished simply by taking the Tth power of the $T \times T$ binary matrix that summarizes the direct revealed preference relation. However, it turned out the computer scientists had a much more efficient algorithm. Warshall (1962) had shown a few years earlier how to use dynamic programming to compute the transitive closure in just T^3 steps.

Combining the work of Afriat and Warshall effectively solved the problem of finding a computationally efficient method of testing data for consistency with utility maximization. One could simply construct the matrix summarizing the direct relation, compute the transitive closure, and then check GARP.

6.5.1 Empirical Analysis

Several authors have tested revealed preference conditions on different sorts of data. The 'best' data, in some sense, is experimental data involving individual subjects since one can vary prices in such a setting and so test choice behavior over a wide range of environments.

Battalio *et al*. (1973) was, I believe, the first paper to look at individual human subjects. The subjects were patients in a mental institution who were offered payments for good behavior. Cox (1989) later examined the same data and extended the analysis in several ways.

Kagel *et al*. (1995) summarizes several studies examining animal behavior. Harbaugh *et al*. (2001) examined choice behavior by children, and Andreoni and Miller (2002) looked at public goods experiments to test for rational behavior in this context.

Individual household consumption data is the next best set of data to examine in the context of consumer choice theory. I believe that Koo (1963) was the first paper to look at household data. See also the subsequent exchange between Dobell (1965) and Koo (1965). Later studies using household budgets include Manser and McDonald (1988) and Famulari (1995). Dowrick and Quiggin (1994, 1997) look at international aggregate data.

Finally, we have time series data on aggregate consumption. I used these methods described above to test revealed preference in Varian (1982a). To my surprise, the aggregate consumption data easily satisfied the revealed preference conditions. I soon realized that this was for a trivial reason: the change in expenditure from year to year were large relative to the changes in relative prices. Hence budget sets rarely intercepted in ways that would generate a GARP violation (or so it seemed).

Bronars (1985) offered a novel contribution by investigating the power of the GARP test. Power, of course, can only be measured against a specific alternative hypothesis, and Bronars chose the Becker (1962) hypothesis of random choice on the budget set. He found that Becker's random choice model violated GARP about 67 percent of the time. Contrary to my original impression, there were apparently enough budget intersections in aggregate time series to give GARP some bite.

GARP was even more powerful on per capita data. Of course, another interpretation of these findings is that Becker's random choice model is not a very appealing alternative hypothesis. But, for all the criticism directed at the classical theory of consumer behavior, there seem to be few alternative hypotheses other than Becker's that can be applied using the same sorts of nine data used for revealed preference analysis.

6.5.2 Goodness of Fit

It is of interest to consider ways to relax the revealed preference tests so that one might say "these data are almost consistent with GARP." Afriat (1967) defines a "partial efficiency" measure which can be used to measure how well a given set of data satisfies utility maximization.

Definition 5 (Efficiency levels) *We say that x^t is directly revealed preferred to x at efficiency level e if $ep^t x^t \geq p^t x$.*

We define the transitive closure of this relation as in the usual way. If $e = 1$ this is the standard direct revealed preference relation. If $e = 0$ nothing is directly revealed preferred to anything else, so GARP is vacuously satisfied. Hence there is some critical level e^* where the data just satisfy GARP.

It is easy to find the critical level e^* by doing a binary search. Varian (1990) suggests defining e^t separately for each observation and then finding those e^t that are as close as possible to 1 (in some norm). I interpret these e^t as a "minimal perturbation." They can be interpreted as error terms and thus be used to give a statistical interpretation to the goodness-of-fit measure.

Whitney and Swofford (1987) suggest using the number of violations as a fit measure, while Famulari (1995) uses a measure which is roughly the fraction of violations that occur divided by the fraction that could have occurred. Houtman and Maks (1985) propose computing the maximal subset of the data that is consistent with revealed preference. These measures are reviewed and compared in Gross (1995) who also offers his own suggestions.

6.6 Form

The issue of testing for various sorts of separability had been considered by Afriat in unpublished work and independently examined by Diewert and Parkan (1985). The Diewert–Parkan work extended the linear inequalities described in Afriat's Theorem. They showed that if an appropriate set of linear inequalities had positive solutions, then the data satisfied the appropriate form restriction.

To get the flavor of this analysis, suppose that some observed data (p^t, x^t) were generated by a differentiable concave utility function $u(x)$. Differentiability and concavity imply that

$$u(x^t) \leqslant u(x^s) + Du(x^t)(x^s - x^t) \quad \text{for all } s,t$$

The first-order conditions for utility maximization imply

$$Du(x^t) = \lambda^s p^t \quad \text{for all } t$$

Putting these together, we find that a necessary condition for the data to be consistent with utility maximization is that there is a set of positive numbers (u^t, λ^t), which can be interpreted as utility levels and marginal utilities of income that satisfy the linear inequalities

$$u^t \leqslant u^s + \lambda^s (p^s x^t - p^t x^t) \quad \text{for all } s, t$$

Furthermore, the existence of a solution to this set of inequalities is a sufficient condition as well. This can be proved by defining a utility function as the lower envelope of a set of hyperplanes defined as follows:

$$u(x) = \min_{s} u^s + \lambda^s p^s (x - x^s)$$

Afriat (1967) had used a similar construction but went further and showed that cyclical consistency (i.e. GARP) was a necessary and sufficient condition for a solution to this set of linear inequalities to exist. Thus the computationally demanding task of verifying that a positive solution to a set of T^2 linear inequalities could be replaced by a much simpler calculation: checking GARP. Suppose now that the data were generated by a homothetic utility function. Then it is well known that the indirect utility function can be represented as a multiplicatively separable function of price and income: $v(p)m$. This means that the marginal utility of income is simply $v(p)$, which also equals the utility level at income 1.

If we normalize the observed prices so that expenditure equals 1 at each observation, we can write the above inequalities as

$$u^t \leqslant u^s + u^s (1 - p^s x^t) \quad \text{for all } s, t$$

We have shown that the existence of a positive solution to these inequalities is a necessary condition for the maximization of a homothetic utility function. This can also be shown to be sufficient.

One immediately asks: is there an easier-to-check combinatorial condition that is equivalent to the existence of a solution for these inequalities? Varian (1982b) found such a condition. Simultaneously, Afriat (1981) published essentially the same test.

To get some intuition, consider Figure 6.1. The data (p^1, x^1) and (p^2, x^2) are consistent with revealed preference. However, if the underlying preferences are homothetic, then x^3 would be demanded at the budget set p^3 creating a violation of revealed preference.

In general, the necessary and sufficient condition for an observed set of choices to be consistent with homotheticity is given by HARP:

Definition 6 (Homothetic Axiom of Revealed Preference) *A set of data (p^t, x^t) for $t = 1, \ldots, T$, satisfy the Homothetic Axiom of Revealed preference (HARP) if for every sequence r,s,t, . . . ,u,v*

$$\frac{p^r x^s}{p^r x^r} \frac{p^s x^t}{p^s x^s} \cdots \frac{p^u x^v}{p^u x^u} \geq 1.$$

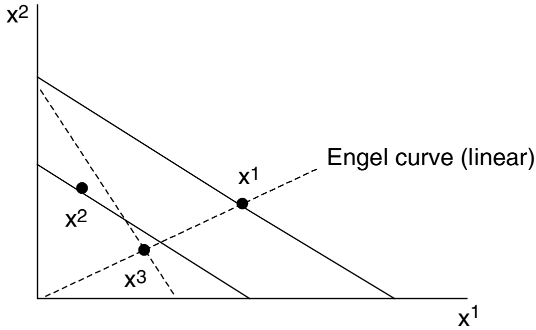

Figure 6.1 GARP with homothetic preference

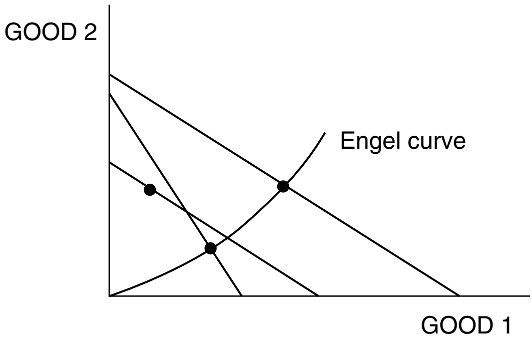

Figure 6.2 GARP with arbitrary Engel curve

It turns out that there is an easy computation to check whether or not this condition is satisfied that uses methods that are basically the same as those in Warshall's algorithm.

Using similar methods, Browning (1984) came up with a nice test for life-cycle consumption models which rests on the constancy of the marginal utility of income in this framework.

Subsequently Blundell *et al.* (2003) recognized that the logic used in the homotheticity tests could be extended to a much more general setting.

Suppose one had estimates of Engel curves from other data. Then these Engel curves could be used to construct a set of data that could be subjected to revealed preference tests. The logic is the same as that described in Figure 6.1, but uses an estimated Engel curve rather than the linear Engel curve implied by homotheticity. See Figure 6.2 for a simple example.

The Blundell–Browning–Crawford approach is very useful for empirical work since cross-sectional household data can be used to estimate Engel curves, either parametrically or nonparametrically. See Blundell (2005) for further developments in this area.

Other restrictions on functional form, such as various forms of separability, have been examined by Varian (1982a). Tests for expected utility maximization and related models are described in Green and Srivastava (1986), Osbandi and Green (1991), Varian (1983, 1988), and Bar-Shira (1992).

6.7 Forecasting

Suppose we are given a finite set of observed budgets and choices (p^t, x^t) for $t = 1, \ldots, T$ that are consistent with GARP and a new price p^0 and expenditure y^0. What are the possible bundles x^0 that could be demanded at (p^0, x^0)?

Clearly all that is necessary is to describe the set of x^0 for which the (expanded) data set (p^t, x^t) *for* $t = 0, \ldots, T$ satisfy GARP. Varian (1982a) calls this the set of supporting bundles. Figure 6.3 shows the geometry.

In an analogous way, one can choose a new bundle y^0 and ask for the set of prices at which this bundle could be demanded. This is the set of supporting prices. Formally,

$$S(x^0) = \{p^0 : (p^t, x^t) \text{ satisfy GARP for } t = 0, \ldots, \mathrm{T}\}$$

Of course, one could also ask about demanded bundles or prices that are consistent with utility functions with various restrictions imposed such as homotheticity, separability, specific forms for Engel curves, and so on.

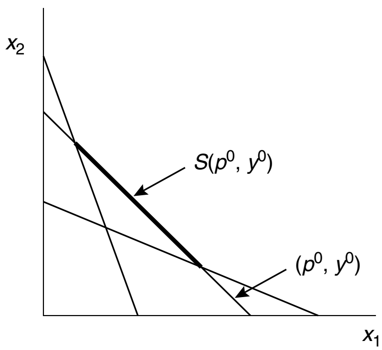

Figure 6.3 Supporting bundles

6.8 Recoverability

As we have seen, Afriat's methods can be used to construct a utility function that is consistent with finite set of observed choices that satisfy GARP. However, this is only one utility function. Typically there will be many such functions. Is there a way to describe the entire set of utility functions (or preferences) consistent with some data?

Varian (1982a) posed the question in the following way. Suppose we are given a finite set of data (p^t, x^t) *for* $t = 2, \ldots, T$ that satisfies GARP and two new bundles x^0 and x^1. Consider the set of prices at which x^0 could be demanded, that is, the supporting set of prices. If every such supporting set makes x^0 revealed preferred to x^1 then we conclude that all preferences consistent with the data must have x^0 preferred to x^1.

Given any x^0, it is possible to define the sets of x's that are revealed preferred to x^0 $(RP(x^0))$ and set of x's that are revealed worse than x^0 $(RW(x^0))$. A very simple example is shown in Figure 6.4. The possible set of supporting prices for x0 must lie in the shaded cone so every such set of prices imply that x^0 is revealed preferred to the points in $RW(x^0)$. Similarly, the points in the convex hull of the bundles revealed preferred to x^0 must themselves be preferred to x^0 for any concave utility function that rationalizes the data.

Of course Figure 6.4 uses only one observation. As we get more observations on demand, we will get tighter bounds on $RP(x^0)$ and $RW(x^0)$, as shown in Figure 6.5. Another approach, also suggested by Varian (1982a) is to try to compute bounds on *specific* utility functions. A very convenient

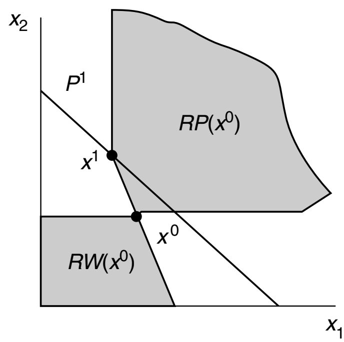

Figure 6.4 $RP(x^0)$ and $RW(x^0)$: simple case

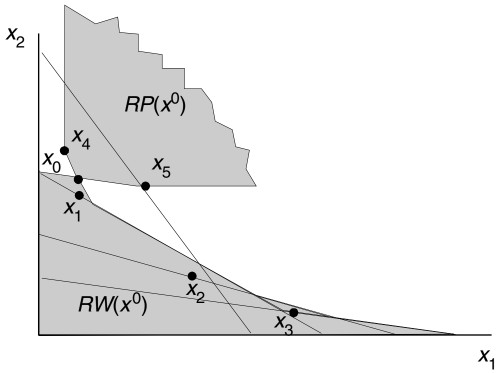

Figure 6.5 $RP(x^0)$ and $RW(x^0)$: a more complex case

choice in this case is what Samuelson (1974) calls the *money metric utility function*. First define the expenditure function

$$e(p,u) = \min pz \text{ such that } u(z) \geqslant u.$$

It is not hard to see that under minimal regularity conditions $e(p,u)$ will be a strictly increasing function of u. Now define

$$m(p,x) = e(p, u(x)).$$

For fixed p, $m(p,x)$ is a strictly increasing function of utility, so it is itself a utility function that represents the same preferences. Varian (1982a) suggested that given a finite set of data $(p^t x^t)$ one could define an upper bound to the money metric utility by using

$$m^+(p, x) = \min pz^t \text{ such that } x^t Rx.$$

Subsequently, Knoblauch (1992) showed that this bound was in fact tight: there were preferences that rationalized the observed choices that had $m^+(p,x)$ as their money metric utility function. Varian (1982a) defined a lower bound to Samuelson's money metric utility function and showed that it was tight.

Of course, using restrictions on utility form such as HARP allow for tighter bounds. There are several papers on the implications of such restrictions in the theory and measurement of index numbers, including Afriat (1981), Afriat (1981), Diewert and Parkan (1985), Dowrick and Quiggin (1994, 1997), and Manser and McDonald (1988).

6.9 Summary

Samuelson's 1938 theory of revealed preference has turned out to be amazingly rich. Not only does the Strong Axiom of Revealed Preference provide a necessary and sufficient condition for observed choices to be consistent with utility maximization, it also provides a very useful tool for empirical, nonparametric analysis of consumer choices.

Up until recently, the major applications of Samuelson's theory of revealed preference have been in economic theory. As we get larger and richer sets of data describing consumer behavior, nonparametric techniques using revealed preference analysis will become more feasible. I anticipate that in the future, revealed preference analysis will make a significant contribution to empirical economics as well.

Notes

1. As Richter (1966) has pointed out, "selected over" has the advantage over "revealed preference" in that it avoids confusion about circular definition of "preference." Unfortunately, the original terminology did not catch on.
2. I once asked Samuelson whether he thought of revealed preference theory in terms of a finite or infinite set of choices. His answer, as I recall, was: "I thought of having a finite set of observations . . . but I always could get more if I needed them!"

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7

Samuelson's "Dr. Jekyll and Mrs. Jekyll" Problem: A Difficulty in the Concept of the Consumer

Robert A. Pollak

In 1956 Samuelson posed what he memorably called the "Dr Jekyll and Mrs Jekyll" problem. Observing that most individuals live in families, Samuelson asked: "...how can we expect family demand functions observed in the market place to obey the consistency axioms of revealed preference or any other regularity conditions?" (Samuelson, 1956, p. 9). Samuelson went on to establish the conditions under which family demand functions obey the revealed preference axioms, but he clearly thought that these conditions were unlikely to be satisfied.

In the 50 years since Samuelson posed the Dr Jekyll and Mrs Jekyll problem, the economics of the family has sharpened our understanding of family decision-making. In this chapter I begin by describing Samuelson's formulation of the Dr Jekyll and Mrs Jekyll problem. I then discuss the implications of developments in the economics of the family for our understanding of family demand functions.

Recent work is transforming the neoclassical theory of consumer's behavior by incorporating insights from the economics of the family. Unlike the neoclassical theory of consumer's behavior, which focuses on the demand functions for goods and supply functions for labor, the economics of the family uses the tools of economics to analyze a wide range of nonmarket behaviors, including marriage, divorce, and fertility.[1] More relevant for demand analysis, the economics of the family analyzes the

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allocation of goods and time within the family. This analysis has implications for the family's market behavior, that is, for the demand functions for goods and supply functions for labor.

The neoclassical theory of consumer's behavior begins with "economic agents" who have preferences and face constraints. To operationalize this theory requires specifying the empirical counterparts of economic agents. Economists have traditionally identified these agents with individuals, and for this reason Samuelson (1956, p. 8) insists that it is properly called the theory of "consumer's" behavior, not the theory of "consumers'" behavior. But, as Samuelson points out, once we recognize that most individuals live in families, the identification of economic agents with individuals poses theoretical difficulties.[2] The theoretical difficulty Samuelson exposed, which he felicitously called the Dr Jekyll and Mrs Jekyll problem, arises in using the theory of consumer's behavior to provide a foundation for demand analysis. Samuelson began by assuming that each spouse has a utility function. This assumption marked a radical departure from the standard assumption of a household or family utility function. Samuelson then asked: "...how can we expect family demand functions observed in the market place to obey the consistency axioms of revealed preference or any other regularity conditions?" (p. 9).[3]

Samuelson posed the Dr Jekyll and Mrs Jekyll problem in a throw-away section in his classic 1956 paper, "Social Indifference Curves." The paper is concerned primarily with international trade and, more specifically, the conditions under which the demand functions of a country can be treated as if they were the demand functions of an individual. Samuelson saw that aggregating the demands of the individuals in a family into the family's demand functions is formally analogous to aggregating the demands of the individuals in a country into the country's demand functions. He showed that the consistency axioms would be satisfied if the spouses somehow agreed on a family Bergson–Samuelson welfare function that specified how alternative distributions of utility are evaluated and, hence, how to trade off the utility of one spouse against the utility of the other. If the spouses could agree to maximize a family welfare function subject to a family budget constraint, then the family's demand functions would be indistinguishable from the demand functions of a utility-maximizing individual. More precisely, if the family welfare function and the spouses' utility functions are well-behaved, then the demand functions that solve this constrained maximization problem depend only on prices and family income, and exhibit all the standard properties of neoclassical demand functions. I call any model in which a family Bergson–Samuelson welfare function is defined over the spouses' utilities a "consensus model."

Samuelson did not purport to explain how spouses might achieve a consensus regarding a family welfare function or, once achieved, how that consensus might be maintained. Indeed, he is openly skeptical, pointing out that constructing a family welfare function for spouses requires solving the two-person version of the problem that Arrow (1950) investigated in the context of aggregating individuals' preferences into a social welfare function. Arrow's paper, "A Difficulty in the Concept of Social Welfare," began by stating a set of appealing and apparently weak axioms. His celebrated "impossibility" theorem asserts that the preferences of individuals can be aggregated into group preferences in a way that satisfies these axioms if and only if one member of the group is a dictator.

Samuelson, however, was not interested in constructing family welfare functions but in establishing the conditions under which a family's demand functions are indistinguishable from the demand functions of a neoclassical consumer. Nerlove (1974) describes Samuelson's resolution to the Dr Jekyll and Mrs Jekyll problem as the "Samuelson finesse," and I suspect that Samuelson would not object to this description. Samuelson did not claim that the assumption of a consensus family welfare function was plausible, nor did he investigate the properties of family demand functions in the absence of a family welfare function.

Although Samuelson posed the Dr Jekyll and Mrs Jekyll problem, Becker was responsible for putting the family on the research agenda of the economic profession. Becker (1974, 1981) began by proposing a model of family interaction—the "altruist model." Becker's altruist model provides an alternative to, or at least an alternative interpretation of, Samuelson's consensus family welfare function. Unlike Samuelson, who was skeptical of the consensus assumption, Becker was a vigorous advocate of the altruist model.

Becker introduced the altruist model in the context of a family consisting of a group of purely selfish but rational "kids" and one altruistic "parent" whose utility function incorporates the utility functions of the kids. Unlike Samuelson, for whom the paradigmatic family is a married couple, for Becker the paradigmatic family is an altruistic father and his children. Becker's assumption of interdependent preferences departs from the usual neoclassical assumption that each individual is concerned only with his or her own consumption. Samuelson (1956, pp. 9–10) anticipated the need for such a departure: "Where the family is concerned the phenomenon of altruism inevitably raises its head: if we can speak at all of the indifference curves of any one member, we must admit that his tastes and marginal rates of substitution are contaminated by the goods that other members consume." Indeed, Samuelson's verbal formulation is compatible with a much more general specification than Becker's highly restrictive definition of altruism.[4]

Becker's Rotten Kid Theorem and Samuelson's consensus model reach virtually identical conclusions about the properties of family demand functions. The Rotten Kid Theorem asserts that the presence of a parent with altruistic preferences who makes positive transfers to each of the selfish kids will induce the kids to act so as to maximize the altruist's utility function subject to the family's resource constraints. Hence, Becker's analysis implies that the altruist's utility function is the family welfare function. The benevolence of the altruist is reflected in the weight he gives to his own consumption relative to the well-being of other family members. In Pollak (1988) I call Becker's altruist the "husband-father-dictator-patriarch." Although Becker's altruist is not a dictator in the technical sense in which social choice theorists use that term, the altruist model comes uncomfortably close to implying that one member of the family is a dictator.

Although Becker introduced the altruist model in the context of parents and children, he reinterprets it in terms of husbands and wives. The reinterpretation casts the husband in the role of the altruistic parent and the wife as the selfish kid; the husband's utility function can then be equated with Samuelson's consensus family welfare function. Becker's altruist model, like Samuelson's consensus model, implies that the family's demand functions for goods and its supply functions for labor are indistinguishable from those of a neoclassical consumer. Samuelson's consensus model does not purport to derive the family's welfare function or relate it to the spouses' utility functions or bargaining power. In contrast, Becker's altruist model purports to derive the family's welfare function and claims that it coincides with the altruist's utility function. The power of the altruist arises not from assumptions about the altruist's preferences but from assumptions about the structure of the implicitly specified bargaining game. The bargaining game underlying Becker's model is best interpreted as a one-shot noncooperative game in which the altruist moves first and confronts the other family members with take-it-or-leave-it offers. Without this take-it-or-leave-it structure proposed by Pollak (1985) or some alternative special conditions, the conclusion of the altruist model fails to hold. The take-it-or-leave-it structure of the altruist game is essentially that of an "ultimatum game."

Both Samuelson's consensus model and Becker's altruist model imply that families act as if they are maximizing family utility functions subject to family budget constraints. Models with this property are called "unitary models."[5] Unitary models provide a rationale for treating families as economic agents and thus are simple, powerful mechanisms for generating demand and supply functions with familiar properties for use in applied economics. Only serious deficiencies could justify replacing unitary models with more complicated alternatives. Although unitary models remain the dominant theoretical framework for analyzing labor supply and goods demand, in recent years they have come under a barrage of methodological, theoretical, and empirical criticisms.

Much of the criticism of treating families as economic agents is a byproduct of the study of marriage and divorce. Models of marriage and divorce require a theoretical framework in which individuals compare their expected utility or well-being inside marriage with their expected utility or well-being outside marriage. The need to recognize the individual utilities of spouses implies the inadequacy of simply postulating that families are economic agents. Approaches that postulate household preferences directly, however, have limitations that go beyond their inadequacy for studying marriage and divorce. In virtually all bargaining models the possibility and anticipated consequences of divorce constrain equilibrium allocations within marriage.[6] For example, when unilateral divorce is possible, individual rationality implies that if the spouses remain together, then the equilibrium allocation in the marriage must be better (or, more precisely, at least as good) for each spouse as divorce. Models that ignore individuals and begin by postulating that families are economic agents cannot be used to study marriage, divorce, or allocation within marriage.

At the level of methodology or "meta-theory," Chiappori (1992, p. 440) argues that economics is committed to "methodological individualism" and that this commitment is inconsistent with simply postulating that the household is an economic agent. In response it might be argued that neither Samuelson's consensus model nor Becker's altruist model postulate household preference directly, but derive them from more primitive assumptions. Alternatively, it might be argued that methodological individualism is better treated as a research strategy than a sacred principle: for some purposes economists can usefully treat households and firms as fundamental units of analysis, just as biologists, for some purposes, treat plants and animals rather than genes or molecules as fundamental units. Finally, it might be argued that unitary models such as Samuelson's consensus model that begin with the utility functions of individuals are consistent with the tenets of methodological individualism and thus immune to Chiappori's methodological critique. In response to this, Chiappori could assert that methodological individualism requires not only utility functions for individuals but also an economic model relating individuals' preferences to family behavior.

The development of bargaining or game theoretic alternatives to the consensus model and the altruist model has changed the theoretical landscape. Proponents of bargaining models do not claim that Samuelson's consensus model or Becker's altruist model are formally incorrect—everyone recognizes that there could be worlds in which families behave as these models predict. The claim is that game theoretic bargaining models are equally consistent with the modeling conventions of economics and, hence, that theory does not favor the consensus model or the altruist model over bargaining models—even though some bargaining models are inconsistent with the unitary model.

Bargaining models from cooperative game theory dominate family economics.[7] A typical bargaining model of marriage begins by assuming, with Samuelson, that each spouse has a utility function that depends only on his or her own consumption. If the spouses fail to reach agreement, both husband and wife receive the utilities associated with a default outcome. The utilities associated with the default outcome are usually described as the "threat point." In some models the threat point is interpreted as divorce, in others as a noncooperative equilibrium within the marriage.

The Nash bargaining model provides the leading solution concept in cooperative bargaining models of marriage. The Nash bargaining solution is the allocation that maximizes the product of the gains to cooperation, measured in utility, subject to the constraint that the spouses' joint income equal joint expenditure. More precisely, the Nash product function is given by: $N = (U^h - T^h)\,(U^w - T^w)$, where U^h and U^w denote the utilities of the husband and wife. Nash (1950) shows that a set of four axioms—including Pareto efficiency, which ensures that the solution lies on rather than inside the utility possibility frontier—uniquely characterizes the Nash bargaining solution.[8]

The utilities received by husband and wife in the Nash bargaining solution depend upon the threat point; the higher a spouse's utility at the threat point, the higher the utility that spouse will receive in the Nash bargaining solution. This dependence is the critical empirical implication of Nash bargaining models: family demands depend not only on prices and total family resources, but also on the threat point. Thus, the precise empirical implications of the Nash bargaining model depend on whether the threat point corresponds to divorce, as in the divorce threat models of Manser and Brown (1980) and McElroy and Horney (1981), or to a noncooperative outcome within the marriage, as in the separate spheres model of Lundberg and Pollak (1993).[9] In both the divorce threat and separate spheres models, Nash bargaining may imply violations of income pooling.

Income pooling implies a restriction on family demand behavior that is simple to explain and apparently simple to test: if family members pool their incomes and allocate the total to maximize a single objective function, then only total family income will affect family demand behavior. That is, the fraction of income received or controlled by one family member does not influence those demands, conditional on total family income. A large number of recent empirical studies have rejected income pooling, finding instead that the fraction of earned or unearned income received by the husband or the wife significantly affects demand patterns when total family income or expenditure is held constant. Rejections of family income pooling have been most influential in undermining economists' attachment to the unitary approach.

With 20/20 hindsight, I am tempted to say that resource pooling is obviously the crucial empirical issue. But it was not obvious to Manser and Brown, to McElroy and Horney, nor to me when, as editor of the *International Economic Review*, I accepted their papers for publication. The earliest attempts to investigate the compatibility of bargaining models with the unitary model focused on whether the Slutsky matrix corresponding to the family's demand functions was symmetric and negative semi-definite. The focus on the Slutsky conditions was understandable given the influence of Samuelson's formulation of consumer's demand theory in *Foundations of Economic Analysis*. In the 1980s, revealed preference tests were the only alternative to Slutsky conditions as a test of whether demand functions could be derived by maximizing a family utility function subject to a family budget constraint.[10] Pooling, like intrafamily allocation, had yet to be discovered.

The ideal test of the pooling hypothesis would be based on an experiment in which some husbands and some wives were randomly selected to receive income transfers. A less-than-ideal test could be based on a "natural experiment" in which some husbands or some wives received exogenous income transfers. Lundberg, Pollak, and Wales (1997) examine the effects of such a natural experiment—a policy change in the United Kingdom that transferred a substantial child allowance from husbands to wives in the late 1970s. They find strong evidence that a shift toward relatively greater expenditures on women's clothing and children's clothing followed this policy change, and they interpret this as a rejection of the income pooling hypothesis. These empirical results provide convincing evidence against unitary models. Using the same natural experiment, Ward-Batts (2000) provides further evidence. Ward-Batts used disaggregated data that distinguished between cigarettes and "other tobacco"—that is, pipe tobacco and cigars—which she characterized as "men's tobacco." Ward-Batts found that the policy change was followed by a substantial and significant decrease in expenditure on men's tobacco, providing further evidence that control over resources affects household expenditure patterns and allocation within households.

The "collective model" proposed in Chiappori (1988, 1992) characterizes intrafamily allocation by a single-valued, Pareto-efficient "sharing rule" that is assumed to satisfy certain regularity conditions. Both Chiappori's collective model and Samuelson's consensus model are defined by requiring that the solutions to a particular problem exhibit certain properties, but neither examines the conditions under which solutions will exhibit the required properties. Chiappori's sharing rule can be regarded as the reduced form of an unspecified bargaining game. As such, it can provide a convenient mechanism for bracketing some problems in order to focus on others. If we accept the Coasian view that bargaining leads to Pareto-efficient outcomes and if we assume that the Pareto-efficient equilibrium is unique, then we are close to Chiappori's single-valued, Pareto-efficient sharing rule.

The assumption that family behavior can be characterized by a Pareto-efficient sharing rule, although it has important advantages, has three significant limitations. First, because the collective model does not specify a particular bargaining model or class of bargaining models, it offers no guidance for choosing which variables to include in the sharing rule as determinants of bargaining power. Second, as Lundberg and Pollak (2003) argue, unless family members can make binding agreements, the assumption that bargaining outcomes are efficient is implausible when the family must take major decisions that affect future bargaining power. The efficiency of family decisions needs to be investigated, not assumed. Third, both cooperative and noncooperative bargaining models can yield multiple equilibria.

When Samuelson posed the Dr Jekyll and Mrs Jekyll problem in 1956, he recognized that his consensus model did not provide a satisfactory resolution. Fifty years later, economists are still grappling with the difficulty in the concept of the consumer that Samuelson exposed. In the intervening years, the economics of the family emerged as an established field with its own North-Holland Handbook. Where Samuelson focused on the family's demand functions for goods and supply functions for labor, the economics of the family now takes as its domain a wide spectrum of family behaviors: household production, the allocation of time and goods within the family, marriage, divorce, fertility, investment in children's human capital, and care of disabled family members.

Samuelson recognized that the unitary model, which provides a rationale for treating families as economic agents, is valid only under highly restrictive conditions. Samuelson's consensus model provides one such set of conditions and Becker's altruist model provides another. The development of cooperative and noncooperative bargaining models of the family has demoted unitary models by clarifying the assumptions—about the rules of the game or its outcome—under which the unitary model is valid. Theorists may debate the plausibility of alternative models, but the unitary model has been mortally wounded by empirical evidence that married couples do not pool their resources. I believe the demise of the unitary model would not have surprised Samuelson.

Notes

1. Samuelson appears to have been hostile to Becker's economic approach to the family, or at least to its application to the study of fertility. Praising Easterlin's relative income hypothesis, Samuelson (1976) goes on to criticize Becker and T. W. Schultz: "Thus, the Easterlin hypothesis can explain fertility waves not unlike those actually experienced in the United States during the last 40 years [footnote omitted]. The Easterlin theory is all the more valuable for its scarcity among economic theories, standing out in welcome relief from the rather sterile verbalizations by which economists have tended to describe fertility decisions in terms of the jargon of indifference curves, thereby tending to intimidate non-economists who have not mis-spent their youth in mastering the intricacies of modern utility theory." To make clear the targets of his criticism, Samuelson followed this sentence with a footnote citing an article by Leibenstein "for a survey of economists' theories of fertility, including that of the Chicago School theorists, Gary Becker and T. W. Schultz . . ."
2. It also poses empirical difficulties because consumption data are almost always collected for households or "consumer units," rather than for individuals.
3. In this chapter I treat the term "family" as synonymous with "household." I also treat utility functions and preference orderings—preferences, for short—as equivalent.
4. In Pollak (2003) I argue that much of Becker's analysis rests on a special case of interdependent preferences that he calls "altruistic" and I call "deferential."

5. Sometimes they are called "common preference models."
6. In divorce threat bargaining models, the anticipated utilities of the spouses in the event of divorce determine allocation within marriage.
7. Lundberg and Pollak (2005) survey cooperative and noncooperative bargaining models of the family.
8. The standard Nash axioms are Pareto efficiency, invariance to linear transformation of individuals' von Neumann–Morgenstern utility functions, symmetry (i.e. interchanging the labels on the players has no effect on the solution), and a contraction consistency condition.
9. Woolley (1988) appears to have been first to use a noncooperative Cournot-Nash equilibrium within marriage as the threat point in a bargaining model.
10. Samuelson, of course, invented revealed preference, but that is a story for another chapter.

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8

Paul Samuelson on Karl Marx: Were the Sacrificed Games of Tennis Worth It?

G. C. Harcourt

> ...around 1955 I volunteered mentally...to investigate whether [Marxian economics] was truly as lacking in merit as seems to be thought the case. (Mark Twain: Wagner is not as bad as he sounds)...colleagues and friends thought it strange of me to waste good tennis time on so irrelevant a subject.
>
> (Samuelson, 1997a, p. 190)

8.1 Introduction and motivation

In his ninth decade Paul Samuelson said in an address to the Bank of Italy on October 2, 1997, in which he compared the difference experiences of present-day American and European economies:

> I lay stress on two main factors, both new and neither one predicted...by any economists writing in the 1980s.
>
> One. In America we now operate...the Ruthless Economy.
>
> Two. In America we now have A Cowed Labor Force...two features...interrelated...[yet]...somewhat distinguishable (Samuelson, 1997b, pp. 6–7).

When I read this, I thought: "Karl, that you should have lived to see this hour." So, when I was asked by Michael Szenberg to contribute to the volume in honor of Paul's 90th birthday, I thought it would be interesting and certainly appropriate to sketch Samuelson's views on Marx as an economist, and any changes in them, over Samuelson's working lifetime (to date, of course).[1] As well as rereading some of his papers on Marx, I went through the references to Marx and topics related to him cited in the indexes of the various editions of Paul's famous introductory textbook (since the 14th edition of 1992, Samuelson and Nordhaus) in order to trace both the waxing and waning over time of the space given to Marx, to see whether and, if so, how his views have changed. Because, with Prue Kerr (see Harcourt and Kerr, 1996; 2001), I have tried to explain to business people and managers what *we* think the essence of Marx's legacy is, I have taken our evaluations as the backdrop against which to assess agreement and disagreement with Samuelson's interpretations and evaluations. I hope he will find the chapter topic acceptable, not least because his contribution (Samuelson, 1997a), to volume I of the Festschriften for me was on Marx.

8.2 Overview of Samuelson's views on Marx

Starting with the introductory text, Marx certainly receives mentions from the first edition (1948) on, but discussions of him and topics related to him—for example, Friedrich Engels, the labor theory of value (LTV), simple and expanded reproduction in the context of modern (now dubbed "old") growth theory, the iron law of wages, and so on—really took off in the late 1960s and the 1970s: whole sections were devoted to the contributions of Marx, the economist. Furthermore, running through other chapters and discussions of other topics are references to, for example, Marx's predictions about history, the nature of technical progress, and the role of the reserve army of labor. There are also sections discussing the economic principles of ideal socialist states vis-à-vis those of pure competitive capitalist economies and the like. In the ninth edition (1973) there is introduced for the first time an eight-page appendix on the rudiments of Marxian economics (pp. 858–866).

Overall, Marx does not get that bad a press from Samuelson, either as told to beginning students or in his various papers[2]: it is true that in his 1961 Presidential Address to the American Economic Association, Samuelson damned Marx with faint praise—"From the viewpoint of pure economic theory, Karl Marx can be regarded as a minor post-Ricardian" (Samuelson, 1962, p. 12; 1966b, p. 1510)—but he quickly backed off—a little—from this in later evalu-ations, for example, his article in the *American Economic Review* celebrating the centenary of the publication of volume I of *Das Kapital*. While "only the Good Fairies should be invited [to] such a birthday party . . . , a great scholar deserves the compliment of being judged seriously" (Samuelson, 1967; 1972a, p. 268). In the latest editions of the introductory textbook, while the number of references to Marx are much reduced, they are still not *that* unfavorable. Indeed, they are more favorable in the Samuelson and Nordhaus editions than in the latest "Australian Samuelson" (Samuelson *et al.*, 1992), some of whose authors are unrepentant cold war warriors!

As is to be expected, Samuelson comes to his evaluation of Marx as an economic theorist par excellence. In an autobiographical essay published in 1972, Samuelson records that he felt he "was made for economics." "To a person of analytical ability, the world of economics was his oyster in 1935 . . . a terrain strewn with beautiful theorems begging to be picked up and arranged in unified order" (Samuelson, 1972b, p. 161). The same enthusiastic approach characterizes his writings on Marx. While he is careful never to put Marx down because Marx did not have access to the same technical and mathematical tools that Samuelson had, he nevertheless emphasizes what he sees as lack of basic logic in many of Marx's arguments, especially in relation to the labor theory of value. Samuelson takes his most severe stance in his well-known 1971 *Journal of Economic Literature* survey of the literature relating to the Marxian transformation problem. Marx argued that the pattern of labor values of commodities in the sphere of production and the pattern of competitive prices of production in the sphere of distribution and exchange are integral to each other, but that labor values are dominant in a logical sense because of the essential nature of the capitalist mode of production. It was necessary therefore to explain the nature of the deviations of one pattern from the other.

In the survey, Samuelson gives a virtuoso performance in his technical discussion of the issues. He provides wonderful diagrams for teaching and the profession alike; he likens labor values of commodities to the outcome of mark-ups akin to value added taxes on the stages of their production, and prices of production to the outcome of mark-ups akin to turnover taxes, making clear the nature of the inevitable divergences of the relative prices of production from the labor values Marx claimed underlay them.

Samuelson will have none of this. He argues that the essential solution to the transformation problem is contained in an eraser (a rubber to Australians and Limeys) with which to erase (rub out) the value scheme and, making a new start, replace it with the prices of production scheme—"the 'transformation algorithm' is. . . : 'Contemplate two alternative and discordant systems. Write down one. Now transform by taking an eraser and rubbing it out. Then fill in the other one. *Voilà!* You have completed your transformation algorithm' " (1971; 1972a, p. 277).

8.3 The transformation problem

Generations of students introduced to economics through Samuelson's textbook are told that the LTV is a theory of prices, the proposition that commodities exchange in proportion to the labor directly and indirectly embodied in them, and that as Smith knew—Ricardo too (but he did not want to)—once we leave Smith's "early and rude state of society," these labor values are both inaccurate and unnecessary in an explanation of the pattern of the relative (long-period) prices that a competitive situation will tend to establish. Moreover, labor is not alone the source of value and therefore price (a meaningless distinction to modern readers but not to the classical political economists (see Cohen and Harcourt (2003)) once land and capital goods come into the story. Then, we need a general equilibrium system in order to determine *simultaneously* both prices of commodities and payments for the services of the factors involved in producing commodities, so that labor values as defined by Marx are both misleading and unnecessary.

No one has ever established, though many have tried, that Marx himself solved the transformation problem in an agreeable way, and Engels behaved badly in not paying up to those who anticipated or even improved upon Marx's proposed solution after he and Marx had issued just such a challenge. Nevertheless, it is wrong to give the impression that Marx thought the LTV was a literal theory of the pattern of prices of production—we know (and Samuelson makes this explicit) that he had written volume III of *Capital* before the publication of volume I in which, probably understandably but nevertheless wrongly, the price system is identified with a simple LTV. What Marx claimed—this is well documented by, for example, William Baumol, Maurice Dobb, and Ronald Meek, as Samuelson acknowledges—was that he could predict the deviations of the prices of production around the underlying labor values once we considered more complicated models of the pure competitive capitalist mode of production in which organic compositions of capital differed as between industries even though rates of exploitation tended to be the same.

Prue Kerr's and my interpretation of Marx's method of analysis and of the LTV is as follows. Marx came to political economy from philosophy; he was crucially influenced by the philosophical views of Hegel and the principle of dialectical change. The use of the dialectic led him always to look for internal contradictions both in systems of thought and in the working out of social processes. His organizing concept when he came to political economy was the notion of *Surplus*—how it was created, extracted, distributed, and used in different societies. Marx looked at human history as succeeding epochs of different ways of surplus creation, and so on; he was determined to find by analysis of the power patterns of each the seeds of both their achievements and their internal contradictions and eventual destruction and transformation as, through the endogenous processes thus discovered, one form gave way to the next. The jewel in his crown was his analysis of capitalism.

Marx's method of analysis may be likened to an onion. At the central core underlying the overlapping outer layers of skin is the pure, most abstract yet fundamental model of the mode of production (Marx's phrase) being analyzed. All fossils from the past, all embryos of what is to come, are abstracted. The system is revealed in its purest form. Yet, the aim is to show that the fundamental characteristics and relationships thus revealed are robust—that they survive intact the complications provided by adding back (in analysis) the inner and outer layers of skin of the onion, that they still remain the ultimate determinants of what is observed on the surface. Thus, if we may illustrate from the transition from volume I to volume III of *Capital*, though there is little explicit mention in volume I of the (near) surface phenomenon of prices of production of volume III, the links from the underlying labor values of volume I are always at the forefront of Marx's intention—not in the mainstream sense of providing a theory of relative prices (the mainstream interpretation of what the LTV is about) but in making explicit the link as a necessary part of the story of production, distribution, and accumulation in capitalism.

Let us now see what we understand by the dreaded term LTV. As we said, the principal task Marx set himself was to explain the creation and so on of the surplus in capitalism. Naturally he linked this in capitalism with an explanation of the origin of profits and the determination of the system-wide rate of profits. He identified in previous modes the role of classes in each, one dominant, one subservient, with reference to the creation of wealth and so social and economic power, and the connection of their relationship to the creation and so on of the surplus by a process of explicit exploitation of one class by another. For example, in feudalism, the process was obvious: its institutions and laws ensured that the lords of the manor could physically extract from the serfs part of the annual product, either by making the serfs work for set periods on the lords' lands or because the serfs were tenants, requiring them to "hand over" part of the product of the land which their labor had brought forth.

When we get to pure *competitive* capitalism, such a process seems impossible. For one aspect of capitalism, purified in modern theory to become price-taking behavior by all agents with prices set by the impersonal forces of the market, in classical and in Marx's times more robustly specified as wide diffusion of power amongst *individual* capitalists and *individual* wage-earners, seems to make it impossible for individual capitalists to coerce free wage-earners into doing what they do not wish to do. They could always leave one and work for another, just as any one capitalist and his or her capitals could leave or enter any activity—hence the *tendency* for rates of profits to be equalized in all activities and the need to explain what determines the origin and size of the systemic rate of profits to which their individual values tended. Moreover, each free wage-laborer was paid a definite money wage for all the hours he or she worked. Under these conditions, how could exploitation occur or a surplus arise, and where *did* profits come from?

Marx answered this by distinguishing between necessary and surplus labor time associated with the class relations of capitalist society. Capitalists *as a class* (subset into industrial, commercial, and finance capital) had a monopoly of the means of production and finance. Workers *as a class*, having only their labor power to sell, had to do as they were told in the workplace. As property-less, landless but free wage-laborers, whose creation was the by-product of feudalism giving way to capitalism, they had but one choice—*either* to work under the conditions established by the capitalist class *or* to withdraw from the system entirely, and starve. Therefore the working day could conceptually be split into two parts: the hours needed with the existing stock of capital goods, methods, and conditions of production to produce wage goods (necessary labor) and the rest (surplus labor) which was the source of surplus value in the sphere of production, and of profits in the sphere of distribution and exchange. Marx adopted the classical idea, especially Ricardo's, that all commodities had an embodied labor value to explain how labor services, a commodity saleable just like any other in capitalism, would tend to sell at a price determined by its value. But human labor had the unique property that it would create more value—produce more commodities—than was needed for its own reproduction and this was embodied in the commodities corresponding to this surplus labor time.

A subsidiary part of the story was that the actual operations of capitalism resulted in the waxing and waning of the reserve army of labor (RAL)—a much more suitable euphemism for the unemployed than the modern description of the same phenomenon as flexible labor markets—causing actual wages to tend toward (or fluctuate around?) their natural values (a purely classical story). But the main story was that while the surface phenomenon seemingly reflected fairness and efficiency—people being paid fully for what they did and all the hours they worked—this masked the underlying exploitation process arising from the situation of class monopoly. In the sphere of production there was a tendency to equality in the length of the working day (week, year) and intensity of work too. In the sphere of distribution and exchange, abstracting from actual (market) prices, there was a tendency for the prices of production to be such that a uniform rate of profits was created (the first great empirical generalization of classical political economy) and for the profit components of the prices of production to be such as to constitute uniform rates of return on total capitals, similarly measured, in all activities.

Many, including Samuelson, have come to see the "transformation problem" as a sterile exercise and debate. Yet viewed in this way I think it makes sense, both in explaining a fundamental characteristic of capitalism and in illustrating the power of Marx's method and approach. In order to show that anything classical political economy could do Marx could do as well and better, it was necessary to reconcile the pure theory of the origin of profits in the capitalist mode of production with the other major "finding" of political economy—the tendency to a uniform rate of profits in all activities—and also to "explain" what determines the size of the system-wide rate of profits. (Piero Sraffa, who had a deep knowledge of and admiration for Marx's work, always spoke of the rate of profits, indicating that it *was* the system-wide concept which needed to be explained within the classical and Marxist system. As Luigi Pasinetti said of his own modern variant of the theory of the rate of profits, "It is macro-economic because it could not be otherwise" (Pasinetti, 1974, p. 118)).

The various conundrums arise because, while competition would ensure a uniform rate of exploitation (s/v, where s = surplus labor and v = necessary labor) in all industries because, as we have seen, free wage-laborers can always move from one occupation to another, there is nothing obvious or even not obvious in the forces of competition and their impact on technical progress to ensure that the corresponding organic compositions of capital (c/v) (with some license, the capital-labor ratios) should also tend to equality. But since a well-known Marxist result is that $r = s/v/(1 + c/v)$, when all variables are measured in terms of abstract socially necessary labor time, *if* the LTV meant that commodities exchanged in proportion to their embodied labor mounts, there would not be a tendency, not even a long-run one, to equality of rates of profit (so measured) in all activities.

Therefore it became necessary to explain the deviations of the prices of production with their uniform profit components around the underlying labor values, at the same time requiring the explanation to embrace the magnitudes of surplus value *et al.* in the sphere of production.

This step is what the various proposed "solutions" of the transformation problem were meant to establish—Sraffa's is the most satisfying as Ronald Meek pointed out in his 1961 review article of *Production of Commodities* (Meek, 1961; 1967). Baumol in his comment (Baumol, 1974a) in 1974 on Samuelson's *Journal of Economic Literature* survey (in which he was only concerned to establish what Marx really said, not that he was necessarily correct) set out a parable which I believe is consistent with our interpretation.[3] Baumol (1974a, p. 153) wrote:

> My contention is that Marx['s] interest in the transformation problem analysis as a sequel to his value theory was not a matter of pricing. [He] sought to describe how *non* wage incomes are *produced* and then how this aggregate is redistributed... first... substantive issue to Marx and the one he discusses in volume I, while the latter is the surface manifestation known to all bourgeois economists and which Marx only deigns to consider in volume III.
>
> The substance of Marx's analysis can be summarised in a simple parable... the economy is described as an aggregation of industries each of which contributes to a storehouse containing total surplus value... Each industry's contribution is proportionate to the quantity of labor it uses... how society's surplus value is *produced*.
>
> The distribution of surplus value from the... storehouse takes place via the competitive process which assigns to each industry for profit... an amount... proportionate to its capital investment... the heart of the transformation process—the conversion of surplus value into profit... takes from each according to its workforce and returns to each according to its total investment (emphasis in original).

"Values," adds Baumol, "are not approximations to prices nor a necessary step in their calculation... one is a surface manifestation—the latter... intended to reveal an underlying reality" (p.55). (Baumol (1974b) and Samuelson (1974a) subsequently had an exchange over Baumol's comment. Samuelson, in effect, restated his original position and Baumol thought Samuelson's reply was to an article he never wrote.)

I would also argue that Piero Sraffa's solution of the transformation problem (Sraffa, 1960), the nature of which is brought out beautifully in Ronald Meek's illuminating review article of Sraffa's classic (Meek, 1961; 1967), arises because Sraffa's analysis is set in a context that accepts Marx's general approach, even though he is critical of specific incoherence and his analysis is also directed at unfinished business. Meek showed the striking similarity between Marx's, ultimately unsatisfactory, concept of the average industry and Sraffa's precise account of the Standard system, Standard commodity and the relationship $r = R\,(1 - w)$, where r = rate of profits, R = the maximum rate of profits in the Standard system (and actual economy) and w = share of wages in the Standard national income. "What both economists are trying to show . . . is that the average rate of profits, *and therefore the deviations of price ratios from embodied labour ratios*, are governed by the ratio of direct to indirect labour in the industry whose conditions of production represents a sort of 'average' of those prevailing in the economy as a whole" (Meek, 1961; 1967, pp. 176–178, emphasis added).

Furthermore I would submit that, in general terms, Samuelson's (1997a) address in Rome draws on the idea that it is the creation of the potential surplus in the sphere of production via the activities of the business class, now with international as well as national aims, that provides the backdrop to the realization (or not) of profits and accumulation in the sphere of distribution and exchange. The person who has set this out extremely clearly is Donald Harris in his 1975 *American Economic Review* article and 1978 book. Joan Robinson, who was contemptuous of the LTV—she used to say in effect that she could not see why the LTV is needed to explain that chaps who owned the means of production and had access to finance could push around chaps who did not—nevertheless was at one with Harris on this interpretation. Samuelson also seems to be at one with Marx in recognizing the internal contradictions in the operation of capitalism, whereby in the attempt through harsh monetary policy to create a larger potential surplus, the impact on Keynesian "animal spirits" maybe such as to prevent the potential surplus becoming an actual surplus.[4] Witness the disappointing rates of accumulation in many economies in recent years, relatively to those of the years of the Long Boom (or Golden Age of capitalism).

8.4 Samuelson on Marx: Accumulation and growth

One of the headings in the 1960s and 1970s editions of *Economics*, for example, the eighth edition (1970, p. 718), at first sight may tend to cause raised eyebrows. Samuelson refers to "Ricardo–Marx–Solow models of capitalist accumulation"! But on further reflection, Samuelson has made an incisive point here (marred only by his omission of Trevor Swan's name from the list), as Bob Rowthorn, for one, has often argued. For if we ignore the adjectives involved, there *is* a family resemblance between, for example, the narrative that James Meade told in his neoclassical theory of growth (Meade, 1961)—itself explicitly based on Swan's work, especially on the feedback between relative factor prices, choice of technique, and induced technical change—and the narratives that we find in volumes II and III of *Capital*. All these authors were striving toward what we now call a theory of endogenous technical change to which, of course, Samuelson has also made important contributions.

From some of his earliest postwar writings, Samuelson has always been keen to test whether classical and Marxian conjectures are confirmed when the latest technical advances, for example, linear programming, are used to specify their ideas in models. Thus, we have here his papers in the 1950s on Marx and Ricardo (Samuelson, 1957, 1959a, 1959b, 1960, all reprinted in Samuelson, 1966a, Part IV), and later in his textbooks, on schemes of simple and expanded reproduction in Marx. Samuelson sees these classical and Marxian contributions as early forerunners of modern (now "old") growth theory associated with Harrod (1939, 1948) and with Solow and Swan's responses to the puzzles thrown up by Harrod's contributions: that is to say, the analysis of steady states and their stability, especially the stability of the warranted rate of growth, g_w, itself, and the possibilities of g_w finding its way to the natural rate of growth, g_n, if initially their values are not equal.

But perhaps this is a misreading of what Marx principally had in mind. Claudio Sardoni (1981) has pointed out that Marx was asking the question: what conditions have to be satisfied in order that aggregate demand and its components match aggregate supply and *its* components, period by period, and that this did not necessarily imply advance in a steady state at a constant rate of growth. Marx did the analysis in terms of his three departmental schema, which Samuelson reproduces in his discussions. Marx asked in effect what conditions will ensure that each department either directly (i.e. from within itself) or indirectly can find markets for its output. The point is that by establishing the very special conditions involved, Marx illustrated how unlikely it was that a competitive capitalist economy, with each individual capitalist doing his/her own thing concerning production, employment, and accumulation, would lead to an uncoordinated collective outcome that satisfied the conditions for balance of both totals and compositions.

The next question then was what would the nonfulfilment lead to as far as systemic behavior was concerned? Marx thought it would create cycles and even crises. Joan Robinson once pointed out that Harrod had rediscovered this insight when he set out what were the consequences of the nonattainment of the warranted rate of growth, that is, of the actual rate of growth not immediately (and thus not ever) coinciding with the warranted rate.

8.5 Marx and Samuelson's method and approach

Samuelson's Italian address reflects, I think, a central characteristic of Marx's procedure. As pointed out above (see pp. 130–132), Marx divided history into distinct periods classified by the specific way the surplus was created and so on. Allied with this insight was the view that each period has a dominant class, the nature of whose dominance determined the nature of surplus creation. Nothing in Marx's account of surplus creation in capitalism is inconsistent with the view, shared by Samuelson in many of his comments in his textbook and essays, that capitalism was an extraordinarily dynamic mode of production which through accumulation and innovation raises productivity and at least potential standards of living over time.

Those scholars who were more favorable to Marx in general, for example, Dobb, Meek, Sraffa, took this narrative for granted when they contributed their formal analysis of the characteristics of the two spheres of operation in capitalism. So too does Samuelson now, or so I interpret him as doing; but in his earlier discussions at least he seems not to have accepted the prior narrative. Partly, this may be due to Samuelson's comparative advantage in presenting technical analysis, the establishing of pleasing lemmas and theorems; partly, it may be due to his being adverse to "the late Prof. Wildon Carr's admirable motto . . . 'It is better to be vaguely right than precisely wrong.' "[5]

Samuelson and Marx have some, perhaps many, things in common in their methodology and general approach. Both always steeped themselves in what had gone before, provided an internal critique, and then built their own often innovative but also synthetic contributions on the basis of their absorption of the writings of their predecessors. Samuelson is very careful to name predecessors of ideas, both in his textbook and in his articles, before he uses his powerful technical skills to see whether conjectures or less rigorous (in his terms) arguments of predecessors go through. Usually this proves to be most illuminating and helpful, especially in teaching, by extracting in a simple precise way the essence of some conventional doctrines. Though it runs the risk sometimes, as

Keynes said of Russell, Wittgenstein, and Ramsey's "gradual perfection of the treatment [of logic]," to run the risk of reducing it to "mere dry bones" (Keynes, *CW*, Vol. X, 1972, p. 338); dry bones into which, moreover, life may not be breathed. That is to say, while the systems formed may be self-consistent, they are too far removed from the reality from which they started to serve to provide illuminations.

8.6 Increasing misery and skilled labour

Samuelson used his method and approach to criticize the "doctrine of increasing misery" (Meek, 1967, p. 113) of Marx, whereby both the rate of profits tends to fall over time and the situation of the wage-earners to worsen, as one prediction too many. I suppose a riposte could be that if accumulation too were falling as a result so that unemployment was rising, both tendencies could be present. But this is not a convincing response, especially when we take, as we must in this context, long-term advances in technical progress into account. We must agree therefore with Samuelson's contention that Marx was wrong and so must be content with being "just another genius" as Meek (1967, p. 128) told us many years ago.[6]

Samuelson lists in many places as one of his criticisms of the LTV the incoherence introduced by the presence of skilled labor, in that a way of measuring its amounts in a common unit of socially necessary labor cannot be found or, at least, not in a manner that is independent of the wage structure which it ought to be one of the tasks of economic theory to explain. There are perhaps two comebacks. First, in order to make the point at a high level of abstraction that surplus labor and value are both the source of profits and vital determinants of their size, assuming homogeneous labor may be a legitimate simplification. Second, Rowthorn (1988) has set out an ingenious way of getting over the problem of reduction of skilled to unskilled labor. He follows a suggestion of Hilferding. This involves tracing back and adding together the amounts of socially necessary labor needed to produce skilled labor. Rowthorn provides an elegant formal exposition using simultaneous equations, which no doubt would warm Samuelson's heart. It, moreover, allows technical advances to be incorporated and so "the reduction of skilled to unskilled labour can be performed quite independently of the level of wages and the analysis avoids Böhm-Bawerk's charge of circularity" (Rowthorn, 1980, p. 233).

8.7 Conclusion

To conclude: Samuelson in his Italian Address has, to some extent anyway, accepted Marx's desire to explain in simple pure (or ideal) models the various processes at work in capitalism then and now. He has also accepted some of the ingredients of Marx's own explanations while not jettisoning his own use of more mainstream ingredients.

I hope that I have not done Paul Samuelson any gross injustice in this evaluation. If I have, no doubt he will respond by emulating Marx (but, I am sure, much more politely) in the manner in which Samuelson at the end of his essay celebrating the Centenary of the publication of volume I of *Capital* describes Marx's own likely response:

> But this is a birthday party and I [PAS] approach the boundaries of good taste. Let me conclude by wishing that, like Tom Sawyer attending his own funeral, Karl Marx could be present at his own centennial. When the 'the Moor' rose to speak, how we would all pay for our presumptuousness! (Samuelson, 1967; 1972a, p. 275).

Notes

1. May I say how honored and delighted I feel to have been asked to contribute to the volume? Though we do not agree on some issues, I have always admired Paul and his work. I have especially appreciated the graciousness he has always shown to me, despite—or perhaps because—of our differences over the years: the graciousness he also showed my mentor, Joan Robinson, despite *their* many intellectual battles over the years. He is a role model and an inspiration to our profession.
2. I must admit, though, that after having done my homework, I looked again at Paul's essay (1997a), "Isolating sources of sterility in Marx's theoretical paradigms," only to read that "at the end of the day I never could find analytical pearls to cast before orthodox economist swine . . . *Capital's* volume 2 tableaux of reproduction and balanced exponential growth [is] . . . Karl Marx's sole contribution to economic theory" (p. 190)—and then only in a (very) special case!
3. Indeed, in my case—I cannot speak for Prue—I was very much influenced by Baumol's comment and parable, as well as by many discussions with and the writings of my three former research students, Prue Kerr, Allen Oakley, and Claudio Sardoni on these and related matters.
4. Samuelson surely would also appreciate Tommy Balogh's quip that "Monetarism is the incomes policy of Karl Marx" (Balogh, 1982, p. 177).

5. This was quoted by Gerald Shove in his evaluation of Marshall's *Principles* 50 years on from their publication. Shove thought it "might well have been Marshall's" as well (see Shove, 1942, p. 323).
6. In fairness to Marx it may be said that all the major classical political economists thought there would be a long-run tendency for the rate of profits to fall, though they differed fundamentally in their explanations why.

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9

Paul Samuelson and the Stability of General Equilibrium

Franklin M. Fisher

The study of the stability of general equilibrium is not a popular indoor sport among present-day economists. Yet, the lack of a fully satisfactory stability analysis is a gaping hole in microeconomic theory. In particular, the First and Second Welfare Theorems on which so much policy depends are theorems about the efficiency properties of general equilibrium. If general equilibrium is not satisfactorily stable, then the usefulness of those theorems is in question. Further, to assume that the economy is always at or near equilibrium is to beg the question of why that is so, and to fail to notice that relative prices do change in fact.

In short, economists' concentration on equilibrium analysis seems grounded only in elegant convenience rather than in soundly based proof. It looks at economies only after the Invisible Hand has ceased its activity and ignores the way in which such activity operates. The Rational Expectations school of thought is the prime example of this error, but it is far from the only one.

Despite the fact that stability theory has yet to reach a satisfactory conclusion, it remains my hope that the twenty-first century will see more attention paid to this area and considerable progress made. If that turns out to be the case, it will have been Paul Samuelson who long ago set the subject on the right path, even while, at the same time, introducing a then necessary construct whose dismantling appears now to be a crucial step.[1]

The pre dawn of stability theory occurred with the publication of John Hicks's *Value and Capital* (Hicks, 1939). In the appendix to that influential volume, Hicks set forth a set of conditions that he regarded as important (perhaps even necessary and sufficient) for the stability of prices in a general equilibrium system of multiple markets. He began with the proposition that stability in a single market depended on excess demand being a decreasing function of the price of the commodity there traded, and then added conditions to ensure that this property would be preserved even when other markets and prices were permitted to adjust in reaction to a non-equilibrium price in the first one.

But however interesting the Hicks Conditions might be,[2] Samuelson pointed out that their relation to stability of anything was questionable (Samuelson, 1941; see also Samuelson, 1947, chap IX, especially pp. 269–270). Indeed, more importantly, he pointed out that the study of stability necessarily requires consideration of dynamics and hence of a process that unfolds over time. This observation and his suggestion of how such a process should be modeled were the foundation of the stability literature.

Samuelson suggested that one should model the behavior of prices in disequilibrium according to the following equation (somewhat generalized and updated):

$$\dot{p}_i = F^i(Z^i(p)) \quad \text{when } p_i = 0 \text{ and } Z^i(p) < 0 \text{, in which case } \dot{p}_i = 0.$$

Here, p_i denotes the price of the ith commodity, p is the price vector, $Z^i(p)$ is the excess demand for the ith commodity, and $F^i(\bullet)$ is a continuous, sign preserving function. This means that each price moves in the direction indicated by the excess demand for the corresponding commodity—up when demand exceeds supply and down when supply exceeds demand. (The second part of (9.1) is a later addition, designed to take account of the fact that prices cannot be negative.)

Samuelson went on to point out that, for local movements, this could be represented as a linear differential equation, and, following that, the literature went on to consider whether the Hicks Conditions were either necessary or sufficient for the local stability of the process of price adjustment being modeled. It was shown (Metzler, 1945) that this could be analyzed under the assumption that all commodities are gross substitutes, and eventually shown (Hahn, 1958; Negishi, 1958) that the gross substitutes assumption implied the Hicks Conditions and hence local stability. It was then shown (Arrow and Hurwicz, 1958) that the gross substitutes assumption itself implies global stability.[3]

Where Equation (9.1) is the only equation of motion, only prices adjust, no disequilibrium trade takes place, and the process is known as "*tâtonnement*." It is not generally stable. But Samuelson's reasonable-appearing suggestion survived into the far more fruitful non-*tâtonnement* literature, where trade does take place out of equilibrium and thought has been given as to *how* it takes place.

The problem with (9.1), however, is that, appealing as it is, we have no good reason for believing in it. This can be described as follows:

We are dealing with a competitive economy, in which all participants take prices as given. But, as Arrow, among others, aptly remarked, if everyone takes prices as given, how do prices ever change?[4]

Indeed, the same problem exists even when dealing with a single competitive market in isolation. The explanation commonly given in elementary classes is that, if demand exceeds supply, unsatisfied buyers offer higher prices, and that, if supply exceeds demand, unsatisfied sellers offer lower ones. At some level, this is doubtless correct, but it lacks rigor.

The common fiction in the general equilibrium literature is that this is the job of an "auctioneer" who compares demand and supply and behaves according to (9.1). But such auctioneers generally do not exist.

This problem is symptomatic of a larger one. Economic theory provides a truly elegant explanation of the behavior of the individual maximizing agent in a competitive economy and of how all plans can be consistent in general competitive equilibrium. But it is practically silent on how agents behave when their planned actions do *not* turn out to be feasible. Yet the question of out-of-equilibrium behavior is central to any examination of dynamics and stability. Ironically, Samuelson pointed out that the modeling of disequilibrium behavior over time was essential, but also suggested an adjustment equation that has little or nothing to do with such behavior.

In some ways, this may have been fortunate, however. With the methods known at the time, analysis of stability with individuals setting prices might have been even more frustratingly difficult than it turned out to be in the late twentieth century. At that time, I was able to show that price-setting depended on each seller's (to make it simple) perception of its monopoly power. Unfortunately, even if one can prove stability this means that convergence of the system to a *Walrasian*, *competitive* equilibrium will only happen if such perceived monopoly power asymptotically lessens to zero (Fisher, 1983, chapter 6). Interestingly, this is related to liquidity problems and to the liquidity trap of Keynes' *General Theory*.

But it also turns out that the stability proof involved depends on a strong assumption about individual perceptions generally—an assumption that new favorable opportunities do not arise as sudden surprises in the course of the adjustment process. I believe that assumption to be probably inevitable when dealing with a process that depends on individual behavior and expectations, but that does not make the result very appealing.

Still, if progress is to be made, economists cannot refrain from considering such issues. One cannot understand the workings of the Invisible Hand by examining only situations where the Hand has already done its work. Paul Samuelson may have been the first to truly understand this.

Notes

1. For a general review of the subject and a detailed analysis of its importance and problems, see Fisher (1983), especially chapter 2.
2. It is now known that they ensure uniqueness of general equilibrium and follow from the assumption that all goods are gross substitutes—hence also ensuring global stability of the *tâtonnement* process.
3. Much later, McFadden (1968) showed that the Hicks Conditions could be interpreted in terms of relative speeds of adjustment in a model in which they imply global stability without the Gross Substitutes assumption.
4. I have been unable to track down the exact reference.

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10

Paul Samuelson and Piero Sraffa—Two Prodigious Minds at the Opposite Poles

Luigi L. Pasinetti

10.1 Introduction

This chapter is concerned with two giants in the history of economic thought, who stand at the opposite poles. Paul Samuelson is one of the main architects—perhaps *the* main architect, and in any case the leading symbol—of what nowadays is known as neoclassical economics—that is, dominant economics. Piero Sraffa has been the most acute critical mind of Marginal, and hence neoclassical, economics and the leading promoter of a resumption of that classical economic analysis, which—born at the eve of the Industrial Revolution—was (as he claims) "nipped in the bud," and unduly submerged by an over-flowing of (Marginal) economic theory. If the mark of a great leader is the recognition he or she obtains by the leader of the opposite camp, we must admit that Paul Samuelson never spared enthusiastic appreciations of the greatness of Piero Sraffa. "Did any scholar have so great impact on economic science as Piero Sraffa did in so few writings? One doubts it. [...] Piero Sraffa was much respected and much loved. With each passing year, economists perceive new grounds for admiring his genius" (Samuelson, 1987, p. 460).

Yet, while Samuelson, with his articles—hundreds of them—and with his successful textbook—the most successful textbook ever written in

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economics—has become the undisputed icon of mainstream economics, in the United States and throughout the world, Sraffa remains almost an unknown soldier, above all, in Samuelson's homeland. Sraffa never wrote any textbook, even less did he write popular articles in influential magazines or newspapers. He was always reluctant to publish anything, and the very few times he did publish, he even eschewed—unlike his mentor, John Maynard Keynes—any attempt to actively promote the diffusion of his publications. Nevertheless, the influence of Sraffa on shaping the course of economics science in the twentieth century is unquestionable. He was the mind behind "the imperfect competition revolution" of the 1920s and behind the famous *Economic Journal* discussion on "returns to scale" in the 1930s. His *Works and Correspondence of David Ricardo* (Sraffa, 1951–72) has made him the most celebrated critical Editor in economics ever. He was the inspirer of the debate on capital theory of the 1950s and 1960s. And just when almost nobody expected anything from him anymore, he published his masterpiece—an amazingly compact 99-page book, with an apparently innocent title, *Production of Commodities by means of Commodities* (Sraffa, 1960, *PCmC* from now on).

Samuelson acknowledged Sraffa's achievements with no hesitation. In a commemoration article, "A genius with few works," published in Italian at the time of Sraffa's death, Samuelson (1983) remarked that each single page Sraffa published has left a mark in the economic literature. Samuelson has even gone to the point of calling the two decades that followed the Second World War, "the age of Leontief and Sraffa" (Samuelson, 1971). Yet, three years later, the same *Journal* offered its readers another article—"The Age of Leontief and Who?"—in which its author, Levine (1974), showed surprise, claiming that the work of Sraffa "does not appear to have penetrated very deeply into the consciousness of the economics profession in North America, nor . . . into that of the United Kingdom" (Levine, 1974, p. 872).

All this makes any parallel between Samuelson and Sraffa even more intriguing. The keen interest that Samuelson showed in Sraffa's contributions is puzzling, as much as genuine. Samuelson was one of the few economists to read the proofs of *PCmC*. They were sent to him by the Cambridge University Press—as he tells us—with the question: "Shall we bring out a separate American publication?" And his reply was an "enthusiastic affirmation" (Samuelson, 2000a, p. 113). Ever since, he never ceased writing and conjecturing on Sraffa's arguments and findings. So much so that Samuelson has devoted to Sraffa's *PCmC* a number of pages that are more than double the length of Sraffa's book itself. Where does this unusual interest originate from? And why does Samuelson remain one of the few US economists who continues to be keenly concerned with the Cambridge Italian economist? The present chapter makes an attempt at peeping into this sort of mystery.

10.2 Different Styles and Reciprocal Fascination

As far as sheer personalities are concerned, we could not imagine a wider gap between Samuelson and Sraffa. Paul Samuelson's extrovert temperament and great communicative disposition are well known. A delightful occasion such as the present celebration could never have been imagined to happen in the case of Sraffa. Again, on sheer ground of research organization, the central place that Samuelson has always held in the economics Department of M.I.T., his participation to innumerable meetings, congresses, conferences, at home and abroad, stand out as diametrically opposite to the unobtrusive, almost hidden role, to the solitary life, of Piero Sraffa, as a near anonymous Fellow of a Cambridge college. At the same time, Sraffa's contacts with, and remarkable influence on, outstanding intellectuals of his generation (think of Wittgenstein, of several notable Cambridge mathematicians and economists of his time) are well known and widely acknowledged, even if they took place in lonely conversations, restricted encounters in college rooms, or in the college backs, or in esoteric places, such as mountains, seaside secluded places, or accidental spots. All this being said, the reciprocal intellectual fascination and attraction of Paul Samuelson and Piero Sraffa are an undeniable fact, even if they lived far away from each other. (Piero did not even ever visit the United States.) Perhaps, the longest time they happened to be together was confined to the few days of the Corfù Conference on the *Theory of Capital* (1958), a very rare (perhaps unique) occasion in which Austin Robinson succeeded in convincing Piero (thanks to the sheer beauty of the chosen island) to attend one of his numerously organized International Economic Association Conferences. One can have some ideas of the problems they may have been talking about in their exchanges from the (published) minutes of the Conference. An amusing impression of what may have gone on between the two can also be glimpsed from the correspondence (rather restricted, but not insignificant) that followed those encounters.[1] Reading the jokes that the two were improvising in their exchanges is exhilarating delight. But of course it is to their works that one must look to find the source of their intellectual attraction and reciprocal fascination.

Samuelson is one of "the last generalists economists"—as he defined himself.[2] His interests ranged over so varied and numerous fields as to make it impossible even to list them synthetically here. But there is one major work—his masterpiece, *Foundations of Economic Analysis* (1947)—that, besides having established his fame all over the world, is at the very basis of his scientific contributions. This work, more than any other, shaped the direction of modern economic analysis and brought infant Walrasian economics to its mature neoclassical shape. Sraffa, on his part, by distilling his words in his rare publications and by leaving us an enormous heap of unpublished notes, has equally proved to aim at fundamental results, though on a strikingly different route. He is the (so far unsurpassed) champion of a reconstruction of classical economics, and the major critic of the (marginal economics) theories that have spread since the end of the nineteenth century. His masterpiece, *PCmC*, is, as Samuelson's *Foundations*, of equally foundational character. It is not therefore so surprising that it should have attracted Samuelson's attention since the beginning.

10.3 Alternative "Foundations"

"Comparison is a death knell to sibling harmony." It may be wise to proceed with caution. *Foundations of Economic Analysis* (Samuelson, 1947) and *Production of Commodities by means of Commodities* (Sraffa, 1960) are two books that tackle economic theory at its foundations; but they do so by proceeding on two quite different tracks.

Samuelson has, in a sense, chosen an easier route. His foundational contribution is covering, and crowning, an enormous number of works carried out over decades, since the 1870s, by hundreds of economists. His genius has led him to discover that all these works are carrying out the same basic conceptual process, and are applying it over and over again to a whole variety of economic problems. He has thus been able to single out a central (*mathematical)* principle—maximization under constraints—that allows him to insert all those works into a grand general theoretical frame. In this way, his scheme, ambitiously and—from the result he has achieved, we must say—successfully pursues the aim of absorbing all specific economic theories in order ideally to unify them, under the same comprehensive over-all (neoclassical) umbrella.

The route which Sraffa decides to take is a much thornier one. He goes back, so to speak, to where modern economics began (under the strong impact—we must remember—of a major historical event, the Industrial Revolution). He goes back to classical economics, which, when it was started and developed, got enmeshed into no few analytical problems (left unsolved). Sraffa is convinced that he must, first of all, eliminate all ambiguities and definitely solve those analytical problems in an unassailable way. This task constrains him to a preliminary stage, which, to be successful, must be placed on absolutely solid bases, even if it must be left open in many directions. He feels this so strongly as to append an apparently disconcerting subtitle—*Prelude to a critique of economic theory*—to the title of his book. If taken literally, this would confine his investigation to a *destructive* stage, which—we must acknowledge—he accomplishes rather successfully. But of course this cannot be the whole thing; and he says so. Of course, one should then proceed beyond the *Prelude to the critique*. And even this cannot be enough. We are reasonably entitled to ask: When and in which way will the *constructive* phase set in? And to what extent? And in which way will it be able to absorb, and thus include, into its ideally more general framework whatever acceptable contributions may already have been achieved elsewhere?

The Levine (1974) quotation mentioned above shows the “normal” way to react to questions of this type. Levine simply records the lack of interest from the (American and U.K.) profession. But this is not Samuelson’s reaction! He responds in an exceptionally perceptive and concerned way. This is the intriguing point.

There must be a clue (or clues). I can immediately see at least a methodological one. Under the full title of his *Foundations*, in the front page of his book, Samuelson appends, as epigraph, a proposition by J. Willard Gibbs: “Mathematics is a language,” which sounds as a sort of *manifesto*. No doubt, Sraffa’s book is perfectly in line with this declaration of intents. It even goes beyond it; and in two ways. First, by pursuing the conviction that absolutely water-tight, coherent, logic is even more important than strict mathematics itself, as he always argued (with some vindication) with his mathematician colleagues in Cambridge. Second, by claiming that even strict, logically consistent, mathematical language may not be enough.

But at this point I am risking to begin to walk on a ground “where angels fear to tread.” For the limited purposes of this chapter, I shall limit myself to briefly consider two historical instances in which, between the two prodigious minds we are considering, there was a clash. Confronting disagreements may sometimes turn out to be more helpful in improving understanding than considering common views.

10.4 The Re-switching of Techniques Debate

The first example I shall consider is the one in which there was a clash to begin with, which was then followed by a meeting of the minds, in spite of differences in interests and methods. This is the case of the well-known debate that took place in the 1960s, on the phenomenon of the *re-switching of techniques*.

For brevity's sake, I shall condense the essential points in five steps:

1. A preliminary stage, in which Joan Robinson (1953–54) broke on the scene with a vehement critique of the use of the concept of an aggregate "quantity capital" in the theory of economic growth. Almost by chance, she came across a *curiosum* in capital theory, which she could not herself clearly explain (but the point became clear later on). Robert Solow (1955–56) and Trevor Swan (1956) replied to the critique by strenuously defending the concept of a neoclassical production function. Both sides of the debate remained adamant on their positions, without succeeding in gaining conviction from the other side.

2. The publication of Piero Sraffa's slim book, *PCmC* (1960), where—in a very concise seven-page chapter XII, "Switch in methods of production"—Sraffa showed that the same method of production can become the most profitable one, as against many other alternatives, at different, separate, levels of the *w/r* (wage-rate/rate-of-profit) distribution curve, thus contradicting the neoclassical basic contention of a monotonic inverse relationship between capital and its "price" (the rate of profit);

3. Samuelson's *RES* (1962) article, where he constructed a *surrogate production function*, that is generated when there is a succession of linear *w/r* alternative relations. Such *surrogate* function mimics—it was hoped for the general case; this was the conjecture—all the relevant features connected with a full, "well-behaved", neoclassical production function;

4. A *QJE* article by David Levhari (1965), in which the author, at that time a graduate student at MIT, developed Samuelson's conjecture into a full *non-switching theorem*. The claim was that a succession of techniques, each of which yielding a polynomial (not a linear!) *w/r* relation, could cross each other only once in the positive quadrant. This theorem, if true, would have falsified Sraffa's result and would have given full generality to Samuelson's claim of a "well-behaved" production function;

5. A paper presented by Pasinetti at the Rome 1st Congress of the *Econometric Society* (1965), in which the Levhari–Samuelson nonswitching

theorem was proved to contain a logical flaw, with the devastating implication (for neoclassical economics) that, in general, the *w/r* relations of any two techniques may intersect each other more than once in the relevant quadrant. Other disproofs of the same theorem followed in a *QJE Symposium* (Samuelson, 1966a).

To Samuelson's merit we must record that his response astonished the world of professional economists. He immediately admitted that "the non-switching theorem is false" (Levhari-Samuelson, 1966), and went on to draw the implied logical consequences. In his concluding words,

> Pathology illuminates healthy physiology . . . re-switching is a logical possibility in any technology, indecomposable or decomposable . . . There often turns out to be no unambiguous way of characterising different processes as more "capital-intensive", more "mechanized", more "roundabout" . . . Such . . . labelling is shown, in the case of re-switching, to lead to inconsistent ranking between pairs of unchanged technologies, depending upon which interest rate happens to prevail in the market". (Samuelson, 1966b, pp. 582–83.)

This provided in fact the basic conclusion of the debate. A vast literature followed, in the 1960s and 1970s, which did refine, or try to minimize (or try to extend), the results stated above. But the logic (and Samuelson's conclusion) of the whole debate had made the point clear. Samuelson and Sraffa, though with different visions, and moving from different premises, had been confronted with an important theoretical issue. When it became possible to state the problem in clear, logical terms, they reached the same analytical conclusions, though with inevitably different nuances, accentuation of details, or shades of emphasis.

10.5 The "Standard Commodity" Case

There is another notable case in which the two minds came to a clash. In this case, alas, they never succeeded in meeting, and remained wide apart from each other, in spite of the fact that the subject itself entailed advanced mathematical analysis, which by itself would have made one expect it to be a helpful feature in closing up the differences.

The case concerns one of the most cherished of Sraffa's brain children—what he called the *Standard commodity*, an entirely novel and original concept that he had coined.

Let me explain briefly the problem at stake in the simplest of all cases—the case of circulating capital only—which is sufficient for our purposes.

I shall be using what appears to me Sraffa's point of view, and a terminology as close as I can imagine to his own.

As we all know, the Sraffa price system contains two degrees of freedom, which can be closed by fixing, from outside, the price of any particular (single or composite) commodity as the *numeraire*, and the rate of profit at any point between zero and its technical maximum, R (which is yielded by the relation $R = \lambda_m^{-1} - 1$, where, λ_m is the maximum eigenvalue of the technical coefficient matrix). When we fix r from outside,[3] we in fact fix the distribution of income between the wage rate (w) and the rate of profit (r). The set of all such possible income distributions is in general expressed by a rather complex relation between w and r (a ratio of polynomials of degrees depending on the rank of the technical coefficient matrix).

Let us review this set of possibilities. Consider first the two extreme cases in which $r = 0$ or $r = R$, respectively. When $r = 0$, all prices (let us call them by vector $p(l)$) turn out to be proportional to the (direct and indirect) quantities of labor embodied in each commodity. We may say that prices, in this ($r = 0$) extreme case, embody a *pure labor theory of value*. Profits play no role in them. When $r = R$ (and thus $w = 0$), all prices (let us call them by vector p^*) turn out to be proportional to the current values of the capital goods. If we want to follow the same logic, we should say that, in this second ($r = R$) extreme case, prices embody a *pure capital theory of value*. Wages, and hence labor, play no role in determining them. Consider now all the other cases, in which $0 < r < R$. Prices will be determined *both* by wages on (direct and indirect) labor inputs *and* by profits on the value of capital goods inputs, and will vary in a very complicated (polynomial) way as r is hypothetically increased from the one extreme (zero) to the other extreme (R).

If we continue to use Sraffa's approach, we might at this point realize that Ricardo had discovered a very peculiar technical case in which the *pure labor* prices of the first extreme happen to coincide with the *pure capital* prices of the second extreme, which implies that they also coincide with the labor *cum* capital prices of all intermediate cases. This case (in our modern terms) is the very particular case in which the labor coefficients happen to coincide with the left-hand-side eigenvector of the technical coefficient matrix. The economic meaning of this case is that all production processes require the same proportion of labor to capital (in Marxian terms they happen to have the same "organic composition" of capital). In this peculiar technical case, the w/r relation, which in general is a ratio of complicated high degree polynomials, reduces to the, by now well-known, linear relation:

$$r = R(1 - w). \tag{10.1}$$

Relation (10.1) allows us to talk of the distribution of income between wages and profits in physical terms, and thus *independently of prices*. If 3/4 of the net output goes to labor, 1/4 goes to profits, and vice versa; and so on for any other complementary fractions of the net output.

It should be stressed that, in classical-like economic reasoning, the relevance of (10.1) derives—not from its linearity, but from the fact that it is independent of the system of prices. The left-hand-side eigenvector of the coefficient matrix ensures that all prices remain *invariant* in the whole range of the income distribution alternatives, from $r = 0$ to $r = R$.

What has Sraffa discovered? He has discovered that there exists a dual, symmetrical, case to the Ricardo case, which shares the same analytical properties. The right-hand-side eigenvector of the technical coefficient matrix defines a very particular composition of output—let us call it by vector y^*—which is precisely the one that Samuelson calls the von Neumann output composition, and which Sraffa's calls the *Standard commodity*. This particular commodity, if adopted as the *numeraire* of the price system (and we are always entitled to do this, since the choice of the *numeraire* is arbitrary), yields exactly the same linear relation (10.1) between w and r. Again it is important to repeat that the relevance of (10.1) is due not to its linearity, but to the fact that it is independent of prices—it makes all prices disappear from the w/r relation.[4] To underline this remarkable property, I shall mention that, if we were to change Sraffa's assumption on the timing of wage payment and suppose that wages are paid at the beginning (instead of being paid, at the end) of the production period, the w/r relation would be transformed from a line to a hyperbola,[5] but its basic property would persist: the w/r relation (no longer a line, but a hyperbola) would continue to remain entirely independent of prices.

Sraffa rightly points out that there is no need that wages should actually be paid with the physical quantities constituting the Standard commodity. The only requirement that is needed is that the Standard commodity is chosen as the *numeraire* of the price system. By the same token, there would be no need to pay wages, let us say, with "gold," if gold were to be chosen as the *numeraire*, since the choice of the *numeraire* is arbitrary. Sraffa goes on to propose to choose the Standard commodity as the *numeraire* of the price system. We would be able to take advantage of all its unique properties, among which is the one of yielding a w/r relation entirely independent of prices.

This is an extraordinarily beautiful analytical result. An added remarkable circumstance is that Sraffa reached it without using vector and matrix algebra and even without knowing the Perron–Frobenius theorems on nonnegative matrices; yet with an impeccable logical reasoning that has astonished mathematicians—as I have had the opportunity of realizing, for instance, through my personal contacts with Carlo Felice Manara.

Why hasn't such an analytically beautiful result impressed Paul Samuelson? It may well have at first; but further reflection must have led him to a quite different stance.

10.6 Samuelson's Difficulties with Sraffa's Cherished Brainchild

It took some time, at least to my knowledge, before Paul Samuelson explicity reacted to Sraffa's concept of Standard commodity. Apparently there was a sort of uneasiness on his part at first, which was then turned into open hostility. The singling out of the reasons for this attitude is the most intriguing puzzle that prompted the present investigation.

Samuelson's first open criticism of the Standard commodity is to be found in "Sraffian Economics" (Samuelson, 1987), published in the *New Palgrave Dictionary* four years after Sraffa's death.[6]

Samuelson opens his criticism with what appears to me a key proposition:

> for reasons not easy to understand, Sraffa thought that [(10.1)]'s truth somehow provided Ricardo with a defence of his labour theory of value. (p. 456).

I see three relevant points in this sentence:

(1) Samuelson acknowledges that there is something in Sraffa that he finds "not easy to understand";

(2) He does not attempt in the least to refute Sraffa's analytical achievement. Relation (10.1), yielded by using the Standard commodity as the *numeraire*, is "true."

(3) He attributes to Sraffa the intention of using the Standard commodity for a defense of Ricardo's labor theory of value.

Let me start with point (2). Samuelson acknowledges the analytical correctness of Sraffa's Standard commodity construction. But then, in his article, he proceeds to reject it outright on the ground that it is "useless." This claim is made even stronger and more emphatic in his later writings. "Uselessness," "irrelevance," "futility" of the Standard commodity are all terms that are repeatedly used, even in the titles of his articles' sections (see Samuelson, 1990, 2000b,c). This is an unusually severe verdict from Samuelson, especially if one thinks that he could not have corresponded with, and thus elicited any a reaction from, Sraffa. I find it disconcerting.

The objections that Samuelson gives to substantiate his claim are *not* of an analytical nature. They are all focused on what he calls the "restrictiveness" of Sraffa's concepts. He gives a long list—by mentioning, among others, aspects concerning the joint product cases, the nonproduced factors of production, the composition of market quantities deriving from intertemporal preferences, the availability of many techniques of production, etc. These objections do not point out anything analytically "wrong" in Sraffa's arguments. They all essentially concern—not just the (mathematically well-defined) concept of Standard commodity but—the whole of Sraffa's way of doing economics. It is significant that, to conclude the list, Samuelson should return to Ricardo:

> The purported defence of Ricardo's absolute standard has collapsed (Samuelson, 1987, p. 456)

So, we must infer that what Samuelson is really concerned with is Ricardo's labor theory of value—the above point (3). This is puzzling.

I myself have not been able to find in *PCmC* any place where Sraffa defends Ricardo's labor theory of value. Yet the clue to the puzzle must precisely be here. By carefully considering the use of the very term "labor theory of value," as used by Samuelson, one realizes that it does not fit into what Sraffa is doing with his *w/r* relation in terms of the Standard commodity.

It seems that Samuelson has in his mind the same *w/r* relation, but referred to Ricardo's particular case of homogeneous proportion of capital to labor in all production sectors. If this were so, it would not apply to Sraffa's case. The *w/r* relation yielded by the use of the Standard commodity as the *numeraire* has for Sraffa the crucial property of being independent of the price system. No more. It has nothing to do with the labor theory of value, except at the single, extreme, point $r = 0$. At all other points, in Sraffa's analysis, prices contain *both* wage *and* profit components, in varying proportions. Significantly, at the other extreme point, $r = R$, they contain only *profits* components, and thus they embody, as we have seen, a *pure capital* theory of value! If we were to persist in thinking in terms of a labor theory, we would fall into the same contradiction, which I had the occasion of attributing to both Ricardo and Marx, when I suggested that it was presumably such contradiction that induced them to adopt the obviously unrealistic assumption of a homogeneous technical composition of labor and capital in all sectors, since this assumption is the only one that can save them from that contradiction.[7]

Has Samuelson by any chance fallen into the same trap, from a symmetrical, dual, point of view? Since vector price p^*, associated with the net product composition y^*, which defines both von Neumann output composition and Sraffa's Standard commodity, embodies a *pure capital* theory of value, then—by extending Samuelson terminology to Sraffa's results—we should (incorrectly) say that the whole *w/r* relation, expressed in terms of the Standard commodity, is representing a generalized version of the *pure capital* theory of value! This seems unconceivable.

We are here putting our fingers on some terminological paradoxes that stand to indicate that the two minds cannot meet because they are following conceptions, and lines of thought, each of which does not fit into the other. They do not seem to talk the same language. Curiously enough, it is Samuelson (not Sraffa) that, in this case, has abandoned the mathematical language. If this is so, a clash (or rather a lack of understanding) is inevitable.

An explanation of this curious way in which the two opposite views are clashing seems to be revealed by the concluding sentence which Paul Samuelson states at the end of his arguments:

> MORAL. The Walrasian paradigms are in general unavoidable in the most unrestricted von Neumann paradigm (Samuelson, 1987, pp. 456–457) [which one must presume should also include the Sraffa paradigm]

But are they? At this point, one must go right back to point (1) at the beginning of the section. There is something in Sraffa which Samuelson finds "not easy to understand." This may well be so. But if the clash, in this case, is revealed to concern two incommensurably different "paradigms," two different ways of conceiving economics as a whole, what else could we expect?

10.7 Two Different Foundational Conceptions of Economics

Thomas Kuhn (1962, 2000), who invented the concept of "paradigm," in order to explain the discontinuous way in which sciences evolve (through *breaks* from one "paradigm" to another), relates many instances of lack of communication that have taken place when scientists have been trying to talk to each other across different paradigms. (The process may turn out to be even more complex, and not so linear in time, in the social sciences.) Just to mention one of them (which may be helpful for our purposes), let me recall the episode, in chemistry, that Kuhn describes, concerning

Joseph Priestley and Antoine Laurent Lavoisier. Through their experiments, they observed the appearance of the same new gas (oxygen) and yet, by moving within entirely different paradigms, they gave entirely different explanations—the one in terms of common air with less than the usual quantity of phlogiston, the other in terms of a gas of a distinctly new discovered species. Perhaps Priestly might even be entitled to claim priority in the discovery, and yet—immersed as he was in the prevailing phlogiston-based paradigm—was never able to accept the idea of the existence of the new gas—a total breakdown of possibility of communication. I have no intention to suggest, by quoting this episode, who could be prefigured by whom in our case. The only point I want to stress is the inherent lack of communication across paradigms.

In presenting his further developments, in *The Road since Structure* (2000), Kuhn has elaborated the problem further. When scientific arguments are based on different foundational conceptions, they face both methodological incommensurability (because each scientist takes the fundamentals of his/her paradigm for granted) and a semantic incommensurability, that terribly complicates the efforts of understanding each other. The methodological incommensurability has the effect, within each paradigm, to marginalize or even disregard the significance of the contradictions between theory and reality. The semantic incommensurability is even trickier, as it may lead scientists, moving within radically different paradigms, to use the very same words and phrases with entirely different meanings.

It does not require much effort to realize that what has been described in Sections 10.4–10.6 could be explained quite beautifully in Kuhn's terms. The basic results of the capital controversy have been accepted because they have been kept within an unambiguous logical–mathematical framework, and Samuelson must be given credit for achieving this. It must be admitted, at the same time, that ever since, all efforts on the neoclassical side have been in the direction of minimizing the results which Samuelson had acknowledged, so as to shift these results out of the protected belt of the neoclassical theoretical core. In practice, in today's mainstream economics textbooks, that controversy is simply not mentioned, and the underlying problems are simply ignored.

The misunderstandings on concepts and language that have emerged from the Standard commodity clash are more serious and at the same time more significant. They concern directly the two very different conceptual foundations of economics as they are intended by Samuelson (1947) and Sraffa (1960) respectively.

Two quotations may suffice, I hope, to explain clearly enough how far apart Samuelson and Sraffa stand in this respect. The first quotation comes from one of the most recent comments of Samuelson's. The other comes from early notes retrieved in the *Sraffa Papers*.

Here is Samuelson's:

> I strongly believe on the evidence, that Smith, Ricardo and J.S. Mill used essentially *the same logical paradigm* as did Walras and Arrow and Debreu [...]. Until missing papers surface in the Sraffa files with *new* devastating critiques of "marginalism," or until living Sraffians produce such new critiques not yet to be found in the literature, there will seem no need to qualify the first two sentences of this paragraph. [italics in original] (Samuelson, 2000b, p. 140)

Samuelson is (with justification) proud of the generalizations he carried out in his *Foundations*. The basic principle that he had proposed (expressed in the mathematical language of maximization under constraints) is here hinted at in bold terms, as a powerful all-inclusive unifying algorithm. Unlike what the early Marginalists had thought[8] Samuelson is stoutly—and no doubt justifiably—sure of his achievements. His *Foundations* are for him strong enough, as to be susceptible to bear extensions backward in the history of economic thought, so as to absorb classical (including Ricardian) economics, and at the same time as susceptible to support extensions forward, so as to absorb even the consequences of the "Keynesian Revolution" of the 1930s.

Quite sensibly, Samuelson can claim that the *onus* of a disproof falls on his opponents. Let Sraffians produce arguments, or even unpublished papers, if they can; let them come out into the open and state their case. From his strong, lofty standpoint, almost from a pinnacle-like altitude, Paul Samuelson is challengingly ready to stand back and listen, and only then, if necessary, to react. To many of his colleagues it appears almost unbelievable that he should consider (widely unknown) Sraffa as a possible challenger. That he should do so and take the challenge seriously is—to me—only a sign of his prodigious mind.

But let me come to the second excerpt, which I want to quote. It comes indeed from Sraffa's unpublished papers.[9] It dates back to the late 1920s, or at most to the very early 1930s, at a time when Sraffa had begun to consider resignation from his Cambridge lectureship (from which he then actually did resign), out of disillusionment with the then current state of economic theory:

> It is terrific to contemplate the abysmal gulf of incomprehension that has opened itself between us and the classical economists. Only one century separates us from

them: [then the following sentence, here reproduced in italics, is added as a footnote] *I say a century; but even 1/2 a century after, in 1870, they did not understand it. And during the preceding century an obscure process of "disunderstanding" had been going on.* How can we imagine to understand the Greeks and the Romans? [then the following sentence, again here reproduced in italics, is added as a footnote] *Or rather, the extraordinary thing is that we do understand, since we find them perfect: Roman law and Greek philosophy.* The classical economists said things which were perfectly true, even according to our standards of truth: they expressed them very clearly, in terse and unambiguous language, as is proved by the fact that they perfectly understood each other. We don't understand a word of what they said: has their language been lost? Obviously not, as the English of Adam Smith is what people talk today in this country. What has happened then?

This was written, as we may now realize, three decades before Kuhn (1962) could have given us some clues in justifying Sraffa's troubling reflections.

10.8 Perspectives for Economic Theory in the Twenty-First Century

The twenty-first century has opened with the legacy of a "Samuelsonian economics" at the apex of its success. "Sraffian economics" is striving for survival in the economics dictionaries. The former dominates the valleys of many social sciences—not only economics. The latter is a niche territory fighting for existence in—not even all—University economics departments and in some learned journals.

In the last fifty years, the Samuelson way of doing economics has become orthodoxy. If nowadays one finds the term "Samuelsonian economics" unusual, this is only because, by and large, it has become a synonym of mainstream economics. Yet we know how attentive and perceptive (no matter how critical) Samuelson has been to the claims of alternatives.

Sraffian economics started less than fifty years ago, with a *prelude to a critique* of orthodoxy. On the critical front it has left a mark, as Samuelson has acknowledged. But Sraffa seems to have offered us—not only a critique, but also a *seed* of a theory that revives the classical method and looks at the history of economic thought in terms of discontinuity. Admittedly, Sraffa's theory—as we have it at present—is far from being a complete theory. In *Production of Commodities by means of Commodities* many blocks are missing or are just sketched out or hinted at. (Samuelson has called it a "one leg theory," owing to its avoidance of the demand side aspects.) But the laid foundations have proven solid.

Our investigation has, I think, unveiled something more. Why has Samuelson been so interested in Sraffa? I think a better answer can be given at the end of this chapter than one could have imagined at the beginning. Samuelson's stated desire "of formulating a general theory of economic theories" (Samuelson, 1947 [1983, p. xxvi]) would seem to imply the absorption and inclusion also of Sraffa. But what has been reviewed above gives us several reasons to say that, despite Samuelson's skills, this attempt has not succeeded in the past and may prove impossible even in the future. "Samuelsonian economics" and "Sraffian economics" are running, as we have seen, on two different tracks. They start from different roots and look at the world from two different perspectives. "Samuelsonian economics" seems to me the latest ring in the chain of evolution of what elsewhere (Pasinetti, 1986) I had the opportunity of calling an "exchange paradigm." Sraffian economics belongs to what I called a "production paradigm." The reference point of the former is a world of pure markets. The reference point of the latter is, more fundamentally, a world of pure production.

What can the future of these two paradigms be? Events will of course tell. But my conviction is that Sraffa's theory has future contributions in reserve. The economies in which we live at present are "production economies" and they are likely to remain production economies, at a global level, for some time to come. The Classical School made its mark just after the First Industrial Revolution. We seem to be under the effects, on a larger scale, of a more acute expression of the same revolution, with consequences not dissimilar from, and in fact deeper than, those of two centuries ago. This may offer an opportunity—an open window we may say—for an originally framed resumption and revival of concepts typical of classical economics. True, the course of economic history will not be sufficient by itself to induce forsaking "Samuelsonian economics," but it might well teach us to use it in a different way. To supercede a theory, one necessarily needs an alternative theory. The one proposed by Sraffa, though solid on its bases, does not yet show all the legs necessary to run on its own. The crucial question therefore is to see to what extent *Production of Commodities by means of Commodities* may be armed with those parts it is still lacking. The economic profession should not be disappointed if a new paradigm, openly alternative to the present mainstream one, is allowed to be built and to be developed. I am sure Paul Samuelson would be the last economist not to accept the proposition that, also for economic theory, healthy competition is bound to be superior to a situation of monopoly.

Notes

1. A dozen of letters or so may be found in the *Sraffa Papers* in the Wren Library of Trinity College Cambridge and in the manuscripts Library of the Mattioli Foundation in Milan.
2. Quoted in the biographical presentation of his Nobel Prize lecture (Samuelson, 1970a, 1970b).
3. An alternative procedure would of course be to fix the wage rate, instead of the rate of profit, from outside. I leave this alternative aside to keep near Sraffa's arguments.
4. An interesting and elegant way of showing how all prices cancel out in, and disappear from, the *w/r* relation, when the Standard commodity is used as the *numeraire*, has recently been presented by Bellino (2004).
5. I may refer for convenience to Pasinetti (1977), pp. 78–80.
6. There are hints that Samuelson (perhaps provisionally) tried ways to deal with the problem much earlier, but in seminars (see Burmeister, 1968, 1984).
7. See Pasinetti (1977), pp. 78–80.
8. Let me recall that, at the dawn of Marginal economics (in the 1870s), the early Marginalists, with William Jevons (1871) in the fore-front, stressed the deep break between Marginalism and the earlier (especially Ricardian) economics. Samuelson clearly thinks that, with his method, he has overcome this split.
9. I have had already the opportunity of reproducing this excerpt in Pasinetti (2003), p. 153. I renew my thanks to Sraffa's literary executor (Pierangelo Garegmani) for allowing me to reproduce it here.

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11

Paul Samuelson as a "Keynesian" Economist

L.R. Klein

11.1 The Meaning of a Keynesian Economist

To brand or label someone as a Keynesian economist or to define a school of thought or a branch of economic analysis as Keynesian is not very constructive, from my point of view. There are groupings such as New or Neo Keynesian, Liberal, Progressive, or various adjectives connected to the thinking of John Maynard Keynes, who was, in my opinion, the most significant economist of the first half of the twentieth century. Paul Anthony Samuelson is unquestionably my choice as the most significant economist of the second half of the twentieth century. Since Keynes died prior to 1950 and Samuelson began his professional career before 1950, the split between the two half century periods is not precise; perhaps prewar and postwar (Second World War) would be a better designation.

In the course of this essay, I am going to come to the concept of the *Neoclassical Keynesian Synthesis*, which I believe provides a more accurate designation for the role and influence of Paul Samuelson. Though I have found in practice that many economists look upon this longer classification as clumsy and not sufficiently decisive, I believe that it is much more inclusive of Paul Samuelson's achievements in the economics profession.

There is also an important distinction to be made between economic analysis and economic policy (also known as political economy, applied economics, or just plain *politics*). Media writers, in particular, are very liberal in treating Keynesian economics (or economists) as advocating (advocates of) public spending to promote production and jobs. This way of looking at Keynesian economics was, at the beginning of use of the concept, known as depression economics or, more broadly, economics of state intervention. The latter concept would cover anti-inflation policy as well as anti-recession policy.

11.2 The Origins of Keynesian Economics

The economic turbulence of the period after the First World War laid the foundation for new thinking about how to bring afflicted countries out of the grip of the Great Depression that followed the crises in financial markets after the 1929 Crash. There were two seemingly unrelated developments. One dealt with the rising tide of mathematics and, more generally, quantitative economics, particularly in the form of econometrics, which was meant to unify economic theory, mathematical methods of analysis (mathematical economics), and statistical methods of analysis of economic information (econometrics). These developments occurred mainly in North America and Europe, including of course the United Kingdom and Ireland as well as continental Europe.

Somewhat later than the mathematical developments, Keynesian economics grew out of the informal groupings inspired by Keynes in Cambridge, England. This became known as Keynes's *Circus*, and culminated in the publication of the *General Theory of Interest, Employment and Money* in 1936.

In the United States, at Harvard University in Cambridge, Massachusetts, there was a study group that included Paul Samuelson, other advanced students of economics, and some members of Harvard's faculty. The American group members were analyzing the deep meaning and potential importance of the ideas emanating from the Circus, and some visiting participants from the group in England, notably a Canadian economist, Robert Bryce, who prepared a set of notes about the discussions that were taking place in Cambridge, England.

A remarkable feature of the activities of these two study groups is how much detail of the thinking of the Circus group in England was known to the Harvard group. When I went to Cambridge, England for a short visit in summer, 1948, I met Richard Kahn, Joan Robinson, Piero Sraffa and other participants from the Circus, all of whom marveled at the precise detail that Paul Samuelson knew about their thinking in the decade of the 1930s, while the *General Theory* was being formulated. They said to me that Paul had the discussions in Cambridge, England, just right, without being there in person.

As an undergraduate student in California between 1938 and 1942, I had only one significant exposure to Keynesian economics. That was in the

1940–41 academic year when Wassily Leontief visited for an evening seminar. I was struck by the amount of time he devoted to Keynesian economics in his presentation dealing with contemporary economic issues. He was surely influenced by the Harvard discussion, although he would not be considered as a Keynesian economist, for their mode of analysis was too aggregative.

11.3 Paul Samuelson, an Architect of Modern Mathematical Economics

From my perspective, the development of Keynesian economics is closely related to the parallel development of mathematical methods in economics, both theoretical and statistical. In fact, my going to MIT for their new graduate program in economics, introduced in the academic year 1941–42, was motivated by an interest in the general issue of formulating economic analysis in mathematical terms. On browsing through some issues of *Econometrica* I was genuinely stimulated by some recent articles of Paul Samuelson. They did not deal with Keynesian economics, but with subjects that eventually appeared in *Foundations of Economic Analysis*. In terms of the present essay this material fits well with the case for classifying Paul Samuelson as an exponent of the *Neoclassical Keynesian Synthesis*. My first substantive experience with Paul ties together his work in general economic analysis and Keynesian economics.

The economics program at MIT was only one year old, when I arrived, and there were approximately ten students for the 1942–43 academic year. Each student was assigned to a faculty member for research guidance, and I had the extremely good fortune to be assigned to Professor Samuelson. He immediately suggested a small research investigation concerning the statistical estimation of savings and investment functions for the United States, in particular, because he was dissatisfied with the methodology used in a recently published piece in the *American Economic Review* by Mordecai Ezekiel. His insight into the econometrics of that paper as a contribution to Keynesian economic analysis was very perceptive. His reasoning was as follows: In early econometric studies of supply and demand functions in economics, generally, but in agriculture, in particular, because Dr Ezekiel was a statistician in the US Department of Agriculture, Paul presumably knew the *identification* issues from his contact with Henry Schultz at the University of Chicago.

The typical or classical problem in the early days of econometrics had been to identify, separately, estimates of demand and/or supply functions

$$q_t = S(p_t) + e_t$$

$$q_t = D(p_t) + u_t$$

where S denotes a supply function, while D denotes a demand function, on the basis of regressions between q_t (quantity at time t) and p_t (price at time t). Random errors are denoted by e_t and u_t. There is not enough information given here to distinguish the estimated supply function, especially in linear form, from the estimated demand function, also in linear form, without there being what econometricians now call *identifying* restrictions on the two equations. This problem is well understood now but was not clearly and generally formulated in published econometric theory at that time.

Mordecai Ezekiel considered the macroeconomic relation, in Keynesian theory, between savings and investments as

$$S_t = S(Y_t, \Delta Y_t, t) + e_t$$

$$I_t = I(Y_t, \Delta Y_t, t) + u_t$$

$$S_t = I_t$$

The identification issues with respect to this problem are practically the same as those for the agricultura1 supply-demand relationships being examined by agricultural economists.

Dr Ezekiel argued that by disaggregating I_t into many subcategories, the aggregate saving and investment functions could be identified. I was assigned the research task of determining whether Dr Ezekiel had satisfactorily dealt with the same problem that the agricultural statisticians had to confront. They had the vagaries of nature (climate, pestilence, use of fertilizer, use of insecticide, etc.) to put in the supply functions as additional variables or to cause large variances of supply function error (e_t) and obtain identification that way, by information on relative error variances. Dr Ezekiel estimated a single relationship between savings and income, but a four-fold decomposition of investment into different types with separate effects coming from ΔY_t and t in each case. Thus by disaggregation of investment he hoped to be able to estimate an investment function from the sum of separate subinvestment equations. Paul Samuelson could see this approach as a possible violation of identification principles. Over the years of subsequent savings studies, trend and income dynamics have also been used in savings functions. Econometricians later pointed out that disaggregation, as in the case of Dr Ezekiel's specification of investment was not, by itself, a satisfactory route for achieving identification. Paul Samuelson's original insight was that "the savings-investment cross" from Keynesian economics posed the same identification problem as "the supply-demand cross" from agricultural economics.

11.4 Keynes the Investor and Samuelson the Investor

There have been many stories about Keynes's judgment and foresight in amassing wealth, during his lifetime. Some of the stories focus on Keynes's failures in speculation; others feature his insight into the value of art, in particular, that associated with well-known painters, often of ballet scenes. It is undoubtedly the case that Keynes *earned* and *lost* in his speculative investments, but he died a wealthy person, with a country estate, the founding of an art theater in Cambridge and the strong institutional portfolio following his sage investment advice to King's College.

In his famous textbook, *Economics*, Paul Samuelson has this to say about Keynes's ability as an investor: "He was also an economist who knew how to make money, both for himself and for King's College Cambridge." In spite of some point of disagreement by biographers with Keynes's early speculation and comments on investment methodology, there is no doubt that Paul Samuelson appreciated the end result.

It is evident that the royalties from the publication of so many editions of *Economics*, coupled with the explicit admiration for Keynes as an investor, may have influenced Paul Samuelson to follow Keynes's footsteps. Paul Samuelson, however, respects the insight and techniques of modern mathematics and econometrics of investment, as well as the sage investment strategy of Warren Buffet and his devotion to Graham's principles of investing that seem to be far removed from some of the speculative ventures of Keynes. Roy Harrod notes in his biographical volume on Keynes that the greatest contribution to Keynes's very significant portfolio at the time of his death came from investment in American utilities during the Great Depression. This fact suggests that Keynes was a very wise value-investor who recognized fine bargains at times when the financial markets were in a great slump. It suggests that Keynes was, in fact, a very astute investor for the medium to long run. There can be no doubt that Paul Samuelson emulated this aspect of Keynes's life. After having served on the Finance Committee of the National Academy of Sciences for almost three decades with Paul, I can fully attest to his investment insight, not by the methodology of Keynes, but by the successful attention to investment principles. In many aspects, the above quotation from *Economics* about Keynes as an investor would be eminently suitable for Paul A. Samuelson's biographer.

11.5 Antecedents and Cranks in the Keynesian Spirit

Keynes refers explicitly to Gesell, Malthus, and Hobson in the *General Theory*. These are all early thinkers who had some ideas about problems with the prevailing economic system and either particular policies for making it perform better, or warnings about approaches of the economy to conditions of basic conflicts or incompatibilities.

For example, Malthus feared that population growth would overtake the world's ability to provide sustenance. In more than two centuries, Malthusian warnings have not been heeded, nor has widespread world hunger appeared, although there have been some special, but limited, examples.

Paul Samuelson, at times, has shown some interest in the writings of people who anticipated some aspect of a Keynesian system of thought, without bringing the issues to the point of self-contained systems that would be able to encounter an episode as serious as that of the Great Depression, which clearly brought Keynesian thinking to the fore.

There are, however, two important academic economists who played very important roles in the founding of the Econometric Society or figured in early meetings of the Society in Europe. They are well recognized in other facets of Paul Samuelson's view of economics in the time of the rising Keynesian tide, but they do not figure significantly in his analysis of Keynesian economics, as do Gesell, Malthus, Foster and Catchings, and Hobson. My recollections of Professor Samuelson's classroom macroeconomics, or suggestions in the role of dissertation supervisor, is that one should look for grains of truth in their writings.

There are, however, deeper analytical reasons for mentioning the work of Frisch and Kalecki in the context of anticipating Keynes. The two cases are quite different. Ragnar Frisch was a key figure in the formation of the Econometric Society, at the time when Keynes was developing the *General Theory*, but also during this period Frisch was making policy suggestions to the Norwegian public on reasons for the unusual economic collapse and on ways of emerging toward recovery. On the occasion of the 100th anniversary of Frisch's birth in 1895, there was a series of lectures and discussions about his work, and at a dinner (hosted by Trygve Haavelmo), I was asked to address the audience. I took up the subject of Frisch's policy advice, to a Norwegian radio audience in 1932, in which he explained to the public the need to encourage consumer spending, completely in the Keynesian fashion. The significant point about these lectures is that they spelled out clearly the macroeconomic reasoning of the "paradox of thrift," namely if the society tries to save (not spend) more, they are likely to end up by saving less. This paradox can be illustrated simply by analysis of the savings-investment cross, which of course Paul Samuelson knows so well. The only issue is that Frisch did not take his message in English, beyond the scope of Scandinavian listeners.

Kalecki's case is more complex, but in a short period of time he, by himself, developed a small macro model that was capable of generating cycles and covering the same ground as the early Keynesian models. His system consisted of a consumption equation, an investment-orders equation, and a velocity-interest rate equation. With slight variations, these are practically the same as the Hicks and Lange representations of the Keynesian system. The works of Frisch and Kalecki were developed independently of the discussions in the Keynesian Circus, but they arose in the same time period and when both econometricians were prominent in the early years of the Econometric Society, while Paul Samuelson was framing his own version of the Neoclassical Keynesian synthesis.

11.6 The Existence of An Unemployment Equilibrium

A major theoretical inquiry of the new Keynesian doctrines being discussed in the two Cambridges was whether the system being proposed by Keynes could have an equilibrium position at less-than-full-employment. This is one of the first analytical problems of macroeconomic analysis that I took up when I started work on my dissertation, *The Keynesian Revolution*. The debate with Mordecai Ezekiel on identification, in connection with the savings-investment cross was mainly an issue of identification, as that subject was then being refined by Trygve Haavelmo and, later, econometricians at the Cowles Commission.

As I was speaking, from time to time, with Professor Samuelson about a fresh topic for my dissertation, he suggested that I consider a thesis on "The Keynesian Revolution." When he suggested that, I found a close fit with my own interests and immediately took it up since it was a subject that had a great appeal to me. Early in my thesis research, I was asked to make a seminar presentation before an economic study group at Harvard.

I chose the Keynesian model that was shaping in my thinking and ran into a dispute at the Harvard session whether one would need to specify an unusual nonlinear labor supply function or an inhomogeneous function in which labor supply depended on the nominal wage rate rather than the real wage rate, in order to have unemployment in equilibrium.

On the morning-after, Professor Samuelson inquired about the course of discussion at the seminar. When I told him about the issues of labor supply specification, he immediately suggested that maybe the long-run equilibrium point of the final system, reduced, after substitution, into two equations depending on two variables would have a logical intersection point only in an invalid quadrant—one where the real wage or some other positive variable would have to be negative. He then said it would be impossible to get the economy to that point, but in the process of trying to do so, there would be unstable deflationary movements with wages being competitively bid downward. In terms of the IS–LM diagram, the curves would be shifted through a search for an equilibrium solution that exists only in a quadrant that permits negative interest rates. In *The Keynesian Revolution*, this situation was depicted graphically as shown in Figure 11.1.

Later, in continuing discussions at the Cowles Commission, among Don Patinkin, Trygve Haavelmo, and myself, Trygve suggested that the Keynesian model be specified as one that always had a valid solution, in which negative wages or interest rates would not be present, as long as the system was in motion, in a dynamic sense, but when one imposed equilibrium conditions which included full employment a solution would not exist. Personally, I find that explanation attractive. There is yet another approach to this problem, through the Pigou effect. Pigou introduced another variable, the real stock of cash balances, in the savings equation,

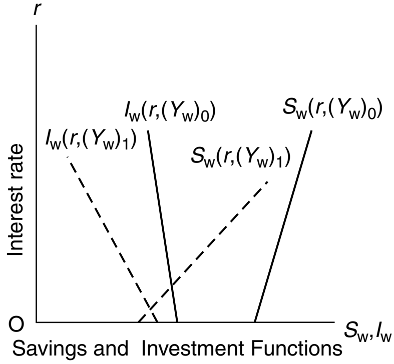

Figure 11.1 Saving and Investment functions

essentially as an indicator of real wealth, in addition to real income. In such a modification of the Keynesian system, flexible wage bargaining—as long as employment is not full—would expand the value of real wealth with falling wage rates until full-employment equilibrium is found.

The Pigou effect, putting real cash balances in the saving or consumption function became an important policy issue immediately after the Second World War because households in the United States had accumulated large reserves of government bonds, a popular type being Series E, purchased in units costing $18.75 and maturing in ten years at $25.00 to yield 2.9 percent. Paul Samuelson remarked on many occasions that US workers could be nicely rewarded for being patriotic in financial support of the war effort, receiving a very secure investment at 2.9 percent interest. For many people, the process was made easy, through payroll deduction plans.

There were limited goods to purchase with the savings; so the war ended with many households in possession of a significant stock of liquid assets, by adding the values of Series E bonds to ordinary cash balances. An immediate question arose, would American consumers draw upon their liquid assets for spending on goods that were not available during the war? Statistical correlations with both disposable personal income and liquid assets (as enlarged cash balances) could motivate consumer spending beyond the amounts that were being estimated by conventional Keynesian consumption functions. The answer was not clear, especially because Series E bonds did not have a long history in a statistical sample. Most calculations indicated existence of some positive effect, but not with a high degree of statistical significance. There undoubtedly was some positive effect, and I can recall inconclusive discussions with Professor Samuelson about the importance of Pigou's article soon after it was published, but before peace settlements and demobilization had begun.

11.7 Keynesian Policy

In 1944, I went to meetings of the Econometric Society which were just being reconvened after postponement during the war. I was asked to present a paper dealing with my discussion of the approach by Mordecai Ezekiel on the savings-investment cross and my dissertation work on *The Keynesian Revolution*. I was deeply interested in the program listing of a paper entitled "Will There Be Business Cycles after the War?" Unfortunately that paper was cancelled, but Jacob Marschak, after inquiring about professor Samuelson and his latest professional activities, said to me, "What this country needs is a new Tinbergen model to forecast the performance of the American economy after the War." This remark excited me, and I was more than pleased to consider his offer of my coming to the Cowles Commission to take up this task.

These events have relevance for another side of Paul Samuelson's views as a "Keynesian" economist. Keynes was both an academic economist and a popular economist who contributed often to the media, one source being the *New Statesman*. In 1944, when the issues surrounding the end of the war were being intensively discussed, Paul Samuelson contributed two articles to the *New Republic* in the United States.

11.8 Unemployment Ahead

I. A Warning to the Washington Expert
September 11, 1944, pp. 297–99

II. The Coming Economic Crisis
September 18, 1944, pp. 333–35

The predominant view at the Econometric Society meetings was that the United States would face the problem of a weak civilian economy after the war. Very elementary Keynesian models, emphasizing the consumption function, were presented at the professional meetings. In Europe some economists, particularly in Sweden, suggested a new orientation to the East instead of the West because they feared that the United States would not have a robust economy after the stimulus for production on a war footing was taken away.

In the *New Republic*, Paul Samuelson did not make such a judgment. He was much more cautious about the uncertainty of the pessimistic forecasts. He concluded "the government under any party will have to take extensive action in the years ahead." The emphasis was on his graph of the enormous ratios of federal government war expenditure to nonwar expenditures, and his reaction was to remark on "the startlingly large proportion of the former."

How did things work out after 1945? There was no large increase in unemployment. During that year, after our preliminary calculations with the first version of the Tinbergen-type model at the Cowles Commission (on request from the Committee for Economic Development), my position was completely changed to one of no immediate serious recession. In office after office in Washington, economic analysts, such as those anonymously

referred to in Paul Samuelson's *New Republic* articles, responded to the calculations from the Cowles Commission, "Just wait until mid 1946; there will be 6 million unemployed." A better response would have been about 2 million (Table 11.1).
As the military sector expanded, employment became "overfull," but the reconversion was gradual.

This brings me to Paul Samuelson's view of Jan Tinbergen, whose 100th anniversary of birth was memorialized two years ago in Rotterdam, and for which Paul Samuelson sent a fond statement of admiration and achievement. In the first place, Tinbergen received unfortunate review commentary from Keynes for the work at the League of Nations in constructing an econometric model of the United States during the interwar period. Tinbergen told me a few years earlier that he genuinely admired Keynes and was simply trying to find empirical support for the abstract model of the *General Theory*. Professor E. B. Wilson, a distinguished professor at Harvard and MIT, who was admired by Paul Samuelson, told me and other MIT students who attended Wilson's lectures at Harvard in 1942, that Keynes should be regarded in the role of "the theoretical physicist," while Tinbergen filled the role of "the experimental physicist." A careful examination of Tinbergen's US model reveals that he made two improvements over the conventional Keynesian consumption function: (1) He paid explicit attention to the distribution of incomes. (2) He generalized the Pigou effect by allowing for the influence of wealth in the form of equities. He introduced the rate of change of stock market prices in an equation for the rate of change of consumption. In a sense he led Keynesian scholars in one step toward generalizing the Pigou effect to a broader wealth effect. A combination of wealth and government spending for demobilized military (the GI Bill—investing in human capital) and support for Europe, Korea, and Japan turned the end of the Second World War into an immediate gain,

Table 11.1 Forces to be Demobilized and Unemployment

	Armed forces (million)	Unemployment (million)
1942	4.0	2.7
1943	8.9	1.1
1944	11.4	0.7
1945	11.6	1.0
1946	3.8	2.3
1947	1.7	2.4
1948	1.5	2.3

Source: Statistical Abstract of the United States plus a longer sustained gain for the US economy. Also the new technological spin-offs of the military research (radar, jet engines, large, high speed computers, etc.) paved the way for technical change, which is not at all incompatible with Keynesian economics, but not emphasized. Once an explicit production function is introduced, with allowance for ample technical change, the system can go far toward enhancing Keynesian analysis of aggregate demand with aggregate supply and emphasizing the Neoclassical Keynesian Synthesis.

11.9 The Early Postwar Period

A leading expositor and analyst of Keynesian economics in the United States was the Harvard economist, Alvin H Hansen, and his large coterie of students and colleagues contributed to a Festschrift entitled *Income, Employment, and Public Policy, Essays in Honor of Alvin H Hansen*. (W W Norton Inc., 1948). A leading contributor was his student, Paul Samuelson. In an article in the volume on "The Simple Mathematics of Income Determination" one can readily see the deep meaning of describing Paul Samuelson as a Keynesian economist. He wrote that the heart of income determination was the simple equation that stated: for a given level of investment I, the simple equation $Y = C(Y) + I$, which Paul Samuelson described as "the nucleus of the Keynesian reasoning." He compared this simple equation, which is basic to the Keynesian system, with $D(p) = S(p)$, the "equating of supply and demand to determine market price." For me, it is a clear reminder of his concern over Mordecai Ezekiel's analysis of estimation of savings and investment equations as functions of income, to determine Y_i.

The chapter in the volume honoring Hansen deals very much with such things as various multiplier concepts and formulas, the balanced-budget theorem, to show how stimulus for an underperforming economy can be realized even though deficit spending is not involved. In addition, Paul Samuelson's leaning was toward fiscal policy of public spending instead of tax reduction for moving a weak economy toward stronger performance. As for international trade, he wrote that this component of total production and demand "will be implicitly rather than explicitly in the income system." In the present globalized world economy, conditions have changed so much that full explicit treatment of international trade is of top priority in many or most economies.

If this chapter in honor of Alvin Hansen shows clearly the Keynesian side of Paul Samuelson's macro policy analysis, one can find other clear expressions of neoclassical reasoning, especially in his admiration for the work of Keynes's student, at an earlier time than the Circus discussions, Frank Ramsey. While Paul Samuelson thought highly of Keynes, he simultaneously was keen on the neoclassical reasoning of medium-to-long-term growth studied by Ramsey. On the occasion of the World Congress of the Econometric Society in Cambridge, England in 1970, Paul gave a lecture, billed as an examination of the work of an important Cambridge economist. There was a guessing game taking place about who was this person. It turned out to be Frank Ramsey. He was a student of Keynes, but not for Keynesian economic analysis as we know it today (or even then).

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12

Samuelson and the Keynes/ Post Keynesian Revolution

Paul Davidson

For most students who studied economics in any American university during the last half of the twentieth century, Paul A. Samuelson was thought to be a direct disciple of Keynes and his revolutionary general theory analysis. Samuelson is usually considered the founder of the American Keynesian school which he labeled neoclassical synthesis Keynesianism because of the classical microeconomic theory that Samuelson believed was the foundation of Keynes's macro analysis. As we will explain, Samuelson's neoclassical synthesis brand of "Keynesianism" was not analytically compatible with the theoretical framework laid out by Keynes in *The General Theory of Employment Interest and Money* (1936).

Explaining the differences between Samuelson's version of Keynesianism and Keynes's *General Theory* is the essence of this chapter. Given Samuelson's dominance of the American macroeconomic scene after the Second World War, the analytical different foundation of Samuelson's Keynesianism vis-a-vis Keynes's *General Theory* aborted Keynes's truly revolutionary analysis from being adopted as mainstream macro economics. Consequently in the 1970s academic literature, the Monetarists easily defeated the Samuelson's neoclassical synthesis Keynesianism on the grounds of logical inconsistency between its microfoundations and its macroeconomic analysis and policy prescriptions. The effect was to change the domestic and international choice of policies deemed socially acceptable to prevent unemployment, to promote economic development, and even the method to finance government social security systems away from prescriptions founded on Keynes's *General Theory* and to the age-old laissez-faire policies promoted by classical theory that had dominated nineteenth and early twentieth century thought.

As a result of the Monetarist victory over Samuelson's neoclassical Keynesianism in the 1970s, New Keynesian theory was developed to replace Samuelson's Keynesianism. Just as Friedman's Monetarism had conquered Samuelson's brand of Keynesianism, New Classical theory easily made a mockery of the New Keynesians approach which relied on the rigidity of wages and prices to achieve Keynesian-like results. New Classicists argued that price and wage rigidity was associated with government interference in the competitive market place. The result was to lead policy makers to dance to the Panglossian siren song that "all is for the best in the best of all possible worlds provided we let well enough alone" by encouraging adoption of policies of liberalizing all markets.

Accordingly, as we entered the twenty-first century, only the Post Keynesians remain to carry-on in Keynes's analytical footsteps and develop Keynes's theory and policy prescriptions for a 21st century real world of economic globalization.

12.1 The Coming of Keynesianism to America

In their wonderful book *The Coming of Keynesianism to America*, Colander and Landreth (1996, p. 23) credit Paul Samuelson with saving the textbook pedagogical basis of the Keynesian Revolution from destruction by the anti-communist spirit (McCarthyism) that ravaged America in the years immediately following the Second World War.

Lori Tarshis, a Canadian who had been a student attending Keynes's lectures at Cambridge during the early 1930s had, in 1947, written an introductory textbook that incorporated Tarshis's lecture notes interpretation of Keynes's *General Theory*. Colander and Landreth note that despite the initial popularity of the Tarshis textbook, its sales declined rapidly as it was attacked, by trustees of and donors to American colleges and universities, as preaching an economic heresy. The frenzy about Tarshis's textbook reached a pinnacle when William Buckley, in his book *God and Man at Yale* (1951), attacked the Tarshis analysis as communist inspired.

In August 1986 Colander and Landreth (hereafter C–L) interviewed Paul Samuelson, (C–L, 1996, pp. 145–178) about his becoming an economist and a "Keynesian". Samuelson indicated that he recognized the "virulence of the attack on Tarshis" and so he wrote his textbook "carefully and lawyer like" (C–L, 1996, p. 172). The term "neoclassical synthesis Keynesianism" did not appear in the first edition of Samuelson's textbook, *Economics An Introductory Analysis* (1948), which was published after the attack on

Tarshis's text. This neoclassical synthesis terminology, however, does appear prominently in the later editions of Samuelson's textbook. From hindsight it would appear that Samuelson's assertion that his brand of Keynesian macroeconomics is synthesized with (and based on) traditional neoclassical microeconomic assumptions made the Samuelson version of Keynesianism less open to attacks of bringing economic heresy into University courses on economics compared to Tarshis's Keynesian analysis.

Unlike Tarshis's analysis which was based on separate aggregate supply and demand functions, the analytical foundation of Samuelson's Keynesianism was imbedded in Samuelson's 45 degree Keynesian cross. Samuelson derived this cross analysis from a single equation aggregate demand function. This mathematical derivation in conjunction with the claimed synthesis of neoclassical theory made it more difficult to attack the Samuelson version of textbook Keynesianism as politically motivated. Thus for several generations of economists educated after the Second World War, Samuelson's name was synonymous with Keynesian theory as various editions of Samuelson's neoclassical Keynesian textbook was a best seller for almost a half century. Even those younger economists who broke with the old neoclassical synthesis Keynesianism and developed their own branch of New Keynesianism based their analytical approach on the Samuelson's *Foundation of Economic Analysis* (1947) and its classical microeconomic foundations.

From an historical perspective it appears to me that Samuelson may have saved the textbook pedagogical basis of the Keynesian Revolution from McCarthyism destruction simply by ignoring the axiomatic foundation of Keynes's analytic revolution.

12.2 How did Samuelson Learn Keynes's Theory?

In his 1986 interview Samuelson indicated that in the period before the Second World War, "my friends who were not economists regarded me as very conservative" (C–L, 1996, p. 154). Samuelson graduated the University of Chicago in June 1935 and, as he explained to Colander and Landreth, were it not for the Social Science Research Council fellowship that he received upon graduation, he would have done his graduate studies at the University of Chicago (C–L, 1996, pp. 154–155). Consequently, it was the visible hand of a fellowship offer that placed Samuelson at Harvard when Keynes's *General Theory* was published in 1936. What information about Keynes's *General Theory* was Samuelson exposed to at Harvard?

Robert Bryce, a Canadian, had attended the same Keynes Cambridge lectures as Tarshis between 1932 and 1935. In a 1987 interview with Colander and Landreth (1996, pp. 39–48) Bryce indicated that in the spring of 1935 he (Bryce) spent his weeks at the London School of Economics and Cambridge. At LSE Bryce used his Cambridge lecture notes to write an essay on Keynes's revolutionary ideas—without having read *The General Theory*—for the people at the LSE. This essay so impressed Hayek that Hayek let Bryce have four consecutive weeks of Hayek's seminar to explain Keynes's ideas as Bryce had written them out in this essay. Bryce's lectures were a huge success at the LSE (C–L, 1996, p. 43).

In the fall of 1935 Bryce went to Harvard and stayed for two years. During that time, an informal group met during the evenings to discuss Keynes's book. Bryce, using the same pre-General Theory essay that he had used as the basis for his talks at the LSE, presented to this group what he believed was Keynes's *General Theory* analysis—although he still had not read the *General Theory*. As Bryce put it "In most of the first academic year (1935–36) I was the only one who was familiar enough with it [Keynes' theory] to be willing to argue in defense of it" (C–L, 1996, pp. 45–46). So in 1936 Bryce's essay became the basis of what most economists at Harvard, probably including Samuelson, thought was Keynes's analysis—even though Bryce had not read *the* book when he made his presentations. Even in 1987, Bryce stated that, "anyone who studies that book is going to get very confused. It was . . . a difficult, provocative book" (C–L, 1996, pp. 44–46). The immediate question therefore is: "Did Bryce ever really comprehend the basis of Keynes's analytical framework?" And if he did not, how did that affect how the young Samuelson and others at Harvard in 1936 learn about Keynes's analytical framework.

Bryce's presentations at the LSE and Harvard were supposed to make Keynes's ideas readily understandable—something that Bryce believed Keynes could not do in his *General Theory* book. Bryce indicated that in his first year at Harvard "I felt like the only expert on Keynes's work around" (C–L, 1996, p. 45).

Samuelson has indicated that his first knowledge of Keynes's *General Theory* was gained from Bryce (C–L, 1996, p. 158). Moreover, even after reading the *General Theory* in 1936, Samuelson perhaps reflecting Bryce's view of the difficulty of understanding Keynes's book, found the *General Theory* analysis "unpalatable" and not comprehensible (C–L, 1996, p. 159). Samuelson finally indicated that "The way I finally convinced myself was to just stop worrying about it [about understanding Keynes's analysis]. I asked myself: why do I refuse a paradigm that enables me to understand the Roosevelt upturn from 1933 till 1937? . . . I was content to assume that there was enough rigidity in relative prices and wages to make the Keynesian alternative to Walras operative" (C–L, 1996, pp. 159–160).

Keynes's biographer, Lord Skidelsky (1992, p. 512) recognized the problem with this Samuelson interpretation of Keynes when he wrote "the validity of Keynes's 'general theory' rests on his assertion that the classical theory . . . is, as he put it in his lectures, 'nonsense.' If it [Walrasian classical theory] were true, the classical 'special case' would, in fact, be the 'general theory'[1] and Keynes's aggregative analysis not formally wrong, but empty, redundant. It is worth noting, at this point, that mainstream economists after the Second World War treated Keynes's theory as a 'special case' of the classical [Walrasian] theory, applicable to conditions where money wages and interest were 'sticky'. Thus his theory was robbed of its theoretical bite."[2]

Apparently Samuelson never tried to comprehend Keynes's analytical foundation and framework. For in 1986 Samuelson was still claiming that "we [Keynesians] always assumed that the Keynesian underemployment equilibrium floated on a substructure of administered prices and imperfect competition" (C–L, 1996, p. 160). When pushed by Colander and Landreth as to whether this requirement of rigidity was ever formalized in his work, Samuelson's response was "There was no need to" (C–L, 1996, p. 161).

Yet, specifically in chapter 19 of *The General Theory* and even more directly in his published response to Dunlop and Tarshis, Keynes (1939b) had already responded in the negative to this question of whether his analysis of underemployment equilibrium required imperfect competition, administered prices, and/or rigid wages. Dunlop and Tarshis had argued that the purely competitive model (i.e. the Walrasian model) was not empirically justified, therefore it was monopolistic price and wage fixities that was the basis of Keynes's unemployment equilibrium. Keynes's reply was simply: "I complain a little that I in particular should be criticised for conceding a little to the other view" (Keynes, 1973b, p. 411). In chapters 17–19 of his *General Theory*, Keynes explicitly demonstrated that even if perfectly flexible money wages and prices existed ("conceding a little to the other side"), there was no automatic mechanism that could restore the full employment level of effective demand. In other words, Keynes's general theory could show that, as a matter of logic, less than full employment equilibrium could exist in a purely competitive economy with freely flexible wages and prices.

Obviously, Samuelson, who became the premier American Keynesian of his time, had either not read, or not comprehended, (1) Keynes's response to Dunlop and Tarshis or even (2) chapter 19 in *The General Theory* which was entitled "Changes in Money Wages." In chapter 19 Keynes explicitly indicates that the theory of unemployment equilibrium did not require "a rigidity" in money wages (Keynes, 1936a, p. 257). As Keynes put it:

> For the classical theory has been so accustomed to rest the supposedly self-adjusting character of the economic system on the assumed fluidity of money wages; and, when there is rigidity, to lay on this rigidity the blame of maladjustment . . . My difference from this theory is primarily a difference of analysis (Keynes, 1936a, p. 257).

Keynes (1936a, p. 259) indicated that to assume that rigidity was *the* cause of the existence of an unemployment equilibrium lay in accepting the argument that the micro-demand functions "can only be constructed on some fixed assumption as to the nature of the demand and supply schedules of other industries and as to the amount of aggregate effective demand. It is invalid, therefore to transfer the argument to industry as a whole unless we also transfer the argument that the aggregate effective demand is fixed. Yet, this assumption reduces the argument to an *ignoratio elenchi*."

An ignoratio elenchi is a fallacy in logic of offering a proof irrelevant to the proposition in question. Unfortunately Samuelson invoked the same classical ignoratio elenchi when he argued that Keynes's general theory was simply a Walrasian general equilibrium system where, if there is an exogenous decline in effective demand, rigid wages and prices created a temporary disequilibrium that prevented full employment from being restored in the short-run.[3]

As Keynes went on to explain, "whilst no one would wish to deny the proposition that a reduction in money wages *accompanied by the same aggregate effective demand as before* will be associated with an increase in employment, the precise question at issue is whether the reduction in money wages will or will not be accompanied by the same aggregate effective demand as before measured in term of money, or, at any rate, by an aggregate effective demand which is not reduced in full proportion to the reduction in money-wages" (Keynes, 1936a, pp. 259–260). Keynes then spent the rest of chapter 19 explaining why and how a general theory analysis must look at the relationship between changes in money wages and/or prices and changes in aggregate effective demand—an analysis that, by assumption, is not relevant to either a Walrasian system or Samuelson's neoclassical synthesis Keynesianism.

At the same time that Samuelson became a Keynesian by convincing himself not to worry about Keynes's actual analytical framework, Tarshis had obtained a position at Tufts University, a mere half-hour of travel from Harvard. Tarshis would often met with the group at Harvard, including Bryce, who were discussing Keynes. Tarshis notes that "Paul Samuelson was not in the Keynesian group. He was busy working on his own thing. That he became a Keynesian was laughable" (C–L, 1996, p. 64).

Yet, Paul Samuelson has called himself a "Keynesian" and even a "Post-Keynesian" in several editions of his famous textbook. Nevertheless, as we will explain in section 12.4 infra, Samuelson's theoretical neoclassical synthesis axiomatic foundations is logically not the general theory spelled out by Keynes.

12.3 The Axiomatic Differences between Samuelson's Neoclassical Keynesianism and Keynes/ Post Keynesian Theory

At the same time that Samuelson was developing his neoclassical synthesis Keynesianism, he was working on his masterful *Foundations of Economic Analysis* (1947). In his *Foundations*, Samuelson asserts explicitly (or implicitly) certain specific classical axioms are the basis of both classical micro theory and his neoclassical Keynesian macroeconomic analysis. For example, Samuelson noted that "in a purely competitive world it would be foolish to hold money as a store of value as long as other assets had a positive yield" (Samuelson, 1947, pp. 122–124). This statement means that (1) any real producible capital goods that produce a positive yield are a gross substitute for money and (2) money is neutral. Thus as he was promoting his pedagogical brand of Keynesianism in his textbook Samuelson was arguing that the gross substitution axiom and the neutral money axiom are the foundations upon which all economic analysis including neoclassical synthesis Keynesianism must be built. (We shall indicate infra that Keynes specifically rejected these two classical axioms as a foundation for his *General Theory*.)

Furthermore, in an article published in 1969, Samuelson argued that the "ergodic hypothesis [axiom]" is a necessary foundation if economics is to be a hard science (Samuelson, 1969, p. 184). (As explained in section 12.4 infra, Keynes also rejected this ergodic axiom.) What is this ergodic hypothesis?

If one conceives of the economy as a stochastic (probability) process, then the future outcome of any current decision is determined via a probability distribution. Logically speaking, to make statistically reliable forecasts about future economic events, the decision maker should obtain and analyze sample data from the future. Since that is impossible, the assumption of an ergodic stochastic process permits the analyst to assert that samples drawn from past and current data are equivalent to drawing a sample from the future. In other words, the ergodic axiom implies that the outcome at any future date is the statistical shadow of past and current market data.

A realization of a stochastic process is a sample value of a multidimensional variable over a period of time, that is, a single time series of recorded outcomes. A stochastic process provides a universe of such time series. *Time statistics* refers to statistical averages (e.g. the mean, the standard deviation, etc.) calculated from a singular realization over an indefinite time space. *Space statistics*, on the other hand, refers to statistical averages calculated at a fixed point of time observation and are formed over the universe of realizations (i.e. space statistics are calculated from cross-sectional data).

If the stochastic process is ergodic, then for an infinite realization the time statistics and the space statistics will coincide. For finite realizations of ergodic processes, time and space statistics coincide except for random errors, that is, they tend to converge (with the probability of unity) as the number of observations increase. Consequently, if the ergodic axiom is applicable, statistics calculated from either past time series or cross-sectional data are statistically reliable estimates of the space statistics that will occur at any future date.

The ergodic axiom therefore assures that the outcome associated with any future date can be reliably predicted by a statistical analysis of already existing data. The future is therefore never uncertain—it can always be reliably predicted by a sufficient statistical analysis of already existing data. Future outcomes, in an ergodic system, are probabilistically risky but reliably predictable. (In a nonstochastic deterministic orthodox economic model, the classical ordering axiom plays the same role as the ergodic axiom of classical stochastic models.[4])

In an ergodic world, in the long run, the future is predetermined and cannot be changed by anything human beings or governments do. It follows that any government market regulation or interference into normal competitive market (assumed ergodic) processes, may, in the short run, prevent the system from achieving the full employment level assured by the axioms of a classical Walrasian system. In an ergodic system where the future can be reliably predicted so that future positive yields of real assets can be known with actuarial certainty, and where the gross substitution axiom underlies all demand curves, then as long as prices are flexible, money must be neutral and the system automatically adjusts to a full employment general equilibrium. If, on the other hand, prices are sticky in the short run, then it will take a longer time for the gross substitution theorem to work its way through the system but, at least in the long run, a full employment general equilibrium is still assured. In Keynes's general theory analysis, a full employment equilibrium is not assured in either the short run or the long run.

Samuelson (C–L, 1996, p. 163) has stated that in his view Keynes's analysis is a "very slow adjusting disequilibrium" system where the "full Walrasian equilibrium was not realized" in the short run because prices and wages do not adjust rapidly enough to an exogenous shock. Nevertheless, the economic system would, if left alone, achieve full employment in the long run.

In contrast, on the very first text page of *The General Theory*, Keynes (1936a, p. 3) explained "that the postulates of the classical [Walrasian] theory are applicable to a special case only and not to the general case . . . Moreover the characteristics of the special case assumed by the classical theory happen not to be those of the economics society in which we actually live, with the result that its teaching is misleading and disastrous if we attempt to apply it to the facts of experience."

In the preface to the German language edition of *The General Theory* (1936b, p. ix) Keynes specifically noted "This is one of the reasons which justify my calling my theory a *general* (emphasis in the original) theory. Since it is based on *fewer restrictive assumptions* ('weniger enge Voraussetzunger stutz') than the orthodox theory, it is also more easily adopted to a large area of different circumstances" (Second emphasis added). In other words, Keynes argued that what made his analytical system more general than the classical (or more recent Walrasian general equilibrium) analysis is that Keynes's general theory requires a smaller common axiomatic base (fewer restrictive axioms) than any other alternative theory. Alternative theories then are special cases that impose additional restrictive axioms to the common axiomatic foundation of the general theory. The onus is therefore, on those who add the restrictive axioms to the general theory to justify these additional axioms. Those theorists who invoke only the general theory axiomatic base are not required, in logic, to prove a general negative, that is, they are not required to prove the additional restrictive axioms are unnecessary.

12.4 Samuelson's Keynesian Axioms that Keynes and the Post Keynesians Overthrow in their General Theory Revolution

Keynes was primarily a monetary theorist. The words money, currency, and monetary appear in the titles of most of his major volumes in economics.

Post Keynesian monetary theory evolves from Keynes's revolutionary approach to analyzing money-using economy where money was never neutral even if a hypothetical pure competitive market conditions including instantaneously flexible wages and prices exist. Keynes (1936a, p. 26) argued that even if such a purely competitive market existed it would not automatically achieve a full employment general equilibrium in a money-using economy.

Keynes compared those economists whose theoretical logic was grounded on the classical special case additional restrictive axioms to Euclidean geometers living in a non-Euclidean world

> who discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight—as the only remedy for the unfortunate collisions which are taking place. Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required today in economics. (Keynes, 1936a, p. 16)

To throw over an axiom is to reject what the faithful believe are "universal truths." The Keynesian revolution in economic theory required economists to "throw over" three restrictive classical axioms from its theoretical foundation. Post Keynesian monetary theory has followed Keynes's fewer restrictive axiom analytical framework. In light of Keynes's analogy to geometry, Post Keynesian monetary theory might be called non-Euclidean economics.

The classical axioms that Keynes threw out in his revolutionary general analysis were (1) *the neutrality of money axiom*, (2) *the gross substitution axiom*, and (3) *the axiom of an ergodic economic world*.

In 1935 Keynes explicitly noted that in his analytic framework money matters in both the long and short run, that is, money is never neutral. Money affects real decision making. In 1935 Keynes wrote:

> the theory which I desiderate would deal . . . with an economy in which money plays a part of its own and affects motives and decisions, and is, in short, one of the operative factors in the situation, so that the course of events cannot be predicted either in the long period or in the short, without a knowledge of the behavior of money between the first state and the last. And it is this which we mean when we speak of a monetary economy. (Keynes, 1935, pp. 408–409).

As Keynes's developed his theory of liquidity preference he recognized that his theory of involuntary unemployment required specifying "The Essential Properties of Interest and Money" (1936a, ch. 17) that differentiated his results from classical theory. These "essential properties" assured that money and all other liquid assets are never neutral. These essential properties (Keynes, 1936a, pp. 230–231) are:

(1) the elasticity of production of all liquid assets including money is zero or negligible, and

(2) the elasticity of substitution between liquid assets (including money) and reproducible goods is zero or negligible.

A zero elasticity of production means that money does not grow on trees and consequently workers can not be hired to harvest money trees when the demand for money increases. Or as Keynes wrote: “money . . . cannot be readily reproduced;-labour cannot be turned on at will by entrepreneurs to produce money in increasing quantities as its price rises” (Keynes, 1936a, p. 230). In other words, when the demand for money (liquidity) increases, private sector entrepreneurs cannot hire labor to produce more money to meet this increase in demand for a nonreproducible (by the private sector) good.

In classical theory, on the other hand, money is a reproducible commodity. In many neoclassical textbook models as well as in the Walrasian system, peanuts or some other reproducible product of industry is the money commodity or *numeraire*. Peanuts may not grow on trees, but they do grow on the roots of bushes. The supply of peanuts can easily be augmented by the hiring of additional workers by private sector entrepreneurs.

The zero elasticity of substitution assures that portion of income that is not spent on by the products of industry for consumption purposes, that is, savings, will find, in Hahn’s (1977, p. 31) terminology, “resting places” in the demand for nonproducibles. Some 40 years after Keynes, Hahn rediscovered Keynes’s point that a stable involuntary unemployment equilibrium could exist *even in a Walrasian system with flexible wages and prices* whenever there are “resting places for savings in other than reproducible assets” (Hahn, 1977, p. 31).

Hahn rigorously demonstrated what was logically intuitive to Keynes. Hahn (1977, p. 37) showed that the view that with “flexible money wages there would be no unemployment has no convincing argument to recommend it. . . . Even in a pure tatonnement in traditional models convergence to [a general] equilibrium cannot be generally proved” if savings were held in the form of nonproducibles. Hahn (1977, p. 39) argued that “any non-reproducible asset allows for a choice between employment inducing and non-employment inducing demand.” Accordingly, the existence of a demand for money and other liquid nonreproducible assets (that are *not* gross substitutes for the products of the capital goods producing industries) as a store of "savings" means that all income earned by households engaging in the production of goods is not, in the short or long run, necessarily spent on the products of industry. Households who want to store that portion of their income that they do not consume (i.e., that they do not spend on the products of industry) in liquid assets are choosing, in Hahn's words "a non-employment inducing demand" for their savings.

If the gross substitution axiom was universally applicable, however, any new savings that would increase the demand for nonproducibles would increase the price of nonproducibles (whose production supply curve is, by definition, perfectly inelastic). The resulting relative price rise in non-producibles vis-a-vis producibles would, under the gross substitution axiom, induce savers to increase their demand for reproducible durables as a substitute for nonproducibles in their wealth holdings. Consequently nonproducibles could not be ultimate resting places for savings as they spilled over into a demand for producible goods (cf. Davidson, 1972).

Samuelson's assumption that all demand curves are based on an ubiquitous gross substitution axiom implies that everything is a substitute for everything else. In Samuelson's foundation for economic analysis, therefore, producibles must be good gross substitutes for any existing nonproducible liquid assets (including money) when the latter are used as stores of savings. Accordingly, Samuelson's *Foundation of Economic Analysis* denies the logical possibility of involuntary unemployment[5] as long as all prices are perfectly flexible.

Samuelson's brand of Keynesianism is merely a form of the classical special case analysis that is "misleading and disastrous" (Keynes, 1936a, p. 3) if applied to the real world. In the absence of a restrictive universally applicable axiom of gross substitution, however, income effects (e.g. the Keynesian multiplier) can predominate and can swamp any hypothetical classical substitution effects. Just as in non-Euclidean geometry lines that are apparently parallel often crash into each other, in the Keynes-Post Keynesian non-Euclidean economic world, an increase demand for "savings" even if it raises the relative price of nonproducibles, will not spill over into a demand for producible good and hence when households save a portion of their income they have made a choice for "non-employment inducing demand."

Finally, Keynes argued that only in a money-using entrepreneur economy where the future is uncertain (and therefore could not be reliably predicted) would money (and all other liquid assets) always be nonneutral as they are used as a store of savings. In essence Keynes viewed the economic system as moving through calendar time from an irrevocable past to an uncertain, not statistically predictable, future. This required Keynes to reject the ergodic axiom.

Keynes never used the term "ergodic" since ergodic theory was first developed in 1935 by the Moscow School of Probability and it did not become well known in the West until after the Second World War and Keynes was dead. Nevertheless, Keynes's main criticism of Tinbergen's econometric "method" (Keynes, 1939a, p. 308) was that the economic data "is not homogeneous over time." Nonhomogeneous data over time means that economic time series are nonstationary, and nonstationary is a sufficient (but not a necessary condition) for nonergodic circumstances. Consequently, Keynes, with his emphasis on uncertainty had, in these comments on Tinbergen, specifically rejected what would later be called the ergodic axiom—an assumption that Samuelson has declared is a foundation necessary to make economics a hard science.

In sum, Samuelson theoretical foundations requires three classical axioms that are the equivalent of the axiom of parallels in Euclidean geometry. Clearly then Samuelson's macroeconomics is not applicable to the "non-Euclidean" economics of a money-using entrepreneurial system that Keynes developed in his *General Theory*.

12.5 Liquidity and Contracts

Nevertheless, the question may remain "Does applying Keynes's smaller axiomatic base make any difference in our understanding of the real world in which we live vis-a-vis applying Samuelson's classical axiomatic foundation version of Keynesianism?" The answer is definitely yes because only if we overthrow these three classical axioms that are an essential part of Samuelson's foundations of economic analysis can the concept of liquidity play an important role in our analysis—as it does in our lives.

Important decisions involving production, investment, and consumption activities are often taken in an uncertain (nonergodic) environment. Hiring inputs and buying products using forward contracts in money terms are a human institution developed to efficiently organize time consuming production and exchange processes. Since the abolition of slavery the money-wage contract is the most ubiquitous of these contracts. Unemployment, rather than full employment, is a common laissez-faire outcome in such a market oriented, monetary production economy.

The economy in which we live utilizes money contracts—not real contracts—to seal production and exchange agreements among self-interested individuals. The ubiquitous use of money contracts is an essential element of all real world entrepreneurial economies. Moreover *recontracting without income penalty* (an essential characteristic of the Walrasian system) whenever parties have entered into a contract at a price other than the implicit full employment general equilibrium price *is never permitted under the civil law of contracts*. Why, one might ask Samuelson, do economies continue to organize production and exchange on the basis of money contracts, if such use interferes with the rapid achievement of a socially optimal general Walrasian equilibrium?

The use of money contracts has always presented a dilemma to classical theorists. Logically consistent classical theorists must view the universal use of money contracts by modern economies as irrational, since such agreements fixing payments over time in nominal terms can impede the self-interest optimizing pursuit of real incomes by economic decision makers. Mainstream economists tend to explain the existence of money contracts by using noneconomic reasons such as social customs, invisible handshakes, etc.—societal institutional constraints which limit price signaling and hence limits adjustments for the optimal use of resources in the long run.

For Post Keynesians, on the other hand, *binding* nominal contractual commitments are a sensible method for dealing with true uncertainty regarding future outcomes whenever economic activities span a long duration of calendar time. In organizing production and exchange on a money contractual basis, buyers need not worry about what events happen in the uncertain future as long as they have, or can obtain, enough liquidity to meet these contractual commitments as they come due. Thus liquidity means survival in a money-using contractual entrepreneurial directed market economy. Bankruptcy, on the other hand, occurs when significant contractual monetary obligations cannot be met. Bankruptcy is the equivalent of a walk to the economic gallows.

Keynes's general theory that emphasizes money and liquidity implies that agents who planned to spend in the current period need not have earned income currently, or previously, in order to exercise this demand in an entrepreneur system. All these buying agents need is the liquidity to meet money contractual obligations as they come due. This means that investment spending, which we normally associate with the demand for reproducible fixed and working capital goods, is not constrained by either actual income or inherited endowments. This type of exogenous spending is constrained, in a money-creating banking system, solely by the expected future *monetary* (not real) cash inflow (Keynes, 1936a, ch. 17) upon which banks are willing to make additional loans.

In a world where money is created only if someone increases his/her indebtedness to banks in order to purchase newly produced goods, then real investment spending will be undertaken as long as the purchase of newly produced capital goods are expected to generate a future of cash inflow (net of operating expenses) whose discounted present value equals or exceed the money cash outflow (the supply price currently needed to purchase the capital good).

For any component of aggregate demand not to be constrained by actual income, therefore, agents must have the ability to finance purchases by borrowing from a banking system that can create money. This Post-Keynesian financing mechanism where increases in the nominal quantity of money are used to finance increased demand for producible goods results in increasing employment levels. Money, therefore, cannot be neutral and can be endogenous.

To reject the neutrality axiom does not require assuming that agents suffer from a money illusion. It only means that "money is not neutral" (Keynes, 1935, p. 411) in the sense that money matters in both the short run and the long run, affecting the equilibrium level of employment and real output. If it were not for Samuelson's insistence on neutral money as foundations for all economic theory, economists might recognize that in a money-using entrepreneurial economy that organizes production and exchange with the use of spot and forward money contracts, money is a real phenomenon. The money neutrality axiom must be rejected.

Arrow and Hahn (1971, pp. 356–357) implicitly recognized this necessity of overthrowing the neutral money axiom when they wrote:

> The terms in which contracts are made matter. In particular, if money is the goods in terms of which contracts are made, then the prices of goods in terms of money are of special significance. This is not the case if we consider an economy without a past or future . . . *if a serious monetary theory* comes to be written, the fact that contracts are made in terms of money will be of considerable importance [italics added].

Moreover Arrow and Hahn demonstrate (1971, p. 361) that, if production and exchange contracts are made in terms of money (so that money affects real decisions) in an economy moving along in calendar time with a past and a future, then *all general equilibrium existence theorems are jeopardized*. The existence of money contracts—a characteristic of the world in which we live—implies that there need never exist, in the long run or the short run, any rational expectations equilibrium or general equilibrium market clearing price vector. Samuelson's Walrasian foundation is not a reliable base for real world economies that use money and money contracts to organize economic activities.

12.6 Conclusion

Paul Samuelson saved the term “Keynesian” from being excoriated from post-Second World War textbooks by the McCarthy anti-communist movement at the time. But the cost of such a saving was to sever the meaning of Keynes’s theory in mainstream economic theory from its *General Theory* analytical roots that demonstrated that, in a money-using economy, flexible wages and prices and pure competition are neither necessary nor sufficient conditions to assure full employment equilibrium, even in the long run.

Samuelson’s view of Keynesianism resulted in aborting Keynes’s revolutionary analysis from altering the foundation of mainstream macroeconomics. Consequently what passes as conventional macroeconomic wisdom of mainstream economists at the beginning of the twenty-first century is nothing more than a high-tech and more mathematical version of the nineteenth century classical theory.

In winning the battle against the forces trying to prevent the teaching of suspected communist inspired “Keynesian” economics in our universities, Samuelson ultimately lost the war that Keynes had launched to eliminate the classical theoretical analysis as the basis for real world economic problems of employment, interest, and money. In 1986 Lorie Tarshis recognized this when he noted “I never felt that Keynes was being followed with full adherence or full understanding of what he had written. I still feel that way” (C–L, p. 72).

Mainstream economics—whether espoused by Old Neoclassical Keynesians, New Keynesians, Old Classical, or New Classical theorists, etc[6]—relies on the three classical axioms that Keynes discarded in his general theory attempt to make economics relevant to the real world problems of unemployment and international trade and international payments. As a result these problems still plague much of the real world in the globalized economy of the twenty-first century.

Notes

1. As Weintraub (2002, p. 113) noted, Debreu was a Student of Bourbakian mathematics and Bourbakians believe “good general theory does not search

for the maximum generality but for the right generality". Keynes searched for a "maximum" general theory, that is a theory built on the smallest axiomatic foundation that could be applied to the real world. Debreu's *Theory of Value* (1959) was a "direct analogue of Bourbaki's (analysis) right down to the title . . . .{Debreu] sought to establish the definitive analytic mother-structure from which all further work in economics would depart . . . But this required one very crucial maneuver that was nowhere explicitly stated, namely that the model of Walrasian equilibrium was the root structure [the right level of generality] from which all further work in economics would eventuate" (Weintraub, 2002, p. 121).

2. Mainstream economists called this sticky interest rate argument the "liquidity trap" where at some low, but positive, rate of interest the demand to hold money for speculative reasons was assumed to be perfectly elastic (i.e. horizontal). After the Second World War, econometric investigations could find no empirical evidence of a liquidity trap. Had mainstream economists read *The General Theory*, however, they would have known that on page 202 Keynes specifies the speculative demand for money as a rectangular hyperbola—a mathematical function that never has a perfectly elastic segment. Moreover eyeball empiricism led Keynes (1936a, p. 207) to indicate that he knew of no historical example where the liquidity preference function became "virtually absolute", that is, perfectly elastic. In sum, from both an empirical and theoretical view, Keynes denied the existence of a liquidity trap.
3. The particular proof that Keynes claimed was irrelevant was the classical assertion that a fixed and unchanging downward sloping marginal product curve of labor was the demand curve for labor and so that falling wages must increase employment. In chapter 20 of *The General Theory* Keynes specifically develops an "employment function" that is not the marginal product of labor curve and does not assure that aggregate effective demand is fixed.

   What the marginal productivity of labor curve indicates is that if in response to an expansion of aggregate effective demand, private sector entrepreneurs hire more workers to produce an additional flow of output per period, then in the face of diminishing returns (with no change in the degree of competition), the rise in employment will be associated with a fall in the real wage rate. In other words, the marginal product of labor curve is, for any given the level of effective demand and employment, the real wage determining curve. For a complete analysis of this point see Davidson (1998) or Davidson (2002).
4. True uncertainty occurs whenever an individual cannot specify and/or order a complete set of prospects regarding the future, either because: (1) the decision maker cannot conceive of a complete list of consequences that will occur in the future; or, (2) the decision maker cannot assign probabilities to all consequences because "the evidence is insufficient to establish a probability" so that possible consequences "are not even orderable" (Hicks, 1979, pp.113, 115). In such cases ordering is not possible.

5. To overthrow the axiom of gross substitution in an intertemporal context is truly heretical. It changes the entire perspective as to what is meant by "rational" or "optimal" savings, as to why people save or what they save. It would deny the life-cycle hypothesis. Indeed Danziger *et al*. (1982–83) have shown that the facts regarding consumption spending by the elderly are incompatible with the notion of intertemporal gross substitution of consumption plans which underlie both life cycle models and overlapping generation models currently so popular in mainstream macroeconomic theory.
6. Some economists, for example behavioral theorists, have tried to erect *ad hoc* models suggesting that agents do not always act with the economic rationality of classical theory's decision makers although there is nothing in their analysis that denies the possibility that rational decision making is possible. Unfortunately, such theories have no unifying underlying general theory to explain why such "irrational" behavior exists. Behavioral theorists can not explain why those who undertake non-rational behavior have not been made extinct by a Darwinian struggle with those real world decision makers who take the time to act rationally.

   Had behavioral theorists adopted Keynes's general theory as their basic framework, irrational behavior can be explained as sensible if the economy is a nonergodic system. Or as Hicks (1979, p. vii) succinctly put it, "One must assume that the people in one's models do not know what is going to happen, and know that they do not know just what is going to happen." In conditions of true uncertainty, people often realize they just don't have a clue as to what rational behavior should be.

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13

Paul Samuelson and International Trade Theory Over Eight Decades

Avinash Dixit

Paul Samuelson has made seminal contributions to every major field of economics, but I think international trade theory can rightfully claim to be one of his favorite areas of research. He keeps returning to it after detours into other fields, and invariably finds something new and thought-provoking to say.

He started in the 1930s with pathbreaking work on gains from trade. Then, in a series of celebrated articles over a period of more than a decade, he established the two-by-two model, now called the Heckscher–Ohlin–Samuelson model, as the standard tool for thinking about international trade for the following two decades or longer. The 1950s brought what is my personal favorite article (Samuelson, 1953), which elucidated the interaction between the prices of goods and factors in a general equilibrium with trade. It extended much of the earlier analysis beyond the two-by-two case, and gave us new tools, particularly duality and the revenue or GDP function. Samuelson continued the theme of "beyond two-by-two" with the sector-specific factor model, now called the Ricardo–Viner–Samuelson model, in the late 1960s and the early 1970s, and the Dornbusch–Fischer–Samuelson model of trade with a continuum of commodities in the late 1970s; both models now constitute an essential part of the international trade economists' toolkit. His concern about the normative effects of trade continued with new insights on the transfer problem, further contributions to the issues of gains from trade, and critiques of doctrines of unequal exchange. He has continued to examine and develop classical ideas and theories, and to write perceptive and generous assessments of historical and contemporary pioneers of trade theory. And his most recent explorations of Ricardian theories have brought him into the current policy controversies concerning globalization and outsourcing.

The community of international trade economists has recognized and celebrated Samuelson's role in the development of our subject before. Noteworthy among these writings is Ronald Jones' (1983) survey of Samuelson's impact on trade theory. The last twenty years have brought new contributions from Samuelson, and have seen his influence continue in the work of others; therefore a new overview is justified. But there is a second and perhaps more important reason for a new encomium. Old Nordic and Celtic bards told and retold the deeds of heroes; these rituals were an important part of the collective memory or even identity of their people. The tribe of professional economists should likewise continue to sing of our heroes.

Jones' excellent article saves me the need to describe the technical details of Samuelson's contributions to trade theory. Therefore I can concentrate on my chosen role of a bard or balladeer, which is much more fun.

13.1 Trade Theory and Economic Theory

Samuelson has always seen economics as a unified whole, and indeed has always striven to place it in the even larger context of its connections with other sciences in matters of concepts and techniques of analysis. I believe that the explanation of his special love for international trade theory is to be found in the same trait. He saw from the earliest days that trade theory and general equilibrium theory were very closely linked, and that the linkages flowed both ways. The opening sentence of one of his earliest publications (Samuelson, 1938) is: "Historically the development of economic theory owes much to the theory of international trade." He used the Ricardian model of comparative advantage as the starting point of his exposition of linear programming for economists (Samuelson, 1949b). And recently he said (Samuelson, 1995) "If you've got it, flaunt it. Well, we in trade theory do have a lot to display. . . . Thus, the first general equilibrium was not by Léon Walras. Sixty-five years earlier it was by John Stuart Mill—and in connection with international equilibrium. . . . Stan Ulam[1] . . . once challenged me, saying 'Paul, name me one proposition in the social sciences that is both true and non-trivial.' My reply was: 'Ricardo's theory of comparative advantage.' . . . Trade theory leads the way."

13.2 Gains from Trade

Samuelson's early articles on the gains from trade (1938, 1939), and a later one (1962b) stimulated by Kemp (1962), laid down the basic comparisons (free trade versus autarky, free trade versus restricted trade, Pareto efficiency versus social optimality) and the techniques (revealed preference inequalities, consumption, and utility possibility frontiers) that have guided thinking on this issue ever since. Most importantly, Samuelson showed that the utility possibility frontier with free trade lies outside the utility possibility frontier with autarky, provided the aggregate quantities of goods available in both situations can be distributed among the country's consumers by the government. Thus, given any allocation under autarky, a move to free trade accompanied by suitable distribution of commodities can achieve a Pareto superior outcome, whereas, given any allocation under free trade, no move to autarky accompanied by any redistribution can be Pareto improving.

The simplicity and generality of this analysis was a shining contrast to the muddled thinking of many earlier writers, who had been mired in doctrinal debates about the nature of costs, and had been misled into believing that the arguments for trade depended in crucial ways on there being only two goods, or on all goods being tradeable, or on factors being in fixed supply, or on factors being perfectly mobile across sectors within a country, or any of a number of other restrictive assumptions often made for convenience of exposition. Alas, misunderstandings about the normative case for trade persist and continue to resurface periodically even in professional writings, not to mention policy debates in the media where the grasp of logic is feeble. Trade economists must be ever vigilant to combat such errors. Nothing illustrates this better than the reaction in the media to Samuelson's latest (2004) foray into trade theory. He set out to make a perfectly valid point—while there are positive gains from trade, various developments in the rest of the world can either increase or decrease the size of these gains. If the rest of the world gets better at producing the goods that we currently export, our terms of trade decline and our gains from trade are smaller than they were before. He must have been particularly delighted in making this point using a Ricardian model with simple numerical calculations, a method that has always been one of his favorites. And it was a useful reminder to some participants in debates about trade policy who sometimes leave the impression that under free trade things always and continuously get even better and better. However, his work was misinterpreted and hijacked by the truly dangerous participants at the other extreme of the same debates, who favor a fortress America shut off from the rest of the trading world. Nothing in Samuelson's paper denied the existence of gains from trade. He compared two situations, call them B and A, respectively before and after the rest of the world improves its productivity in America's export sectors. He showed that in the aggregate, B was better for America than A. But there is no feasible policy to restore B; how can we reverse the productivity gains in the rest of the world short of conquest and suppression? The relevant policy comparison is of A with a third alternative F, fortress America. And all the analysis stemming from Samuelson's own papers of the late 1930s shows that A is better for America as a whole than F. To be sure, A is not as much better than F as is B, but it would be a mistake to respond to the technological shift from B to A by making a policy shift that would take us from A to F. This logic was either beyond grasp of the protectionists or was deliberately ignored by them, and it is taking Samuelson and others much time and effort to correct the error. Eternal vigilance is the price of good economic policy, and perhaps even an 89-year-old has something to learn about the need to anticipate and avoid such distortions and hijackings of one's ideas by the policy community.

What about research following Samuelson's pioneering analyses of gains from trade? In my opinion, two lines of thought merit special mention. First, in constructing his utility possibility frontiers, Samuelson assumed that the aggregate quantities of commodities could be allocated among consumers without any restrictions, as personalized lump-sum transfers in kind. But a more natural way to distribute gains from trade in a market economy would be to transfer purchasing power, and then let each individual buy the goods or services according to his or her own preference. Can this be done, using personalized lump-sum transfers that are balanced within each country? This requires a careful construction and proof of existence of equilibrium; it was done by Grandmont and McFadden (1972). Second, personalized lump-sum transfers have long been known to be unrealistic, and thanks to Hammond (1979), we now know the precise nature of the difficulty. They are not incentive-compatible: each individual has the incentive and the ability to misrepresent private information about his or her preferences and abilities in an attempt to secure a larger transfer receipt. Commodity taxes or subsidies of the Diamond–Mirrlees kind depend only on statistical or aggregate information about demands; therefore any individual who is small in the economy has negligible ability to gain by such misrepresentation. Dixit and Norman (1986), in exchange with

Kemp and Wan (1986), examined when Pareto superior outcomes can be achieved using such instruments. It emerged that if the full set of commodity tax and subsidy instruments is available, and the Diamond–Mirrlees condition for productive efficiency under commodity taxation is met, then any positive aggregate production gains from trade can be distributed in a Pareto-beneficial way. Subsequent research has examined what happens when this fails, for example, Feenstra and Lewis (1991) and Spector (2001).

13.3 Ricardian Models of Trade

Ricardo's ideas and theories have a natural appeal for Samuelson, given his manifold interests in all aspects of economics. First and foremost, as a serious historian of thought, who has the proper combination of respect for, and critical assessment of, the contributions of the giants who laid the foundations of our subject, he recognizes the pivotal importance of the concept of comparative advantage. That did not stop him being critical of other aspects of Ricardo's theories; see his Presidential Address to the American Economic Association (Samuelson, 1962a, p. 9). He gave Ricardo credit for formulating "a rigorously handled general equilibrium model of primitive type,"[2] but recognized many less formal predecessors, including Adam Smith, when it comes to the policy implication favoring freer trade. And in his masterly[3] exegesis of Ricardo's overall theory of production and prices (Samuelson, 1962a, b), he took Ricardo to task for his "flirtations" with a labor theory of value.

I think the most useful lesson from these articles for posterity, though, is his conclusion about how modern economists compare with classical economists:

> [Ricardo] would have made a most excellent modern economist! Despite though the high native ability of the ancients, we have advanced a long way ahead of their discussions. . . . In particular, we are more humble. They declared so many things to be necessarily so that we today recognize as not having to be so. This is, in a sense, a step backward. How exciting to be able to assert definitely that invention of a machine cannot do this and must do that! But alas, dull as it may be, the modern theorist must face the facts of life—the infinite multiplicity of patterns that can emerge in actuality. Good, advanced theory must be the antidote for overly-simple, intuitive theory. (Samuelson, 1962b, p. 231)

What we now know as "the" Ricardian model of comparative advantage has logical delights and expository appeal for economic theorists more generally, not just trade theorists. Samuelson (1949b) saw and exploited its potential for explaining many of the intuitions and techniques of linear programming to economists. The same model and the same geometry of linear programming also serve to express in a simple way the concepts of production efficiency in a world economy when goods can cross international boundaries but labor cannot. The standard decentralized implementation of such an efficient production plan in turn establishes the efficiency of free trade in this context. These technical contributions of Samuelson have been so well expounded by Jones (1983) that I have nothing to add (or subtract).

Another expository use of the Ricardian model is its ability to make many of its points using numerical values and arithmetic calculations, without the need to deploy algebra or calculus. Samuelson has used this device on many occasions, including in his most recent article (2004) on the gains from trade.

13.4 Factor Endowment Models of Trade

Samuelson seems to have regarded differences of factor endowments among countries as a better explanation of trade than Ricardian productivity differences. In his first paper on factor price equalization (Samuelson, 1948), he says: "instead of relying upon such crypto explanations as 'Yankee ingenuity' to explain patterns of comparative advantage, Ohlin would attribute America's comparative advantage in food production—a land-intensive industry—to the fact that each unit of American labor has relatively much land to work with."

He gleaned an astonishingly rich harvest from his work on the factor endowment theory of trade. The two-by-two model is now justly called the Heckscher–Ohlin–Samuelson model. This work produced two of the "four theorems of trade theory." With Stolper (1941), he showed how changes in the international prices of goods lead to magnified changes in the domestic prices of factors, and generated unambiguous predictions about the effects of tariffs on the real returns to factors; this came to be called the Stolper–Samuelson effect. In two papers (Samuelson, 1948, 1949a) he found that free trade in goods will lead to complete equalization of the prices of factors even though factors trade in separate country-specific markets. The question was whether the nonlinear equations relating the world prices of outputs to the domestic unit cost functions for all the goods being produced had a unique solution for the domestic input prices.

This analysis of "global univalance," with more general mathematical analysis in an appendix to Samuelson (1953), actually led to some new mathematics—a global inverse function theorem. Mas-Colell (1985) gives a detailed discussion of this.

The 1949 paper contains another argument, which I believe gets much more directly at the economics of the factor price equalization issue. This is the wonderful "angel and recording geographer" device:

> Let us suppose that in the beginning all factors were perfectly mobile, and nationalism had not yet reared its ugly head. . . . [T]here would be one world price of food and clothing, one real wage, one real rent, and the world's land and labour would be divided between food and clothing production in a determinate way, with uniform proportions of labour to land being used everywhere in clothing production, and a smaller—but uniform—proportion of labour to land being used in production of food. Now suppose an angel came down from heaven and notified some fraction of all the labour and land units producing clothing that they were to be called Americans, the rest to be called Europeans. . . . Obviously, just giving people and areas national labels does not alter anything; it does not change commodity or factor prices or production patterns. . . . [W]hat will be the result? Two countries with quite different factor proportions, but with identical real wages and rents and identical modes of commodity production (but with different relative importance of food and clothing industries). . . . Both countries must have factor proportions intermediate between the proportions in the two industries. The angel can create a country with proportions not intermediate between the factor intensities of food and clothing. But he cannot do so by following the above-described procedure, which was calculated to leave prices and production unchanged. (Samuelson, 1949a, pp. 194–195)

The question is whether or when incomplete markets suffice to ensure the full efficiency of complete markets. Even though markets for factors of production are not unified across countries, will trade in unified world markets for goods suffice to equate prices of factors across countries? Put this way, the idea is very similar to that of spanning in financial markets (see Ekern and Wilson (1974) for an early statement and Duffie and Huang (1985) for a more general later development). If a full set of markets for Arrow–Debreu securities corresponding to all states of the world exists, then a competitive general equilibrium will be Pareto-efficient (with the usual caveats about satiation and externalities). Now suppose such a direct set of complete markets does not exist, but there are markets for other composite securities, for example, shares in firms whose production decisions generate particular profit patterns across the states of the world so that the shares constitute prepackaged bundles of Arrow–Debreu securities. This alternative set of markets suffices for Pareto-efficiency if the available set of securities spans the space of purchasing powers in all states of the world. Similarly, when countries differ in their factor endowments, full production efficiency is ensured if they can trade the factors directly. But suppose they can only trade prepackaged bundles of these factors, namely those embodied in units of each of the goods. This suffices if the vectors of factors comprising these bundles together span the factor space in a suitable sense. Of course this is an equilibrium concept. In finance one must find the real choices of firms to know the patterns of profits in the available securities and see if they span the full space; in trade one must solve for the factor proportions in the hypothetical equilibrium of an integrated world with international factor mobility and see if these factor bundles suffice for the purpose. One difference between finance and trade is that finance theory usually allows short sales of securities, while production quantities in trade must be inherently nonnegative in each country. Therefore we must incorporate this restriction and refine the concept of spanning to an appropriately nonnegative spanning; in the two-by-two model this requires that the factor proportions in the two countries should not be too different. But the analogy captures well the economic idea that trade in goods is serving as a substitute for trade in factors, and generates a more useful intuition than the mathematics of univalence. This approach to factor price equalization was developed for the competitive factor endowment models by Dixit and Norman (1980, pp. 110–125, 289–291), and was used in many other contexts including foreign direct investment by Helpman and Krugman (1985).

The Stolper–Samuelson and factor price equalization papers did not actually produce the Heckscher–Ohlin theorem, namely the prediction that the pattern of trade will correspond to relative factor abundance, although the idea was implicit there. As Jones (1983, p. 89) says, "it was left to the next generation to explore this 2×2 model in more detail for the effect of differences in factor endowments and growth in endowments on trade and production patterns." That, plus the Rybczynski theorem which arose independently, completed the famous four theorems. Jones' own article (1965) is my favorite exposition of the complete story; there are also important surveys by Bhagwati (1965) and Chipman (1965, 1966).

All this and much more came together in Samuelson (1953). With any number of goods and factors, he established a duality between prices and quantities (more precisely, a reciprocity relationship linking the Stolper–Samuelson and Rybczynski effects), and studied the univalence question with any number of goods and factors. In the process, he developed tools, most notably the revenue or GDP function, that have found numerous uses in trade theory. The subsequent research can be mostly subsumed in the general theme of "beyond 2-by-2."

Some explored questions such as "Under what conditions do the four theorems directly generalize to the case of many goods and factors?" and "Can the many goods and factors model be reduced to the 2-by-2 case by aggregation?" (see Jones and Scheinkman, 1977 and Neary (1985)). Others explored how the general intuition behind some of the four theorems can be adapted to the multidimensional situation, to obtain results involving alignments or correlations between the vectors of factor endowment differences or autarkic price differences and the vector of trade patterns (see Deardorff (1980, 1982) and Dixit and Norman (1980, pp. 94–102)).

Two of the most influential advances beyond two-by-two came from Samuelson himself. Both were special models that combined a richness of structure and the simplicity of tractability, and led to numerous uses. One was the two-by-three model where one factor was mobile across uses within a country, and the other two factors were each specific to the production of one of the two internationally tradeable goods; this was soon labeled the Ricardo–Viner–Samuelson model. Samuelson developed this in an article that launched the new *Journal of International Economics* (1971a). His aim was to provide a structure that combined the interconnections of general equilibrium essential for trade and the simplicity of rising supply curves in partial equilibrium analysis. He succeeded brilliantly, and the model has been much used in this way, but it soon found implications and significance beyond the original purpose. Samuelson himself found (1971c) that with more factors than goods, factor price equalization became an unlikely or exceptional case, and that Ohlin's arguments for partial but incomplete convergence of factor prices across countries could then be rehabilitated. Most importantly, the model was interpreted as the short run when a slow-adjusting factor (capital) was temporarily sector-specific. The resulting view of a process of adjustment that links the totally sector-specific-factor or pure exchange models on the one hand, and the Heckscher–Ohlin–Samuelson model with full intersectoral mobility of factors on the other, enriched our understanding of the dynamics of production (see Mayer, (1974), Mussa (1974) and Neary (1978)). And in a little-known but excellent paper, Mussa (1984) showed that factor specificity or adjustment costs are not by themselves distortions: in dynamics with rational expectations, producers will make socially efficient decisions about relocation of factors.

Samuelson's second foray beyond two-by-two, joint with Dornbush and Fischer (1977, 1980), is equally influential. They went to the limiting case of a continuum of goods, in Ricardian and Heckscher–Ohlin–Samuelson structures of production with one and two inputs. This created a smooth margin for adjustment in response to changes in underlying conditions or policies; therefore these models were more easily amenable to comparative statics methods.

Recently the generalizations of factor endowment models have gone in a new direction. Empirical research in trade as well as industrial organization showed that there is great intra-industry heterogeneity among firms. This opened up the possibility that in a given country and a given industry, some firms are more productive and are successful exporters, whereas other firms are less productive and struggle to compete against imports and against other domestic producers. Theoretical work developing the implications of firm heterogeneity, as well as empirical work testing the resulting models, constitutes a very active research area these days. And the theoretical models continue to use many of the concepts and tools that have their origins in Samuelson's work: integrated equilibrium, unit cost functions, and more. Some examples of this research are Davis and Weinstein (2003), Melitz (2003).

13.5 And Much More

Even within the field of international economics, I have touched upon Samuelson's contributions to only a few, albeit central, subfields and topics. I have left out his work on the transfer problem, for example Samuelson (1952b, 1954, 1971b), because Jones (1983) has treated the developments through the 1970s so well, and perhaps because the less said about some of the subsequent debates on this topic, the better. I have omitted Samuelson's work on the Hume mechanism and balance of payments, because I am only a microeconomist who cannot hope to do justice to the topic. And I have left out his battles with purveyors of doctrines of "unequal exchange" and the like, for example Samuelson (1975, 1976), because this should have been unnecessary; anyone who had understood the Arrow–Debreu theory of intertemporal equilibrium, especially the article of Malinvaud (1961), should never have been confused in the first place and it should not have been necessary to use up so much of Samuelson's valuable time to correct the errors. Finally, the Balassa–Samuelson (Samuelson, 1964, 1994) effect got short shrift because everyone knows it and it did not fit neatly into any of the above sections.

But I do want to mention another aspect of Samuelson's contribution to the profession—his thoughtful, generous, and witty articles about many seniors, contemporaries and, relative youngsters: Ohlin (Samuelson, 2002), Haberler (Samuelson, 1990a), Stolper (Samuelson, 1990b), and Kemp (Samuelson, 1993), to mention just a few. As if his seminal contributions to our stock of ideas and techniques were not enough, he constantly helps us refresh and celebrate our folk memories. It is only fitting, and a small repayment in kind, that we should continue to remember and celebrate his achievements.

Notes

1. A physicist who played a crucial role in the development of the hydrogen bomb.
2. See also his response to Ulam quoted above.
3. I would like to indulge in an opportunity to criticize the modern tendency to say "masterful" (whose primary meaning is "domineering, imperious") instead of "masterly" ("showing the ability or skill of a master, expert"). (The definitions are from Webster's New World Dictionary.)

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14

Paul Samuelson's Contributions to International Economics

Kenneth Rogoff

14.1 Introduction

Paul Samuelson's contributions to trade theory and international economics are simply breathtaking. Virtually every undergraduate or graduate student, anywhere in the world, is asked to understand his Stolper–Samuelson and factor–price equalization theorems. These theorems tell us, of course, why trade liberalization tends to benefit the relatively abundant factor of production (skilled labor, in the case of the United States), and why trade in goods can, in many respects, equalize opportunities just as effectively as trade in people and capital. Indeed, it is a very safe bet that whoever the great economist of the twenty-second century turns out to be, he or she will be teaching and reinvigorating ideas Samuelson articulated during the middle part of the twentieth century.

Achieving eternal life in the pantheon of trade giants is already an extraordinary feat. What is perhaps even more remarkable about Samuelson's trade contributions is their vitality in today's globalization debate. Whereas few taxi drivers in Shanghai have ever been to college much less graduate school (something one cannot assume in Cambridge, Massachusetts), they will still understand that trade with the United States is raising the wages of Chinese workers, just as most Americans understand that the country's shrinking manufacturing base has more

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than a little to do with international trade. Indeed, the rising wage differential between skilled and unskilled workers in the United States (and throughout the advanced economies) stands as one of the most contentious and difficult economic and political issues of our day. There is still a great deal of disagreement about what drives this growing differential, and in particular how much is due to globalization, and how much is due to changing technologies that favor skilled labor. Regardless, Samuelson's ideas contributed greatly to building the framework that economists use for asking such questions and for quantifying potential answers.

In this chapter, I will not attempt a technical exposition of Samuelson's core trade theories, since one can find these (at various levels) in any economics textbook, from introductory to advanced (including, of course, the many generations of Samuelson's own celebrated book, first published in 1948). Rather, I will concentrate on highlighting a few main ideas in his work, and saying how they are influencing the contemporary policy debate. My discussion is necessarily selective and omits some areas others might have chosen to focus on. At the end of the chapter, I attach an extensive list of Samuelson's contributions to international economics and international finance.

14.2 International Trade

In his earliest work on trade, including (1), Samuelson used his theorem of revealed preference to show that in a representative agent economy (where everyone is the same), free trade must be welfare improving for all parties. If trade were not welfare improving, a country could choose to continue in autarky, ignoring the rest of the world. This may seem like a trivial result, but with it Samuelson began to lay the foundations of the general equilibrium approach he would ultimately use to prove many other trade theorems. For example, later in (14) he was able to show that whereas trade typically generates winners and losers, there is always, in principle, a way for winners to make sidepayments to compensate the losers so that everyone comes out ahead. (Viner and Lerner had earlier intuited this idea, while Kemp (1962) simultaneously published a closely related analysis.) Even today, this is really the core result around which all trade policy discussions take place. The modern conundrum, of course, is that in practice, it is very hard to find ways to pay off the losers in trade, at least not without creating incentive distortions almost as egregious as the tariff barriers being eliminated in the first place. So all too often, special interests will lobby for trade protection despite the fact that it is a hugely inefficient and expensive way for governments to buy off small groups (see for example Grossman and Helpman, 2002). The most spectacular example, really, has to be the agricultural supports that Organization for Economic Cooperation and Development (OECD) countries lavish on their farmers, making it far more difficult for poor developing countries to export farm products. (One calculation, from a 2003 IMF–World Bank study, showed that the $300 billion dollars rich countries lavish on farm subsidies would be enough to fly every cow in the OECD around the world first class each year, with lots of spending money left over.)

Perhaps the cornerstone of Samuelson's early trade work, however, is his widely celebrated paper (3) with Stolper. This paper was the first to demonstrate the "Heckscher–Ohlin theorem" in a two-good, two-country, two-factor (labor and capital) model. The H–O theorem, of course, shows that with identical technologies at home and abroad, the country with the larger endowment of labor relative to capital should export the labor-intensive good. Obvious? Hardly. Even today, it is amazing how many people seem convinced that China (which, with 1.3 billion people, is clearly a labor-rich country) is going to export everything to everybody as free trade opens up. Admittedly, demonstrating that the Heckscher–Ohlin theorem holds empirically has proven a lot trickier than anyone expected (see, for example, Trefler, 1995), but the bottom line is that it is extremely helpful for thinking about trade between countries with widely different capital-labor ratios. From a policy perspective, the major result of (3) was to confirm the intuitive analysis of Ohlin about who wins and who loses when a country opens up to trade. The answer, as we now well understand, is that the relatively abundant factor gains, and the relatively scarce factor loses, not only in absolute terms but in real terms. Thus if capital is the relatively abundant factor (compared to the trading partner), then an opening of trade will lead the return on capital to rise more than proportionately compared to the price of either good, whereas the wage rate will *fall* relative to the price of either good. Admittedly, many of the simple $2 \times 2 \times 2$ results do not generalize so easily where there are more factors and more goods but they do typically go through in a weaker sense (e.g. Deardorff, 1980), and the broad intuition remains critical to helping us understand how trade impacts welfare.

Whereas Stolper and Samuelson's paper laid the cornerstone of modern trade theory, and contains many of the core results we use today, the real show-stopper in Samuelson's trade contributions has to be his famous factor price equalization theorem (6). Before Samuelson, economists recognized, of course, that factor mobility would help equalize wage rates and returns on capital across countries, at least up to a point. During the latter 1800s as Britain poured money into the rest of the world (with current account surpluses often topping 9 or 10%), capitalists in Britain garnered higher returns on their wealth, while workers in the colonies saw their wage rates rise. Similarly, the great waves of migration from Europe to the Americas in the nineteenth and early twentieth centuries played a significant role in equalizing rates of return on capital between the old world and the new world. Indeed, at times, labor mobility has played a bigger role than capital mobility. But, as is still the case today, international labor and capital mobility is far from perfect, for a host of reasons (see Obstfeld and Rogoff, 1996 for an overview). But is factor mobility the only channel for helping equalize relative wages across countries? Again, leading trade economists understood the possibility that trade in goods might also play a role, if labor-poor countries export capital-intensive goods, and labor-rich countries export labor-intensive goods. Because free trade equalizes relative prices of various goods (up to trade costs, as Samuelson was always careful to emphasize), the result has to be to put equalizing pressure on relative factor returns as well (or so Ohlin and others conjectured). But could one prove this? Samuelson not only proved this result but much more; he developed conditions under which trade in goods could fully substitute for trade in factors themselves. That is, he demonstrated conditions under which trade in goods, and only trade in goods, could fully equalize wages and rates of return on capital across countries! (One important caveat is that the two countries' endowments of capital relative to labor cannot be too different. Otherwise, at least one country will specialize and the logic of the result would break down.) This is one of those rare but powerful insights that just knocks people's socks off when they see it; many were so incredulous they thought that there must be an error in Samuelson's mathematics. But his logic was flawless.

Of course, in practice, one does not typically see factor price equalization, or indeed anything close to it. The 1992 North American Free Trade Agreement between Mexico, Canada, and the United States did not fully equalize wages across the United States and Canada, much less between Mexico and the United States. Numerous factors, including different quality of institutions (Mexico is still a young state where the rule of law is progressively strengthening), different levels of technology and other factors still drive a wedge that keeps Mexican wages far below US levels (despite the fact that there are large immigration flows going on at the same time). One assumption of Samuelson's analysis that is perhaps strained in practice is that labor and capital are perfectly mobile across sectors; in practice, workers often require extensive retraining or relocation, and a great deal of capital is industry specific. Nevertheless, the result gives a critical benchmark for illustrating the extraordinary importance and power of free trade. All in all, Samuelson's results still guide the trade debate, and his results still provide the benchmark for the subsequent literature. Indeed, this author has no doubt that if and when interplanetary trade ever commences (say, via radio beam exchanges of technological blueprints and music), economists of the day will quickly find themselves trotting out expositions of Samuelson's 1948 paper (6).

Though the contribution is more methodological than practical, one can hardly survey Samuelson's contributions to trade without mentioning his clever (11) device of modeling trade costs as "iceberg costs" so that when a good is shipped from country X to country Y, a fraction of it dissipates in transit costs. This simple yet elegant device allows trade economists to introduce trade frictions while keeping their models simple and tractable. Virtually every other trade paper today uses it in some form, and the trick has been widely applied in other fields as well. A small thing, perhaps, but this is precisely the kind of clever device that helps propel whole new fields of inquiry.

Although Samuelson made many other critical contributions to trade theory, perhaps the next truly giant step was (31) his 1980 paper with (much junior) MIT colleagues Rudiger Dornbusch and Stanley Fischer, in which they (henceforth DFS) developed a so-called Ricardian model of trade with a continuum of goods. By "Ricardian" model, of course, they meant a model with only one factor of production (for Ricardo, that was usually labor), where differences in technology drive comparative advantage. This is in contrast to the Heckscher–Ohlin inspired framework developed in Stolper and Samuelson, where there are two factors (labor and capital) and (in the classic setup) countries have identical technologies. In a Ricardian model, one cannot think of countries as exporting, say, labor-intensive goods, because that is all any country has. Rather, trade arises due to different technologies (which could in turn be traced to different endowments of land or weather). Of course, a Ricardian model is all one needs to develop the theory of comparative advantage, which Samuelson famously quipped (including in his text) is one of the few results in economics that is simultaneously true and not obvious. The theory of comparative advantage also explains why xenophobic politicians should not worry that China will some day produce everything in the world. Rather, the theory tells us that China will only export what it is *relatively* good at even if, some day, it really does gain an absolute advantage in producing everything. People who have not taken trade theory often seem stunned when they hear the theory of comparative advantage. But, of course, most people in our highly specialized society have come to terms with the principle of comparative advantage in their daily lives (for example, even if a high-paid investment banker is very good at doing her shopping, she may find it advantageous to pay someone to do it in her stead, so as to be able to devote more time to highly paid investment banking activities).

Prior to DFS, the Ricardian approach had been dormant for years, not because the assumptions were so unreasonable, but because the model had been viewed as intractable for all but illustrative purposes. Through the brilliant device of introducing a continuum of goods, DFS were able to enormously simplify the standard Ricardian model, and allow one to do comparative statics exercises with an elegance that had previously seemed impossible. At first, the DFS model was greatly admired, but did not lead to any flowering of new research. In recent years, however, the research line following the DFS model has become an explosion. The DFS model has become the starting point for a number of applied papers (e.g. see Copeland and Taylor, 1994). In addition, the DFS model has formed the basis for an important and exciting resurgence of empirical work in trade (e.g. see Eaton and Kortum, 2002; Kehoe and Ruhl, 2002; and Kraay and Ventura, 2002; Yi, 2003; Ghironi and Melitz, 2005). One interesting application is Feenstra and Hanson (1996), who apply the continuum-of-goods model to show how direct foreign investment flows from a capital-abundant country to a labor-abundant economy may actually increase the skill premium in both countries. Whereas the migrating industries may be skill-intensive from the point of view of the recipient country, they might not be so from the point of view of the country losing the industries—a very Samuelson-like result!

14.3 International Finance

Samuelson has also made important contributions to the field of international finance. First and foremost, in (15) he is codeveloper of the famous Balassa–Samuelson theorem (Obstfeld and Rogoff, 1996, note Harrod's (1933) contribution as well, and I will follow their convention here). Simply put, the Harrod–Balassa–Samuelson (H–B–S) theorem predicts that fast-growing countries will tend to have appreciating real exchange rates, and that rich countries will have high real exchange rates relative to poor countries. Underlying the H–B–S model is a fact that Samuelson had emphasized throughout his early trade writings: trade is costly, and for some goods, it is prohibitively costly. Second, the analysis assumes that fast-growing countries tend to see faster rates of productivity improvement in their (highly) traded goods industries than in their (relatively) nontraded goods industries. Assuming that labor and capital are mobile across sectors, factor prices will get bid up by the fast-growing traded goods sector. But this, in turn, will make production in the nontraded goods more expensive, and bid up prices there. Then assuming (a third assumption) that traded goods prices tend to be equalized across countries, higher nontraded goods prices must translate into a higher real exchange rate. Simply put, as a poor country gets better at manufacturing, haircuts and hotels will have to become more expensive as the general level of wages in the economy starts to rise. The H–B–S model is useful because it gives a framework for say, trying to understand why the price of McDonald's Big Mac hamburger is five dollars in Switzerland but just over one dollar in China. Again, like the Heckscher–Ohlin theorem, the H–B–S theorem is at best a loose description of reality, since many complex forces work together to create price differentials, including pricing to market, slow adjustment of factors across sectors, sticky prices, etc. Also, in a world where many countries have a degree of monopoly power in the goods they produce, the H–B–S result can also become weaker or even be stood on its head (Fitzgerald, 2003). Nevertheless, it is a very useful benchmark.

Indeed, the logic of H–B–S is arguably the central idea behind the International Comparison of Prices project that began in the 1950s (see Rogoff, 1996) which later culminated in the celebrated Heston–Summers comparisons of incomes and prices across countries (see Summers and Heston, 1991). The Heston–Summers data base, of course, attempts to compare different countries' incomes in terms of a common relative price matrix (the United States). For example, if one measures the relative size of Japan and China using market exchange rates and national prices, then the Chinese economy is only 1/3 the size of Japan's. However, an alternative way to compare these economies uses "Purchasing Power Parity" exchange rates, which are constructed to set equal, on average, the values of identical goods in different countries (such as the Big Mac). Using Purchasing Power Parity exchange rates, rather than market rates, China is twice the size of Japan (in this case, arguably a better description of its influence in the world). The Heston–Summers data set has been very important in empirical research on growth since it allows much more meaningful comparisons across countries than do national income accounts. Increasingly, it has also become important in policy circles as well (e.g. the International Monetary Fund *World Economic Outlook* projections for global and regional growth are all based on purchasing power parity aggregations that are motivated by very similar considerations as H–B–S). (Robert Summers, of course, is Paul Samuelson's brother, having once changed his name.)

Another area of international finance where Samuelson's work remains widely cited and enormously influential is in studies of the "Transfer Problem," famously debated in the early 1920s by Keynes and Ohlin. The central question of the Keynes–Ohlin debate was whether the vast wartime reparations being demanded from Germany would lead to a secondary burden due to induced price effects. In (10) and (11), Samuelson basically settled the issue, showing that neither of them were quite right. On the one hand, Samuelson showed that from a policy perspective, Keynes was right in the sense that, under reasonable assumptions, the real cost of Germany's postwar reparations would likely be magnified by price effects. Lower wealth in Germany would reduce domestic demand for German goods, but higher wealth abroad would increase demand for its goods. However, since Germans tend to prefer their own tradeable goods to imports (a home bias), they consume a disproportionately large amount of them. So as Germany transferred money to the Allies, higher foreign demand for its goods would not fully substitute for reduced domestic demand and the relative price of German goods would fall. On the other hand, Samuelson showed that Ohlin was right from a methodological viewpoint, in that income effects are what matter most. To understand how the wealth transfer would impact prices, one needed to know who is giving money and who is receiving, and how, at the margin, the two groups will tend to adjust to these income changes. Samuelson's work on the transfer problem is enormously influential today in theory and policy. For example, transfer problem type analysis underlies the analysis of Obstfeld and Rogoff (2000, 2005). Their analysis strongly suggests that when the US trade deficit finally closes up from its astounding current 6 percent of GDP value, the real value of the trade-weighted dollar will almost surely plummet. Foreign demand for American goods will rise, but not by as much as American demand will fall, and foreign demand will not substitute at all in the case of nontraded goods. Hence, at least until factors can migrate across sectors (which will take years if not decades), large relative price movements are needed, which in turn implies large movements in exchange rates if central banks are stabilizing overall inflation rates.

14.4 Conclusions

It is impossible in this brief space to do justice to Paul Samuelson's stunning contributions to international economics, or to adequately characterize their profound policy impact. I trust, however, that the reader will at least gain a flavor of the remarkable span of ideas this man has generated, and the profound policy influence he has had. Finally, I have not even mentioned Samuelson's role as a teacher in trade; many of us in this volume have been his students.

Paul A. Samuelson's Main Articles on International Trade and Finance

1. "Welfare economics and international trade" (*The American Economic Review*, June 1938)
2. "The gains from international trade" (*Canadian Journal of Economics and Political Science*, May 1939)
3. "Protection and real wages," with W.F. Stolper (*The Review of Economic Studies*, November 1941)
4. Review of Jacob L. Mosak, *General Equilibrium Theory in International Trade* (*The American Economic Review*, December 1945)
5. "Disparity in postwar exchange rates" (Seymour Harris, ed., *Foreign Economic Policy for the United States*, Harvard University Press, 1948)
6. "International trade and equalization of factor prices" (*Economic Journal*, June 1948)
7. "International factor-price equalization once again" (*Economic Journal*, June 1949)
8. "A comment on factor-price equalization" (*The Review of Economic Studies*, February 1952)
9. "Spatial price equilibrium and linear programming" (*The American Economic Review*, June 1952)
10. "The transfer problem and the transport costs: the terms of trade when impediments are absent" (*Economic Journal*, June 1952)
11. "The transfer problem and the transport costs: analysis of effects of trade impediments" (*Economic Journal*, June 1952)
12. "Prices of factors and goods in general equilibrium" (*The Review of Economic Studies*, 1953–1954)
13. "Intertemporal price equilibrium: a prologue to the theory of speculation" (*Weltwirtschaftliches Archive*, December 1957)
14. "The gains from international trade once again" (*The Economic Journal*, December 1962)
15. "Theoretical notes on trade problems" (*The Review of Economics and Statistics*, May 1964)

16. "Equalization by trade of the interest rate along with the real wage" (*Trade, Growth and the Balance of Payments*, in honor of Gottfried Haberler, Rand McNally, 1965)
17. "Summary of factor-price equalization" (*International Economic Review*, October 1967)
18. "An exact Hume-Ricardo-Marshall model of international trade" (*Journal of International Economics*, February 1971)
19. "On the trail of conventional beliefs about the transfer problem" (J. Bhagwati et al. (eds), *Trade, Balance of Payments, and Growth: Papers in International Economics in Honor of Charles P. Kindleberger*, Amsterdam, North-Holland Publishing Co., 1971)
20. "Ohlin was right" (*Swedish Journal of Economics*, 73(4), 1971, 365–384)
21. "Heretical doubts about the international mechanisms" (*Journal of International Economics*, 2(4), 1972, 443–454)
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23. "Deadweight loss in international trade from the profit motive?" (C. Fred Bergsten and William G. Tyler (eds), *Leading Issues in International Economic Policy: Essays in Honor of George N. Halm*, Lexington, Mass., D.C. Heath and Co., 1973)
24. "Equalization of factor prices by sufficiently diversified production under conditions of balanced demand" (*International Trade and Finance: Essays in Honor of Jan Tinbergen*, Willy Sellekaerts, ed., London, Macmillan, 1974)
25. "Trade pattern reversals in time-phased Ricardian systems and intertemporal efficiency" (*Journal of International Economics*, 5, 1974, 309–363)
26. "Illogic of Neo-Marxian doctrine of unequal exchange" (D. A. Belsley, E. J. Kane, Paul A. Samuelson, Robert M. Solow (eds), *Inflation, Trade and Taxes: Essays in Honor of Alice Bourneuf*, Columbus, Ohio State University Press, 1976)
27. "Interest rate equalization and nonequalization by trade in Leontief–Sraffa models" (*Journal of International Economics*, 8, 1978, 21–27)
28. "Free trade's intertemporal pareto-optimality" (*Journal of International Economics*, 8, 1978, 147–149)
29. "America's interest in international trade" (*New England Merchants Company, Inc.*, 1979 Annual, pp. 4–5)
30. "A corrected version of Hume's equilibrating mechanism for international trade" (John S. Chipman and Charles P. Kindleberger (eds), *Flexible Exchange Rates and the Balance of Payments: Essays in Memory of Egon Sohmen*, Amsterdam: North-Holland, 1980, pp. 141–158)
31. "Comparative advantage, trade and payments in a Ricardian model with a continuum of goods," with Rudiger Dornbusch and Stanley Fischer (*American Economic Review*, 95(2), 1980, 203–224)
32. "To protect manufacturing?" *Zeitschrift für die gesamte Staatswissenschaft* (*Journal of Institutional and Theoretical Economics*, Band 137, Heft 3, September 1981, 407–414)

33. "Summing up on the Australian case for protection" (*The Quarterly Journal of Economics*, 96(1), 1981, 147–160)
34. "Justice to the Australians" (*The Quarterly Journal of Economics*, 96(1), 1981, 169–170)
35. "Japan and the world at the century's end" (*NEXT Magazine*, August 1984, 4–15, Original English version provided for translation into Japanese)
36. "The future of American industry in a changing economy" (*The Journalist*, Fall 1984, 3–5, 19)
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38. "US economic prospects and policy options: impact on Japan–US relations" (Ryuzo Sato, and John A. Rizzo (eds), *Unkept Promises, Unclear Consequences: US Economic Policy and the Japanese Response*, Cambridge University Press, 1989)
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40. "Factor-price equalization by trade in joint and non-joint production" (*Review of International Economics*, 1(1), 1992, 1–9)
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42. "The past and future of international trade theory" (Jim Levinsohn, Alan V. Deardorff, and Robert M. Stern (eds), *New Directions in Trade Theory*, Ann Arbor, MI: The University of Michigan Press, 1995, pp. 17–23)
43. "Economic science grapples with dilemmas of international finance"
44. "Recurring quandaries in international trade"
45. "A Ricardo-Sraffa paradigm comparing gains from trade in inputs and finished goods" (*Journal of Economic Literature*, 39 (4), 2001, 1204–1214)
46. "The state of the world economy" (Paul Zak, and Robert A. Mundell (eds), *Monetary Stability and Economic Growth: A Dialog Between Leading Economists*, Edward Elgar Publishing, 2003)
47. "Pure theory aspects of industrial organization and globalization" (*Japan and the World Economy*, 15 (1), 2003, 89–90)
48. "Where Ricardo and Mill rebut and confirm arguments of mainstream economists supporting globalization" (*Journal of Economic Perspectives*, 18 (3), Summer 2004, 135–146)

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15

Protection and Real Wages: The Stolper–Samuelson Theorem

Rachel McCulloch

Second only in political appeal to the argument that tariffs increase employment is the popular notion that the standard of living of the American worker must be protected against the ruinous competition of cheap foreign labor. . . . Again and again economists have tried to show the fallaciousness of this argument.

Thus begins Stolper and Samuelson's (1941) analysis of the effect of protection on real wages, a landmark contribution to the modern theory of international trade. The central result, now known as the Stolper–Samuelson theorem, is that "international trade necessarily lowers the real wage of the scarce factor expressed in terms of any good." The paper signals a transition in the debate among international economists concerning the welfare consequences of free trade, from largely verbal reasoning toward the use of formal general-equilibrium models. Derived in a simple framework of two homogeneous factors, each freely mobile between two domestic industries, the Stolper–Samuelson theorem is striking because it demonstrates that a productive factor's ability to relocate from an import-competing to an export industry does not prevent a loss in real income due to expanded trade. Moreover, it shows that the sharp redistributive consequences of trade do not depend on tastes or expenditure patterns.

Chad Bown and Gary Chamberlain provided helpful comments.

15.1 Birth of a Theorem

According to Samuelson (1994), the collaboration arose from Wolfgang Stolper's efforts to reconcile the new general-equilibrium trade theory with the work of earlier economists: "How can Haberler and Taussig be right about the necessary harm to a versatile factor like labor from America's tariff, when the Ohlin theory entails that free trade must hurt the factor that is scarce relative to land?" Stolper's friend and junior colleague, first the sounding board, eventually became "the midwife, helping to deliver Wolfie's brain child." The infant prospered.

Earlier analyses of the effect of free trade on real wages had emphasized the implications of trade for productive efficiency. In the long run, free trade would increase demand for the country's comparative-advantage goods and thereby shift employment toward the domestic industries where labor is most productive. The classical economists had typically assumed a one-factor model or, equivalently, that productive factors were used in unvarying proportions both within and across industries. In either case, trade could have no redistributive consequences within a country. Although Stolper and Samuelson's teachers and contemporaries recognized the implications of changing factor proportions for income shares, their analyses were based on a partial-equilibrium model of a protected industry. While elimination of tariffs might cause the money wage to fall, the resulting reduction in the prices of the goods workers buy with their wages was presumed to be larger. The *real* wage was thus anticipated to rise, at least in terms of imported goods and most likely overall, though the effect would depend on the relative importance of imported and exported goods in workers' total expenditure.[1]

The general-equilibrium trade theory introduced by Eli Heckscher and Bertil Ohlin opened a new line of inquiry focusing on differences in relative factor intensity across industries and differences in relative factor abundance across countries.[2] Stolper and Samuelson adopted this approach and coined the now standard terminology "Heckscher-Ohlin theorem" to refer to the proposition that "each country will export those commodities which are produced with its relatively abundant factors of production, and will import those in the production of which its relatively scarce factors are important."

15.2 The Stolper–Samuelson Analysis

Formalizing the logic of the Heckscher–Ohlin model, Stolper and Samuelson assumed two homogeneous goods A and B, each produced under constant returns to scale using labor L and capital K , but with good A using more capital relative to labor than good B. The two factors were assumed fixed in total supply but freely mobile between the country's two industries:

$$L_A + L_B = \bar{L} \quad \text{and} \quad K_A + K_B = \bar{K}$$

The two full-employment conditions together imply that the economy's overall capital–labor ratio $\bar{k}$ can be expressed as the weighted average of the capital–labor ratios k_A and k_B used in the two industries:

$$\lambda_A k_A + \lambda_B k_B = \bar{k},$$

where $\lambda_A = L_A/\bar{L}$ and $\lambda_B = L_B/\bar{L}$ are the shares of the total labor supply used in the two industries, $\lambda_A + \lambda_B = 1$. Thus, as the production mix moves toward specialization in good A and λ_A approaches unity, the capital–labor ratio used in A production must fall toward $\bar{k}$. Factor mobility and perfect competition together imply that the equilibrium factor returns w and r are equal across industries, and the return to each factor is equal to the value of its marginal product in that industry:

$$w = p_A \frac{\partial A}{\partial L_A} = p_B \frac{\partial B}{\partial L_B}, \qquad r = p_A \frac{\partial A}{\partial K_A} = p_B \frac{\partial B}{\partial K_B}.$$

The ratio of the marginal physical products of the two factors must therefore be equal across industries:

$$\frac{\partial A/\partial K_A}{\partial A/\partial L_A} = \frac{\partial B/\partial K_B}{\partial B/\partial L_B}.$$

Stolper and Samuelson used an Edgeworth–Bowley box diagram to represent the model geometrically. Each point in the box represents a feasible full-employment allocation of the factors between the two industries.[3] Points along the contract curve indicate alternative efficient allocations of the two factors between industries and thus alternative efficient output combinations for the economy, with a one-to-one correspondence between points on the contract curve and points on the economy's production possibility frontier. At the corners of the box representing specialization in one of the two products, the capital–labor ratio in the industry of specialization must equal the country's overall capital–labor ratio. In between, where both goods are produced, the capital–labor ratios in the two industries change systematically, with both falling monotonically as the economy moves from production only of labor-intensive B toward production only of capital-intensive A. As a consequence of the changing capital–labor ratios in the two industries, the physical marginal product of labor must fall, and the physical marginal product of capital must rise, in both industries as the economy produces more A and less B.

The actual output combination produced depends on the relative price p_A / p_B. Although their original motivation was to shed new light on the effect of protection on wages, Stolper and Samuelson avoided further consideration of the details of trade by focusing on the resulting change in the domestic relative price of the goods.[4] Their result is thus applicable to a change in relative price that occurs for any other reason. Trade would reduce the relative price of the import-competing good, which by the Heckscher–Ohlin theorem was assumed to be labor-intensive B for the United States, a labor-scarce country.[5] The lower relative price of good B would cause a shift in the economy's production toward good A—a movement along the production possibility frontier and the contract curve in the Edgeworth–Bowley box. If each industry were to use the same factor proportions as before, the change in output mix would raise the country's total demand for capital and reduce its total demand for labor. Given fixed total factor supplies and full employment of both factors before and after the rise in relative price of good A, the new output mix would thus be feasible only if both industries were now to employ a lower capital–labor ratio, or equivalently, if there was a rise in the rental-wage ratio facing the firms in both industries. These lower capital–labor ratios imply a lower marginal physical product of labor in both industries and thus an unambiguously lower real wage (and higher real rental) measured in terms of *either* good. This outcome is independent of the pattern of consumption.

15.3 Stolper–Samuelson and the Simple General–Equilibrium Model

Although the Stolper–Samuelson argument based on varying factor demand and fixed factor supply is intuitively appealing, their key result does not actually require fixed factor supplies. An alternative proof hinges on the observation that with constant returns and perfect competition, both industries can maintain positive output only if both yield equal (zero) economic profits. As neatly laid out in Jones (1965),[6] the price of each good produced must in equilibrium be equal to its unit production cost:

$$p_A = a_{LA}(w/r)w + a_{KA}(w/r)r$$

$$p_B = a_{LB}(w/r)w + a_{KB}(w/r)r,$$

where $a_{ij}(w/r)$ indicates the cost-minimizing input of factor i in producing one unit of good j. With the assumption that the two industries differ in relative factor intensity, and given the money prices of the two goods, these two equations can be solved to obtain unique equilibrium factor rewards r and w consistent with production of both goods, as well as the real returns expressed in terms of either good.[7]

Jones derived corresponding "equations of change" that show the comparative statics of the model. To restore equilibrium, any change in the price of either good must be matched by an equal change in its unit cost of production. The proportional change in each good's production cost can be expressed as a weighted average of the proportional changes in the factor rewards, with a larger weight on the change in wages for the labor-intensive good:

$$\theta_{LA}\hat{w} + \theta_{KA}\hat{r} = \hat{p}_A$$

$$\theta_{LB}\hat{w} + \theta_{KB}\hat{r} = \hat{p}_B,$$

where θ_{ij} indicates factor i's share in the total cost of producing good j and $\hat{x}$ is the proportional change in x. For the case considered by Stolper and Samuelson, where trade raises the relative price of capital-intensive good A, these conditions imply:

$$\overline{w} < \overline{p}_B < \overline{p}_A < \bar{r}.$$

Jones called this relationship the magnification effect—a rise in the relative price of a good is accompanied by a *magnified* increase in the equilibrium return to the factor used intensively in its production and a decrease in the real return to the other factor.

Jones's reformulation of the Stolper–Samuelson theorem highlights its broad applicability. In the context of the basic model of two goods, two factors freely mobile between industries, constant returns to scale, and diversified production, Jones's version shows the "magnified" consequences for equilibrium real factor prices of *any* change in the relative price of the goods. Regardless of its cause, and even in a closed economy, a fall in the relative price of the labor-intensive good must be accompanied by a decrease in the corresponding equilibrium real wage and a rise in the real return to the other factor. The redistributive effect of adding or removing a tariff, or of moving toward or away from autarky, is a special case.

The proof based on equality of cost and production price also shows that the theorem holds even when each industry uses factors in fixed proportions, that is, when the production isoquants are L-shaped rather than smoothly curved, as had been assumed by Stolper and Samuelson.[8] With additional assumptions (free trade, no factor-intensity reversal, a second country with the same production technology), Samuelson's factor-price equalization theorem follows directly from the same formulation of the model. When free trade equalizes product prices between countries, factor rewards in each country must satisfy the same set of equations (unit cost must equal price for each of the two goods). This argument is similar in spirit to Lerner's (1952) geometric proof of factor-price equalization.[9]

15.4 Stolper–Samuelson's Seminal Role

As with other path-breaking papers, "Protection and Real Wages" did not immediately find favor with journal editors. Howard Ellis and Paul Homan of the *American Economic Review* read the paper and agreed (as stated in Homan's rejection letter to Samuelson) that it "is a brilliant theoretical performance" but also "a very narrow study in formal theory, which adds practically nothing to the literature," not to mention "practically a complete 'sell-out' "—this no doubt because the key result might offer intellectual comfort to protectionists.[10] Still, a positive response from Ursula Hicks at the *Review of Economic Studies* came less than half a year later—and the rest is history.[11]

The huge literature built upon the Stolper–Samuelson theorem has proceeded in many directions, with contributors constituting a veritable Who's Who of international trade theory.[12] Theoretical papers, including several by Samuelson, have systematically explored the robustness of the result by relaxing each of the assumptions used in the original derivation.

One important strand focuses on a question that Stolper and Samuelson raised in their paper but did not subject to detailed analysis: how well does the theorem generalize beyond the special world of two goods and two factors? As summed up in Wilfred Ethier's (1984) survey of this literature, the Stolper–Samuelson theorem survives, but in a "nonexclusive" sense. With more goods and more factors, *at least one* factor stands to gain unambiguously from trade, and *at least one* factor stands to lose unambiguously. The basic message of the original theorem is thus maintained: even when free trade raises national income overall, some factors may lose in the absence of compensation. But identifying specific gainers and losers becomes more complex, and intuition based on the two-by-two case may prove to be an unreliable guide; Edward Leamer (1994) demonstrates the failure in a three-by-three world of several plausible generalizations of the two-by-two version of Stolper–Samuelson. For example, it is not necessarily true that a country's "scarce" factors will lose from trade.

Another direction of inquiry returns to the original sharp focus of the theorem and asks how well its predictions can explain observed behavior in the political sphere. In voting or lobbying, do factor owners act as if they believe the Stolper–Samuelson theorem? Stephen Magee (1980) showed that the rival "specific factors" model, with two immobile industry-specific factors and one mobile factor, is more consistent with the lobbying positions of labor and capital. In retrospect, this result should not be surprising. The Stolper–Samuelson theorem is based on perfect factor mobility within a country, and thus its implications are best understood as long-term tendencies. Even assuming that factor owners seek to maximize the present discounted value of their lifetime earnings, the more immediate impact is likely to dominate.[13] Later work with William Brock and Leslie Young (Magee, Brock, and Young, 1989), which modeled protection as endogenous, again found that the specific-factors model explains short-run lobbying (time series data), but that Stolper–Samuelson works better in explaining patterns of protection across countries (cross-national data).[14] A new generation of scholars has continued the debate, for example, Eugene Beaulieu and Christopher Magee (2004).[15]

15.5 An Essential Tool for Economists

Notwithstanding the hundreds or perhaps by now thousands of scholarly contributions that the Stolper–Samuelson paper has inspired,[16] its real significance may be somewhat different. By linking output prices to equilibrium factor rewards, Stolper and Samuelson filled an important gap in the general-equilibrium model. Together with the other key elements of the Heckscher–Ohlin model, the stripped-down basic version of Stolper and Samuelson has become an essential part of the intellectual toolkit of every international economist and is now found in every international trade textbook.[17] Like the supply and demand curves of partial-equilibrium analysis, the simple Heckscher–Ohlin model provides the first back-of-the-envelope attack on an endless variety of questions. The framework thus continues to be used to cast light on important policy issues relating to income distribution. For example, Lawrence and Slaughter (1993) chose the Stolper–Samuelson framework to contrast price changes due to increased international competition with biased technical change as alternative explanations of an increasing gap between the wages of skilled and unskilled workers. As long as economists maintain a lively interest in the division of national income among factors of production, Stolper and Samuelson will be there. The end is not in sight.

Notes

1. Stolper and Samuelson provide illustrative quotations and references. One quote from Haberler rejects the possibility of equalization of wages across countries unless labor is internationally mobile. As of 1941, Stolper and Samuelson agreed, noting that "there will be a tendency—necessarily incomplete—toward an equalisation of factor prices" due to trade. A few years later, however, Samuelson (1948, 1949) would show that, under stipulated conditions, free trade alone is sufficient to equalize factor prices. A footnote to Samuelson (1949) indicates that Abba Lerner presented essentially the same result in a 1933 paper prepared for a seminar at the London School of Economics. Perhaps due to Samuelson's acknowledgment, the paper was finally published as Lerner (1952).
2. Ohlin's landmark treatise was published by Harvard University Press in 1933. The basic work by Heckscher and by his student Ohlin had been available a decade earlier, but only in Swedish. Heckscher's seminal 1919 article finally appeared in English translation in 1950 in a collection of fundamental contributions to the theory of international trade published by the American Economic Association.
3. This appears to be the first use of the Edgeworth–Bowley box to analyze efficient production—earlier uses of the diagram had dealt with efficiency in exchange.
4. Samuelson (1939) used the same simplification in examining a country's gains from trade.
5. This was of course long prior to Leontief (1954) and illustrates the ready acceptance by international economists of the empirical validity of the Heckscher–Ohlin theory.

6. Ronald Jones was Stolper's student at Swarthmore and then Samuelson's student at MIT.
7. Factor supplies do enter by the back door. The required equilibrium condition that the price of each good must equal its production cost applies only if both goods are produced at home, that is, if the country's factor endowment lies within its "cone of diversification."
8. The production possibility frontier in this case is made up of two linear segments, and the output combination at their intersection is the only one consistent with full employment of both factors. This output combination is consistent with a range of relative prices and corresponding factor rewards, even though the capital–labor ratios used in producing the goods do not change.
9. Lerner's paper and a comment by I. F. Pearce in the same issue of *Economica* introduced the Lerner–Pearce diagram into general use. The Lerner–Pearce diagram can be used to prove the Stolper–Samuelson theorem, as demonstrated in Deardorff (1994). Because this proof does not require calculus, it is now used in some undergraduate textbooks. Although the proof appears in my 1967 lecture notes from Harry Johnson's course in international trade theory at the University of Chicago, I have been unable to track down its earliest appearance in the literature.
10. As published, the paper ends with an effort to defuse any potential "political ammunition for the protectionist" by noting that "it is always possible to bribe the suffering factor . . . so as to leave all factors better off." Does a bigger pie really allow everyone to enjoy a larger slice? It is difficult to identify even a single case in which losers from a government policy choice have received full compensation, and in fact proposed changes in trade policy are often rationalized in terms of their anticipated redistributive consequences.
11. Both letters are reproduced in Deardorff and Stern (1994). Young economists coping with today's publish-or-perish environment may take heart from the initial rejection of this iconic work but may weep with envy over the speed with which the paper went from inspiration to print.
12. Several key contributions are reprinted in Deardorff and Stern (1994). The volume's annotated bibliography lists many others.
13. Robert E. Baldwin (1984) examined an intermediate model in which sector-specific labor skills give rise to labor rents. As a consequence, workers may not find it worthwhile to move between industries when relative output prices change.
14. Magee (1994) provides a brief summary of the results.
15. In this case it is literally a new generation; Christopher Magee is the son of Stephen P. Magee. He is also the student and coauthor of Robert E. Baldwin, an early and influential contributor to the literature on the political economy of trade policy.
16. The inspiration has evidently continued into the twenty-first century. Econlit reports (as of March 14, 2005) mentions of Stolper–Samuelson in forty-one new

items published since 2000. This is an underestimate of continuing impact, as political scientists are making increasing use of the result.

17. Given its enduring influence on subsequent economic analysis, the Stolper–Samuelson theorem may be appropriately regarded as a Schumpeterian innovation. Stolper and Samuelson were both students of the legendary Joseph Schumpeter.

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16

Samuelson and the Factor Bias of Technological Change: Toward a Unified Theory of Growth and Unemployment

Joseph E. Stiglitz

It is a great pleasure for me to be able to write this chapter in honor of Paul's ninetieth birthday. On such occasions, one's students traditionally write an essay inspired by one's work. Paul's long and prolific career—which continues almost unabated—makes this both an easy and a difficult task: easy, because on almost any subject one reflects upon, Paul has made seminal contributions; all of MIT's students—indeed, much of the economic profession for the past half century—has been simply elaborating on Paul's ideas. But by the same token, the task is difficult: there are so many of his ideas the elaboration of which remain on my research agenda, forty two years after leaving MIT, it is hard to make a choice.

Take, for instance, his development of the overlapping generations model, which has played such a central role in macroeconomics. Social security is one of the central issues facing American public policy, and his model remains the central model for analyzing theoretically the consequences of various proposals. Obviously, the results obtained in that

This essay was written on the occasion of Paul Samuelson's ninetieth birthday. I had the good fortune of being asked to write a preface for a book in honor of Paul Samuelson (*Paul Samuelson: On Being an Economist*, by Michael Szenberg, Aron Gottesman, and Lall Ramrattan. Jorge Pinto Books: New York, 2005), in which I describe my days as a student of Paul Samuelson and my huge indebtedness to Paul—and the indebtedness of my fellow students and the entire economics profession. I will not repeat here what I said there. The research on which this essay is based was supported by the Ford, Mott, and MacArthur Foundations, to which I am greatly indebted. The influence of my teachers, Paul Samuelson, Robert Solow, and Hirofumi Uzawa, as well as those with whom I discussed many of these ideas more than forty years ago, including Karl Shell, David Cass, and George Akerlof, should be evident. I am indebted to Stephan Litschig for excellent assistance. I am also indebted to Luminita Stevens for the final review of the manuscript.

model are markedly different from—and far more relevant than—those in an infinitely lived representative agent model.

Recently, I used the model in a quite different context,[1] to study the impact of capital market liberalization, one of the central issues under debate in the international arena. Again, the results are markedly different from those obtained in the perfect information, perfect capital markets, representative agent models, where liberalization allows a country facing a shock to smooth consumption: it helps stabilize the economy. The evidence, of course, was overwhelmingly that that was not the case, and using a variant of the overlapping generations model, one can understand why. Without capital market liberalization, a technology shock, say, to one generation is shared with succeeding generations, as savings increase, wages of successive generations increase, and interest rates fall (in response to the increased capital stock). But with capital market liberalization, the productivity shock may simply be translated into increased income in the period, and increased consumption of the lucky generation. By the same token, capital market liberalization exposes countries to external shocks from the global capital market. I had thought of using this occasion to elaborate on the life-cycle model in a rather different direction: a central feature of the standard life-cycle model and some of the subsequent elaborations (such as Diamond[2]) is the possibility of oversaving: if capital is the only store of value, then the demand for savings by households may be such that the equilibrium interest rate is *beyond* the golden rule, and the economy is dynamically inefficient. Introducing a life-cycle model with land, however, can have profound implications. Take the case, for instance, with no labor force growth and no technological change; being beyond the golden rule would imply a negative real interest rate, which would, in turn, mean an infinite value to land. Obviously, this cannot be an equilibrium. The problems of oversaving, on which so much intellectual energy was spent in the 1960s, simply cannot occur when there is land (and obviously, land does exist). Samuelson was the master of simple models that provided enormous insights, but the result shows the care that must be exercised in the use of such models: sometimes, small and realistic changes may change some of the central conclusions in important ways.

But I have chosen in this chapter to focus on another topic on which I remember so vividly Paul lecturing: endogenous technological change. Long—some two decades—before the subject of endogenous growth theory (which really focuses on growth theory where the rate of technological change is endogenous) became fashionable, Paul Samuelson, Hirofumi

Uzawa, and Ken Arrow and their students were actively engaged in analyzing growth models with endogenous technological progress, either as a result of learning by doing[3] or research.[4]

Of particular interest to Paul was the work of Kennedy[5] and Weizacker[6] (and others) on the *bias* of technological progress—whether it was labor or capital augmenting. Earlier, Kaldor[7] had set forth a set of stylized facts, one of which was the constancy of the capital output ratio. It was easy to show that that implied that technological change was labor augmenting. But what ensured that technological change was labor augmenting—if entrepreneurs had a choice between labor and capital augmentation? These authors had posited a trade-off between rates of capital and labor augmentation, and shown an equilibrium with pure labor augmentation.

Contemporaneously, economic historians, such as Salter[8] and Habakkuk,[9] had discussed economic growth arguing that it was a *shortage* of labor that motivated labor saving innovations, for example, in America. Of course, in standard neoclassical economics, there is no such thing as a shortage—demand equals supply. One might be tempted to say what they meant to say was "high" wages. But how do we know that wages are high? What does that even mean? Of course, with productivity increases, wages are high, but not relative to productivity.

Once we get out of the neoclassical paradigm, of course, markets may be characterized by "tightness" or "looseness." There can be unemployment. Firms may have a hard time finding employees. Moreover, if the unemployment rate is low, workers are more likely to leave, so firms face high turnover costs; what matters is not just the wage, but total labor costs.[10] Worse still, if the unemployment rate is low, workers may shirk—the penalty for getting caught is low.[11] Some economies are plagued by labor strife, again increasing the total cost of labor. One of the motivations for the model below was to try to capture (even if imperfectly) some aspects of this as affecting the *endogenous direction* of technological progress.

There is another motivation for this chapter. The early beginnings of growth theory derive from the basic model of Harrod and Domar, where there was a fixed capital-output ratio, a. With savings, s, a fixed fraction of output (income), Y,

$$I = sY = \mathrm{d}K/\mathrm{d}t \tag{16.1}$$

where K is the capital stock, so the rate of growth of capital is

$$\mathrm{dln}\ \mathrm{K}/\mathrm{d}t = sY/K = s/a. \tag{16.2}$$

Moreover, as machines become more efficient, each machine requires less labor, so the number of jobs created goes up more slowly than the capital stock. If

$$L/Y = b \tag{16.3}$$

is the labor requirement per unit output, then b/a is the labor required per unit capital, and job growth is

$$\text{dln}\, L/\text{d}t = s/a - \beta - \alpha \tag{16.4}$$

where

$$-\text{dln}\, b/\text{d}t = \beta \tag{16.5}$$

$$\text{dln}\, a/d\text{t} = \alpha \tag{16.6}$$

s/a was sometimes referred to as the warranted rate of growth, what the system would support. Once technological change was incorporated, the warranted rate of growth is modified to $s/a - \beta - \alpha$.

By contrast, labor was assumed to grow at an exogenous rate, n. The problem was that, in general, n was not equal to $s/a - \beta - \alpha$.[12]

If (in the model without technological change), $s/a < n$, unemployment would grow continually; and if $s/a > n$, eventually the economy reached full employment—after which it would be profitable to invest only enough to keep full employment, an amount less than s/a.

The "dilemma" was resolved by Solow (1956), who proposed that the capital output ratio depended on the capital labor ratio, k: $a(k)$; and technological change was purely labor augmenting, so in equilibrium

$$s/a(k^*) = n + \beta \tag{16.7}$$

There is a capital labor ratio such that capital and *effective* labor (the demand for jobs and the supply of labor) grow precisely at the same rate.

The problem with Solow's "solution" is that it does away with the very concept of a job; alternatively, if there were ever a job shortage, simply by lowering the wage, more jobs would be created until the economy reached full employment. In developing countries, this means there is never a capital shortage; if there is unemployment, it must simply be because wages are too high. By the same token, there is never "technological unemployment." Technology may reduce the demand for labor *at a particular wage*, but whatever technology does, wage adjustments can undo. In practice, of course, at least in the short run, there is not such flexibility.[13]

In this short note, we take seriously the notion of jobs (perhaps more seriously than the concept should be taken). Given today's technology and capital stock, wage adjustments will not lead to full employment. There is a maximum employment which they can support.

In the model here, it is the combination of changes in capital stock and technology which drive changes in employment. Wages make a difference, through their effects on technology (and possibly capital accumulation). In short, we construct a model where, over time, technological change leads to either increases or decreases in the capital output ratio, so that *eventually*

$$s/a^* = n + \beta \tag{16.8}$$

That is,

$$a = a^* = s/(n+\beta). \tag{16.9}$$

It is technological change that ensures that jobs grow at the same rate as the labor force.

The problem with standard versions of the fixed coefficients model (where, at any moment of time, a and b are fixed) is that the distribution of income is very fragile: if N is the supply of labor and L is the demand,

$$L = (L/Y)\,(Y/K)\,K = (b/a)\,K \tag{16.10}$$

$$\text{If } (b/a)\,K < N, \quad \text{then } w = 0$$

$$\text{If } (b/a)\,K > N, \quad \text{then } r = 0$$

where w = (real) wage, r = (real) return on capital. If $(b/a)K = N$, the distribution of income is indeterminate.

Here, however, we present an alternative version, based on agency theory (Shapiro and Stiglitz, 1984). If workers are paid too low a wage, they prefer to shirk; there is the lowest wage which firms can pay at any unemployment rate to induce them not to shirk. That wage depends on the payment an unemployed worker receives. We write this as

$$w = f(v)w_{\min} \tag{16.11}$$

where v is the employment ratio,

$$v = L/N = (b/a)(K/N) \tag{16.12}$$

So

$$\mathrm{dln}\, v/\mathrm{d}t = s/a - n - \beta - \alpha \tag{16.13}$$

Finally, firms have a choice of innovations. Total cost of production per unit output is

$$c = ar + bw \tag{16.14}$$

The firm has a given technology today $\{a_0, b_0\}$. It can, however, decide on the nature of the technology by which it can produce next period (Figure 16.1).

Technology defines next year's feasibility locus. Taking for a moment r and w as given, the firm can reduce (next year's) cost by balancing out changes in a and b:

$$\mathrm{d}c/\mathrm{d}t = \alpha m - \beta(1 - m) \tag{16.15}$$

where m is the share of capital in costs, or the share of capital in income:

$$m = ar = rK/Y \tag{16.16}$$

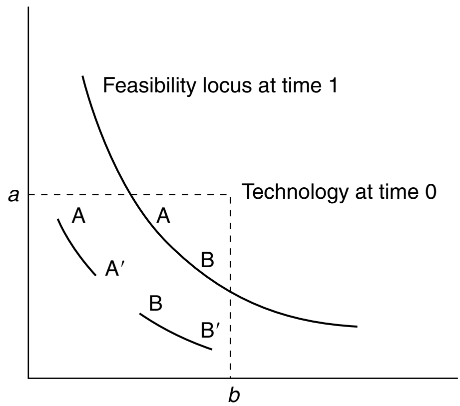

Figure 16.1 Technology feasibility locus.

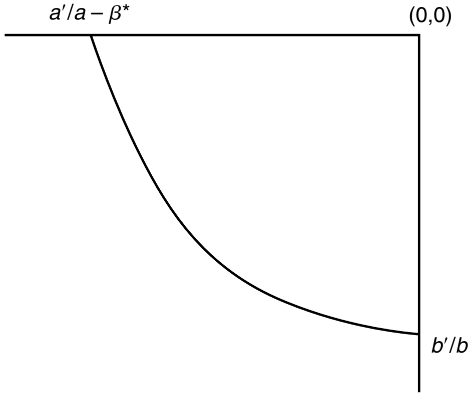

Figure 16.2 Labor vs. capital augmentation.

Assume that there is a trade-off between labor and capital augmenting progress, so that

$$\beta = \beta(\alpha). \quad \beta' > 0 \quad \beta'' < 0 \tag{16.17}$$

depicted in Figure 16.2.

Then cost reductions are maximized when

$$\beta'(\alpha) = m/(1 - m) \tag{16.18}$$

16.1 Steady State Equilibrium

We can now fully describe the steady state equilibrium. In the long run, we have argued that a must converge to a^*, which means that

$$\alpha^* = 0, \tag{16.19}$$

which in turn means that

$$m^*/(1 - m^*) = \beta'(0) \equiv \beta^* \tag{16.20}$$

or

$$r^* = m^*/a^*. \tag{16.21}$$

a^*, in turn, solves

$$s/a^* = n + \beta(\alpha^*) \tag{16.22}$$

We can similarly solve for the wage (conditional on productivity):

$$w^* b_0 = (1 - m^*) \tag{16.23}$$

Finally, we can use this to solve for the equilibrium unemployment rate:

$$w^*/w_{\min} = (w^* b_0)/(b_0 w_{\min}) = (1 - m^*)/(b_0 w_{\min}) = f(v^*) \tag{16.24}$$

That is, once we set the unemployment compensation ($w_{\min}$) relative to labor market productivity, then we know what the unemployment rate is.

16.2 Heuristic Dynamics

In this model, there is a simple adjustment process. If the capital output ratio is too high, too few jobs will be created (given the savings rate) and unemployment grows. Growing unemployment means that wages will become depressed—in the story told here, firms can pay a lower wage without workers' shirking, but there are other stories (such as bargaining models) which yield much the same outcome. Lower wages mean, of course, that the return to capital is increased. As wages get depressed, and labor becomes easier to hire, and the return (cost) of capital increases, firms seek ways of economizing on capital, and pay less attention to economizing on labor. The new technologies that are developed are capital saving and labor using. The capital output ratio falls, and the labor output ratio increases. Given the savings rate, more jobs are created, and the unemployment rate starts to fall.

16.3 Formal Dynamics

The convergence to equilibrium, however, may neither be direct nor fast. Figure 16.3 depicts the phase diagram, in $\{a, v\}$ space.

The locus of points for which $\mathrm{d}a/\mathrm{d}t = 0$ is a vertical line, given by

$$\alpha = 0, \text{ that is,} \tag{16.25}$$

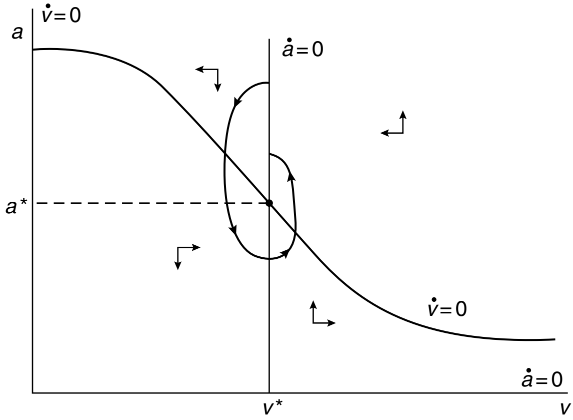

Figure 16.3

from Equations (16.24), (16.23) and (16.20), given $b_0 w_{\min}$ (which we will take as set), there is a unique value of v^* for which $\alpha = 0$, that is, a is constant; and if

$$v > v^*, \quad da/dt > 0$$

$$v < v^*, \quad da/dt < 0$$

when employment is high, wages are high; firms economize on labor, and the capital output ratio increases, and conversely when employment is low.

The locus of points for which $dv/dt = 0$ is the negatively sloped curve defined by

$$a = s/(n + \beta + \alpha) \tag{16.26}$$

and it is easy to show that below the curve, v is increasing, and above it, it is decreasing. We show a sample path converging through oscillations into the equilibrium.

In the appendix we provide sufficient conditions for this type of stability of the equilibrium.

16.4 Micro-economics

As noted earlier, at any point of time, the representative firm has a given technology, defined by $\{a, b\}$. Its choice is about its technology next period.

Figure 16.1 showed the original point, and its opportunity set, the locus of points which (given the *technology of change*) it can achieve next period. This is, of course, the framework that Atkinson and I set forth in our 1969 paper.

The firm chooses the point on the feasibility locus that will minimize next period's cost, at expectations concerning next period's factor prices. Thus, what is relevant in the cost minimization described earlier are not current factor prices, but next period's factor prices; but in the continuous time formulation used here, there is no real difference. On the other hand, a firm today should be aware that its choices today affect its choice set tomorrow, that is, in Figure 16.1, if it chooses point A, its choice set tomorrow is the locus AA′, while if it chooses point B, its choice set tomorrow is the locus BB′. And of course, each of those subsequent choices are affected by wages prevailing then and in the future. Hence, in reality, what should matter for a firm is not just tomorrow's wage, but the entire wage profile. The full solution of this complicated dynamic programming problem is beyond the scope of this brief note. The steady state *equilibrium* which emerges is identical to that described here, though the dynamics are somewhat more complicated.

16.5 A Generalization

Earlier, we set forth the notion that what mattered in the choice of technology was not just factor prices, but the total cost of labor, including turnover costs, how hard it was to hire workers, etc. These variables too will, in general, be related to the unemployment rate, so that the point along the feasibility set chosen by the firm will depend not only on relative shares, which depend on v, but also on v directly:

$$\alpha = \Phi(m(v), v). \tag{16.27}$$

Equilibrium still requires that

$$\alpha = 0, \beta = \beta^* = \beta(0) \tag{16.28}$$

and so $a = a^*$. Indeed, once we set the policy variable ($b_0 w_{\min}$), the analysis is changed little, except now, using Equation (16.24), m^* is defined by

$$0 = \Phi\,(m, v) = \Phi\,(m^*, f^{-1}\,(1 - m^*)/(b_0 w_{\min})) \tag{16.29}$$

16.6 A Kaldorian Variant

Kaldor provided an alternative approach to reconciling the "warranted" and "natural" rate of growth, the disparity between $s/a - \beta - \alpha$, the rate at which jobs are created, and n, the rate at which the labor force grows. He suggested that the average savings rate depends on the distribution of income, and by changing the distribution of income, s can be brought into line. Thus, he posited (in simplified form) that none of the wages are saved, but a fraction s_p of profits, so

$$\mathrm{d}K/\mathrm{d}t = s_p r K \tag{16.30}$$

Hence, we replace the differential equation for v, (16.13) with

$$\mathrm{dln}\, v/\mathrm{d}t = s_p r - n - \beta - \alpha \tag{16.13'}$$

$$= s_p(1/a - w(v)b/a) - n - \beta - \alpha$$

and the equilibrium Equation (16.8) defining a^* with

$$s_p r^* = n + \beta(0) \tag{16.8'}$$

defining the equilibrium interest rate. Before, given a, we used (16.21) to solve for r. Now, we use (16.21) to solve for a, given r:

$$a^* = m^*/r^* \tag{16.21'}$$

The dynamics are also modified only slightly. As (16.13′) makes clear, it is still the case that above the locus $\mathrm{d}v/\mathrm{d}t = 0$ (i.e. for higher values of a), the rate of growth of capital is lower (as before), so v (the employment rate) is falling; below the curve, it is rising. Hence, the qualitative dynamics remains unchanged.[14]

16.7 A Related Model

Some years ago, George Akerlof and I formulated a related model of the business cycle (another area in which Samuelson's contributions were seminal.)[15]

Real wages were portulated to depend positively on capital per worker, As here, an increase in capital accumulation led to increases in wages which reduced funds available for savings, which slowed growth and led to lower wages.[16] In that model, we again obtained oscillatory dynamic behavior.

16.8 Why it Matters: a Distinction with a Difference

At one level of analysis, the difference between this model and the standard Solow model is small. In the standard model, firms choose the *current* technology among a set of available technologies so that the capital output ratio adjusts and eventually the warranted and natural rate are equated. Here, firms choose *future technologies*, and again, eventually the warranted and natural rates are equated. In both models, at the microeconomic level, firms are choosing technologies in response to maximizing profits (minimizing costs), given factor prices.

There are, of course, important differences in dynamics: in the Solow model, convergence is monotonic. Here, the dynamics are far more complicated. Convergence may be oscillatory.

But there are some more profound differences, some of which relate to economic policy, to which I want to call attention. The first relates to the determination of the distribution of income and the choice of technique. In the Solow model, wages adjust *so that there is always full employment.* The choice of technique is, in effect, dictated by factor supplies. Though firms *choose* the technology to employ, factor prices always adjust so that the technology they choose is such that factors are fully employed. Thus, the distribution of income really plays no role—and in Solow's exposition, one could tell the entire dynamic story without reference to it, or without reference to firms "choosing" a technology. If there is unemployment, it is only because wages are too high and lowering wages would eliminate the unemployment (but increase growth only slightly and temporarily).

In the model here, the choice of (future) technology is central. Wages are not determined by marginal productivities, but by firms, as the lowest wage they can pay to avoid shirking on the part of workers. If minimum wages pushed wages above this level, they would result in increased unemployment; but for most workers, the minimum wage is set below that level so that lowering the minimum wage has little effect on wages actually paid, and hence on unemployment or growth. (An increase in unemployment compensation in this model does, however, increase the unemployment rate, by forcing firms to pay higher wages to avoid shirking.)

High wages do have an effect on unemployment, through the impact on the evolution of technology. This has two implications. First, it takes considerable time before any action to lower wages (even if it were successful) has any effect. The short-run effect on unemployment is nil.[17] Second, there are other ways by which the government could affect the evolution of the system and the creation of jobs. There are two ways by which this can be done in the medium run. First, policies which increase the national savings rate would be just as or more effective in increasing employment in the medium term. Second, marginal wage subsidies reduce the cost of labor, and it is the high cost of labor (at the margin) which induces firms to shift the direction of technological developments toward excessive labor savings and capital using technologies.

16.9 Concluding Remarks

For almost half a century, the Solow growth model, in which technological change was exogenous, has dominated discussions of growth theory. But almost half a century ago, Samuelson helped lay the foundations of an alternative approach to explaining the "stylized" facts of economic growth, based on endogenous technological change. What was needed, however, to close the model was a plausible theory of wage determination, which subsequent work in the economics of information (efficiency wage theory) has helped provide. By unifying these two disparate strands of literature, we have provided here a general theory of growth and employment which makes sense of discussions of technological unemployment or job shortages—concepts which have no meaning in Solow's formulation. We have suggested that the policy implications of this theory are markedly different from those arising from Solow's model.

It will be a long time before the fruit of the seeds which Paul sowed so many years ago are fully mature.

Notes

1. Stiglitz (2004)
2. Diamond (1965)
3. Arrow (1962a)
4. Here again, Arrow's (1962b) contribution was seminal. This is not the occasion to go into the large literature, except to mention Karl Shell's volume of essays (1967), Nordhaus' thesis (1969), and my own work with Tony Atkinson (1969).

5. Kennedy (1964)
6. Weizacker (1966)
7. Kaldor (1961)
8. Salter (1960)
9. Habbakuk (1962)
10. Stiglitz (1974)
11. Shapiro and Stiglitz (1984)
12. Harrod and Domar's original analysis did not include technological change. This is a slight generalization of their analysis.
13. Standard models have formalized this in the notion of putty-clay models.
14. The stability conditions are of course changed. See the appendix for details.
15. The accelerator-multiplier model has gone out of fashion, partly because the assumption of fixed coefficients on which it relied has become unfashionable, partly because it was not based on rational expectations (which has become fashionable). But one can obtain much the same results from a model in which investment increases not because sales have increased, but because profits have increased. Stiglitz and Greenwald (1993) have explained both why capital (equity) market imperfections exist and how they can lead to such a *financial accelerator*.
16. That model differed in the wage determination function (we used a real-Phillips curve) and, as in the Solow model, wages determined current choice of technique, as opposed to here, where it affects the evolution of future technology. In some cases, we showed that the economy could be characterized by a limit cycle.
17. Early students of growth theory recognized that this would be true even within the neoclassical model; they focused on putty-clay models in which after investments have been made, the ability to change its characteristics (the labor required to work it) is very limited. Dynamics of growth in putty-clay models are markedly different from those of standard neoclassical models. See Cass and Stiglitz (1969). Unfortunately, the models were not easy to work with, and the distinction seems to have been lost in discussions of growth in recent decades.

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Appendix: Stability Conditions

In order to analyze stability, we simplify by writing

$$\mathrm{d}\ln a/\mathrm{d}t = \alpha(v); \quad \alpha' > 0$$

$$-\mathrm{d}\ln b/\mathrm{d}t = \beta(\alpha); \quad \beta' > 0$$

The locus of points for which $dv/dt = 0$ is the negatively sloped curve defined by

$$a = s/(n + \beta(\alpha(v)) + \alpha(v)) \quad da/dv = -a^2 (\beta'\alpha' + \alpha')/s \tag{16.27}$$

Below the $dv/dt = 0$ curve, v is increasing and above the curve, v is decreasing.

To evaluate the stability conditions of the pair of differential equations:

$$v' = v\,(s/a - n - \beta(\alpha(v)) - \alpha(v))$$

$$a' = a\,\alpha(v)$$

we look at the Jacobian evaluated at $\{v^*, a^*\}$ as follows:

$$J(v^*, a^*) = \begin{Bmatrix} -v^*(\beta'(0)\alpha'(v^*) + \alpha'(v^*)) & -v^*s/a^{*2} \\ a^*\alpha'(v^*) & 0 \end{Bmatrix}$$

The conditions for the steady state to be a stable spiral (converging to equilibrium through oscillations) is:

$$-v^*(\beta'(0)\alpha'(v^*) + \alpha'(v^*)) < 0$$

which is always satisfied; and

$$[v^*(\beta'(0)\alpha'(v^*) + \alpha'(v^*))]^2 - 4(v^*s/a^{*2})a^*\alpha'(v^*) < 0$$

which can be simplified to

$$v^*\alpha'(1 + \beta')^2 < 4(n + \beta(0)).$$

Provided the limit as α goes to zero of $d\ln \alpha/d\ln v$ is finite, then the limit of the LHS of the above condition is always satisfied.[1]

16A.1 Kaldorian Variant

For the Kaldorian variant, the dv/dt equation is now:

$$v' = v\,(s_p r - n - \beta - \alpha) \text{ from Eq. (13') in the text}$$

$$= v\,(s_p(1/a - w(v)b/a) - n - \beta(\alpha(v)) - \alpha(v)) \text{ from the fact that } Y = rK + wL$$

[1] If lim $d\ln \alpha/d\ln v$ is infinite, then the stability condition will be satisfied only if the derivatives of the technology functions with respect to employment are sufficiently small, that is in the limit, as α goes to zero

$$(1+\beta')^2\,\alpha' < 4(n + \beta(0))/v^*$$

The Jacobian under the Kaldorian variant becomes:

$$J(v^*, a^*) = \begin{Bmatrix} -v^*(s_p(b_0/a^*)w' + \beta'(0)\alpha'(v^*) + \alpha'(v^*)) & -v^*s_p(1 - w(v^*)b_o)/a^{*2} \\ a^*\alpha'(v^*) & 0 \end{Bmatrix}$$

The conditions for local stability with oscillations in this case are:

$$[v^*(s_p(b_0/a^*)w' + \beta'(0)\alpha'(v^*) + \alpha'(v^*))]^2 - 4(v^*s_p(1 - w(v^*)b_0)/a^{*2})a^*\alpha'(v^*) < 0$$

$$\text{and } - v^*(s_p(b_0/a^*)w' + \beta'(0)\alpha'(v^*) + \alpha'(v^*)) < 0$$

Again, the latter condition is always satisfied, but now if the limit as α goes to zero of dln α/dln v is finite, the former condition is never satisfied; but if the limit is finite, the former condition requires that real wages not be too sensitive to employment. To see this, we rewrite the former condition as

$$[v^*(s_p(b_0/a^*)w' + \beta'(0)\alpha'(v^*) + \alpha'(v^*))]^2 < \{(v^*s_pr^*\alpha' = 4v^*(n + \beta(0))\alpha'\}$$

$$\text{LHS} = [v^*s_p(b_0/a^*)w' + v^*\alpha'(1 + \beta')]^2$$

$$= [v^*s_pb_0/a^*w']^2 + [2(v^*s_p(b_0/a^*)w'(v^*\alpha(1 + \beta')] + [v^*\alpha(1 + \beta')]^2$$

It is apparent, first, that if dln α/dln v is finite, the condition for stable oscillations is never satisfied (in marked contrast to the standard case), because the LHS is strictly positive, the RHS is zero. If lim $\alpha'(v)$ is strictly positive, the condition can be satisfied only if w′ is not too large. If the condition is not satisfied, the equilibrium is locally stable and the approach is not oscillatory.

17

Samuelson and Investment for the Long Run

Harry M. Markowitz

When I was a student in the Economics Department of the University of Chicago, Karl Brunner and I diligently read through Paul Samuelson's *Foundations of Economics*. Karl and I found a bug in the book and wrote Professor Samuelson concerning it. Samuelson replied that several other people had called his attention to this bug, but we were the first non-Asians to do so. Years later, I was surprised and delighted to see Samuelson cite my work and write about portfolio theory, albeit sometimes critically as well as creatively.

Through the years, Paul and I have had one ongoing debate on the following topic. If an investor invests for the "long run" should she or he choose each period the portfolio which maximizes the expected logarithm of 1+ return for that period? I say yes; Paul says no. Our written works on the subject include Samuelson (1963, 1969, 1979) and Markowitz (1959, 1976). We also debated the matter at a meeting in Vale, Colorado many years ago. To this day both of us feel that our respective views have been completely vindicated. But, I must admit, Samuelson's (1979) article titled "Why We Should Not Make Mean Log of Wealth Big Though Years To Act Are Long" is a particularly remarkable expository achievement. As he explains in the last paragraph, "No need to say more. I've made my point. And, save for the last word, have done so in prose of but one syllable." It is hard not to feel intimidated in a debate with an opponent who is a combination of Albert Einstein and Dr Seuss.

In the present chapter I note the primary positions of the two sides and give one example that illustrates their differences. I chose an example which most simply supports my side of the debate. I believe that any other example will, upon close examination, also support my side but not necessarily as directly. I make no attempt to provide arguments on the other side since Paul, despite or because of his ninety years, is perfectly capable of doing so.

17.1 Background

The expected log criteria was proposed by Kelly (1956) and embraced by Latane (1957, 1959). Markowitz (1959) accepts the idea that the expected logarithm (of one plus return) of a portfolio is its rate of growth in the long run. Markowitz concludes that the cautious investor should not choose a mean-variance combination from the mean-variance efficient frontier with higher arithmetic mean (and therefore higher variance) than the mean-variance combination which approximately maximizes expected log, or, equivalently, geometric mean return. A portfolio higher on the frontier subjects the investor to more volatility in the short run and no greater return *in the long run*. The cautious investor, however, may choose a mean-variance combination lower on the frontier, giving up return in the long run for greater stability of return.

Breiman (1960, 1961) supplied a strong law of large numbers argument supporting the expected log rule. Samuelson (1963, 1969, 1979) provides an expected utility argument which contradicts the expected log rule. Markowitz (1976) provides an alternative expected utility argument which supports the expected log rule.

17.2 The Expected Log Rule in General and Particular

Throughout this chapter I consider an investor who starts with an initial wealth W_0 and allocates resources, without transaction costs, at discrete times 0, 1, 2, . . . separated by a day, month, year, or millisecond. The return on a portfolio P during time interval t—between time point $t-1$ and t—is denoted r_t^P. In general, as of time $t-1$, the probability distribution of r_t^P may depend on the values of state variables as of time $t-1$ and may be jointly distributed with the values of state variables as of t. The max E log rule says that, whatever this dependence on and joint distribution with state variables, as of time $t-1$ choose the portfolio P which maximizes current, single-period

$$E \log(1 + r_t^p) \tag{17.1}$$

where E is the expected value operator.

The issues which separate the Kelly, Latané, Brieman, and Markowitz arguments *for* and the Samuelson arguments *against* are already present, and can be discussed more simply, in the special case wherein the returns r_t^P on a given portfolio are i.i.d. (independent and identically distributed) and the investor must rebalance to the same portfolio P (therefore the same probability distribution of r_t) at every point in time. We shall deal only with this special case in this chapter. See Markowitz (1976) for the more general case.

17.3 First Argument *For* Max *E* log

If an investor repeatedly draws from a rate of return distribution without adding or withdrawing funds beyond the initial W_0, then at time T the investor's wealth is

$$W^T = W_0 \prod_{t=1}^{T} (1 + r_t^P) \tag{17.2}$$

where r_t^P here represents the rate of return actually achieved on the portfolio at time t. The rate of return, g^P, achieved during the entire history from 0 to T satisfies

$$\begin{aligned}(1 + g^P) &= (W_T^P / W_0)^{1/T} \\ &= \left(\prod_{t=1}^{T} (1 + r_t^P) \right)^{1/T}\end{aligned} \tag{17.3}$$

g^P is the rate of return which, if earned each period, would grow wealth from W_0 to W_T in T periods. Thus, wealth at time T is a strictly increasing function of

$$\log(1 + g^P) = (1/T)\left(\sum_{t=1}^{T} \log(1 + r_t^P) \right) \tag{17.4}$$

The assumption that r_t^P is i.i.d. implies that $\log(1 + r_t^P)$ is i.i.d. If $r_t^P = -1$ is possible then $\log(1 + r_t^P)$ is an "extended real" random variable defined on $[-\infty, \infty)$. If $\log(1 + r_t^P)$ has expected value

$$E \log(1 + r_t^P) = \mu \in \left[\infty, \infty\right] \tag{17.5}$$

then the strong law of large numbers says that—with probability 1—

$$\lim_{T\to\infty} \sum_{t=1}^{T} \Big(log\,(1 + r_t^P) \Big) / T \to \mu. \tag{17.6}$$

In particular, if μ is finite then, with probability 1, for every positive ϵ there is a T_0 such that the difference between μ and the sample average of log $(1 + r_t^P)$ is less than ϵ, for all T greater than T_0:

$$\forall \varepsilon > 0\ \exists T_0 \quad \text{such that } \forall T > T_0$$

$$\sum_{t=1}^{T} \log\,(1 + r_t^P)/T - \mu \Big| < \varepsilon \tag{17.7}$$

T_0 is random, and will typically vary from one randomly drawn sequence to the next (independently drawn) random sequence. If expected log $(1 + r_t^P) = \infty$ (or$-\infty$) then, with probability 1, for every $b>0$ there is a random time T_0 such that for all time thereafter average log $(1 + r_t^P)$ is greater than b (respectively, less than $-b$):

$$\forall b > 0\ \exists T_0 \quad \text{such that } \forall\, \mathrm{T} > T_0$$

$$\sum_{t=1}^{T} \log\,(1 + r_t^P)/T > b \tag{17.8}$$

$$(<-b, \text{ respectively})$$

One of the principal justifications for identifying max E log $(1 + r)$ with investment for the long run follows from the above. If $r_1^P, r_2^P, \ldots$ and r_1^Q, r_2^Q are two rate of return sequences, each i.i.d. but r_t^P may be correlated with r_t^Q, and the first has a higher E log $(1 + r)$ than the second, that is,

$$\begin{aligned} \mu_P &= E \log\,(1 + r_t^P) \\ &> E \log(1 + r_t^Q) \\ &= \mu_Q \end{aligned} \tag{17.9}$$

then (17.3), (17.4) and (17.7) or (17.8) imply that—with probability 1—there is a time T_0 such that W_T^P exceeds W_T^Q ever after

$$\exists T_0\ \forall\, T > T_0\ W_P^T > W_Q^T. \tag{17.10}$$

Since, with probability one, there comes a time such that forever after the wealth of the investor who rebalances to portfolio P exceeds that of the investor who rebalances to portfolio Q, surely one can say that P does better than Q in *the long run*. This does not necessarily imply that any particular investor, with a finite life and imminent consumption needs, should invest in P rather than Q. But it seems an unobjectionable use of language to summarize relationship (17.10) by saying that portfolio P does better than portfolio Q "in the long run."

17.4 Argument *Against* Max *E* log

Consider an investor who invests W_0 at time 0, and lets this investment "ride," without additional investments or withdrawals until some fixed, distant time T. At time T the investor, or his or her heirs, will "cash in" the investment. The investor must decide whether the trustees of this investment are to be instructed to rebalance each period to portfolio P or Q. We continue to assume that successive returns to a given portfolio are i.i.d., although the simultaneous returns r_t^P, r_t^Q may be correlated. Suppose that the investor seeks to maximize expected utility of final wealth, where the utility function is the form

$$U = \operatorname{sgn}(\alpha)\, W_T^{\alpha} \tag{17.11}$$

for some $\alpha \neq 0$. Since returns to a given portfolio are i.i.d., expected utility equals

$$\begin{aligned} EU &= \operatorname{sgn}(\alpha)\, E\left(\prod_{t=1}^{T}(1 + r_t^P)\right)^{\alpha} \\ &= \operatorname{sgn}(\alpha)\left(E(1 + r^P)^{\alpha}\right)^{T} \end{aligned} \tag{17.12}$$

Thus, the expected utility maximizing portfolio is whichever has greater $E(1 + r)^{\alpha}$. This is not necessarily the one with greater $E \log (1 + r)$.

Samuelson (1969) and Mossin (1968) show a much stronger result than shown above (in which it is *assumed* that the investor rebalances to the *same* portfolio each period). Even if the investor may switch portfolios, for example, choose one when there is a long way to go and another when the end is imminent, the optimum strategy for the utility function in (17.11) is to stay with the same portfolio from beginning to end, whether "time is long" or not.

Thus, no matter how distant the goal, the optimum strategy is not the max E log rule.

17.5 Example

Consider two portfolios P and Q. P provides 6 percent per year with certainty. Q provides, each year, a fifty-fifty chance of 200 percent gain or 100 percent loss. The expected return and expected log(1 + return) of P are 0.06 and $\log_e(1.06) = 0.058$, respectively. The expected return and expected log of Q are

$$\tfrac{1}{2}(2.00) + \tfrac{1}{2}(-1.00) = 0.50 \text{ (50 percent) and } \tfrac{1}{2}\log(3.00) + \tfrac{1}{2}\log(0.0) = -\infty.$$

An investor who followed the max E log rule would prefer P. For any fixed investment horizon T, the investor who maximized expected utility of form (11) with $\alpha = 1$, that is, an investor who maximized expected terminal wealth, would prefer Q.

The arguments for and against the max E log rule can be illustrated with this example. Imagine that the return on Q is determined by a flip of a fair coin, heads being favorable. If the coin is flipped repeatedly, with probability 1, eventually a tail will be tossed. From that time on $0 = W_T{}^Q < W_T{}^P = (1.06)^T$. Thus, in the particular case, as in general, with probability 1 there comes a time when the max E log strategy pulls ahead and stays ahead of the alternative strategy, forever.

On the other hand, pick some point in time, such as $T = 100$. At that time P provides $(1.06)^T$ with certainty. Q provides nothing if a tail has appeared in the first 100 tosses. If not, $W_T{}^Q = 3^T$. Since this has probability $(½)^T$ expected wealth (equals expected utility here) is

$$EW_T^Q = (\tfrac{1}{2})^T 3^T = (1.50)^T > (1.06)^T = W_T^P \qquad (17.13)$$

Thus, in the particular case as in general, the portfolio which maximizes EU for $T = 1$ also maximizes EU for arbitrarily large T fixed in advance.

17.6 Another Argument *For* Max *E* log

Markowitz (1976) argues that an assertion that something is best (or not best) in the long run should be an asymptotic statement that some policy or strategy does (or does not) approach optimality as $T \to \infty$. The Samuelson argument against Max *E* log is presented in terms of a (long) game of fixed length. Since this fixed length is arbitrarily long, the Samuelson argument can be transformed into an asymptotic argument as follows. Imagine a sequence of games $G_1, G_2, G_3, \ldots, G_{100}, \ldots$. The second game G_2 is "just like" the first except it is two periods long, $T = 2$, rather than one period long $T = 1$. The third game G_3 is just like the first two except that it is three periods long, $T = 3$, and so on.

In general, the notion that game G_T is "just like" game G_{T-1}, only longer, would require that the same opportunities be available in the first $T - 1$ moves of game G_T as were available in all of G_{T-1}. For the simple example in the last section, it implies that the same two probability distributions, P and Q, be used T times instead of $T - 1$. Let EU_T^P and EU_T^Q represent the expected utility of the T-period game, obtained by repeatedly investing in distribution P or Q respectively. Samuelson's complaint about identifying P as the better investment for the long run is that it is *not* true that

$$\lim_{T\to\infty} EU_T^P \geq \lim_{T\to\infty} EU_T^Q$$

even though P has greater $E \log(1 + r)$ on each spin of the wheel.

One way in which the Samuelson games stay the same as T varies is that each is scored by the expected value of *the same function* of *final wealth*. We could instead score the games by the same function of rate of return g defined in (17.3). In the example of the last section, P always supplies a rate of return of 0.06. The rate of return supplied by Q is

$$q^Q = \begin{cases} -1.0 & \text{with probability } 1-\left(\tfrac{1}{2}\right)^T \\ 2.0 & \text{with probability } \quad \left(\tfrac{1}{2}\right)^T \end{cases}$$

Let f be any strictly increasing function of g. Let us define a sequence of games, $H_1, H_2, \ldots, H_{100}, \ldots$ which are just like the Samuelson games except that they are each scored by expected value of the same function $V = f(g)$. Then

$$EV_T^P = f(0.06) \rightarrow f(0.06)$$

$$EV_T^Q = (1-(\tfrac{1}{2})^T)\, f(-1.0)$$

$$+(\tfrac{1}{2})^T f(2.0)$$

$$\rightarrow f(-1.0)$$

Thus, indeed,

$$EV_T^P > EV_T^Q \quad \text{as } T \rightarrow \infty.$$

If we score each game by the same function of g (rather than the same function of W_T) then the max E log rule *is* asymptotically optimal.

Suppose we wish to compare the performances of two investment strategies for varying horizons: for example, for a 5-year period, a 10-year period, . . . , a 50-year period, etc. How should we decide whether increasing time is more favorable to one than the other? No matter how long or short the horizon, there is *some* chance that one will do better, and *some* chance the other will do better. The question is how to "add up" these various possibilities. One way—the constant utility of final wealth way—assumes that the trade-offs should be the same between making a dollar grow to \$1.10 versus \$1.20 versus \$1.30 after 50 years as after 5 years. The other way—constant utility of rate of growth—assumes that the trade-offs should be the same between achieving a 3 percent, 6 percent and 9 percent rate of growth during the 5 or 50 years. For a fixed T, any utility of final wealth $U(W_t)$ can be expressed as a utility of growth $f(g) = U(W_0(1+g)^T)$. But, as our example illustrates, assuming that U remains the same versus assuming f remains the same as T increases has very different implications for the asymptotic optimality of the max E log rule.

17.7 Summary

One argument in favor of the *E* log rule is that (under broad assumptions) eventually the wealth of the investor who follows the rule will become greater than, and stay greater forever than, an investor who follows a distinctly different strategy. Samuelson's argument against the rule is that if the investor seeks to maximize the expected value of a certain kind of function of final wealth, for a long game of fixed length, then maximizing *E* log is *not* the optimal strategy. Indeed, if we let the length of the game increase, the utility supplied by the max *E* log strategy does not even approach that supplied by the optimum strategy. This assumes that utility of final wealth remains the same as game length varies. On the other hand, if we assume that it is the utility of rate-of-growth-achieved, rather than utility of final wealth, that remains the same as length of game varies, then the *E* log rule *is* asymptotically optimal.

As Keynes said, "In the long run we are all dead." Even if you buy the notion, for either reason, that the max *E* log rule is asymptotically optimal for the investor who lets her, his, or its money ride, it may not be optimal for the individual or institution with fixed or random cash flow needs. Perhaps this is a sufficient caveat to attach to the observation that the cautious investor should not select a mean-variance efficient portfolio higher on the frontier than the point which approximately maximizes expected log (1 + return); for a point higher on the frontier subjects the investor to greater volatility in the short run and, almost surely, no greater rate-of-growth in the long run.

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18

Paul Samuelson and Financial Economics

Robert C. Merton

18.1 Introduction

It has been well said that Paul A. Samuelson is the last great general economist—never again will any one person make such foundational contributions to so many distinct areas of economics. His profound theoretical contributions over nearly seven decades of published research have been ecumenical and his ramified influence on the whole of economics has led economists in just about every branch of economics to claim him as one of their own. I am delighted to take part in this celebration of his life and work.

This volume provides a special opportunity to honor this universal man of economics as he enters his tenth decade. On such Festschrift occasions, the common practice is to write a substantive piece building upon the honoree's work. However, here I try my hand at a different format: synthesizing Samuelson's work in financial economics itself. As everyone knows, Paul Samuelson is his own best synthesizer and critic, and so the format as executed will only be at best second-best.[1] Synthesis, we know, involves abstraction from the complex original. With Samuelson, we must be severely selective since even with confinement to a single branch of economics, the wide-ranging scope and unflagging volume of his researches allows only a few elements of the work to be examined. Within that brute reality, I limit my discussion to just three of his chief contributions to the field of financial economics: (1) The Efficient Market Hypothesis; (2) Warrant and option pricing; and (3) Investing for the long run.

Happily, I had the great good fortune to explore this same synthesizing theme in print nearly a quarter century ago (Merton, 1983), covering early major contributions of Samuelson—a number of which are not discussed here—such as expected utility theory (from reconciling its axioms with nonstochastic theories of choice to its reconciliation with the ubiquitous and practical mean-variance criterion of choice), the foundations of diversification and optimal portfolio selection when facing fat-tailed, infinite-variance return distributions.[2] As we shall see, however, it is remarkable how much of Samuelson's early research remains in the mainstream of current financial economic thought decades later, having gained even greater significance to the field with the passage of time.[3] Samuelson's discoveries in finance theory, as in economic theory generally, constitute the manifest core of his multiform writings. His accomplishments in both the problem-finding and problem-solving domains of theory are legend. Another, latent but no less deep, theme of Samuelson's writings is trying to divert us away from the paths of error, whether in finance research, private-sector finance practice, or public finance policy.

Samuelson's attacks on error are not limited to engagements in the economics arena. He has, upon occasion, used the life works of other economists to discredit the widely held myth in the history of science that scientific productivity declines after a certain chronological age. The strongest debunking of this ill-founded belief would, of course, have been the self-exemplifying one. While my brief search of the literature produced neither an exact cutoff age where productivity is purported to decline nor whether this decline is to be measured by the flow of research output per unit time or by its rate of change, the data provided by Paul Samuelson's lifetime pattern of contributions are robust in rejecting this proposed result on all counts. Representing twenty-seven years of scientific writing from 1937 to the middle of 1964, the first two volumes of his Collected Scientific Papers contain 129 articles and 1772 pages. These were followed by the publication in 1972 of the 897-page third volume, which registers the succeeding seven years' product of seventy-eight articles published when he was between the ages of 49 and 56. A mere five years later, at the age of 61, Samuelson had published another eighty-six papers, which fill the 944 pages of the fourth volume. A decade later the fifth volume appeared with 108 articles and 1064 pages. Simple extrapolation (along with a glance at his list of publications since 1986) assures us that the sixth and even a seventh volume cannot be far away.

Nearly a quarter century ago, I presented Paul with a list of his then thirty articles in financial economics and asked him to select his favorite ones, leaving the criteria for choice purposely vague. By the not-so-tacit demanding criterion that was evidently applied, he was drastically selective, choosing only six. I list these below. Four of the six articles appear in journals not on the beaten path of most economists, but happily they are reproduced in Samuelson's Collected Scientific Papers.

18.1.1 Paul Samuelson's 1982 Selection of his Favorite Financial Economics Papers

1. "Probability, utility, and the independence axiom," *Econometrica*, 20 (4), 1952, 670–678; (1952b, I, Chap. 14).
2. "General proof that diversification pays," *Journal of Financial and Quantitative Analysis*, 2(1), 1967, 1–13; (1967a, III, Chap. 201).
3. "The fundamental approximation theorem of portfolio analysis in terms of means, variances, and higher moments," *Review of Economic Studies*, 37(4), 1970, 537–542; (1970a, III, Chap. 203).
4. "Stochastic speculative price," *Proceedings of the National Academy of Sciences, U.S.A.*, 68(2), 1971, 335–337; (1971a, III, Chap. 206).
5. "Proof that properly anticipated prices fluctuate randomly," *Industrial Management Review*, 6(2), 1965, 41–49; (1965a, III, Chap. 198).
6. "Rational theory of warrant pricing," *Industrial Management Review*, 6(2), 1965, 13–39; (1965b, III, Chap. 199).

Perhaps a bit selfishly, we in financial economics are especially thankful that Paul paid no heed to the myth of debilitating age in science. Five of the six articles he selected in 1982 as his most important papers in our branch of economics and all but six of his more than three-score contributions to our field to date were published after he had reached the age of fifty.

Along with his foundational research and important directives on avoiding the paths of error, there are the characteristic Samuelsonian observations in the history of economic science. Samuelson's writings on Smith, Ricardo, Marx, and his many essays on the evolution of more contemporary economic thought provide much grist for the mill-of-the-historian of science. But, to focus exclusively on those explicit undertakings in the history of economic science is to miss much. Part of an unmistakable stamp of a Paul Samuelson article is the interjections of anecdotes and stories around and between his substantive derivations, which serve to entertain and enlighten the reader on the developmental chain of thought underlying that substantive analysis.

One happy example in financial economics is Samuelson's brief description in the "Mathematics of Speculative Price" (1972a, IV, Chap. 240, p. 428) of the rediscovery of Bachelier's pioneering work on the pricing of options. In the text, he wrote:

In 1900 a French mathematician, Louis Bachelier, wrote a Sorbonne thesis on the *Theory of Speculation*. This was largely lost in the literature, even though Bachelier does receive occasional citation in standard works on probability. Twenty years ago a circular letter by L. J. Savage (now, sadly, lost to us), asking whether economists had any knowledge or interest in a 1914 popular exposition by Bachelier, led to his being rediscovered. Since the 1900 work deserves an honored place in the physics of Brownian motion as well as in the pioneering of stochastic processes, let me say a few words about the Bachelier Theory.*

The footnote elaborates

*Since illustrious French geometers almost never die, it is possible that Bachelier still survives in Paris supplementing his professional retirement pension by judicious arbitrage in puts and calls. But my widespread lecturing on him over the last 20 years has not elicited any information on the subject. How much Poincaré, to whom he dedicates the thesis, contributed to it, I have no knowledge. Finally, as Bachelier's cited life works suggest, he seems to have had something of a one-track mind. But what a track! The rather supercilious references to him, as an unrigorous pioneer in stochastic processes and stimulator of work in that area by more rigorous mathematicians such as Kolmogorov, hardly does Bachelier justice. His methods can hold their own in rigor with the best scientific work of his time, and his fertility was outstanding. Einstein is properly revered for his basic, and independent, discovery of the theory of Brownian motion 5 years after Bachelier. But years ago when I compared the two texts, I formed the judgment (which I have not checked back on) that Bachelier's methods dominated Einstein's in every element of the vector. Thus, the Einstein–Fokker–Planck–Fourier equation for diffusion of probabilities is already in Bachelier, along with subtle uses of the now-standard method of reflected images.

In addition to providing the facts on how Bachelier's seminal work found its way into the mainstream of financial economics after more than a half century of obscurity, Samuelson's compact description provides a prime example of multiple and independent discoveries across the fields of physics, mathematics, and economics.[4] On the issue of allocating the credit due to innovative scholars, he also provides an evaluation of the timing and relative quality of the independent discoveries. His mention of Poincaré provides a hint that there may be still more to the complete story. Furthermore, note his signature use of a chain of eponyms, the "Einstein–Fokker–Planck–Fourier equation," to compactly remind us of the sequence of scientists to whom we owe credit. And, of course, what economist would not relish this revelation of the great debt owed to this early financial economist by the mathematical physicists and probabilists to be added to the well-known debt owed to Malthus by the Darwinian biologists?

Although most would agree that finance, micro investment theory and much of the economics of uncertainty are within the sphere of modern financial economics, the boundaries of this sphere, like those of other specialties, are both permeable and flexible. It is enough to say here that the core of the subject is the study of the individual behavior of households in the intertemporal allocation of their resources in an environment of uncertainty and of the role of economic organizations in facilitating these allocations. It is the complexity of the interaction of time and uncertainty that provides intrinsic excitement to study of the subject, and, indeed, the mathematics of financial economics contains some of the most interesting applications of probability and optimization theory. Yet, for all its seemingly obtrusive mathematical complexity, the research has had a direct and significant influence on practice. The impact of efficient market theory, portfolio selection, risk analysis, and option pricing theory on asset management and capital budgeting procedures is evident from even a casual comparison of current practices with, for example, those of the early 1960s when Paul Samuelson was just publishing his early foundational papers in finance.

New financial product and market designs, improved computer and telecommunications technology, and advances in the science of finance during the past four decades have led to dramatic and rapid changes in the structure of global financial markets and institutions. The scientific breakthroughs in financial economics in this period both shaped and were shaped by the extraordinary flow of financial innovation, which coincided with those changes. The cumulative impact has significantly affected all of us—as users, producers, or overseers of the financial system.

The extraordinary growth in size and scope of financial markets and financial institutions including the creation of the enormous national mortgage market in the United States were significantly influenced by the models developed in financial economic research. The effects of that research have also been observed in legal proceedings such as appraisal cases, rate of return hearings for regulated industries, and revisions of the "prudent person" laws governing behavior for fiduciaries. Evidence that this influence on practice will continue can be found in the curricula of the best-known schools of management where the fundamental financial research papers (often with their mathematics included) are routinely assigned to MBA students. Although not unique, this conjoining of intrinsic intellectual interest with extrinsic application is a prevailing theme of research in financial economics. Samuelson, once again, did much to establish this theme as a commonplace and to exemplify it in his substantive writings.

It was not always thus. Fifty years ago, before the birth of the economics of uncertainty and before the rediscovery of Bachelier, finance was essentially a collection of anecdotes, rules of thumb, and manipulations of accounting data with an almost exclusive focus on corporate financial management. The most sophisticated technique was discounted value and the central intellectual controversy centered on whether to use present value or internal rate of return to rank corporate investment projects. The subsequent evolution from this conceptual potpourri to a rigorous economic theory subjected to systematic empirical examination was the work of many and, of course, the many included Paul Samuelson.

After this brief overview of Samuelson's multifaceted influence on the ethos of financial economic research, I turn now to that promised discussion of three of his chief contributions to the field.

18.2 The Efficient Market Hypothesis

A question repeatedly arises in both financial economic theory and practice: When are the market prices *of* securities traded in capital markets equal to the best estimate *of* their values? I need hardly point *out* that if value is defined as "that price at which *one* can either buy or sell in the market," then the answer is trivially "always." But, of course, the question is rarely, if ever, asked in this tautological sense, although the distinction between value and price is often subtle. Moreover, as the following examples suggest, the answer to this question has important implications for a wide range of financial economic behavior.

In the fundamentalist approach of Graham and Dodd to security analysis, the distinction between value and price is made in terms of the (somewhat vague) notion of intrinsic value. Indeed, the belief that the market price of a security need not always equal its intrinsic value is essential to this approach because it is disparities such as these that provide meaningful content to the classic prescription for successful portfolio management: buy low (when intrinsic value is larger than market price) and sell high (when intrinsic value is smaller than market price).

In appraisal law, the question is phrased in terms of how much weight to give to market price in relation to other nonmarket measures of value in arriving at a fair value assessment to compensate those whose property has been involuntarily expropriated. In corporation finance, the answer to that question determines the extent to which corporate managers should rely upon capital market prices as the correct signals for the firm's production and financing decisions.

Characteristically, Samuelson's version provides both a clear distinction between value and price and a focus on the broadest and most important issue raised by this question: When are prices in a decentralized capital market system the best estimate of the corresponding shadow values of an idealized central planner who efficiently allocates society's resources? Thus, in "Mathematics of Speculative Price" (1972a, IV, Chap. 240, p. 425), he wrote

> A question, for theoretical and empirical research and not ideological polemics, is whether real life markets—the Chicago Board of Trade with its grain futures, the London Cocoa market, the New York Stock Exchange, and the less-formally organized markets (as for staple cotton goods), to say nothing of the large Galbraithian corporations possessed of some measure of unilateral economic power—do or do not achieve some degree of dynamic approximation to the idealized "scarcity" or shadow prices. In a well-known passage, Keynes has regarded speculative markets as mere casinos for transferring wealth between the lucky and unlucky. On the other hand, Holbrook Working has produced evidence over a lifetime that futures prices do vibrate randomly around paths that a technocrat might prescribe as optimal. (Thus, years of good crop were followed by heavier carryover than were years of bad, and this before government intervened in agricultural pricing.)

As we know, such theoretical shadow prices are "prices never seen on land or sea outside of economics libraries." However, testable hypotheses can be derived about the properties that real-life market prices must have if they are to be the best estimate of these idealized values. Because it is intertemporally different rather than spatially different prices that are of central interest in financial economics, most of Samuelson's analyses in this area are developed within the context of a futures market. In his 1957 "Intertemporal Price Equilibrium: A Prologue to the Theory of Speculation" (1957, II, Chap. 73), however, he does use spatial conditions of competitive pricing as tools to deduce the corresponding conditions on intertemporal prices in a certainty environment. From these local "no-arbitrage conditions," he proves that the current futures price must be equal to the future spot price for that date. In completing his analysis of the price behavior over time, he shows that the dynamics of "allocation-efficient" spot prices can be determined as the formal solution to a particular optimal control problem.[5]

Samuelson underscores his use of the word *Prologue* in the title by pointing out that "A theory of speculative markets under ideal conditions of certainty is Hamlet without the Prince," (p. 970). Indeed, his later papers, "Stochastic Speculative Price" (1971a, **III**, Chap. 206), "Proof That Properly Anticipated Prices Fluctuate Randomly" (1965a, **III**, Chap. 198), and "Rational Theory of Warrant Pricing" (1965b, **III**, Chap. 199), have in common their deriving the stochastic dynamic behavior of prices in properly functioning speculative markets. They also share the distinction of being important papers published in obscure places, which nevertheless found their way into the mainstream. Such occurrences suggest that high visibility of scientific authors may tend to offset low visibility of publication outlets.

Published in the same issue of the *Industrial Management Review*, "Proof That Properly Anticipated Prices Fluctuate Randomly" and "Rational Theory of Warrant Pricing" are perhaps the two most influential Samuelson papers for the field. During the decade before their printed publication in 1965, Samuelson had set down, in an unpublished manuscript, many of the results in these papers and had communicated them in lectures at MIT, Yale, Carnegie, the American Philosophical Society, and elsewhere. The sociologist or historian of science would undoubtedly be able to develop a rich case study of alternative paths for circulating scientific ideas by exploring the impact of this oral publication on research in rational expectations, efficient markets, geometric Brownian motion, and warrant pricing in the period between 1956 and 1965.

In "Proof That Properly Anticipated Prices Fluctuate Randomly," Samuelson provides the foundation of the efficient market theory that Eugene Fama independently and others have further developed into one of the most important concepts in modern financial economics. As indicated by its title, the principal conclusion of the paper is that in well-informed and competitive speculative markets, the intertemporal changes in prices will be essentially random. In a conversation with Samuelson, he described the reaction (presumably his own as well as that of others) to this conclusion as one of "initial shock—and then, upon reflection, that it is obvious." The time series of changes in most economic variables (GNP, inflation, unemployment, earnings, and even the weather) exhibit cyclical or serial dependencies. Furthermore, in a rational and well-informed capital market, it is reasonable to presume that the prices of common stocks, bonds, and commodity futures depend upon such economic variables. Thus, the shock comes from the seemingly inconsistent conclusion that in such well-functioning markets, the changes in speculative prices should exhibit no serial dependencies. However, once the problem is viewed from the perspective offered in the paper, this seeming inconsistency disappears and all becomes obvious.

Starting from the consideration that in a competitive market, if everyone *knew* that a speculative security was expected to rise in price by more (less) than the required or fair expected rate of return, it would *already* be bid up (down) to negate that possibility. Samuelson postulates that securities will be priced at each point in time so as to yield this fair expected rate of return. Using a backward-in-time induction argument, he proves that the changes in speculative prices around that fair return will form a martingale. And this follows no matter how much serial dependency there is in the underlying economic variables upon which such speculative prices are formed. Thus,

> We would expect people in the market place, in pursuit of avid and intelligent self-interest, to take account *of* those elements of future events that in a probability sense may be discerned to be casting their shadows before them. (Because past events cast "their" shadows after them, future events can be said to cast their shadows before them.) (1965a, III, Chap. 198, p. 785)

In an informed market, therefore, current speculative prices will already reflect anticipated or forecastable future changes in the underlying economic variables that are relevant to the formation of prices, and this leaves only the unanticipated or unforecastable changes in these variables as the sole source of fluctuations in speculative prices.

Samuelson is careful to warn the reader against interpreting his conclusions about markets as empirical statements:

> You never get something for nothing. From a nonempirical base of axioms, you never get empirical results. Deductive analysis cannot determine whether the empirical properties of the stochastic model I posit come close to resembling the empirical determinants of today's real-world markets. (1965a, III, Chap. 198, p. 783)

Nevertheless, his model is important to the understanding and interpretation of the empirical results observed in real-world markets.

Suppose that one observes that successive price changes are random (as empirically seems to be the case for many speculative markets). Without the benefit of Samuelson's theoretical analysis, one could easily interpret the fact that these prices wander like a drunken sailor as strong evidence in favor of the previously noted Keynes's view of speculative markets.

Whereas had it been observed that speculative markets were orderly with smooth and systematic intertemporal changes in prices, the corresponding interpretation (again, without Samuelson's analysis) could easily be that such sensible price behavior is (at least) consistent with that of the shadow prices of the idealized rational technocratic planner.

In the light of Samuelson's analysis, we all know that the correct interpretations of these cases are quite the reverse. For speculative market prices to correspond to their theoretical shadow values, they must reflect anticipated future changes in relevant economic variables. Thus, it is at least consistent with equality between these two sets of prices that changes in market prices be random. On the other hand, if changes in speculative prices are smooth and forecastable, then speculators who are quick to react to this known serial dependency and investors who are lucky to be transacting in the right direction will receive wealth transfers from those who are slow to react or who are unlucky enough to be transacting in the wrong direction. More important, under these conditions, current market prices are not the best estimate of values for the purposes of signaling the optimal intertemporal allocation of resources.

In studying the corpus of his contributions to the efficient market theory, one can only conclude that Paul Samuelson takes great care in what he writes. As is evident throughout his Proof paper and in his later discussion of the topic in "Mathematics of Speculative Price," (1972a, IV, Chap. 240) he is keenly aware of the ever present danger of banalization by those who fail to see the subtle character of the theory. Thus, having proved the general martingale theorem for speculative prices, he concludes

> The Theorem is so general that I must confess to having oscillated over the years in my own mind between regarding it as trivially obvious (and almost trivially vacuous) and regarding it as remarkably sweeping. Such perhaps is characteristic of basic results. (1965a, III, Chap. 198, p. 786)

Without Samuelson's careful exposition, the martingale property could easily be seen as either a simple deduction (whose truth follows from the very definition of competitive markets) or as a mere tautology. That is, subtract from any random variable, Y_t, its conditional expectation as of $t - 1$, $E_{t-1}[Y_t]$, and as a truism, the sum of the $\{Y_t - E_{t-1}[Y_t]\}$ will form a martingale. Indeed, in discussing the fair expected returns $\{\lambda_t\}$ around which speculative prices should exhibit the martingale property, Samuelson points out that

> Unless something useful can be said in advance about the $[\lambda_{T-i}]$– as for example, $\lambda_t - 1$ small, or λ_t a diminishing sequence in function of the diminishing variance

to be expected of a futures contract as its horizon shrinks, subject to perhaps a terminal jump in λ_1 as closing-date becomes crucial-the whole exercise, becomes an empty tautology. (1972a, IV, Chap. 240, p. 443)

But, of course, such restrictions can be reasonably imposed (using for example, the capital asset pricing model and the term structure of interest rates), and it is these restrictions that form the basis for testing the theory.

Many less precise discussions of the efficient market theory equate the theory with the property that speculative price changes exhibit a random walk around the fair expected return. However, Samuelson clearly distinguishes his derived martingale property from this much stronger one by showing that such changes need not be either independently or identically distributed for the theory to obtain. He is also careful to make the distinction between *speculative* prices that will satisfy the martingale property and *nonspeculative* prices (as well as other economic variables) that need not exhibit this property in a well-functioning market economy. In his "Stochastic Speculative Price" analysis, for example, the optimal stochastic path for the spot price of a commodity is shown not to satisfy the martingale condition for a speculative price. Indeed, only in periods of positive storage when the spot price also serves the function of a speculative price will the expected changes in the spot price provide a fair expected rate of return (including storage costs). "Thus," Samuelson remarks, "Maurice Kendall almost proves too much when he finds negligible serial correlation in spot grain prices" (1965a, III, Chap. 198, p. 783). I only allude to the import of this message for those in other areas of economics who posit and test models of rational expectations.

In preparing this chapter, I found in my files a 25-year-old unpublished manuscript of Samuelson's, "Nonlinear Predictability Though the Spectrum is White," which he had given to me with a kind invitation to once again become his coauthor and "revise as seems best."[6] As is clear from the title, Samuelson's intent was to provide a specific and empirically plausible model to underscore his point that "white noise" lack of (linear) serial correlation in stock returns is not sufficient to ensure the nonpredictability of those returns. As he describes it

The "efficient markets hypothesis" is sometimes overdramatized by the description that "speculative price behaves like a random walk." More exactly the correct hypothesis is that the speculative price is a *martingale* and therefore has a *zero* autocorrelelogram or "white spectrum" with a zero Pearsonian correlation coefficient between price changes in non-overlapping time periods.

Samuelson elaborates on the implications:

It follows from a zero autocorrelation that any "technical" or "chartist" method of prediction that depends on *linear* multiple correlation is doomed to failure. Econometricians commonly test, and often verify, the white-spectrum necessary condition for the efficient-market hypothesis. This *necessary* condition is *not*, however, *sufficient*. Zero autocorrelation would be equivalent to probabilistic independence (of "excess" returns) if the data were assuredly drawn from multivariate Gaussian distributions. However, for non-Gaussian distributions as with curvilinear functions of Gaussian variates, higher than second-moment tests must also be confirmed. Thus, the whiteness of spectrum with its guarantee of the *impotence of linear multiple regression prediction* is not at all a guarantee that *nonlinear* chartism will fail.

Although the file also contains some mathematical modeling of mine, apparently in anticipated acceptance of his invitation, the paper was neither completed nor circulated. I harbor the hope that with this rediscovery Paul will consider publishing it in full. In the meantime, I sketch out here a simplified version of his central thesis in an example from that modeling.

Let X_t denote the realized return on a stock minus its "fair" expected return between time $t - 1$ and t. If the stock price satisfies the efficient market hypothesis, then the expected excess return on the stock will satisfy the martingale property that

$$\mathrm{E}\left[X_t | X_{t-k}\right] = 0, \quad \text{for } k = 1, 2, 3, \ldots$$

Suppose however that the process for X_t is given by

$$X_t = a\varepsilon_{t-1}\left(\varepsilon^2_{t-1} - b\right) + \varepsilon_t$$

where the $\{\varepsilon_t\}$ are independently and identically distributed Gaussian random variables with zero mean and variance σ^2 and $a > 0$. Consider the linear serial correlation between the excess return from $t - 1$ to t and the excess return from $t - k - 1$ and $t - k$, given by

$$\begin{aligned} \mathrm{E}\left[X_t X_{t-k}\right] &= 0 \quad \text{for } k \geqslant 2 \text{ and all } a \text{ and } b \\ &= a\left[\mathrm{E}(\varepsilon^4_{t-1}) - b\mathrm{E}(\varepsilon^2_{t-1})\right] \quad \text{for } k=1 \\ &= a\sigma^2\left[3\sigma^2 - b\right] \quad \text{for } k=1 \end{aligned}$$

If the stock price is efficient with respect to linear combinations of past returns, then we have that $\mathrm{E}[X_t X_{t-k}] = 0$ for $k = 1, 2, 3 \ldots$ and therefore

$b = 3\,\sigma^2$. Under that white-spectrum condition, we have that the conditional expected excess return is given by

$$E\left[X_t|\varepsilon_{t-1}\right] = a\varepsilon_{t-1}\left(\varepsilon_{t-1}^2 - 3\sigma^2\right)$$

By inspection,

$E\left[X_t|\varepsilon_{t-1}\right] > 0$ and one will earn a greater than fair expected return on the stock, that is, it is "undervalued" when either $\varepsilon_{t-1} > \sqrt{3}\sigma$ or $-\sqrt{3}\sigma < \varepsilon_{t-1} < 0$ and $E\left[X_t|\varepsilon_{t-1}\right] < 0$ and one will earn a less than fair expected return on the stock, that is, it is "overvalued" when either $0 < \varepsilon_{t-1} < \sqrt{3}\sigma$ or $\varepsilon_{t-1} < -\sqrt{3}\sigma$. Put in terms of the directly observable excess returns, we have that

$$E\left[X_t|X_{t-1}\right] = aX_{t-1}\left[X_{t-1}^2 + 18a^2\sigma^6 - 3\sigma^2\right],$$

which will not equal 0 in general and thus, the martingale test condition for the efficient market hypothesis fails.[7]

Thus, Samuelson concludes, "Despite the resulting impotence of *linear* prediction, the experienced eye will soon recognize that the example's white-spectrum series is anything but a random walk, instead being the archetype of a stationary time series that does lend itself to profitable *nonlinear* filtering." In a characteristically careful clarification, he goes on, "The point of this dramatic example is *not* to deny that numerous people in the marketplace may learn to recognize the predictability structure present in this time series—and, in so learning, may subsequently act to wipe out that structure. The point of the example is to illustrate how weak is the power of a test of mere *un*autocorrelation to appraise the efficiency and predictability of market prices."[8]

Samuelson not only exercises great theoretical care himself, but he also tries to induce such in his readers. On his derivation of the efficient market hypothesis, he warns, for example, against reading "too much into the established theorem:"

> It does not prove that actual competitive markets work well. It does not say that speculation is a good thing or that randomness of price changes would be a good thing. It does not prove that anyone who makes money in speculation is *ipso facto* deserving of the gain or even that he has accomplished something good for society or for anyone but himself. All or none *of* these may be true, but that would require a different investigation. (1965a, III, Chap. 198, p. 789)

Samuelson later undertook that investigation (1972b) and demonstrated that uninformed speculators (in later literature, also known as "noise traders") confer less benefit to society than their losses. In an extension of "Proof" (1973), he showed that the dynamics of properly discounted present values of assets must also exhibit the same martingale property.

In the last paragraph of "Proof," Samuelson concludes by raising a number of questions, all of which focus on an issue central to making operational his concept of properly anticipated prices. Namely, where are the basic probability distributions (for which the martingale property of speculative prices applies) to come from? Although he makes no pronouncements on this issue, by identifying it he opened gates to its resolution in the important later work by Fama (1970). Fama defines market efficiency in terms of a hierarchy of information sets that are the basis for forming the probability distributions. He shows that if changes in speculative prices (around their fair expected returns) form a martingale based upon the probability distribution generated by information set Φ then these price changes will also satisfy the martingale property for the distribution generated by any information set Φ' that is a subset of Φ. It therefore follows that if these prices do not satisfy the martingale property for information set Φ', they will not satisfy this property for any information set Φ that contains Φ' as a subset. Thus, Fama makes operational Samuelson's martingale requirement for properly anticipated prices by showing that it is possible to reject the martingale property (and hence, market efficiency) by using only a subset of the information available to any (or for that matter, all) investors. As Fama makes clear in his development of the strong, semi-strong, and weak versions of the efficient market theory, it is also possible for speculative prices to satisfy the martingale conditions for one information set but not to satisfy it for another.

The martingale property of speculative prices is the key element in Fama's development of procedures for testing market efficiency. Indeed, as Fama points out, virtually all empirical studies of speculative price returns (both pre- and post-"Proof") can be viewed as tests of this property and that remains the case to this day, which underscores further the significance of Samuelson's having established it as the crucial one for price behavior in an efficient market.

The early empirical studies focused on tests for serial correlation and comparisons of return performance between buy-and-hold and various simple filter-type trading strategies. While their results were on the whole consistent with market efficiency, these studies were, by necessity, limited to investigations of small numbers of securities and relatively short observation periods. This perhaps explains why the practicing financial community paid little attention to the results of those studies. However, with the development in the late 1960s of large-scale stock return data bases (principally at the University of Chicago Center for Research in Security Prices) and the availability of high-speed computers, there came an avalanche of tests of the efficient market theory, which were neither limited to a few securities nor to short observation periods.

Using return data on thousands of securities over more than forty years of history, some of the studies extended the earlier work comparing buy-and-hold with various mechanical trading strategies. Others, such as the Jensen (1968) study of mutual fund performance, broke new ground and analyzed the performance of real-life portfolio managers. In collectively echoing the findings of the earlier limited examinations, these large-scale studies put to final rest the myth that professional money managers can beat the market by miles, and indeed, cast doubt on whether they could even beat it by inches.

As the evidence in support of the efficient market theory mounted, the results and their implications for optimal strategy were widely disseminated to both the investing professional and the investing public in popular and semi-popular articles written by a number of academics. Included in this number is Paul Samuelson. With the widespread dissemination of this mountain of accumulated evidence, the practicing financial community could no longer ignore the efficient market theory although, as is perhaps not surprising, few (at least among the money managers in that community) accepted it. Here again, Samuelson exercises great care in his writings on this controversial issue by always keeping clear the distinction between "not rejecting" and "accepting" the efficient market theory. In discussing the controversy between practicing investment managers and academics in "Challenge to Judgment" (1974b, IV, Chap. 243, pp. 479–480), for example, he writes:

> Indeed, to reveal my bias, the ball is in the court of the practical men: it is the turn of the Mountain to take a first step toward the theoretical Mohammed, . . . If you oversimplify the debate, it can be put in the form of the question,
>
> Resolved, that the best of money managers cannot be demonstrated to be able to deliver the goods of superior portfolio-selection performance.
>
> Any jury that reviews the evidence, and there is a great deal of relevant evidence, must at least come out with the Scottish verdict:
>
> Superior investment performance is unproved.

With characteristic clarity, Samuelson provides a constructive perspective on the controversy by pointing out that while the existing evidence does not prove the validity of the efficient market theory, the burden of proof belongs to those who believe it to be invalid. In his final paragraph of

"Challenge to Judgment," (1974b, p. 485), he summarizes the point:

> What is interesting is the empirical fact that it is virtually impossible for academic researchers with access to the published records to identify any members of the subset with flair. This fact, although not an inevitable law, is a brute fact. The ball, as I have already noted, is in the court of those who doubt the random walk hypothesis. They can dispose of the uncomfortable brute fact in the only way that any fact is disposed of—by producing brute evidence to the contrary.

Later in the same journal, Samuelson revisits the question of market efficiency in real-world markets measured in terms of possible superior investment performance:

> Fifteen years have passed since my "challenge to judgment." What has been the further testimony of the 1970s and 1980s? What, in sum, is the judgment of 1989 economic science on the challenge to judgment?
>
> Broadly speaking, the case for efficient markets is a bit stronger in 1989 than it was in 1974, or in 1953 when Holbrook Working and Maurice Kendall were hypothesizing that stock and commodity price changes are pretty much a random walk (or a white-noise martingale). (1989b, p. 5)

5 years later, he reconfirms his position:

> To commemorate this Journal's fifteen years of success, I reviewed the cogency and accruing empirical verisimilitude of that agnostic questioning of activistic judgmental investing. By and large, the ball that was put in the court of the would-be judgment-mongers never did get returned with point-winning velocity. The jury of history did not find systematic inefficiency that exercisers of judgment could use to achieve excess risk-corrected returns.
>
> We can expect the debate to go on. And that tells you something about the approximate microefficiency of the organized markets where widely owned securities are traded. (1994, p. 15)

However, Samuelson is discriminating in his assessment of the efficient market hypothesis as it relates to real-world markets. He notes a list of the "few not-very-significant apparent exceptions" to microefficient markets (1989b, p. 5). He also expresses belief that there are exceptionally talented people who can probably garner superior risk-corrected returns . . . and names a few. He does not see them as offering a practical broad alternative investment prescription for active management since such talents are few and hard to identify. As Samuelson believes strongly in microefficiency of the markets, so he expresses doubt about *macro*-market efficiency, supporting the views of Franco Modigliani and Robert Shiller.

There is no doubt that the mainstream of the professional investment community has moved significantly in the direction of Paul Samuelson's position during the 30 years since he issued his challenge. Indexing as either a core investment strategy or a significant component of institutional portfolios is ubiquitous and even among those institutional investors who believe they can deliver superior performance, performance is typically measured incrementally relative to an index benchmark and the expected performance increment to the benchmark is generally small compared to the expected return on the benchmark itself. It is therefore with no little irony that as investment practice has moved in this direction these last 15 years, academic research has moved in the opposite direction, strongly questioning even the microefficiency case for the efficient market hypothesis. The conceptual basis of these challenges come from theories of asymmetric information and institutional rigidities that limit the arbitrage mechanisms which enforce microefficiency and of cognitive dissonance and other systematic behavioral dysfunctions among individual investors that purport to distort market prices away from rationally determined asset prices in identified ways. A substantial quantity of empirical evidence has been assembled, but there is considerable controversy over whether it does indeed make a strong case to reject market microefficiency in the Samuelsonian sense.[9] What is not controversial at all is that Paul Samuelson's efficient market hypothesis has had a deep and profound influence on finance research and practice for the past 40 years and all indications are that it will continue to do so well into the future.

18.3 Warrant and Option Pricing

If one were to describe the important research gains in financial economics during the 1960s as "the decade of capital asset pricing and market efficiency," then surely one would describe the corresponding research gains in the 1970s as "the decade of option and derivative security pricing." Once again, Samuelson was ahead of the field in recognizing the arcane topic of option pricing as a rich area for problem choice and solution. His research interest in options can be traced back at least to the early 1950s when he directed Richard Kruizenga's thesis on puts and calls (1956). As is evident from that thesis, Samuelson had already shown that the assumption of an absolute random walk for stock prices leads to absurd prices for long-lived options, and this before the rediscovery of Bachelier's work in which this very assumption is made. Although Samuelson lectured on option pricing at MIT and elsewhere throughout the 1950s and early 1960s, his first published paper on the subject, "Rational Theory of Warrant Pricing," appeared in 1965 (III, Chap. 199). In this paper, he resolves a number of apparent paradoxes that had plagued the existing theory of option pricing from the time of Bachelier. In the process (with the aid of a mathematical appendix provided by H. P. McKean, Jr), Samuelson also derives much of what has become the basic mathematical structure of option pricing theory today.[10]

Bachelier postulates that stock prices follow a random walk so that the expected change in the stock price over any interval of time is zero. The limit of this stochastic process in continuous time in modern terms is called a Wiener process or a Brownian motion. Bachelier also postulated that the price of a call option (or warrant) that gives its owner the right to buy the stock at time T in the future for an exercise price of *\$a* must be such that the expected change in the option price is also zero. From these postulates, Bachelier deduced that the option price, $W(X;\, T,\, a)$ must satisfy the partial differential equation

$$1/2\sigma^2 W_{xx}\left(X;T,a\right) - W_T\left(X;T,a\right) = 0$$

subject to the boundary condition $W(X;0,a) = \mathrm{Max}[0,\, X - a]$ where X *is* the price of the stock and σ^2 is the variance rate on the stock. The solution of this equation is given by

$$W(X;T,a) = (X-a)\Phi\left(\frac{X-a}{\sigma\sqrt{T}}\right) + \frac{1}{\sqrt{2\pi}} \exp\left[\frac{(X-a)^2}{2\sigma^2 T}\right]\sigma\sqrt{T}$$

where Φ ( ) is the standard normal cumulative density function. For an at-the-money option (i.e. X = a) and relatively short times to expiration T, the Bachelier rule that the value of option grows as $\sqrt{T}$ is a reasonable approximation to observed option prices. However, as Samuelson points out, for long-lived options the formula implies that the option will sell for more than the stock itself, and indeed, for perpetual options, (T = ∞), the value of the option is unbounded.

Samuelson traces this result to the absolute Brownian motion assumption which for T large implies the possibility of large negative values for the stock prices with nontrivial probability. Noting that most financial instruments have limited liability and, therefore, cannot have a negative price, Samuelson introduces the idea of "geometric Brownian motion" to describe stock price returns. By postulating that the logarithmic price changes, log $[X_{t+T}/X_t]$, follow a Brownian motion (with possibly a drift), he shows that prices themselves will have a lognormal distribution and, therefore, this ensures that they will always be nonnegative. Moreover, because lognormal distributions preserve themselves under multiplication, stock returns will have a lognormal distribution over any time interval. Indeed, this geometric Brownian motion has become the prototype stochastic process for stock returns in virtually all parts of financial economics.

Using much the same procedure of Bachelier, but modifying his postulates to include the geometric Brownian motion and the possibility of a nonzero expected rate of return on the stock, α, Samuelson derives a partial differential equation for the option price given by

$$1/2\sigma^2 X^2 W_{xx}(X;T,a) + \alpha X W_x(X;T,a) - \beta W(X;T,a) - W_T(X;T,a) = O$$

subject to $W(X;\, 0,\, a) = \text{Max}[O,\, X - a]$ where β is the required expected return on the option. For the case corresponding to Bachelier's where the required expected return on the option is the same as on the stock (i.e. $\beta = \alpha$), the solution can be written as

$$W(X;T,a) = X\Phi(h_1) - ae^{-\alpha T}\Phi(h_2)$$

$$\text{where } h_1 \equiv [\log(X/a) + (\alpha + 1/2\sigma^2)T]/\sigma\sqrt{T} \quad \text{and} \quad h_2 \equiv h_1 - \sigma\sqrt{T}.$$

Even when $a = 0$, Samuelson's solution satisfies $W(X;\, T,\, a) \leq X$ for all X and T. Hence, the substitution of the geometric Brownian motion for the arithmetic one eliminates the Bachelier paradox. However, as the reader can readily verify for $X = a$ and T small, $W(X;\, T,\, a) \sim \sigma\sqrt{T}$ as in the Bachelier case.

Bachelier considered options that could only be exercised on the expiration date. In modern times, the standard terms for options and warrants permit the option holder to exercise on or before the expiration date. Samuelson coined the terms "European" option to refer to the former and "American" option to refer to the latter.[11] Although real-world options are almost always of the American type, published analyses of option pricing prior to his "Rational Theory" paper focused exclusively on the evaluation of European options and therefore did not include the extra value to the option from the right to exercise early.

Because he only requires that the option price be equal to Max [O, X $-$ a] at the expiration date, Samuelson's ("$\beta = \alpha$") analysis formally applies only to a European type of option. However, he also proves that his solution satisfies the strict inequality $W(X; T, a) > \text{Max}\,[0, X - a]$ for $T > 0$ and $\beta = \alpha \geq 0$. Thus, under the posited conditions, it would never pay to exercise a call option prior to expiration, and the value of an American call option is equal to its European counterpart. In consequence, he views the special "$\beta = \alpha$" case of this theory as incomplete and unsatisfactory. It is incomplete because it provides no explanation of early exercise of options or warrants. Although it resolves the Bachelier paradox, the theory is unsatisfactory because it creates a new one; namely, the value of a perpetual call or warrant, $W(X; \infty, a)$ is equal to the stock price, X, independently of the exercise price. That is, according to the theory, the right to buy the stock at any finite price a (where this right can never be exercised in finite time) is equal to the price of the stock (which in effect is an option to buy the stock at a zero exercise price where the right can be exercised at any time).

Although he rejects the special case *of* his theory when $\beta = \alpha$, Samuelson resolves both its incompleteness and its paradox within the context of *his* general theory by simply requiring that $\beta > \alpha$. He does so by first formally solving his differential equation for the value *of* a European warrant. He then shows that for $\beta > \alpha \geq 0$ and any $T > 0$, there exists a number $C_T < \infty$ such that $W(X; T, a) < X - a$ for all $X \geq C_T$. Thus, for $\beta > \alpha$, there is always a finite price for the stock at which it pays to exercise prior to the expiration date, and hence, the American feature of an option has positive value. He also shows that $\beta > \alpha$, $W(X; T, a) < X$ for a > 0 and the value of a European perpetual call option, $W(X; \infty, a)$ is zero.

Having established that the early exercise provision has value when $\beta > \alpha$, Samuelson then proves that the correct formula for an American call option or warrant will satisfy his partial differential equation subject to the boundary conditions: (1) $W(O; T, a) = 0$; (2) $W(X; 0, a) = \text{Max}\,[0, X - a]$; (3) $W(C_T; T, a) = C_T - a$; (4) $W_X(C_T; T, a) = 1$, which he calls the "high-contact" condition.[12] For those familiar with parabolic partial differential equations *of* this type, it may appear that the boundary conditions are overspecified. However, C_T, which is the time boundary *of* stock prices where the option should be exercised, is not known, and it is precisely the overspecification that permits the simultaneous determination of the option price and the time boundary. Of course, closed-form solutions to such boundary value problems are not easy to derive although Samuelson does solve the perpetual call option case. He also develops a recursive integral technique that is a precursor to the numerical approximation methods used to this day to solve these equations.

While Samuelson mentions the greater riskiness of a warrant over the stock and different tax treatment, his principal argument for the $\beta > \alpha$ case and possible early exercise is that the stock is paying or may pay dividends during the life of the warrant. As formulated in his differential equation, α is the expected rate of price appreciation in the stock and, therefore, will be equal to the expected rate of return on the stock only if there are no cash dividends. In the example he discusses at length, where the dividend rate is a constant fraction, δ, *of* the stock price, he shows that for the expected rate of return on the warrant to just equal that of the stock $\beta = \alpha + \delta$, and therefore, $\beta > \alpha$. This analysis also makes it clear why a perpetual warrant on a currently nondividend-paying stock will not have a price equal to the stock price (as predicted by the $\beta = \alpha$ theory): namely, it could only do so if it were believed that the stock would never pay a dividend.

As Samuelson would be the first to say, his 1965 warrant pricing theory is incomplete in the sense that it simply postulates the first-moment relations between the warrant and stock. Yet, the basic intuitions provided by his theory have been sustained by later, more complete, analyses. For example, his focus on dividends as the principal reason for early exercise of call options and warrants was later justified in his 1969 "A Complete Model of Warrant Pricing That Maximizes Utility" (III, Chap. 200) (He brought me along as his junior coauthor), where it was shown that dividends are the only reason for such early exercise. Still later, an arbitrage argument presented in Merton (1973) proves that this result holds in general. Earlier warrant pricing theories uniformly neglected the possibility of early exercise in the development of their evaluation formulas. Samuelson, in addition to proving that early exercise was a possibility, shows that the effect of this possibility on value can be quite significant especially for long-lived options and warrants. Furthermore, his demonstration that the schedule of stock prices at which the warrant should be exercised can be endogenously determined as part of a simultaneous solution for the warrant price provides one of the cornerstones of modern option pricing theory and its application to the evaluation of more complex securities.[13]

In a subsequent conversation with me, Samuelson contrasted the "Rational Theory" with its companion piece "Proof That Properly Anticipated Prices Fluctuate Randomly" by noting that "the results of the paper were not obvious," and that he "was not sure how they would come out until the work was done." Despite his obvious delight with the paper (I do not doubt that this is his favorite among his contributions to financial economics) and despite the many important contributions it contains, discussion of the paper led Paul to remark that "Too little is written about the 'near misses' in science." While far from unique in the history of science, Samuelson's "Rational Theory" is surely a prime example of such a near miss by an eminent scientist.

Open the financial section of a major newspaper almost anywhere in the world and you will find pages devoted to reporting the prices of exchange-traded derivative securities, futures, warrants and options. Along with the vast over-the-counter derivatives market, these exchange markets trade options and futures on individual stocks, stock index and mutual-fund portfolios, on bonds and other fixed-income securities of every maturity, on currencies, and on commodities including agricultural products, metals, crude oil and refined products, natural gas, and even, electricity. The volume of transactions in these markets is often multiple times larger than the volume in the underlying cash-market assets.[14] Options have traditionally been used in the purchase of real estate and the acquisition of publishing and movie rights. Employee stock options have long been granted to key employees.

In all these markets, the same option-pricing methodology is used both to price and to measure the risk exposure from these derivatives. However, financial options represent only one of several categories of applications for the option-pricing technology. "Option-like" structures are lurking everywhere.[15]

Virtually everyone would agree that the Black–Scholes option pricing model published in 1973 was the breakthrough that led to an explosion in option and derivative security pricing research in the 1970s that has had widespread impact on finance research and practice to the current time. I focus here only upon the development of the Black–Scholes option pricing formula and its relation to Samuelson's "Rational Theory" formula.

The foundation of the Black–Scholes model is that, at least in principle, a dynamic hedging strategy can be derived to form a riskless portfolio of the option, the stock, and riskless bonds. Moreover, if such a portfolio can be created, then to avoid the opportunity for arbitrage, it must yield a return exactly equal to that earned on a riskless bond. From this condition, it follows that there must be a unique relation among the option price, the stock price, and the riskless interest rate.

Of course, hedge strategies using a warrant or other convertible securities and the stock were not uncommon undertakings by practitioners long before 1973. Thorp and Kasouff's *Beat the Market* (1967) is devoted entirely to such hedging strategies. In his "Rational Theory" paper, Samuelson discusses at length (including numerical examples) the use of hedge positions between the warrant and the stock as a means for deriving bounds on the discrepancies between β and α. These bounds translate through his warrant pricing equation into bounds on the range of rational warrant prices. In this discussion, he goes on to mention that the opportunity cost or carrying charges for the hedge should be included and therefore, the riskless rate of interest would enter into the bounds. Thus, Samuelson had in his paper the hedging idea for restricting prices and the possibility that the interest rate would enter into the evaluation, both of them key elements in the Black and Scholes analysis. Yet, neither he nor the others pushed their ideas in this area the extra distance required to arrive at what became the Black–Scholes formula. As Samuelson later wrote in "Mathematics of Speculative Price" (1972a, IV, Chap. 240, p. 438),

> My 1965 paper had noted that the possibility of hedging, by buying the warrant and selling the common stock short, should give you low variance and high mean return in the $\beta > \alpha$ case. Hence, for dividendless stocks, I argued that the $\beta - \alpha$ divergence is unlikely to be great. I should have explored this further!

The most striking comparison to make between the Black–Scholes analysis and Samuelson's "Rational Theory" is the formula for the option price. In their derivation, Black and Scholes assume a nondividend-paying stock whose price dynamics are described by a geometric Brownian motion with a resulting lognormal distribution for stock returns.[16]

This is, of course, the identical assumption about stock returns that Samuelson made. Under these conditions, the Black–Scholes no-arbitrage price for a European call option, $F(X; T, a)$, is shown to be the solution to the partial differential equation

$$1/2\sigma^2 X^2 F_{xx}(X; T, a) + rXF_x(X; T, a) - rF(X; t, a) - F_T(X; T, a) = 0$$

subject to the boundary condition $F(X; 0, a) = \mathrm{Max}[0, X-a]$ and where r is the (instantaneous) riskless rate of interest that is assumed to be constant over the life of the option. By inspection, this equation is formally identical to the one derived in the "Rational Theory" for the special "$\beta = \alpha$" case if one substitutes for the value of "a" the interest rate "r." It follows, therefore, that the Black–Scholes option pricing formula, $F(X; T, a)$, is formally identical to the Samuelson option pricing formula, $W(X; T, a)$, if one sets $\beta = \alpha = r$ in the latter formula.

It should be underscored that the mathematical equivalence between the two formulas (with the redefinition of the parameter α) is purely a formal one. That is, the Black–Scholes analysis shows that the option price can be determined without specifying either the expected return on the stock, α, or the required expected return on the option, β. Therefore, the fact that the Black–Scholes option price satisfies the Samuelson formula with $\beta = \alpha = r$ implies neither that the expected returns on the stock and option are equal nor that they are equal to the riskless rate of interest. Indeed, Samuelson notes in his "Mathematics of Speculative Price" (1972a) that even if α is known and constant, β will not be for finite-level options priced according to the Black and Scholes methodology. It should also be noted that Black–Scholes pricing of options does not require knowledge of investors' preferences and endowments as is required, for example, in the Samuelson–Merton (1969) warrant pricing paper. The "Rational Theory" is clearly a "miss" with respect to the Black–Scholes analysis. However, as this analysis shows, it is just as clearly a "near miss."

This said, it may seem somewhat paradoxical to suggest that the Black–Scholes breakthrough actually added to the significance of Samuelson's "Rational Theory" for the field, yet I believe it did. Before Black–Scholes, there were a number of competing theories of warrant and convertible security pricing. Some, of course, were little more than rules of thumb based on empirical analyses with limited data. Others, however, like the "Rational Theory," were quite sophisticated. The Black–Scholes analysis provides a degree of closure for the field on this issue, and thus renders these earlier theories obsolete. However, as noted here and as shown in detail in the Appendix to "Mathematics of Speculative Price" (Merton, 1972), virtually all the mathematical analysis in the "Rational Theory" (including its formidable McKean appendix) can be used (with little more than a redefinition of parameters) to determine the prices of many types of options within the Black–Scholes methodology. For example, consider options where early exercise can occur. As is shown in Merton (1973), one can solve for the Black–Scholes price of either a European or an American call option on a proportional-dividend-paying stock simply by substituting $\beta = r$ and $\alpha = r - \delta$ into the "Rational Theory" analysis of the "$\beta > \alpha$" case. Similar results obtain for the evaluation of put options.

As a second example, there is the solution in the McKean appendix for the price of an option on a stock whose return is a Poisson-directed process that is discussed in Cox and Ross (1976) and Merton (1976). As still a third example, there is the Samuelson development in the "Rational Theory" of the partial differential equation for option pricing and its solution that uses a limiting process of discrete-time recursive difference equations and a local binomial process for stock price returns. This development is formally quite similar to the simplified procedure for Black–Scholes option pricing presented in Cox–Ross–Rubinstein (1979) and Sharpe (1978) as well as to the numerical evaluation procedure for options in Parkinson (1977). In light of these consequences, Samuelson's "Rational Theory of Warrant Pricing" is some near miss!

18.4 Investing for the Long Run

In so many branches of economics, Paul Samuelson is a kind of gatekeeper. When he is not busy opening gates to new research problems for himself and an army of other economists to attack, he is busy closing gates with his definitive solutions. And in between, he somehow finds the time to convey to both the professional practitioner and the general public those important research findings that have survived the rigors of both careful analytical and empirical examination.

Samuelson's new discoveries in finance are foundational. However, his diligence in trying to subvert error is also deeply important to the field. Just as in investing where the most gold goes to those who show us how to make money, so the most academic gold (or credit) goes to new discoveries. But in investments, as Samuelson's work in efficient markets and portfolio theory amply demonstrates, there is also considerable value to being shown how not to lose money by avoiding financial errors. Just so, there is also considerable value to those who divert us away from the paths of error in research.

By defanging the St Petersburg Paradox, Samuelson (1960, 1977) has taught us not to unduly fear unbounded utility and, thereby, he has left intact the important body of research into the economics of uncertainty that is based upon the Hyperbolic Absolute Risk Aversion (HARA) family of utility functions, most of whose members are unbounded functions. While defending the legitimacy of the HARA family, he has also kept us from becoming enthralled with the enticing geometric mean maximization hypothesis where log utility, a particular member of the family, is proclaimed to be the criterion function for "super" rational choice.[17] Samuelson discriminates among brain children, and his success in saving the profession from being drawn further along these paths of error has been due in no small part to his willingness to reaffirm basic beliefs whenever, like the phoenix, some new version of an old error arises. Disposing of one version in his "The 'Fallacy' of Maximizing the Geometric Mean in Long Sequences of Investing or Gambling" (1971b, III, Chap. 207), Samuelson returned to battle a second one (this time taking me along as coauthor) in

"Fallacy of the Lognormal Approximation to Optimal Portfolio Decision Making Over Many Periods" (1974, IV, Chap. 245). Still later in 1979, he countered a third with his paper of the monosyllabled title, "Why We Should Not Make Mean Log of Wealth Big Though Years to Act Are Long?"

Beginning sometime in the early 1980s, a new fallacy, also associated with long-horizon investing, arose that over the next two decades would have a far greater impact on real-world practice than the fallacy of investing so as to maximize the expected log return on one's portfolio. This new fallacy prescription is that "Stocks are not risky in the long run." That is, over a long enough investment horizon, stocks will outperform risk-free long-maturity bonds and so investors with long-term investment goals such as saving for retirement should invest their retirement savings in equities.

This prescription, like the max expected log strategy, is driven formally by an assessment that one investment strategy will outperform another (or all others) with increasingly greater probability the longer the investment horizon, until in the limit of an infinite horizon, the probability of superior performance approaches 100 percent. As a matter of mere mathematics, it can indeed be shown that under relatively mild assumptions about the expected return on the stock market and its volatility, the probability that stocks will *underperform* bonds goes to zero as the horizon becomes infinite and that indeed over a 25–35 year horizon the estimated probability of such a "shortfall" is in low single digits. The apparent (asymptotic long run) dominance of stocks over bonds permits nearly universal and uniform advocacy for this investment policy, independently of individual economic status. Hence, it is argued that investors with a long-horizon goal should invest in stocks over bonds, *without regard to their risk-tolerance preferences*. Further "practical" support for this prescription was provided by observing that historical returns on the US stock market outperformed bonds over every (or nearly every) 15- or 20-year time period in the last century. Nearly every advice engine on the Internet offers this same age-dependent strategy as a fundamental principle of retirement saving. The same principle is central to asset allocation advice to corporate pension funds.

Characteristically, Samuelson recognized early on that the question of the effect of age on risk-taking and optimal portfolio selection was an important issue, worthy of careful scientific analysis. And so, in 1969, Samuelson published a paper on the optimal intertemporal portfolio selection and consumption problem, which applied the method of stochastic dynamic programming together with the Expected Utility criterion.

Although others studied the problem,[18] it was Samuelson who focused his analytical modeling on the substantive issue of age-dependent influences on portfolio allocations and often-discussed-but-not-well-defined related concepts such as "businessman's risk." He shows that risk-averse investors with constant-relative-risk-aversion (CRRA) utility functions (which includes the heralded log function) and facing the same investment opportunities each period of their investment life, would allocate the same fractions of their optimal portfolio between risky equities and safe short-term debt, *independently of their age*. And while this surely does not rule out age-dependent portfolio behavior for some preferences, it just as surely demonstrates that growing investment conservatism with age is not a robust optimization principle which obtains universally. And in particular, the Samuelson finding provides absolute counter-evidence against the claimed absolute dominance of investing in equities over bonds when the investment horizon becomes very long. And this is so even though the temporally independently and identically distributed returns for equities posited in the Samuelson model also satisfy the probability condition that as investment horizon increases, the probability that equities underperform bonds decreases, asymptotically approaching zero.

The period 1969–82 just after the publication of Samuelson's paper shows no widespread adoption of this prescription for long-horizon investors to allocate a large fraction of their portfolios to equities, perhaps because it was a very poor one for stock performance in the United States. The creation of ERISA and with it, the corporate pension fund industry in 1974 thus did not cause equities to become a significant part of pension fund portfolios immediately. However, by the late 1980s after some strong performing years, institutional pension-fund investors had moved their allocation to equities increasingly to the point of their dominating the typical portfolio. The large shift to equities was encouraged by the actuarial treatment of pension expenses that applied the traditional Law of Large Numbers approach to argue that expected returns on the pension asset portfolio could be treated as virtually sure-thing returns over the long horizon of pension liabilities and so projected pension expense would be reduced by holding larger expected return (and larger risk) equities instead of bonds. This "institutionalization" of the principle that "Stocks are not risky in the long run" was completed when the pension accounting rules were adopted that called for firms to use the *projected* pension expense in computing accounting earnings for the firm instead of the *realized* pension expense based on the actual performance of the pension fund portfolio, with any reconciliation of deviations between the two smoothly amortized over a 10-year period.

In a series of papers, Samuelson (1989b, c, 1990, 1994, 1997a, b) was quick and clear to define the issue: Investing in equities may well be part of an optimal investing strategy for pensions as elsewhere, so long as the risk that goes with the higher expected return on equities is properly accounted for in the decision. What is fallacious... and therefore dangerous... reasoning is the misapplication of the Law of Large Numbers to argue that these higher expected returns will turn into higher realized returns almost certainly, if one has a long enough horizon, and thus with a long horizon one need pay *no* attention to the risk component. Samuelson presented his position both in intuitive fashion and in very formal mathematical terms why the exclusive focus on diminishing probability of a shortfall from equities as horizon lengthens is not sufficient for dominance because it does not take into account the growing magnitude of the present value of the expected shortfall that occurs as horizon increases. That is, what matters is the product of the probability of stocks underperforming times the present value of the conditional expected shortfall when they do, and while the probability is declining, the present value of that expected shortfall is growing and so one needs to consider the net growth or decline of their product with horizon. Furthermore, it turns out that the product grows with longer horizon and thus, the shortfall risk in that sense is not declining at all but increasing.[19]

Samuelson along with others also highlighted the fallacy in simply taking the realized stock returns in the United States for the last century or more as statistically significant empirical proof of the dominance principle by pointing out that from a statistical perspective that long history is only a single sample. He then goes on constructively to specify the proper representation which uses the historical data in what is formally a "bootstrap" process to generate by Monte Carlo techniques the prospective distributions from the past. These distributions demonstrate that a significant shortfall risk does exist, even with a long horizon. Samuelson and others also noted that the data themselves are subject to selection bias in that the United States stock market performance over the twentieth century may not be an unbiased estimate of the future for it or any other country's. Had the focus instead been on the investment history over the same period in other countries, Argentina, Russia, and Japan for instance, the "obvious" empirical evidence for nearly sure-thing outperformance of stocks over bonds in the long run would hardly be so obvious.

Despite the cautioning writings of Samuelson and others on the subject, the influence of the "Stocks are not risky in the long run" principle actually expanded and grew enormously with the creation of

Define–Contribution 401k pension plans in the beginning of the 1990s, in which individuals are directly responsible for allocating their retirement savings. Every advice engine, whether on the Internet or at a mutual fund complex, had this as one of its foundational principles. The extraordinary performance of the US stock market in the 1990s only served to confirm the validity of the principle, even in the not so long run.[20] The related argument for age-dependent growing conservatism exemplified by the rule of thumb "Invest fraction 100 minus your age in stocks" was institutionalized by the mutual fund industry that offers life-cycle funds that adjust the stock-bond mix toward more bonds as one gets older. Having correctly educated investors on the power of diversification among assets as an efficient means for managing risk, intuitive explanations by analogy were put forward claiming that diversification across time works in a similar way to justify the principle of more stock allocation the longer the horizon. Indeed, the principle that one can earn high equity expected returns with virtually no risk if one has a long horizon is tailor-made for arguing that Social Security should consider funding with investments in equities whether in private accounts or the government-controlled fund.[21]

Throughout this period, Samuelson was steadfast in making the case that there are no shortcuts to taking into account risk. Because the performance of 401k plans go directly to individuals, he reiterated the points made by his 1969 paper that sensible preference functions for evaluating the risk-return trade-off do not necessarily lead to ever increasing allocations to stock as one has longer time until retirement. He demonstrated formally and in simple illustrations the fallacy of time diversification (1997b).

Characteristically, Samuelson having made the strong multidimensional case against universal age-dependent arguments for holding a larger fraction in stocks the longer the horizon until retirement, then goes on to investigate what characteristics of the return distribution would cause those counter-example CRRA-utility investors of his 1969 paper to hold more equities the longer the time until retirement. In Samuelson (1989a, 1991, 1997b), he shows that such age-dependent behavior will obtain if one replaces intertemporally independently and identically distributed stock returns (a "white noise" process) posited in his 1969 paper with stock returns that exhibit mean-reversion or negative serial correlation (what he calls a "red noise" process). However, he also shows that the age-dependent behavior can go in the opposite direction with a larger fraction of the portfolio allocated to risky equities the *shorter* the time left before retirement, if stock returns exhibit momentum or positive serial correlation (what he calls a “blue noise” process). Having made these affirmative cases when age-dependent portfolio allocation is optimal, he points out that the evidence for either mean-reversion or momentum in stock returns is hardly overwhelming. He concludes by reaffirming his position that stocks are risky in the short, intermediate, and long runs and that arguments for holding stocks based on a contrary belief are fundamentally flawed.

After the large three-year decline in equity markets and interest rates between 2000 and 2002, there were widespread, deep losses in corporate pension fund portfolios and in individual retirement accounts. Together with the fall in interest rates which caused pension liabilities to increase at the same time, the effect was to cause enormous shifts toward large underfunding of corporate pension plans, which in weakened industries such as steels, airlines, and automobiles has caused, or at least accelerated, bankruptcies. These failures in turn have caused the government insurer of corporate pensions, Pension Benefit Guarantee Corporation (PBGC), to incur enormous losses, going from a large reserve surplus to a huge negative shortfall on its balance sheet, raising the specter of another taxpayer-bailout as was experienced with deposit insurance and the thrift institutions in the 1980s. The ceiling on PBGC insurance coverage has in turn led to large losses in accrued pension benefits by higher-paid workers in these industries.

With these events, corporate plan sponsors, pension regulators, and other overseers have taken notice: Rating agencies are already taking into account pension underfunding on setting credit ratings and it is a safe prediction that they will move from there to recognizing that the risk as well as the expected return of pension fund assets, like any other risky asset of the corporation, needs to be taken into account in assessing the creditworthiness of the firm. The Financial Accounting Standards Board in the United States is currently studying widespread pension accounting reforms with focus on the use of projected pension expenses instead of actual expenses for determining earnings of the firm. Similar reforms are already further underway in the United Kingdom and in the setting of international accounting standards.

Today, 36 years after the publication of Samuelson’s paper identifying and analyzing age-dependent optimal rules for long-horizon investing, we thus find that work at the center of some of the most important private- and public-sector finance-related policy issues around the world.

18.5 Afterword

As noted at the outset of my remarks, a prevailing theme of research in financial economics is the conjoining of intrinsic intellectual interest with extrinsic practical application. This research has significantly influenced the practice of finance whether it be on Wall Street, LaSalle Street, or in corporate headquarters throughout the world. In this regard, Paul Samuelson provides a sterling counterexample to the well-known dictum of Keynes that "practical men, who believe themselves to be quite exempt from intellectual influences, are usually the slaves of some defunct economist." Any attempt to trace *all* the paths of influence that Samuelson has had on finance practice is, of course, doomed to failure—we need only remember the seemingly countless editions of his basic textbook on which so many practitioners were reared.[22]

As in all fields where the research is closely connected with practical application, in financial economics, conflicts in problem choice are not uncommon between those that have the most immediate consequences for practice and those that are more basic. As is evident from the following excerpt from his Foreword to *Investment Portfolio Decision-Making* (1974c, IV, Chap. 244, p. 488), there is surely no doubt how Paul Samuelson resolves such conflicts in his own research.

> My pitch in this Foreword is not exclusively or even primarily aimed at practical men. Let them take care of themselves. The less of them who become sophisticated the better for us happy few! It is to the economist, the statistician, the philosopher, and to the general reader that I commend the analysis contained herein. Not all of science is beautiful. Only a zoologist could enjoy some parts of that subject; only a mathematician could enjoy vast areas of that terrain. But mathematics as applied to classical thermodynamics is beautiful: if you can't see that, you were born color-blind and are to be pitied. Similarly, in all the branches of pure and applied mathematics, the subject of probability is undoubtedly one of the most fascinating. As my colleague Professor Robert Solow once put it when he was a young man just appointed to the MIT staff: "Either you think that probability is the most exciting subject in the world, or you don't. And if you don't, I feel sorry for you."
>
> Well, here in the mathematics of investment under uncertainty, some of the most interesting applications of probability occur. Elsewhere, in my 1971 von Neumann Lecture before the Society for Industrial and Applied Mathematics, I have referred to the 1900 work on the economic Brownian motion by an unknown French professor, Louis Bachelier. Five years before the similar work by Albert Einstein, we see growing out of economic observations all that Einstein was able to deduce and more. Here, we see the birth of the theory of

stochastic processes. Here we see, if you can picture it, radiation of probabilities according to Fourier's partial differential equations. And finally, as an anti-climax, here we see a way of making money from warrants and options or, better still, a way of understanding how they must be priced so that no easy pickings remain.

In short, *first things first*.

There is no need to dwell on the prolific and profound accomplishments of Paul Samuelson, which have become legend—especially when the legend is a brute fact. Rather I close with a few observations (drawn as his student, colleague, and coresearcher over nearly four decades) on some of Paul's modes of thought that perhaps make such super achievement possible. First, there is his seemingly infinite capacity for problem finding and his supersaturated knowledge of just about every special sphere of economics. Second, there is his speed of problem solving together with the ability to put the solution quickly to paper with great skill, great verve, and lack of hesitation. Third, strong opinions and decisive language are characteristic of Samuelson writings, and yet it is his willingness to change his views and admit errors that makes his steadfastness on some issues so credible. Finally, although often masked by the apparent ease with which he produces, there is his diligence. Paul has always worked hard.

On the matter of sustained hard work of this particular kind, Paul is fond of a story (and so, he repeated it in his Presidential Address to the International Economics Association) about the University of Chicago mathematician Leonard Dickson, who was to be found playing bridge all afternoon every afternoon. When a colleague asked how he could afford to spend so much of his time playing, Dickson is said to have replied: "If you worked as hard at mathematics as I do from 8–12, you too could play bridge in the afternoon." As Paul also notes in that address, the same story holds for the mathematician G. H. Hardy, who watched cricket rather than play bridge. I can improve on these yarns with one about Paul from the glorious days when as his research assistant I lived in his office. I was working (not very successfully) on the solution to an equation in warrant pricing that was needed for some research Paul was doing when he left for the tennis courts (as he often did). Sometime later, the phone rang. It was Paul calling from the courts (presumably between sets) to tell me exactly how that equation could be solved. Dickson and Hardy segregated creative work and well-earned play, and so, it appears, does Paul, but with a finite and significant difference. Even at play, he is at work.[23]

Notes

1. Samuelson offers us some brief synthesizing observations on foundational developments in the field in his recent "Modern Finance Theory Within One Lifetime (2002)," but characteristically he confines his remarks only to the contributions of others.
2. See Samuelson (1950, 1952a, b, 1967a, b, 1970a).
3. The explicit content of Samuelson's early work reviewed here, of course, has not changed but its subsequent application and impact on the field, both in breadth and depth, surely has. Hence, even when overlaps with my past writings occur, the substance of Samuelson's work warrants repeating here, especially when the originals appear in obscure places. Thus, when applicable, the text draws heavily on my 1983 essay.
4. See Taqqu (2001) for more on the Bachelier story.
5. As Samuelson notes with his typical great care, without the tranversality or other terminal boundary condition, these local arbitrage conditions are necessary but not sufficient to ensure an optimal path.
6. Samuelson's draft is not dated but I would estimate 1980. The acknowledgment helped pin it down: "We owe thanks to the National Science Foundation for financial aid and to Aase Hugins for editorial assistance. Hal Stern, an MIT senior, kindly tested the data to verify its conformity with theoretical expectations." Hal Stern graduated from MIT in 1981.
7. Note that within this model, excess returns exhibit both mean-reversion and momentum, depending on their size: mean-reversion behavior for small-in-magnitude excess returns and momentum behavior for large-in-magnitude returns. Thus, we have in this early Samuelson work a conditional combination of both his "red noise" and "blue noise" processes for stock returns that he introduces in later work (1989a, 1991, 1997b) to demonstrate possible properties of age-dependent optimal portfolio selection rules.
8. It can be shown that if investor learning is sufficient to wipe out the profitable trading structure, the resulting new excess return process for the model of the example will be $X_t = \varepsilon_t[1 + a(\varepsilon_t^2 - b)]$ for which the martingale property obtains.
9. See Lo and MacKinlay (1999). Merton and Bodie (2005, especially p. 4, footnotes 8 and 9) provide extensive references on both sides of the controversy.
10. Samuelson uses warrants instead of call options as the specific instrument examined in his paper, perhaps because at that time, warrants were listed and traded on exchanges and so price data were available whereas options were only traded through dealers with opaque pricing. Indeed, I tested the Samuelson pricing model in the late 1960s using prices of listed perpetual warrants [Merton, 1969]. Although there is a slight difference between the two in terms of dilution effects depending on whether the company is the issuer or not, the pricing models for warrants and call options are essentially the same and the terms are used interchangeably for purposes of the discussion here.

11. Samuelson started to formulate his theory of warrant and option pricing in the mid-late 1950s. As he often did, and still does, with a new area of research, he began then by talking to those in practice to get a sense of how it all works institutionally before proceeding with the formal model specification and theory development. So he went to New York to see a well-known put and call dealer (there were no traded options exchanges until 1973) who happened to be Swiss. After identifying himself and explaining what he had in mind, Samuelson was quickly told, "You are wasting your time—it takes a *European* mind to understand options." Later on, Samuelson understandably chose the term "European" for the relatively simple(-minded)-to-value option contract that can only be exercised at expiration and "American" for the considerably more-complex-to-value option contract that could be exercised early, any time on or before its expiration date.
12. Later authors refer to this as the "smooth-pasting" condition.
13. Merton (1972, 1973) proves that the Samuelson-posited "high-contact" condition is implied by the unique value-maximizing early-exercise strategy that rules out arbitrage possibilities.
14. A recent Federal Reserve estimate is that $270 trillion notational amount of derivatives are outstanding worldwide.
15. Examples are insurance contracts including deposit and pension insurance, loan guarantees, privatization of Social Security, prepayment of mortgages, farm price supports, oil-drilling and automobile leases, quotas on taxis and fishing, patents, tax and market timing, tenure, labor-force training, health plans, pay-per-view television, retail store shelf space, modularity and flexibility in production processes, drug discovery phasing, and movie sequel timing. See Merton (1992, 1998) for references. Jin *et al.* (1997) provide a live website with an extensive and growing listing of applications.
16. In a 1968 critique of the Thorp–Kasouff book, Samuelson quite correctly warns the reader that their reverse-hedge techniques in expiring warrants are no "sure-thing" arbitrage. Later (1972a, IV, Chap. 240, p. 438, n. 6), he reiterates a similar valid warning in his discussion of the Black–Scholes arbitrage argument. If, however, Samuelson had not discovered this overstatement in the Thorp–Kasouff analysis so quickly, then he might have used the occasion to pursue further his own earlier work in using hedge strategies to restrict the range of rational warrant prices. Perhaps this thought was in his mind when Paul commented to me on his 1968 review as one in which "I won farthings and lost pounds."
17. Cf. Kelly (1956), Latane (1959), Markowitz (1976), and Thorp (2004).
18. Other early developers of this problem include Edmund Phelps, Nils Hakansson, Hayne Leland, and Jan Mossin. I developed a continuous-time version.
19. Bodie (1995) provides an elegant demonstration of this point when he shows that the cost of buying "shortfall insurance" which is structurally a put option on equities with strike price equal to the forward price of the current value of

the portfolio, is an increasing function of the investment horizon. Through his foundational work on options, Samuelson contributed, albeit indirectly, to the Bodie demonstration as well.

20. Unfortunately the experience in Japan, the second largest economy in the world, during this period was quite the opposite: In 1989, the Japanese stock market hit a peak of over 39,000 and today, 16 years later, it is 14,700.
21. It has been noted by a number of observers including Paul Samuelson that the government has an even longer horizon than any pension plan and furthermore, with a central bank it has no short-term liquidity problems, and so if the no-long-term-risk-to-stock-returns principle applies validly to retirement savings, why not apply it to funding *all* government expenditures?
22. Bernstein (1992) nevertheless provides a rich description of the many paths of Samuelson's influence on modern Wall Street.
23. Happily, some things do not change. A few days ago, Paul called me (this time I was on a cell phone in a taxi cab) to discuss a certainty–equivalent calculation he was doing to demonstrate in still another enlightening way why the Kelly Criterion is not even near-optimal for those with nonlog preferences that also do not risk ruin. After he painstakingly described the detailed calculations he was performing in the mere two-period case, he asked whether they were correct. I responded that perhaps I could check them with pencil and paper after reaching my destination. Paul then reminded me that Student was (reputed to be) able to compute Pearsonian correlation coefficients in his head. The message was clear. After my later checking, Paul's calculations were indeed correct.

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Part II

Samuelson's Relevance

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19

Multipliers and the LeChatelier Principle

Paul Milgrom

19.1 Introduction

Those studying modern economies often puzzle about how small causes are amplified to cause disproportionately large effects. A leading example that emerged even before Samuelson began his professional career is the Keynesian multiplier, according to which a small increase in government spending can have a much larger effect on economic output. Before Samuelson's LeChatelier principle, however, and the subsequent research that it inspired, the ways that multipliers arise in the economy had remained obscure.

In Samuelson's original formulation, the LeChatelier principle is a theorem of demand theory. It holds that, under certain conditions, fixing a consumer's consumption of a good X reduces the elasticity of the consumer's compensated demand for any other good Y. If there are multiple other goods, X^1 through X^N, then fixing each additional good further reduces the elasticity. When this conclusion applies, it can be significant both for economic policy and for guiding empirical work. On the policy side, for example, the principle tells us that in a wartime economy, with some goods rationed, the compensated demand for other goods will become less responsive to price changes. That changes the balance between the distributive and efficiency consequences of price changes, possibly favoring the choice of nonprice instruments to manage wartime demand. For empirical researchers, the same principle suggests caution in interpreting certain demand studies. For example, empirical studies of consumers' short-run responses to a gasoline price increase may underestimate their long-run response, since over the long run more consumers will be free to change choices about other economic decisions, such as the car models they drive, commute-sharing arrangements, uses of public transportation, and so on. However, the principle tells us those things only when its assumptions are satisfied, so Samuelson made repeated efforts during his career to weaken the assumptions needed for the principle to apply.[1]

Newer treatments of the LeChatelier principle differ in several important ways from Samuelson's original. First, while the original conclusion applies solely to the choices of an optimizing agent, the newer extensions apply also to many other equilibrium systems. Second, the original conclusion was a local principle that applied only to small parameter changes, while the modern extension is a global principle that applies to all parameter changes, large and small. Finally, the original principle gives at least the appearance of great generality, because it applies locally for any differentiable demand system, while the modern extension depends on a restriction. However, because the restriction always holds locally for differentiable demand systems, the modern principle actually subsumes the original.

All versions of the LeChatelier principle explain how the direct effect of a parameter change can be amplified by feedbacks in the systems in which they are embedded. Thus, the principles provide a foundation for understanding economic multipliers and, more generally, how it may be that small causes can have large effects.

19.2 A Local LeChatelier Principle for Optimization Problems

To explain Samuelson's original LeChatelier principle and set a context for the modern extensions, we restrict attention to the simplest form of the principle—one governing the choices of a profit-maximizing firm with just two inputs. Define the firm's unrestricted and restricted choice functions as follows:

$$x^U(w) \text{ solves } \max_x f(x) - w \cdot x \tag{19.1}$$

$$x^R(w, \bar{x}_2) \text{ solves } \max_x f(x) - w \cdot x \text{ subject to } x_2 = \bar{x}_2 \tag{19.2}$$

In the unrestricted problem (19.1), the firm maximizes profits over a set such as $\Re^2_+$, choosing quantities of both inputs. In the restricted problem (19.2), the firm maximizes profits subject to the additional constraint that its "choice" for input 2 is exogenously given. Clearly, if the maximum is unique at the prices $\bar{w}$ and $\bar{x}_2 = x_2^U(\bar{w})$, then $x^U(\bar{w}) = x^R(\bar{w},\bar{x}_2)$. Then, the traditional LeChatelier principle is the following result.

Theorem 19.1. Suppose that the functions $x^U(w)$ and $x^R(w,\bar{x}_2)$ are well defined and continuously differentiable in w_1 in a neighborhood of $w = \bar{w}$ and that $\bar{x}_2 = x_2^U(\bar{w})$. Then, $(\partial x_1^U/\partial w_1)(\bar{w}) \le (\partial x_1^R/\partial w_1)(\bar{w},\bar{x}_2) \le 0$

Proof. Let $\pi^U(w) = \max f(x) - w\cdot x$ and $\pi^R(w,\bar{x}_2) = \max_x f(x) - w\cdot x$ subject to $x_2 = \bar{x}_2$ be the corresponding unrestricted and restricted profit functions. Since the value is always higher in a problem with fewer constraints, $\pi^U(w) \ge \pi^R(w,\bar{x}_2)$ and, by construction, $\pi^U(\bar{w}) = \pi^R(\bar{w},\bar{x}_2)$.

By the envelope theorem, the profit functions are differentiable at w and the derivatives satisfy $x_1^U(\bar{w}) = -(\partial\pi^U/\partial w_1)(\bar{w}) = -(\partial\pi^R/\partial w_1)(\bar{w},\bar{x}_2) = x_1^R(\bar{w})$. Then, by the results of the previous paragraph, $(\partial x_1^U/\partial w_1)(\bar{w}) = -(\partial^2\pi^U/(\partial w_1)^2)(\bar{w}) \le -(\partial^2\pi^R/(\partial w_1)^2)(\bar{w},\bar{x}_2) = (\partial x_1^R/\partial w_1)(\bar{w},\bar{x}_2)$.

This is a "local" principle, because it allows comparative conclusions only for infinitesimal price changes. It cannot be directly extended to a global principle without extra assumptions. The following simple example, adapted from Milgrom and Roberts (1996), illustrates the problem.

Example. Suppose that a firm can produce one unit of output using two workers or using one worker and one unit of capital, or it can shut down and produce zero. It can also do any convex combination of these three activities. We represent the three extreme points of the firm's production possibility set by triples consisting of labor inputs, capital inputs, and output, as follows: (0, 0, 0), (−1,−1,1), and (−2,0,1). At an initial price vector of (0.7,0.8,2), the firm maximizes profits by choosing (−2,0,1), that is, it demands two units of labor and earns a profit of 0.6. If a wage increase leads to the new price vector (1.1,0.8,2), then the firm's new optimum is (−1,−1,0) that is, it demands one unit of labor and earns a profit of 0.1. If capital is fixed in the short run, however, then the firm must choose between its using two units of labor, which now earns −0.2 or shutting down and earning zero. So, the firm's short-run demand for labor is zero. The important point is that labor demand adjusts *more* when capital is held fixed, in contrast to the conclusion of the LeChatelier principle.

19.3 Positive Feedbacks

We now consider a much more general approach to the LeChatelier conclusion that is not founded in optimization theory at all, but treats the principle as a *global property* of positive feedback systems. We will show below how this theory specializes to yield a global LeChatelier principle for optimization models and how it implies Theorem 19.1.

For comparability with the preceding results, let us limit attention to a simple system of two equations, as follows:

$$x_1 = f_1(\theta, \bar{x}_2); \qquad x_2 = f_2(\theta) \tag{19.3}$$

The variables x_1, x_2, $\bar{x}_2$ and the parameter θ are all real numbers.

We need to assume that f_1 is monotonic in the parameter. Since our central example is one with an input price parameter and the corresponding input choice, let us assume that f_1 is nonincreasing in θ. Then, this system exhibits positive feedbacks if either of the following two conditions holds globally: (1) f_2 is nondecreasing and f_1 is nonincreasing in $\bar{x}_2$ or (2) f_2 is nonincreasing and f_2 is nondecreasing in $\bar{x}_2$. When (1) holds, let us say that "the choices are substitutes" and when (2) holds, that "the choices are complements."[2] This corresponds exactly to the use of these terms in the theory of the firm, subsuming the insight that the relation that two inputs are substitutes (complements) is a symmetric one.

Theorem 19.2. Suppose that (1) or (2) is satisfied (so the choices are substitutes or complements). If $\theta \geq \bar{\theta}$ then $f_1(\theta,f_2(\theta)) \leq f_1(\theta,f_2(\bar{\theta})) \leq f_1(\bar{\theta},f_2(\bar{\theta}))$ and if $\theta \leq \bar{\theta}$ then $f_1(\theta,f_2(\theta)) \geq f_1(\theta,f_2(\bar{\theta})) \geq f_1(\bar{\theta},f_2(\bar{\theta}))$.

According to the theorem, the unrestricted change is in the same direction as the restricted change and larger in magnitude, and this holds globally for any change in the parameter. The proof is quite trivial; it uses the fact that the composition of two nonincreasing functions (or of two nondecreasing functions) is nondecreasing.

To apply this theorem to the model of a firm's input choices analyzed above, fix the price w_2 of input 2 and treat the parameter as being the price of input 1: $\theta = w_1$. Let f_1 and f_2 denote the restricted and unrestricted demands for inputs 1 and 2, respectively. In symbols, this means that $x_1^R(w,\bar{x}_2) = f_1(\theta,\bar{x}_2)$ and $x_2^U(w) = f_2(\theta)$. The unrestricted choice for input 1 is the same as the restricted choice when input 2 is chosen at its unrestricted level, so $x_1^U(w) = f_1(\theta,f_2(\theta))$. With these specifications, the theorem says that, provided inputs are (globally) either substitutes or complements and the price of input 1 increases ($w_1 \geqslant \bar{w}_1$), demand will fall by more in the unrestricted case than in the restricted case: $x_1^U(w) \leq x_1^R(w,x_2^U(\bar{w})) \leq x_1^U(\bar{w})$. The inequalities are all reversed for the case of a price decrease, so in that case demand rises by more in the unrestricted case. In both cases, unrestricted responses are larger.

The counterexample presented earlier, in which the conclusion of the LeChatelier principle fails, is a case where the positive feedbacks condition does not apply globally. In that example, the two inputs (capital and labor) are sometimes complements and sometimes substitutes. When the output price is 2 and capital costs 0.8 per unit, an increase in the wage rate from 0.7 to 1.1 causes the profit-maximizing firm to substitute capital for labor, switching from the production plan $(-2,0,1)$ to the plan $(-1,-1,1)$. For that range of prices, inputs are substitutes. When the wage further increases beyond 1.3, the firm switches to the plan $(0,0,0)$, reducing its use of capital and revealing the inputs to be complements on that portion of the price domain. The pattern displayed in this example is not pathological and represents an economically significant restriction on the scope of the LeChatelier principle.

How can one check whether the complements or substitutes conditions are satisfied? Recall that a smooth function $f(x_1,x_2)$ is *supermodular* if the mixed partial derivative $\partial^2 f/\partial x_1 \partial x_2 \geq 0$ everywhere and is *submodular* if $-f$ is supermodular.

Theorem 19.3. Suppose there are *just two* choice variables. If $f(x_1,x_2)$ is supermodular and the optimal choices are unique, then the choices are complements. If $f(x_1,x_2)$ is submodular and the optimal choices are unique, then the choices are substitutes.

Theorem 19.3 also lends insight into the original Samuelson–LeChatelier principle. In a differentiable demand system, the production function f is twice differentiable. There are three cases, according to whether $(\partial^2 f/\partial x_1 \partial x_2)(\bar{x})$ is positive, negative, or zero. If the mixed partial derivative is positive, it is positive in a neighborhood of $\bar{x}$. In that case, inputs are complements in a neighborhood and, restricting attention to choices in the neighborhood, Theorem 19.2 applies. Similarly, if the mixed partial derivative is negative, then the inputs are substitutes and Theorem 19.2 applies. By continuity, the theorem also applies when the mixed partial derivative is zero although in that case, the inequality of Theorem 19.1 holds as an equality:

$$\frac{\partial x_1^U}{\partial w_1}(\overline{w}) = \frac{\partial x_1^R}{\partial w_1}(\overline{w},\bar{x}_2).$$

The positive feedbacks approach to the LeChatelier principle can be extended to a much richer array of problems. Within optimization models, one can drop the assumption that optimal choices are unique at the cost of a slightly subtler statement about how the *set* of optima changes. One can also drop the assumptions that the objective is smooth and/or that there are just two choice variables. Milgrom and Roberts (1996) develop these generalizations and others.

The LeChatelier conclusion, however, is not limited to optimization problems. One can also apply the positive feedbacks approach to study the behavior of fixed points of systems such as the following one:

$$x_1 = f_1(x_1, x_2, \theta) \tag{19.4}$$

$$x_2 = f_2(x_1, x_2, \theta) \tag{19.5}$$

Suppose that the relevant domain is some product set, say $f:[0,1]^3 \rightarrow [0,1]^2$. If f is nondecreasing in all its arguments, then, by Tarski's theorem, there exist a maximum fixed point and a minimum fixed point and those are given by $x^{\max}(\theta) = \max\{x | f(x,\theta) \geq x\}$ and $x^{\min}(\theta) = \min\{x | f(x,\theta) \leqslant x\}$, and these are nondecreasing functions of θ.[3]

Positive feedback systems like (19.4)–(19.5) arise frequently in economic analysis and game theory (see Milgrom and Roberts, 1990). To simplify our study of the LeChatelier effect in such systems, we focus on the *largest* fixed points of the system (a similar analysis applies to the *smallest* fixed points of the system). Thus, let $\bar{x}_2 = x_2^{\max}(\theta)$. Our goal is to compare changes in $x_1^{\max}(\theta)$ when the parameter changes with the corresponding changes in x_1 in the *restricted* system in which (19.5) is replaced by $x_2 = \bar{x}_2$. By the logic of the preceding paragraph, in the restricted system, the maximum fixed point for x_1 is $g_1(\theta,\bar{x}_2) \equiv \max\{x_1 | f_1(x_1,\bar{x}_2,\theta) \geq x_1\}$, which is a nondecreasing function of both arguments. Let us define $g_2(\theta) \equiv x_2^{\max}(\theta)$. By a direct application of Theorem 2 to the pair of functions (g_1, g_2) , we get the LeChatelier conclusion, as follows:

Theorem 19.4. Suppose that f_1 and f_2 are nondecreasing, $\theta \geqslant \bar{\theta}$, and $x_1^{\max}$, g_1 and g_2 are defined as above. Then, $x_1^{\max}(\theta) \geqslant g_1(\theta, g_2(\bar{\theta})) \geqslant x_1^{\max}(\bar{\theta})$

The conclusion, again, is that the change in the endogenous variable x_1 is larger when x_2 is free to change than when x_2 is restricted. The key is the positive feedback: the change in x_1 pushes x_2 up, and that in turn pushes x_1 up further.

19.4 Conclusion

In modern theory, Samuelson's LeChatelier principle has evolved into a principle for understanding multipliers. The original principle was limited to demand theory applications and reflects the symmetry of the substitution matrix, which implies that the relations of being substitutes or complements are symmetric relations. That symmetry creates a positive feedback system. For example, if capital and labor are complements, then an increase in the wage not only directly reduces the hiring of labor but also reduces the use of capital which leads to a further reduction in the hiring of labor. Alternatively, if capital and labor are substitutes, then an increase in the wage not only directly reduces the hiring of labor but also increases the use of capital which leads to a further reduction in the hiring of labor. Including capital in the model can attenuate the direct effect of the wage increase only when capital is sometimes a substitute and sometimes a complement for labor.

To a modeler who finds that the direct effect of a parameter change cannot explain an observed effect, the LeChatelier principle analysis suggests a line of further analysis. It may be that the variable in question is part of a positive feedback system. Such systems amplify the direct effect of parameter changes. This reasoning is not limited to demand systems, nor to small parameter changes, nor to models with divisible choice variables.

This knowledge is helpful not only for new applications, but also for thinking about the policy and empirical consequences ascribed, only sometimes correctly, to the original LeChatelier principle.

Notes

1. Samuelson (1947, 1949, 1960a, 1960b, 1972).
2. If f_1 is nondecreasing in θ, then the conditions change. In that case, we need that either (1) f_2 is nonincreasing and f_1 is nonincreasing in $\bar{x}_2$ ("decisions are substitutes") or (2) f_2 is nondecreasing and f_1 is nondecreasing in $\bar{x}_2$ ("decisions are complements").
3. For a more complete analysis, see Milgrom and Roberts (1994) and references therein.

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20

The Surprising Ubiquity of the Samuelson Configuration: Paul Samuelson and the Natural Sciences

James B. Cooper and Thomas Russell

Dedicated to Professor Paul A. Samuelson on the occasion of his ninetieth birthday, this chapter is inspired by the principle made clear throughout Professor Samuelson's writings, that a deep unity of mathematical method underlies the study of optimizing systems, be they drawn from the natural, life, or social sciences.

20.1 Introduction

In his 1972 Nobel Prize acceptance speech, Professor Samuelson (1972) summarized the idea that is now associated with his name: that meaningful theorems in economics are often naturally obtained by framing the issue as a problem in constrained optimization. To illustrate the power of this optimization principle, several analogies were made to the natural sciences, attention being given to a geometrical condition (Figure 20.1).

Figure 20.1 gives a criterion for when a set of equilibria are constrained optima. The axes of this diagram are labeled P_1 for the price of input 1 and V_1 for its quantity. Shown are the level curves of the quantities of input 1 demanded at various own prices by a monopolist who hires two inputs, input 1 (shown) and input 2 (not shown), in two regimes of constraint. In constraint regime 1, the quantity of input 2 is held fixed and the

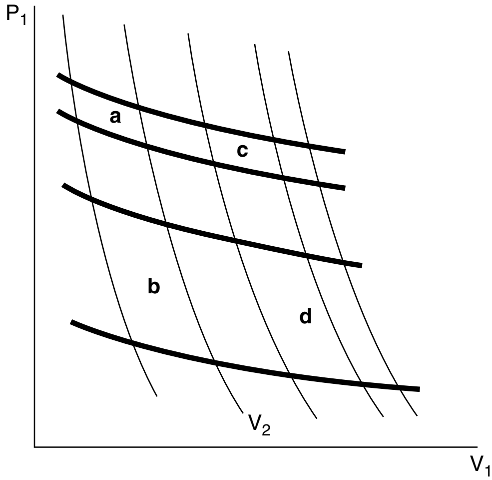

Figure 20.1 The Samuelson configuration

level curves are given by the steeper curves. In regime 2, the price of input 2 is fixed, the level curves being given by the flatter curves shown in bold.

The remarkable property of this diagram is this. If the ratio of the area of any "parallelogram" such as **a** to any area such as **b** is everywhere equal to the ratio of any other "parallelogram" such as **c** to any area such as **d**, the firm hires input 1 to maximize constrained profits. Where does such an unexpected result come from?

Samuelson recalls that in classical thermodynamics, labeling the axes *p* for pressure and *v* for volume, the same level curves represent the equilibrium positions of a heat engine also in two regimes of constraint. In regime 1, shown by the flatter curves, the engine is allowed to equalize its temperature with its surroundings, these curves being called isotherms. In regime 2 the engine is insulated, the steeper level curves being called adiabats.

He then states:

> While reading Clerk Maxwell's charming introduction to thermodynamics, I found that his explanation of the existence of the same absolute temperature scale in every body could be true only if on the p-v diagram that I earlier referred to in connection with LeChatelier's Principle, the two families of curves—steep and light or less-steep and heavy—formed parallelograms like a, b, c, d ... which everywhere have the property area a/ area b = area c/ area d. And so it is with the two different economic curves.

A casual reader of the lecture might be forgiven for thinking that were they to open Maxwell's classic work (1872), they would find there a clear discussion of the area condition as providing the foundation for classical energy minimizing thermodynamics. Such a casual reader, however, would be wrong. It may well be true that Samuelson's Figure 20.1 is lurking in Maxwell's geometry, but Maxwell's treatment is viewed by contemporary physicists as "little used" and "amazingly obscure"[1] and it seems to have had very little influence on the development of modern thermodynamics.

Given this, and recognizing, of course, the inspiration provided by Maxwell, we will now define the area ratio condition embedded in Figure 20.1 as the Samuelson (*S*) area ratio condition. When two families of curves (more precisely a pair of foliations in the plane in general position) satisfy this property, we will say they are a Samuelson (*S*) configuration. This deceptively simple geometric structure does indeed provide a foundation for constrained optimization in both economics and the natural sciences, but reasons for this are far from transparent. In this paper we repay some of the debt which maximizing economics owes to physics by considering the role of the (*S*) condition in a number of physical applications. We begin with that part of physics that is closest to economics, classical thermodynamics.

20.2 The Samuelson Area Condition and Classical Thermodynamics

The history of thermodynamics shows uncanny parallels with the history of economics.[2] In thermodynamics, for example, as much ink was spilled over the meaning of terms such as temperature and entropy as was spilled by economists over the meaning of the term utility. In fact on this issue, economics, remarkably, can claim to have reached the high ground of enlightenment before thermodynamics. In economics it was clear after the contributions of Debreu in the 1950s (Debreu, 1954) that utility, the consumer's maximand, was a numerical representation of a preference ordering.

In thermodynamics it was realized very early of course that temperature was an ordering, the classic Fahrenheit and Celsius scales being just alternative representations. But understanding that the mysterious quantity entropy was also just a representation of an ordering, though certainly glimpsed by Caratheodory (1909), does not seem to have been fully grasped till the work of Giles (1964), and continues to be rediscovered today. There is no indication that mathematical physicists had any awareness of the highly relevant work of Debreu.

Since both temperature and entropy are representations of orderings, just as with the scaling of the economist's utility, these representations will not be unique. Indeed the necessity of a canonical choice of temperature[3] independent of the particular type of thermometer used (absolute temperature versus empirical temperature) was recognized very early and was the subject of one of William Thomson's (Lord Kelvin) most famous contributions (Thomson, 1851).

But now we need to be careful. It is true that we can relabel the temperature and entropy level curves separately in any order preserving the way we please, but, taken together,[4] we must make sure that such recalibrations satisfy whatever equilibrium restrictions of a thermodynamic nature are imposed by constrained energy minimization.

What exactly are these restrictions? In modern thermodynamics it is recognized that this question can be given a very elegant geometric answer. When energy is being minimized, there is a special representation of the two orderings called T (absolute temperature) and S (absolute entropy) for which the mapping from pressure p and volume v to T and S is area preserving.[5] All of the classical laws of thermodynamics follow from this restriction.

However, this area preserving restriction applies only when the underlying orders are represented by the very special coordinates T and S. The workaday experimentalist who plots these level curves can give them perfectly legitimate labels (so-called empirical temperature t and empirical entropy s) but these would correspond to T and S only if the Gods of physics were feeling particularly generous. So what restriction would the workaday empiricist expect to find if indeed energy were being minimized?

As we have shown elsewhere, the answer to this question is precisely the (S) area ratio condition. Indeed, we have the following propositions. (Note that here, and throughout the paper, all statements of equivalence are to be interpreted as local statements. Global statements of equivalence require more delicate analysis.)

Proposition 1. The (S) area ratio condition is necessary and sufficient for the existence of a recalibration of the pair of orderings, $T \to \phi(t)$ $S \to \psi(s)$ such that the map from p, v to T, S is area preserving. We will call these recalibrations (S) recalibrations. Clearly in this case the map has Jacobian matrix with determinant equal to 1. This renumbering gives the isotherms and adiabats a canonical[6] calibration.

Proposition 2. When the temperature and entropy functions are canonically calibrated, then the classical relations known as Maxwell's equations hold, and there exists an energy function E from which these equations can be derived by constrained minimization.

These two propositions are proved in Cooper *et al.* (2001).

The (S) area condition is thus the key geometric test for the presence of an energy function. However, although geometric tests are easy to state, they may be difficult to apply. It is therefore of some interest to give the condition in analytic form.

There are several ways to do this.

1. We can think of the foliations as the level curves of two scalar functions f and g. In this case, in our earlier paper, Cooper *et al.* (2001) we obtained a lengthy nonlinear third order PDE on the functions f and g which is equivalent to the area ratio condition.
2. We can also think of the level curves as the flow lines of vector fields **v** and **w**. In this case we have shown (Cooper *et al.*, 2005) that the area ratio condition is equivalent to the existence of strictly positive scalar functions which we can assume to be of the form $e^{\phi(p,v)}$, $e^{\psi(p,v)}$, and which are such that
   (a) $\operatorname{div} e^{\phi}\mathbf{v} = \operatorname{div} e^{\psi}\mathbf{w} = 0$ and
   (b) $[e^{\phi}\mathbf{v}, e^{\psi}\mathbf{w}] = 0$, where [ ] is the Poisson bracket.

   Once again this can be shown to be equivalent to a third order nonlinear PDE in the components of the vector fields.
3. Both approaches (1) and (2) require that the level curves be calibrated in some arbitrary way. This calibration, however, is not intrinsic to the problem, and often goes beyond the data provided to the experimentalist. Experiments produce level curves with some specific shape, but they do not provide a numbering. Ideally, then, an analytical test should be in terms of the direction fields along the level curves (what economists think of as the marginal rates of transformations), not in terms of the vector fields themselves.[7] As part of a larger study of classical thermodynamics with Professor Samuelson, we have specialized 2a and 2b to direction fields.

So let $(1, a(p,v))$ and $(1, b(p,v))$ be two direction fields in the plane: that is, $a(p,v)$ and $b(p,v)$ are the slope functions of the families. We ask, when do the two families of foliations generated as solutions to the equations

$$\frac{dp}{dv} = a\,(p,v), \text{ respectively, } \frac{dp}{dv} = b\,(p,v),$$

satisfy the Samuelson area ratio condition?

The equation is[8]

$$
\begin{aligned}
&b^3 a_{vv}+a^3 b_{vv}+2(a_p-b_p)^2+b^2(2a_v^2-aa_{vv}+2a_{pv})+2a^2(b_v^2+b_{pv})-a(b_v(3a_p-4b_p)\\
&\quad +a_v b_p+a_{pp}-b_{pp})-b(a^2 b_{vv}+(-4a_v+b_v)a_p+3a_v b_p\\
&\quad +2a(2a_v b_v+a_{pv}+b_{pv})-a_{pp}+b_{pp}=0 \qquad (20.1)
\end{aligned}
$$

It is useful to contrast the treatment of thermodynamics given here with another interesting geometric approach, due to Hermann (1973) and Arnold (1990), noted in footnote 4. In that approach, energy E is added to the system as (initially) an independent variable so that their configuration space M is five-dimensional, measuring energy, pressure, volume, temperature, and entropy. Equilibrium considerations are then captured by adding to this space a contact form. The fundamental equations of state are then incorporated in this form and a thermodynamical substance is what is called a Legendre submanifold of M (see Arnold for details).

Our treatment differs from this one, first due to the fact that we do not presuppose the existence of an energy function. We work with the four quantities that one can, in principle, measure directly, pressure, volume, temperature, and entropy. Second we assume that, for the two latter variables, we can only observe their level curves, that is, the isotherms and adiabats. As mentioned above we can then define empirical temperature and entropy but these are only determined initially up to recalibrations. Thus our mathematical system consists of a four-dimensional configuration manifold (which is just ordinary four space with coordinates (p,v,t,s)). Any thermodynamical substance is then obtained by imposing two restraints on these quantities and is thus described by a two-dimensional submanifold, or more precisely, an equivalence class of such submanifolds (under the actions of recalibrations on the t and s coordinates). Our task is then to determine which (equivalence classes of) submanifolds actually correspond to thermodynamical substances, that is, have representatives that can be embedded as Legendre submanifolds of a suitable five-dimensional contact manifold. We call these Samuelson submanifolds.

We can illustrate the variety of approaches to this question noted above by considering one of the simplest examples of a nontrivial (*S*) configuration, namely the ideal gas $t = pv$, $s = pv^\gamma$. In this case we can compute the various cases explicitly.

In terms of set up 1, above, the manifold can be regarded as the graph of the above pair of functions t and s (and these, of course, satisfy the appropriate PDE).

In terms of set up 2, the curves are the flow lines of the vector fields

$$\mathbf{v} = (-p, v), \quad \mathbf{w} = (-\gamma p v^{\gamma-1}, v^{\gamma})$$

and these can be shown to satisfy the vector equations mentioned there.

In terms of set up 3, the slope functions are given by

$$a(p, v) = -v/p, \quad b(p, v) = -v/\gamma p$$

and these can be shown to satisfy Equation 20.1.

Hence we can compute the recalibrated versions which for t and s are

$$T = pv, \quad e^{(\gamma-1)}S = pv^{\gamma}$$

The recalibrated vector fields are

$$\mathbf{v} = (-p, v) \;\; \mathbf{w} = \left(\frac{-\gamma}{(\gamma-1)v}, \frac{p}{v-1}\right)$$

We can also calculate the relevant energy functions. Finally we remark that when the (S) area ratio condition is satisfied, the families of curves are given by a Lie group representation as discussed below. Lie groups are still somewhat unfamiliar to economists, though they have been used extensively by Sato (1999). Accordingly this approach will not be pursued here.

This characterization of the ideal gas clearly privileges p and v as independent variables. In general, from a purely mathematical point of view, this assignment cannot always be made. All we know is that locally the submanifold is the graph of a function which arises from one of the possible six choices of a pair of independent variables. Hence we require further PDE's to use for the various possible choices.

We call these equations the uncalibrated Maxwell relations. The choices (t, p), (t, v), (s, p) or (s, v) as independent variables give four possibilities. (For example, for the ideal gas one alternative description is $t = sv^{(-\gamma+1)}$, $p = sv^{-\gamma}$.) Then, in addition to the standard case, we have its inverse—independent variables t and s—giving a total of six possible cases.

The need to model the manifold in this way is physically important, since examples of empirical data for suitable substances (e.g. water at high pressures, the van der Waals gas) show that there is no choice of a pair of independent variable that allows a universally valid model for thermodynamical substances, a fact that has led to some polemics among those interested in the foundations of thermodynamics (see Truesdell and Bharatha, 1977).

As an example of this complication, consider the low temperature generalization of the ideal gas. In particular let isotherms be given by $p{\cdot}v = k$ (a constant) and adiabats by $S = p{\cdot}v^{\gamma} = l$ (a constant), but now, unlike the ideal gas for which γ is a constant, let γ depend on temperature t (see e.g. Feynman *et al.* (1963) 40.8 and 40.9).

In this case, of six possible choices of independent variable, only three can be computed explicitly, but it is still possible to verify that the submanifold given by $t = pv$, $s = pv^{\gamma(pv)}$ satisfies the (S) area condition. The recalibrations in this case are given by

$$T = t = \phi(pv) \quad S = \ln\,(pv^{(\gamma\,(pv))})$$

where ϕ is a primitive of the function $1/(\gamma - 1)$.

Remark: Because of the area ratio condition, complete knowledge of both families (isotherms and adiabats) overdetermines the system. It can be shown (Cooper and Russell, (2005)) that if the (S) condition is satisfied, knowledge of the first system of uncalibrated curves (say the isotherms) and just two uncalibrated level curves of the other family (say the adiabats) determines the whole system of curves and the canonical calibration. Put differently, it is possible in principle to reject the energy minimization hypothesis if one has knowledge of the shapes of the level curves of one of the families and just three uncalibrated level curves from the other family. This fact also plays an important role in the theory of equal area map projections discussed below.

20.3 The Samuelson Area Condition and Celestial Mechanics

The intimate connection between area conditions and minimizing systems is not confined to thermodynamics. It occurs in many parts of classical mechanics, most famously in the problem of characterizing planetary motion, the problem that may be said to have given birth to modern physics. In this section we show how the ability to transform an area ratio condition into an equal area condition sheds light on the classical Newtonian problem of explaining planetary motion by the presence of a central gravitational force that follows a power law.[9]

Suppose that we are given an observed orbit of planetary motion that we write in polar coordinates in the rather unusual form $rf(\theta) = 1$ (rather than the more standard $r = g(\theta)$). This simplifies the expressions, which we will obtain below. As is standard with polar coordinates we assume that the origin is the sun, r the radial distance of the orbit from the sun, and θ the angle made by the planet with respect to some given horizon. See Figure 20.2.

Now consider two families of foliations. One family consists of blowups of the given curve, that is, curves described by $rf(\theta) = k$. Note that we are not assuming that these are orbits, although, in the power law case, in fact, they are. The other family consists of rays through the origin. Again, see Figure 20.2. Trivially these two foliations satisfy the Samuelson area ratio condition. Thus we have canonical recalibrations that satisfy the equal area condition and it is easy to see that they are $s = r^2 f^2(\theta)/2$ and v where v is a primitive of $1/f^2(u)$.

If we use the new variable v as time, and write it as t, we see that we have now arranged that our initial arbitrary orbit satisfies the property that, relative to the sun, it traces out equal areas in equal times. This, of course, is Kepler's famous second law, the law from which Newton derived the principle of a central force field with the sun as the center. Changing back to Cartesian coordinates, we can see that the equations of motion of all central force field systems must have the form

$$x(t) = \frac{\cos\theta(t)}{f(\theta(t))} \quad \text{and} \quad y(t) = \frac{\sin\theta(t)}{f(\theta(t))} \text{ where}$$

$$\frac{\mathrm{d}\theta}{\mathrm{d}t} = f^2(\theta)$$

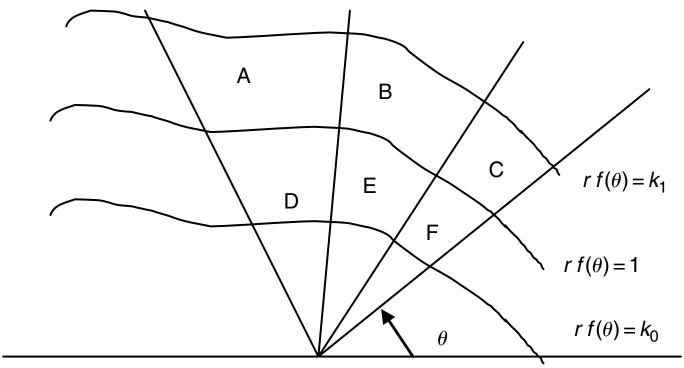

Figure 20.2 Dilations of an arbitrary orbit

Thus, via the Samuelson condition,[10] for any orbit, we have now gained a great deal of information about the underlying dynamics of the path of the planet around this orbit assuming it satisfies Kepler's second law. For example, all derivatives of x and y with respect to time can now be expressed in terms of f and its derivatives. In particular we have

(a) acceleration is given by

$$x''(t) = -\frac{\cos\theta(t) f^2(\theta)}{(f(\theta) + f''(\theta))}, \quad y''(t) = -\frac{\sin\theta(t) f^2(\theta)}{(f(\theta) + f''(\theta))},$$

(b) The curvature, κ_h, of the hodograph[11] is given by

$$\kappa_h = \frac{1}{f(\theta) + f''(\theta)}$$

(c) The curvature, κ, of the curve itself is given by

$$\kappa = f(\theta)^3 \frac{f(\theta) + f''(\theta)}{f(\theta)^2 + f'(\theta)^2}$$

From this we can deduce a wealth of results (some known, even to Newton, some new) in a very quick manner. These include a differential equation for f that characterizes whether the planet is moving under a power law, an explicit formula for the ensuing power, Hamilton's characterization of Keplerian motion by the fact that the hodograph is a circle and a refinement of the so-called duality theory (see Arnold, 1990 b) for power laws (see also Cooper, 2005).

20.4 The Maxwell/Samuelson Area Condition and Cartography[12]

Cartography, of course, is not one of the natural sciences, but it is a branch of applied mathematics which has a great deal in common with the examples discussed in this paper.[13] For practical reasons associated with land use, in many cases maps must be drawn so that they faithfully represent area, that is, regions of equal area on the map correspond to regions of equal area in the object being mapped.[14] Map projections with this property are sometimes called equivalent or authalic.

Many standard projections have the equal area property, see, for example, the list on the United States Geological Survey (no date) web page. The relevance of the (*S*) configuration to mathematical cartography can be summed up as follows. Any system of curves which satisfies the (*S*) area ratio condition leads automatically (by recalibration) to an area preserving mapping of the plane (or a part thereof). Since any equal-area projection can be obtained by composing one of the standard ones (e.g. the famous Lambert projection, see Figure 20.3) with an area preserving map of the plane, we can confine our attention to the latter.

In terms of cartography this means that any suitable family of curves can be the parallels (meridians) of an equivalent projection and that two further transversal curves (i.e. arbitrarily chosen meridians (parallels)) determinethe entire family and the calibrations (i.e. assignment of longitude andlatitude).[15]

The recipe for producing area preserving projection maps is therefore simple. Find an (*S*) transformation and recalibrate. Hence the theory of Samuelson configurations can potentially create all possible equal area projections.

Staying within the class of equivalent maps, for obvious reasons cartographers gave special attention to the class of projections with the property that the parallels and meridians are straight lines or circles. Since the parallels and meridians for the Lambert projection are just the usual Cartesian coordinate system, investigation of this restricted set of projections leads to the problem of characterizing all equal area transformations of the plane which map the parallels to the coordinate axes into circles or lines. This problem was solved by Grave[16] (1896), but his solution is somewhat analytically cumbersome, making it difficult to disentangle the underlying geometry.

What is needed is a unifying principle. This is provided by the fact that all (*S*) configurations consisting of circles and lines, and therefore all area

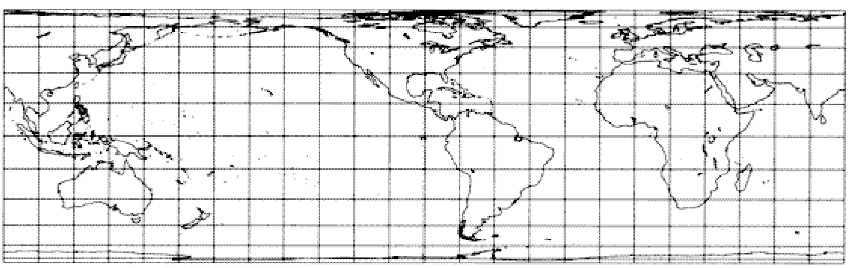

Figure 20.3 Lambert's cylindrical equal area map 1772

preserving map projections with straight lines and circles as meridians and parallels, can be constructed in the same simple way.

Take an arbitrary line or circle. Consider one parameter Lie group actions on the plane[17] whose orbits are circles and lines and which preserve circles and lines. There are three obvious candidates, the translations, the dilations, and the rotations and these generate all the Grave projections in an intuitive geometric manner. In particular it can be shown that every (*S*) circle/ line configuration can be generated by combining

(a) the orbits of the group and
(b) the images of the line or circle under the group.

Once we have the (*S*) configuration we can then recalibrate to obtain an equal area map. Specifically the combinations producing Grave's forms are

(i) translation group and line;
(ii) translation group and circle;
(iii) rotation group and line;
(iv) rotation group and circle;
(v) rotation group and line;
(vi) rotation group and circle.

We now return to the problem of describing the most general form of an equal area projection. The practical question, of course, is to choose suitable families of curves for the meridians and parallels. As we have seen we have no restrictions on the choice of parallels, and having chosen these wisely, we can still choose two meridians at will. To be an (*S*) configuration the geometric form of the other meridians is now specified and all that remains is to recalibrate.

Again from a practical point of view two choices of parallels dominate.

(a) Cylindrical projections in which the parallels are straight lines parallel to the x-axis. In the standard examples, the two extreme meridians are described by curves of the form $x = f(y)$ respectively $x = -f(y)$ where the function f is chosen on the basis of some useful map property (e.g. trueness of length along parallels). In this case the required map is $(x, y) \mapsto (X, Y)$ where $X = x/f(y)$ and Y is the primitive of $1/f$ which vanishes at the origin.
(b) The second class makes parallels concentric circles. In this case one can compute the actual formulae (or, depending on how complex the function f is, display them in a form which is suitable for numerical computations). In order to do this, we use an area preserving mapping of the plane that maps these concentric circles onto the parallels to the x axis and note that this reduces the problem to case a).

For an example of an exotic choice of configuration see Figure 20.4, the classical Stab equal area projection. The presence of the (*S*) configuration is clear to the eye.

20.5 Samuelson Area Condition, Growth and Form

As a final, albeit highly speculative example, we tentatively suggest that the (*S*) configuration can also play a role in the life sciences. In particular, in this section we consider how the (*S*) recalibration can be used to describe the evolution of biological shapes and forms. The section is motivated by observing the diagrams in D'Arcy Thompson's classic work "On Growth and Form" (1942), Chapter 17. The majority of these cases appear to arise as (*S*) transforms of the verticals and horizontals. For example, consider Figure 20.5, taken from Thompson, in which he shows how coordinate transforms, verticals into circles, horizontals into hyperbolae, render the porcupine fish as the otherwise very different looking sunfish. This is one of many examples used by Thompson to show that transformations of form between related species can often be displayed as mathematical transformations between coordinate systems adapted to the individual species. For further discussion see O'Connor and Robertson (no date).

The presence of the (*S*) configuration, of course, could be complete happenstance. D'Arcy Thompson himself, however, believed strongly that the

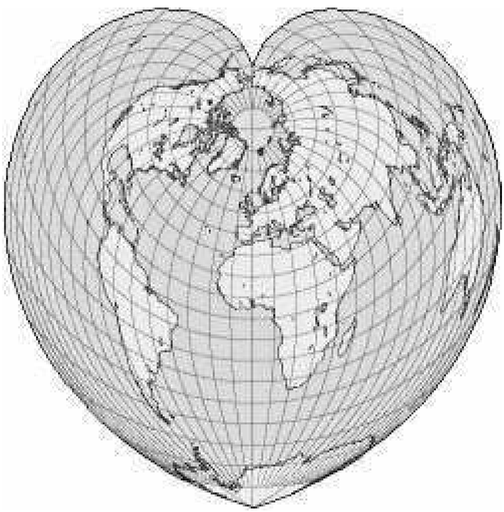

Figure 20.4 The Stab equal area projection

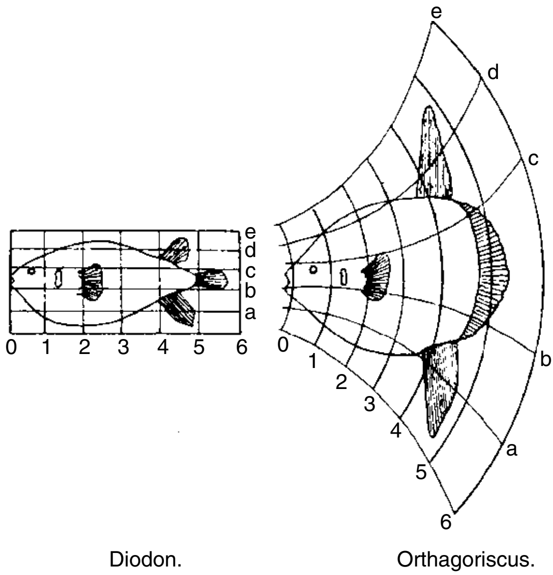

Figure 20.5 Porcupine fish (Diodon) becomes sunfish (Orthagoriscus Mola)

shape of living things was as much conditioned by mechanical forces as was the shape of planetary orbits. As he stated, "The form of any particle of matter, whether it be living or dead, and the changes in form which are apparent in its movements and in its growth, may in all cases be described as due to the action of force." And by force he meant exactly the same forces of gravity and friction introduced by Newton.

Apart from the practical implications of Thompson's transformations, their theoretical significance lies in the fact that the very possibility of constructing such mappings implies the existence of some unifying principle to the effect that the change in form produced by an evolutionary process is the result of a coordination of a relatively small number of genetic factors. Thus if one could show that the transformations are not only smooth (in the mathematical sense) but also have some common structural property, this would presumably lead to more insight into these factors. But just as it is difficult not to be struck by how many of the diagrams between pages 1054 and 1086 of "On Growth and Form" appear to arise from Samuelson configurations, one could hardly expect this to be a universal law. In a subject such as biology, which studies the effects of many causes, one should expect, and, indeed, one finds, examples where the Samuelson condition is clearly violated in subregions.

We would speculate that this occurs when some unusual evolutionary force is at work. A particularly striking example can be found[18] on pp. 1078, 1079 that illustrates the evolutionary development of the modern horse. Here it is tempting to relate the violation of the Samuelson condition which occurs in the region in front of the eye with the well-known transition of the position of this organ from the middle to the back region of the skull (presumably as an evolutionary tactic of combining all-round vision and speed as a defense against predators).

In this respect we note that one way to describe intuitively the transformations of the plane which arise from Samuelson configurations is that they are composed of an area preserving transformation followed by nonuniform deformations along the curvilinear coordinate lines corresponding to the area-preserving transformation. A glance at the diagrams shows that examples of each of these two extreme cases, that is area preserving mappings and nonuniform deformations abound. A particularly striking example of the latter can be found on page 1052, which compares the tibia of an ox, a sheep, and a giraffe. Further remarks on p. 1089 on volume preserving properties (more precisely, conservation of the areas of sections) during the transition between fish which are as distinct as haddock and plaice supports this speculative view.

What D'Arcy Thompson lacked and what geometric morphometrics (as the field is now called) in general lacks is an optimizing principle akin to those derived from Hamiltonians. As Bookstein has noted (1989), more general coordinate transformations, so-called thin plate splines, can be introduced to link two given shapes. These splines can be thought of as energy minimizing, but this is just a statistical analogy akin to the distance minimizing of least squares regressions. It remains to be seen whether or not there exists in nature a true optimizing principle which can be captured by the Samuelson grid.

20.6 Conclusion

One of the fascinating aspects of the Samuelson configuration is the variety of guises in which it appears. The basic geometry is, of course, the simple area ratio condition shown in Figure 20.1. In the applications discussed in that section, as Samuelson pointed out, the configuration is equivalent to the fact that the foliations arise from a constrained optimization problem.

In the thermodynamics case the foliations given by experiment (so-called empirical temperature and empirical entropy) are the level curves of orderings which can be represented by classes of functions in ways familiar to students of preference and utility. Whenever the Maxwell/Samuelson condition is satisfied, however, there is an essentially unique (and so canonical) choice of the calibrations of the scalar functions representing the orders, so-called absolute temperature and absolute entropy. By analogy, in the Newtonian case of planetary motion there is a canonical recalibration of the speed of the velocity vector field tangent to the orbit (absolute time).

When a Samuelson configuration is recognized it will often be useful to compute this canonical recalibration because then on we will have a canonical area preserving map of the plane (or suitable subset) and area preserving maps play a crucial role in modern physics. As we have seen, area preserving maps of the plane also play a major role in cartography where we have discussed the theory and practice of equivalent (equal area) projections. Indeed it has been our experience that (*S*) manifolds are ubiquitous and the above principle—given an (S) configuration compute the canonical recalibration—usually leads to interesting consequences.

What is the general structure underlying these configurations? As we have seen in Section 20.5, we can derive any Samuelson configuration from the usual system of parallels to the coordinate axes. These derive from a Lie group action[19] (the translations parallel to the coordinates which have this system as orbits). And so we get the most sophisticated interpretation. Samuelson configurations are representations of two-dimensional commutative Lie groups in the infinite-dimensional Lie group of area preserving diffeomorphisms of the plane. This fact seems to open the door to a more profound investigation of minimizing systems both in the natural sciences and in economics.

Notes

1. "Interestingly, Maxwell also uses the above-mentioned equality of areas to derive his relations. But he seems not to have cared for or known about Jacobians, and instead uses Euclidean geometry in the tradition of Newton to get his four identities in an amazingly obscure way." V. Ambegaokar and Mermin N.D. (2002) *American Journal of Physics*, 70(2), 2002.
2. For an interesting discussion of this issue see Candeal *et al*. (2001).
3. Alluded to by Samuelson in the Maxwell quote given earlier.
4. The need to consider temperature and entropy together once led Tisza to state that "If someone claims to understand temperature while being mystified by entropy, then his statements may be presumed to be 50 percent accurate," Tisza (1966) p. 75.
5. That dynamic flows are area preserving (or in the higher dimensional generalization, symplectic) is today the central geometric characterization of all Hamiltonian systems. When the thermodynamic model is extended to include energy E, it becomes five-dimensional, p, v, T, S, and E. The mathematically equivalent characterization for what is now an odd dimensional system is the preservation of a contact form (see Hermann, 1973, Arnold, 1990, Samuelson, 1990).

6. This recalibration is unique up to choice of origin for one of the variables and related choice of scale for both.
7. A direction field is related to a geometric object (sometimes confusingly called a line element) defined as an equivalence class of vectors $a\mathbf{v}$, $a > 0$. These objects are the natural differential tool for studying the local behavior of uncalibrated foliations and perhaps should be better known to economists.
8. Here subscripts denote partial differentiation.
9. Remarkably, after 300 years, Newton's work remains controversial and continues to inspire interesting applied mathematical questions (see V. Arnold, 1990, S. Chandrasekar, 1995, and Mittag, L. and M. J. Stephen, 1992).
10. Of course we are not claiming that this is the only way in which these classic results can be obtained. It is simply that starting with the uncalibrated curves then moving to the calibrated version, as in the case of thermodynamics, gives a very general analytical description of the system. For celestial mechanics this means that we have an analytical expression for all central force fields. This makes it easy to ask questions about those central force fields which are given by power or any other special laws.
11. The hodograph, a construction due to Hamilton, is the curve $(x'(t), y'(t))$ traced out by the velocity vector.
12. This section borrows from Cooper and Russell (mimeo).
13. The German artist Dürer, for example, maintained an interest in both form altering transformations (Section 20.4) and cartography.
14. Map projections come in many forms depending on their purpose, but most fit into one of two disjoint classes: conformal ones, which preserve angle and therefore direction (useful for navigation), and the equivalent (area preserving) ones discussed here.
15. Recall from the remark in Section 20.1 that an (*S*) configuration is determined by one family of curves and any two members of the other family.
16. The problem of the straight line and circle conformal maps (i.e. maps which preserve angles) had been solved earlier by Lagrange (1779).
17. For an accessible introduction to Lie groups in the plane, see Hydon (2000).
18. All page references here are to D'Arcy Thompson.
19. The importance of Lie groups in economics has been frequently noted by R. Sato (1999).

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21

Paul Samuelson's Mach

Rod Cross

Methodology involves words about methods. At the cognitive level these words can reflect some automatic, reflexive neural processes running parallel to the methods being pursued to address some problem. This is methodology with a small m. Or these words can reflect some controlled, serial, effortful introspection about methods, almost as though they could exist as things in themselves. This is Methodology with a big M. Then there is the affective response. The automatic, or controlled, response to liking or disliking is to embrace or avoid. There are various ways to respond, such as embracing methodology and avoiding Methodology.

In the brave new world of neuroscience it might be possible to use imaging techniques, scale electrodes and the like to track the role of the little m and the big M in experimental and control tasks performed by eminent scientists such as Paul Samuelson. In the absence of such neural evidence, and leaving deconstruction aside, the evidence from the horse's mouth is that Paul Samuelson has embraced methodology but, with the occasional lapse, has avoided Methodology. "I rather shy away from discussions of Methodology with a capital M. To paraphrase Shaw, those who can, do science, those who can't, prattle about its methodology. Of course I cannot deny that I have a methodology. It is just that there seems little appeal in making it explicit to an outsider. Or, for that matter, in spelling it out to my own consciousness" (Samuelson, 1992, p. 240). To paraphrase poet Browning, do science, shall breed the science, nor wrong the science, missing the mediate word.

The author would like to take this opportunity to thank Paul Samuelson for the assistance he provided in the author's work on hysteresis. This was a mark of the man, as well as of the scientist.

It is tempting to finish an account of Paul Samuelson's relationship with Methodology at this point. The autobiography of the British football player Len Shackleton (1955) contained a chapter titled "The Average Director's Knowledge of Football"—the page was blank. We would thus obey Wittgenstein's injunction to remain silent, wherewithon we cannot speak. Fortunately, or unfortunately as the case may be, there have been lapses. In his voyages of discovery to the lesser known realms of the economic world, this Odysseus has not always remained sufficiently bound to the mast of doing science to avoid the Siren calls of Methodology. Also, as is evident in the writings of Paul Feyerabend, saying no to Methodology can be a Methodological stance.

21.1 Big M

It is evident from Paul Samuelson's still-accumulating collected works that our nonagenarian has read widely outside the boundaries of economics. Given the breadth of the reading, it would have been surprising if Paul Samuelson had not read some writings on the philosophy of science. Amongst such writings mentioned by Samuelson (1992) are those of Ernst Mach, Willard van Orman Quine, a friend and one-time colleague when they were Junior Fellows at Harvard, Karl Popper, and Thomas Kuhn. Pride of place, in relation to Methodology, goes to Mach. "Unpopular these days are the positivists, who deem good theories to be merely economical descriptions of the complex facts that tolerably well replicate those already-observed or still-to-be-observed facts. Not for philosophical reasons but purely out of long experience in doing economics that other people will like and that I myself will like, I find myself in the minority that take the Machian view" (Samuelson, 1992, p. 242). So the big M is that to be found in Mach.

It is interesting that Mach was an eminent physicist, and a psychologist working on the physiology of the senses, as well as a philosopher. Thus he did science, as well as "prattle" about it. And the science done encompassed the nonrepeated worlds of sensations as well as the recurrent patterns of phenomena postulated in the laws of physics. Psychology was central to economic behavior in Adam Smith's *Theory of Moral Sentiments* account of the way the passions have a direct control, but one that can be overridden by individuals scrutinizing their behavior the way an impartial spectator might. In recent work on behavioral economics, and even neuroeconomics, psychology shows signs of coming in from the cold. Physics has long had a place in the economics tent, at least as a source of illustrative metaphors, at most as a constitutive form of borrowing and carrying over of properties. Read Irving Fisher's Ph.D. thesis (1891, 1925) if you want an extreme example. So an account of Methodology that covers psychology as well as physics could well be of interest to economists. As Mach put it in *The Analysis of Sensations*, "only by alternate studies in physics and in the physiology of the senses . . . have I attained to any considerable stability in my views. I make no pretensions to the title of philosopher. I only seek to adopt in physics a point of view that need not be changed the moment our glance is carried over into the domain of another science; for, ultimately, all must form one whole" (1897, 1959, p. 15).

21.2 Ontological Economy

The view attributed to Heraclitus in Plato's *Cratylus* dialogue was that "you don't put your foot into the same river more than once." Such a Heraclitean view of the world as a nonrepeating flux was the starting point for Mach. This world is made up of elemental qualities, such as sounds, colors, and pressures, that are individually unique and nonrepeatable. The mosaic of such qualities appears, then vanishes never to recur in the same form. The elemental qualities exhibit a reciprocal functional dependence on each other, in the form of actions and reactions, but the relationships are instantaneous ones such as $F[\mathrm{a}, \mathrm{b}, \mathrm{c}, \ldots] = 0$, where a, b, c . . . are the elemental qualities, pertaining to the instant between which the qualities appear and vanish.

The key to constructing some ordered account of this flux is memory. "The images of earlier times are tied to the images of present states. States in the memory field are bound to other states in the perceptual field. That which was, we see at the same time with that which is" (Mach notebook 1881, cited in Banks, 2004, p. 28). With memory it is possible to perceive some elemental qualities as recurring facets of past instances. This is a first, ontological, role for "economy" in Mach. Instead of keeping a separate record of each nonrepeating elemental quality, the memory invents element types such as a "red" color that seem to recur in the flux of "fading" "light" in a "cloudless" "sky." The memory applies such ontological economy principles to what come to be taken to be recurring complexes of the world and associations or causal connections, such as a "red sunset." In doing so the memory does injustice to the historically unique, nonrecurrent nature of the underlying elemental qualities. By using conventions to construct recurrent regularities from nonrecurrent elemental qualities, memory paves the way for science. But this still means that the only ultimate reality is one of instantaneous appearances. "If we intended to ascribe the property to nature, that under equal conditions she produces the same effects, we would not know how to find these equal conditions. Nature is but *once* there. Only our reflection produces equal cases. The dependency of certain properties on one another exists only in this. All of our toil to mirror the world were fruitless were it not possible to find something enduring in this brightly coloured flux" (Mach, 1882, 1910, cited in Banks, 2004, p. 29).

"I am primarily a theorist. But my first and last allegiance is to the facts" (Samuelson, 1992, p. 240). Samuelson goes on to explain that, as a student at Chicago, "Frank Knight and Aaron Director planted in me the false notion that somehow deduction was more important than induction" (p. 240). He "grew out of this phase fast. Once Lionel Robbins explained lucidly in the first edition of his *An Essay on the Nature and Significance of Economic Science* his claims for a Kantian a priorism in economics, his case was lost" (p. 241). But escaping from the clutches of the devil of deductive syllogism primacy did not mark a conversion to the deep blue sea of untrammelled induction.

> Wesley Mitchell's empiricisms on the business cycle do seem to me to have been overrated—not because they are empirical, but rather because his was an eclecticism that never had much luck in discovering anything very interesting. Some of the scepticisms of Knight and Jacob Viner regarding the empirical statistical studies that their colleagues Paul Douglas and Henry Schulz were attempting, I readily admit, were well taken—just as some of Keynes's corrosive criticisms of Jan Tinbergen's econometric macromodels were. But it is on *empirical* grounds that these empirical attempts have to be rejected or accepted, and not because deductive syllogisms can claim a primacy to vulgar fact grabbing (p. 241).

In Mach's account of ontological economy the dichotomy between theory and facts is blurred. Inertia was in one manifestation a law of Newton, but in another guise appears as a fact. "What facts one will allow to rank as fundamental facts, at which one rests, depends on custom and on history" (Mach, 1910, p. 56). Quine demolished the theory–fact guillotine that Mach had blurred. Samuelson is aware of Quine's contribution, in this respect. "I am aware that my old friend Willard van Orman Quine, one of this age's greatest logicians, has cast doubt that anyone can in every case distinguish between "analytic" a priorisms and the "synthetic" propositions that positivists take to be empirical facts" (1992, p. 241). Or as Quine put it, "My present suggestion is that it is nonsense, and the root of much nonsense, to speak of a linguistic component and factual component in the truth of any individual statement. Taken collectively, science has its double dependence on language and experience; but this duality is not significantly traceable into the statements of science taken one by one" (Quine, 1980, p. 42).

Among the popular criticisms of economics as a science are that the subject deals with a social world in which events are historically unique, whereas the physical sciences deal with a natural world where phenomena recur; and that in economics the facts are "soft," subject to different subjective interpretations and definitions, whereas in the natural world the facts are "hard" and objective. Mach, as far as I am aware, did not discuss political economy, but in his account of ontological economy these criticisms fail, certainly in terms of differences of substance, though maybe not in terms of degree. In Mach the natural world is also historically unique, and the fact–theory distinction is often a matter of custom, of history. Thus, at the very least, Mach's ontology does not take the economist into unfamiliar territory.

21.3 Epistemological Economy

Ask a philosopher who has not read Mach, and the answer is likely to be that Mach's distinctive contribution to the philosophy of science was to follow Bishop George Berkeley in rebutting the realist or essentialist view of science—that the concepts and relations correspond to, and have real counterparts in, nature—and proposing instead that there is only the world of appearances, with scientific concepts and equations being constructions, conventions, or instruments designed to explain the appearances. Ask an economist who has not read Mach the same question and the answer is likely to be "who?" or "wasn't he an instrumentalist who believed that realism in assumptions doesn't really matter so long as the predictions of a theory are OK?" Ask those trained in the arts of the pub quiz and the answer is likely to be "named the ratio between the velocity of a projectile and the ambient speed of sound." The philosopher's answer is incomplete—no mention of economy principles; the economist's answer is wrong—appearances are to be described, or "saved," not assumed away; only the pub quiz fan would have been right—if the quizmaster had asked about physics.

Mach's actual philosophy of science involves an extension of ontological economy to epistemology. This is stated concisely, obeying his own economy principle, in *The Science of Mechanics*: "it is the object of science to replace, or *save*, experiences, by the reproduction and anticipation of facts in thought . . . science itself . . . may be regarded as a minimal problem, consisting of the completest possible presentment of facts with the *least possible expenditure* of thought" (Mach, 1883, 1960, pp. 577, 586). So, for example, light refraction is an elemental quality that can be sensed in the nonrecurrent instances that constitute nature. Ontological economy in memory allows the variegated instances of refraction to be classified into refraction types and associations. Epistemological economy occurs at a further stage where a scientific law is a "compendious rule" for the mental reconstruction of the refraction types and associations that are now regarded as facts, as in Snel's law of refraction for example. The economizing principle involves explaining a lot from a little, so obeying the common sense of Ockham's razor dictum that "entities are not to be multiplied without need."

Good theories are thus those that provide an economical, or "minimal," description of the facts. Bad theories do not. "What was wrong with the German Historical School was not that it was historical, but rather that its sampling of the facts was incomplete and incoherent. The facts don't tell their own story. You can't enunciate all the facts. And if you could, the job of the scientists would just begin—to organize those facts into useful and meaningful gestalts, into patterns that are less multifarious than the data themselves and which provide economical *descriptions* of the data that afford tolerable accurate extrapolations and interpolations" (Samuelson, 1992, p. 243).

21.4 Some Objections

One line of criticism is that Mach has dispensed with explanation in science, replacing it with description. This problem is sometimes raised by those who favor a hypothetico-deductive model of science. This is partly a matter of semantics, as Samuelson points out: "'Understanding' of classical thermodynamics (the archetype of a successful scientific theory) I find to be the capacity to 'describe' how fluids and solids will actually behave under various specifiable conditions. When we are able to give a pleasingly satisfactory 'HOW' for the way of the world, that gives the only approach to 'WHY' that we shall ever attain" (Samuelson, 1992, p. 242). In Mach's account the theory–fact distinction is partly a matter of convention, of history, as his early resistance to the treatment of atoms as facts demonstrated.

So the notion that the explanans is *completely* independent of the explanandum does not hold in Mach.

Another objection is that Mach's position would eventually involve the abandonment of theoretical terms in science. Mach did propose a reformulation of Newtonian mechanics to eliminate the absolute concepts of motion, space, and time, but the point of the exercise was to produce a more economical description (read explanation if you like). Einstein, indeed, paid Mach a generous obituary tribute for having dispensed with the absolute motion concept. "It is not improbable that Mach would have found the theory of relativity if, at a time when his mind was still young, the problem of the constancy of the velocity of light had agitated the physicists" (Einstein, 1916, cited in Popper 1963, p. 232).

A third objection is the curious argument that surfaced in the debate surrounding what was meant in Milton Friedman's *The Methodology of Positive Economics* (1953). The claim was that instrumentalists, such as Mach, argued that theories were merely instruments for generating predictions about facts, and that whether or not the theories were unrealistic or contained unrealistic assumptions was a matter of no consequence. This is how many economists have taken Friedman's Methodology essay. Samuelson called this the F-Twist, impishly "avoiding his name because this may be, and I hope it is, a misinterpretation of his intention" (Samuelson, 1963, p. 232). The irony here is obvious. The problem is that the "unrealism in assumptions is OK" claim has nothing to do with Mach's instrumentalism, or Berkeley's for that matter. Mach was an instrumentalist in the sense that he rebutted the idea that there are real entities corresponding to the concepts and relationships involved in theories. The task of science is to provide an economical description of the facts. If a theory cannot explain the facts associated with its assumptions it is the worse for that. It is implausible to suppose that a theory could describe or explain everything, but that is another matter.

> A hypothesis's full set of predictions includes its own descriptive contents: so, literally understood, an unrealistic hypothesis entails some unrealistic predictions and is all the worse for those false predictions—albeit it is all the better for its (other) empirically correct predictions. We are left then validly with only the prosaic reminder that few theories have all their consequences exactly correct; and it can be the case that a scientific theory is deemed valuable because we have reason to give great weight to those of its predictions that happen to be true and to give little weight to those that are found to be false. In no case is unrealistic falsity a virtue; and there is danger of self-serving Humpty-Dumptyism in letting the theorist judge for himself which of his errors he is going to extenuate or ignore. (Samuelson, 1992, p. 242)

A related objection is that Mach's instrumentalism (read antirealism, not unrealism in content) is a form of conventionalism which says that theories are human constructions or conventions for explaining facts, rather than being provably true or false in themselves. This is the case, but invites the retort "so what?" If you take a hypothetico-deductive model, the $T \rightarrow I$. $O \rightarrow T$ argument is invalid, where T is theory, I is implications and O is observational evidence or facts. The error would be in "affirming the consequent," so it is not possible to infer that, because the implications of a theory are true, the theory is true. So we are left with Popper's falsificationism: $T \rightarrow I$. $\sim O \rightarrow \sim T$. But the problem is that the arrow of falsity strikes the theory as a whole, and does not isolate which one or more of the constituent hypotheses is responsible for the falsification. As Pierre Duhem, and later Quine, pointed out, which hypothesis or hypotheses are dispensed with or amended in response to a refutation is a matter of convention.

> Total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. . . . Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift, and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (Quine, 1980, pp. 42–43)

Popper's solution was to propose that some hypotheses or parts of the "background knowledge" should be regarded as not open to question, leaving target hypotheses open to falsification. But that is yet another knot on the master's whip, another form of conventionalism, as is the Lakatos typology of scientific research programmes.

21.5 Socratic Misgivings

Some issues still nag in relation to Mach's philosophy of science. One is whether this is a Methodology that is too open to be abused by scientists who want to defend their theories from empirical criticism. To one exposed to Popper and Lakatos whilst at the London School of Economics, the Machian account can appear to be lacking in critical safeguards such as that theories are bold, in the sense that they expose themselves to a wide array of falsifying instances, that they predict novel facts, or that they are revised in a nonadhoc manner, so allowing themselves to be more exposed to empirical criticism. Such safeguards can be seen as applying the Socratic ideal of questioning, of exposing arguments to a wide array of criticism, to science. Is this ideal captured in Mach? This misgiving is maybe more down to the language used by Mach, or the translation there of, than to the substance of the Methodology. The term "description" is usually used to refer to what has happened, not to what might have happened or to what will happen. The connotation is that science just describes what is already known, rather than also anticipates what is still to be known. The language is misleading. Mach does talk about the task of science being "to replace, or *save*, experiences" but also includes "the reproduction, and anticipation, of facts in thought." Similarly, Samuelson talks both of "already-observed" and "still-to-be-observed" facts. The term "economical" also raises doubts. Could it not also be taken to apply to the facts? Once it is made clear that the facts are not to be ignored, however inconvenient, and are to be anticipated, an "economical description" is difficult to distinguish from the falsificationist's excess empirical content. Popper's falsificationist precepts are indeed mentioned by Samuelson. "Long before knowing of Karl Popper's writings, I sought to be my own strictest critic. Why give that fun to the other chap?"(Samuelson, 1992, p. 242).

21.6 Simplicity

Mach's *Denkökonomie* principle suggests that it is possible, at least in certain cases, not only to discard unnecessary theoretical terms, such as absolute motion, but also to distinguish between competing theories in terms of their simplicity. An early criticism, from Herbert Buzello and Edmund Husserl (see Banks, 2004), was that Mach's principle takes the facts as given when discussing what a more economical or simpler theory is. If the theories differ in terms of the implied "still-to-be observed" facts, there is a problem.

It is surprisingly difficult to articulate in general terms what is meant by a simpler theory. A linear relationship has two parameters, a cubic three, so the linear is in one sense simpler. But the linear relationship may require a more complicated correction for error in order to fit the data. One can use an Akaike-type information criterion, such as the encompassing principle advocated in econometrics by Grayham Mizon, David Hendry, and Jean-François Richard, that weights the data likelihoods against the number of parameters. This yields an encapsulation of the Machian economy principle for given data sets. But this leaves open the question of what will happen in the not-yet-observed data sets, and hence "gruesome" paradoxes of the Goodman type.

Then there is the problem of incommensurability. The fruits of theories are multifaceted. There are implications about already-observed facts that have been found to be true or false. There are implications about still-to-be-observed facts that may be found to be true or false. Some of the facts implied are novel, some bring no news; some are bold conjectures, others innocuous; some are qualitative implications, some quantitative; and so on. Then there is silence, neither a yeah nor a nay. There has to be some weighting scheme if an economy principle is to be implemented, as in the size and power trade-off in the Neyman–Pearson account of statistical inference.

The freshwater economist might give you an economical description of what happens in the rivers and lakes. The seawater economist might tell of what happens in the oceans and estuaries. Of course a comprehensively economical description would tell you about both fresh and seawater, about the voluntarily at leisure fish in the one medium and their involuntarily unemployed counterparts in the other, about whether the media are comparable in terms of pollution, and so on. But how do you judge before the "compendious rule" is invented?

Samuelson understands that these are matters of judgment.

> Precision in deterministic facts or in their probability laws can at best be only partial and approximate. Which of the objective facts out there are worthy of study and description or explanation depends admittedly on subjective properties of scientists. Admittedly, a given field of data can be described in terms of alternative patterns of description, particularly by disputing authorities who differ in the error tolerances they display toward different aspects of the data. Admittedly, observations are not merely seen or sensed but rather often are perceived in gestalt patterns that impose themselves on the data and even distort those data. (Samuelson, 1992, p. 244)

21.7 Is Mach Done?

It is quite brave of one who has an aversion to Methodology to adhere to a particular account of scientific method, for this immediately raises questions about whether the preaching or "prattling" is practiced. This leaves Paul Samuelson open to critical commentaries on whether he has done his science the Mach way. In one sense this does not matter too much. Fish can swim without seemingly knowing too much about hydrodynamics, billiard players are not all well versed in Newtonian mechanics. Maybe, though, if the Methodology chorus reacts in a different way to what is happening on the stage, the audience will change its view.

Methodologists such as Stanley Wong (1978), E. Roy Weintraub (1991), and Philip Mirowski (1989) have assessed aspects of the way Samuelson has done his science, but not through Machian spectacles (see also Samuelson's (1998) response). Wong, for example, analyzed the development of Samuelson's revealed preference theory by way of a Popper–Lakatos rational reconstruction. The Google hit count on Mach. Samuelson will no doubt rise at an alarming rate.

Is it necessarily the case that the perception of science, and what scientists have achieved, depends on the Methodological spectacles used? One way out of this is to say no to Methodology: "science at its most advanced and general returns to the individual (scientist) a freedom he seems to lose in its more pedestrian parts" (Feyerabend, 1975, p. 285, my addition in brackets). Otherwise the reader's preferred big M will color the perception of what a great scientist such as Paul Samuelson has done. This is an aspect of Mach's world of appearances.

Samuelson acknowledges that Methodology can make some sort of difference. "When Thomas Kuhn's book, *The Structure of Scientific Revolutions*, came out in 1962, I made two lucky predictions: one, that in the physical and life sciences its thesis would have to be modified to recognize that there is a cumulative property of knowledge that makes later paradigms ultimately dominate earlier ones, however differently the struggle may transiently look; second, that Kuhn's doctrine of incommensurability of alternative paradigms would cater to a strong desire on the part of polemical social scientists who will be delighted to be able to say "That's all very well in your paradigm, but your white is black in my paradigm—and who's to say that we'uns have to agree with you'uns" (Samuelson, 1992, p. 244). Paul Samuelson is right, though, to stress the need for perspective: "Kuhn has correctly discerned the warts on the countenance of evolving science. His readers must not lose the face for the warts" (p. 244).

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Index

Goodness-of-Fit for Revealed Preference Tests

by

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Abstract. I describe a goodness-of-fit measure for revealed preference tests. This index can be used to measure the degree to which an economic agent violates the model of utility maximization. I calculate the violation indices for a 38 consumers and find that the observed choice behavior is very close to optimizing behavior.

Keywords. Demand analysis, revealed preference

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Goodness-of-Fit for Revealed Preference Tests

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Revealed preference analysis offers necessary and sufficient conditions for choice data to be consistent with the neoclassical model of utility maximization. These conditions, as usually formulated, are exact tests: either the data satisfy the relevant conditions, or they don't.

In many applications it is convenient to have tests for ''almost optimizing behavior.'' Such tests indicate the *degree* to which some data are consistent with an optimization model. In this note I describe two measures for goodness-of-fit of revealed preference conditions based on a construction of Afriat (1987) and describe how to calculate some of these measures. I apply these measures to some consumer demand data collected by Battalio (1973) and show that the subjects in this experiment exhibit behavior that appears to be ''almost optimizing.''

1. Revealed preference notation

Let (p^t, x^t) for $t = 1, \ldots, T$ be a set of prices vectors and consumption bundles. Define the *direct revealed preference* relation R^D by $x^t R^D x$ iff $p^t x^t \geq p^t x$. Let R, the revealed preference relation, be the transitive closure of R^D. The Generalized Axiom of Revealed Preference may be stated as follows:

GARP. *If* $x^t R x^s$ *then* $p^s x^s \leq p^s x^t$.

It is easy to see that if the data (p^t, x^t) were generated by maximizing a nonsatiated utility function, then the data must satisfy $GARP$. Afriat (1967) has shown that if the data satisfy $GARP$, it is possible to construct a well-behaved utility function for which the data are optimizing choices. See Afriat (1967) or Varian (1982) for a proof of these assertions.

If the data violate $GARP$ then there is some pair of observations such that $x^t R x^s$ but $p^s x^s > p^s x^t$. In this case there does not exist a nonsatiated utility function that is consistent with the observed choice behavior.

I would like to thank Eduardo Ley for programming assistance.

2. What does it mean to ‘‘almost’’ satisfy GARP?

However, it is possible that the data is very close to passing GARP; in this case we may not want to reject the maximization hypothesis. In such a circumstance it would be very useful to have an index of the degree of violation of GARP.

The problem is to give precise meaning to the idea that the observed choices are ‘‘close to passing GARP.’’ There have been several attempts to do this in the literature. The first, and in many ways the most satisfactory, was proposed by Afriat (1972). Afriat defines an number called the ‘‘critical cost efficiency index’’ which measures the degree to which a set of data fail to satisfy GARP. We will examine Afriat’s efficiency index below. The main difficulty with this index is that it is a single number for the entire data set. Therefore, it gives little information about which observations are responsible for the revealed preference violations.

Houtman & Maks (1985) suggest finding the largest subset of the data that is consistent with GARP. However, this method discards observations that violate revealed preference, even if they only violate it by a small amount.

Varian (1985) shows how to determine a ‘‘minimal perturbation’’ of the data that is consistent with GARP-like conditions. However, this method is very computation intensive and is impractical for larger data sets.

Gross (1989) suggests partitioning the data sets into a set that satisfies GARP and a set that doesn’t, as in the Houtman and Maks construction. Then he computes a violation index for each observation that doesn’t satisfy GARP using the revealed preference information in the subset of consistent observations. Although Gross’s suggestion is quite interesting, it suffers from the same defect as the Houtman-Maks procedure: observations are deemed not to satisfy revealed preference even if they only violate the conditions by a small amount.

Below I define two new measures of violation that do not have the drawbacks. The indices are analogous to residuals in regression theory in that they are a series of numbers that measure how well each observation ‘‘fits’’ the model---in this case, the model of optimizing behavior. The indices use all of the information in the data, and they are easy to compute.

3. An approximate test of revealed preference

We want to define an approximate version of $GARP$ to describe ‘‘almost optimizing behavior.’’ In order to do so, we follow Afriat (1987). Let e^t for $t = 1, \ldots, T$ be a vector of numbers with $0 \leq e^t \leq 1$. Define $R^D(e^t)$ to be $x^t R^D(e^t) x$ iff $e^t p^t x^t \geq p^s x$. If $x^t R^D(e^t) x$ we say that x^t is directly revealed preferred to x at *efficiency level* e^t. Note that $R^D(1)$ is the standard direct revealed preference relation. Afriat (1987) refers to the numbers (e^t) as the *allowable cost efficiencies.*

Let $R(e^t)$ be the transitive closure of $R^D(e^t)$, and define $GARP(e^t)$ to be

GARP($\mathbf{e^t}$). *If* $x^t R(e^t) x^s$ *then* $e^s p^s x^s \leq p^s x^t$.

If $e^t \equiv 1$ we have the standard GARP test; if $e^t \equiv 0$ we all data trivially satisfy the test. A convenient measure of how close the data are to satisfying GARP is to see how close the e^t can be to 1 and still satisfy $GARP(e)$.

Afriat (1972) proposed using a *uniform* bound. That is, find the largest number e^*, such that $x^t R(e^*) x^s$ implies $e^* p^s x^s \leq p^s x^t$.[1] This number, the *Afriat critical cost efficiency index,* measures how much we have to relax every budget constraint in order for the data to appear to be consistent with maximization. In the interest of brevity, we will call this number the *Afriat efficiency index.*

The construction of this index is illustrated in Figure 1. Here we have a violation of $GARP$: observation 1 is revealed preferred to observation 2 and vice versa. However, the magnitude of the violation is not large: a small perturbation of the budget set through observation 2 removes the violation. In the case depicted the Afriat efficiency index is C/D, a number that is close to 1.

One way to think about the Afriat index e^* is as follows. Think of a direct revealed preference comparison such as that in Figure 1, where $p^1 x^1 \geq p^1 x^2$. Since $p^1 x^2$ is much less than $p^1 x^1$, we are pretty sure that the consumer prefers x^1 to x^2. However, $p^2 x^1$ is only a little bit less than $p^2 x^2$, so we are not so sure that the consumer really prefers x^2 to x^1.

The Afriat index allows the consumer to ‘‘waste’’ a fraction $1 - e^*$ of his income at each observation. If e^* is small, the consumer is wasting very little of his income. If e^* is large, he is wasting quite a lot. In this sense e^* measures the overall ‘‘efficiency’’ of his choice behavior.

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[1] This is a slight abuse of notation. By $R(e^*)$ we means the revealed preference relation resulting from setting $e^t = e^*$ for all t.

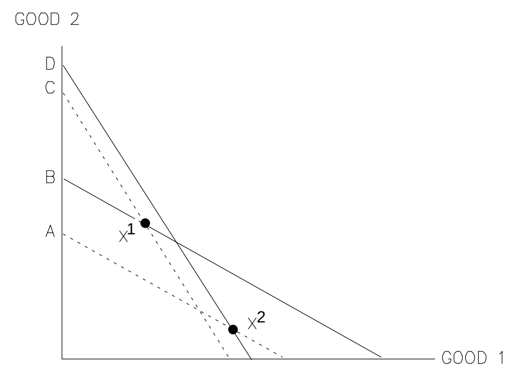

Figure 1. A small violation of revealed preference. A small perturbation of the budget set through observation 2 removes the violation.

The Afriat efficiency index is a nice summary statistic about the overall consistency of the data with optimizing behavior. However, the Afriat index does not give any information about which observations are causing the problems. It would be nice to have a somewhat more disaggregated measure that indicated the minimal amount that one needed to perturb *each* observation in order to satisfy revealed preference conditions.

4. Generalized compensation functions

We approach this problem is a somewhat indirect way. Let S be an *arbitrary* reflexive relation on a set of consumption bundles and define the *generalized compensation function:*

$$m(p, x, S) = \inf_{ySx} py.$$

This function measures the minimal expenditure at prices p to get a bundle that is at least as good as x, according to the relation S.

Let $\succeq$ be a continuous, complete, reflexive, transitive preference ordering of the usual sort. Then $m(p, x, \succeq)$ is essentially the money-metric utility function described in Samuelson (1974) and Varian (1987). Let R be a revealed preference relation that satisfies $GARP$. Then $m(p^t, x^t, R)$ is essentially the overcompensation function in Varian (1982).[2]

[2] Actually, Varian used a somewhat different definition for the overcompensation function called definition the ‘‘approximate overcompensation function,’’ but Knoblauch (1989) has shown that the ‘‘approximation’’ was exact, so that no distinction is necessary.

Now suppose that the relation R is a revealed preference relation that does *not* satisfy $GARP$. As Afriat (1987) points out, the definition of $m(p, x, R)$ still makes sense. Furthermore, since R is reflexive, $m(p, x, R) \leq px$. Suppose that we observe a consumer choosing x^t at prices p^t. If $m(p^t, x^t, R) < p^t x^t$, then the consumer cannot be minimizing his expenditure at observation t. For to say that $m(p^t, x^t, R) < p^t x^t$ is to assert that there exists some x^s such that $x^s R x^t$ and $p^t x^s < p^t x^t$. The *degree* to which the consumer fails to minimize expenditure can be measured by the *violation index*

$$i^t = \frac{m(p^t, x^t, R)}{p^t x^t}$$

If the data are perfectly consistent with $GARP$, we must have $i^t = 1$ for all $t = 1, \ldots, T$. If there is some pair of observations x^t and x^s that violate $GARP$, we will have $x^s R x^t$, but $p^t x^t > p^t x^s$. Hence $i^t < 1$, and the ratio, $i^t = p^t x^s / p^t x^t$ measures the magnitude of this violation.

How does this violation index i^t relate to the idea of ''almost'' satisfying $GARP$ that we discussed earlier? In order to answer this question, it is convenient to establish some properties of $m(p, x, S)$.

Definition. *Let R and S be two arbitrary relations. S is a subrelation of R if xSy implies xRy.*

Fact 1. *If S is a subrelation of R, then $m(p, x, S) \geq m(p, x, R)$.*

Proof. By definition of subrelation, the set $\{y : ySx\}$ is contained in $\{y : yRx\}$. Hence

$$m(p, x, S) = \min_{ySx} py \geq \min_{yRx} py = m(p, x, R).$$

The result now follows. ∎

Fact 2. *If S is a subrelation of R, then the transitive closure of S is a subrelation of the transitive closure of R.*

Proof. If $x^s S x^t$ and $x^t S x^u$ then we must have $x^s R x^t$ and $x^t R x^u$. ∎

Fact 3. *If $f^t \leq e^t$ then $R(f^t)$ is a subrelation of $R(e^t)$.*

Proof. Under the stated condition, if $f^t p^t x^t \geq p^t x$ then $e^t p^t x^t \geq p^t x^t$. Hence if $x^t R^D(f^t) x$ we must have $x^t R^D(e^t) x$. The result now follows from Fact 2. ∎

Proposition 1. *The data satisfy $GARP(i^t)$.*

Proof. Since $R(i^t)$ is a subrelation of $R(1)$, Fact 1 implies

$$i^t = \min_{x^s R(1) x^t} \frac{p^t x^s}{p^t x^t} \leq \min_{x^s R(v^s) x^t} \frac{p^t x^s}{p^t x^t}.$$

Cross multiplying, we see that $i^t p^t x^t \leq p^t x^s$ for $x^s R(i^t) x^t$, which is $GARP(i^t)$ ∎

This says that if we perturb each budget constraint by i^t this will be sufficient to eliminate all revealed preference violations at cost-efficiency level i^t. Hence i^t can serve as a measure of how much the observed choices violation the optimization model.

5. An improved violation index

It is clear from the proof of Proposition 1 that (i^t) is not, in general, not a *minimal* perturbation of the budget sets. If many observations are involved in a cycle the i^t's associated with these observations will all be less than 1. But often perturbing only one of the budget constraints in the cycle will be enough to eliminate the revealed preference violation.

In this section we refine the i^t's described in the last section so as to construct a ''minimal'' perturbation of the budget constraints that will satisfy GARP. Essentially we will start with a vector of 1's, find the revealed preference cycles, and then ''break'' each cycle using the minimum value of i^t over that cycle. We repeat this process until no cycles are left.

The values of i^t that we use will vary as we iterate through the process. Let i^t_k be the value of i^t at the k^{th} iteration. The i^t defined above is i^t_0. Our final output will be a set of numbers (v^t). Again, these will change as we iterate through the algorithm, so we let v^t_k be the value of v^t at stage k.

Given an observation t, and an arbitrary set of numbers (e^t), define $C(t, e^t)$, the *cycle* containing t by

$$C(t, e^t) = \text{all } s \text{ such that } x^s R(e^t) x^t \text{ and } p^s x^s > p^s x^t.$$

If there are no observations that are revealed preferred to x^t and cost less than x^t, then $C(t, e^t)$ is the empty set.

Algorithm for construction (v^t)

0. Initialization stage. Set $k = 0$ and $v^t_0 = 1$ for $t = 1, \ldots, T$.

1. Compute $R(v_k^t)$ and i_k^t. If $GARP(v_k^t)$ is satisfied we are done. Set $v^t = v_k^t$ and terminate.
2. Otherwise, for each observation t find the maximum nonunitary value of i_k^s over all s in the cycle containing t. Let m be the observation where the maximum occurs so that $i_k^m \geq i_k^s$ for all s in $C(t, v_k^t)$ such that $i_k^s < 1$.
3. Let $v_{k+1}^m = i_k^s$. Set $k = k + 1$ and go to step 1.

At each stage the algorithm picks out the largest value of i_k^t that is not 1. Then it uses these maximum of these values over the cycle to ‘‘break’’ each cycle. If $GARP(v_{k+1}^t)$ is not satisfied, it repeats the operations. Eventually all cycles are broken and the algorithm terminates.

Note that breaking all cycles at the first stage doesn’t necessarily eliminate all ‘‘subcycles.’’ For example, suppose that $x^1 R x^2 R x^1$ and $x^1 R x^2 R x^3 R x^1$. At the first stage of the cycle it may be that v_0^3 is the largest nonunitary value of the violation index, so that the budget constraint associated with observation 3 is the one that is perturbed. But it may well happen that this perturbation is not sufficient to remove the cycle involving observations 1 and 2, so the algorithm needs to continue to the second stage in order to break this cycle.

The algorithm to calculate (v^t) involves the same sort of calculations necessary to compute (i_0^t) so it is almost as easy to compute. If we want to use a single number to describe the ‘‘efficiency’’ of the choice data, we can use $v^* = \min_t \; v^t$. This gives us an alternative measure of efficiency to the Afriat index described earlier.

6. An example

Here we describe an example of the computation of the indices described earlier. We use the data set collected by Battalio (1973). This consists of observations of 38 long-term patients who operated in a ‘‘token economy’’ at the Central Islip State Hospital. As part of their treatment, these patients worked for tokens which they could redeem for various items such as cigarettes, candy, milk, locker rental, clothes, admission to a dance, etc. During a seven-week period, the relative of various groups of these goods were doubled or halved. Since the prices of some of the goods were halved some weeks and doubled other weeks, prices varied by a factor of four. Data were collected on how the expenditures of each individual responded to the price changes. For more details on the experimental design, see Battalio (1973).

These data have been examined using revealed preference techniques by the original experimenters and by Cox (1989). Battalio *et. al.* used only the observations on purchases of three groups of goods. Cox attempts to take into account labor supply and savings decisions of the group. Both papers find some violations of revealed preference, but the authors argue that the violations are ‘‘small.’’

Battalio (1973) indicate smallness both as a percentage of total expenditure and in terms of measurement error in tokens. For example, they say ‘‘. . . for at least 5 of these subjects, an error of 1 token would have been sufficiently large to prevent [a violation of revealed preference].’’

Cox (1989) used the Afriat efficiency index to measure the degree of violation of revealed preference. He used only 5 weeks of data and found that most of the subjects satisfied $GARP(1)$. The few that failed $GARP(1)$ had high values of the Afriat index, especially compared to his estimate of the measurement error in the data.

In this paper, I apply the notions of ‘‘smallness’’ developed above. Table 1 presents the values of i^t and v_k^t for the subjects who violated $GARP(1)$. The subscript on v_k^t indicates how many iterations it took to converge to a set of v^t’s that satisfied $GARP$. Note that 9 agents out of the 38 exhibited some violations of GARP during the 7-week experiment. The column labeled ‘‘Index’’ give the values of i^t and of v_k^t. The number v_k^t can be taken as an index of the magnitude of the perturbation on expenditure necessary to make the data consistent with $GARP(v_k^t)$. The number under the subject number in the first column is the minimum value of v_k^t over the t’s. As we have seen, this in general is a lower bound on the Afriat number; however, for these data it turned out that $e^* = v^*$ for all subjects.

Note how close to 1 these numbers are. There were 38 $\times$ 7 = 256 bundles chosen in the seven-week experiment. All but 14 of these were fully efficient. Of the inefficient choices, 8 were 97--99 percent efficient, 4 were 93--96 percent efficient, 1 was 91 percent efficient, and one was 81 percent efficient. If we choose a 95% efficiency level as our critical value---perhaps for sentimental reasons---we find that fully 251 of the 256 choices, or 98% percent of the choices were at least 95% efficient.

7. Summary

We have developed two new measures of the goodness-of-fit of revealed preference conditions. One is the violation index i^t, which measures how much each budget constraint has to be perturbed to eliminate to satisfy the revealed preference conditions. The efficiency indices v^t are a refinement of the i^t measures.

We calculated these measures for 38 subjects in a 7-week experiment in which relative prices varied by a factor of 4. We found that this choice behavior was very close to satisfying $GARP$, which is evidence in favor of the neoclassical model of consumer behavior.

Table 1. Calculation of i^t								
Subject	Index	Week 1	Week 2	Week 3	Week 4	Week 5	Week 6	Week 7
1	i^t	0.98	1.00	0.95	0.81	1.00	1.00	1.00
.98	v_2^t	0.98	1.00	1.00	0.98	1.00	1.00	1.00
8	i^t	1.00	1.00	1.00	0.97	1.00	0.70	0.73
.97	v_1^t	1.00	1.00	1.00	0.97	1.00	1.00	1.00
10	i^t	1.00	1.00	1.00	1.00	0.85	0.95	1.00
.94	v_1^t	1.00	1.00	1.00	1.00	1.00	0.94	1.00
12	i^t	0.71	1.00	1.00	0.77	1.00	1.00	0.91
.91	v_2^t	1.00	1.00	1.00	0.97	1.00	1.00	0.91
17	i^t	0.95	1.00	1.00	0.90	1.00	0.81	0.93
.93	v_3^t	0.95	1.00	1.00	0.94	1.00	1.00	0.93
24	i^t	0.95	1.00	1.00	1.00	1.00	1.00	1.00
.99	v_1^t	1.00	0.99	1.00	1.00	1.00	1.00	1.00
28	i^t	1.00	1.00	1.00	1.00	0.92	1.00	1.00
.99	v_1^t	0.99	1.00	1.00	1.00	1.00	1.00	1.00
29	i^t	0.64	0.59	0.94	1.00	0.61	1.00	1.00
.81	v_2^t	1.00	1.00	1.00	1.00	0.81	0.99	1.00
35	i^t	1.00	1.00	1.00	0.75	1.00	1.00	1.00
.99	v_1^t	1.00	1.00	1.00	1.00	1.00	0.99	1.00

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