8

Individual Decision Making

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I. Introduction

I will review r entexpcrimental studiex of individ\Jfil decision making. with their imphcations for econornJ£s in mind· Decision ma.ltlng ls increasmgly important for economics for at least two reasons.

First, in many CCOTIOmic settings individuals make decisioru. by and among them !ves: consumers save, sell their labor, buy houses and durable goods, form economic and ,;ocial relationships, and bargaim. In these cases !he institutional veils separating people from others are thin. A couple decides whether to buy a house from another couple; Mario hires a college student to work in the grocery store he owns; a daughter borrows money from her mother. (In other settings the institutional veil separating individuals doing business is truck-Monique lends. money to the shareholders of General Motors throogh the concrele veil of Citi• bank, where she has a savings account and GM has a line of credit.)

The thickness of institulional veil is important because there lli a strong intui-tion that institutional fon"eS. correct etrors people make; the *more* directly people trade with each other, that intuition implies, the more likely their errors are to persist. F.conomic analysi;; has increasingly reached into settings with thin veils re.:enlly. Judges are presumed to make law as if they had economic effkkncy in mind; there arc models in whicll people optimin: marriages, sleep, suicide, and extramarital affairs; dle household is modeled :u a unit of production: and so forth. In *these* setting systematic errors by individuals may not be corrected by instimtiooal fun:e-s. Studies of individual decision making can help predict when market prices may be wrong and allocations inefficient and suggest ways to im-prove efficiency,

Second, economic analysis has also reached into incre&ingly complicated do-mains recently. Until thirty years ago there were few formal modcl5 with any uncertainties. Weak assumptions abom agem rationality were adequate tn g,mer• ate strong market level resuils (e.g., Pareto optimality). Now many models pre• sume agents can make choices under risk and uncertainty, over time, keeping in mind subtle game-theoretic effects, As the models grow more and more compli-cated, agent.s are assumed to bave more and more rationality. Then it is more likely individual agents violate the models; srudies like the ones I describe may tell us bow and why.

A. Limited Rationality and Decision Research

For the last thirty years or so, most research on individual decision making has taken nonnative theories of judgment and choice (typically probability rules and utility theories) as null hypotheses about behavior, and tested th se hypo ses in psychology experiments. Much of this work is called "be av1o a! dc1s1n re: search" (a tenn coined by Edwards [1961a]) or, sometimes, 'cogmtJ.ve 11lus10ns. or "cognitive misperceptions." The goal is to test whether nonnativ rules are *systematically* violated and to propose alternative theories to explam any observed violations.

The most fruitful popular alternative theories spring from the idea that limits on computational ability force people to use simplified procedures or "heuristi s" that cause systematic mistakes (biases) in problem solving, judgment, and chol e. The roots of this approach are in Simon's (1955) distinction between *subsrnrive* rationality (the result of nonnative maximizing models) and *procedural* rauonality-people behave coherently by following rea sonable procedures but sometimes make suboptimal decisions as a result.

I. Why Study Errors in Decision Making?

Cataloguing systematic violations of rational models was not always the theme of the psychologists' efforts. In 1967, Peterson and Beach wrote a review of rese h on intuitive statistical judgmenl and concluded that people obeyed nonnative Jaws rather well. Psychologists began focusing on judgment ernm. in the 1970s because they thought judgment errors might reveal how people generally make judgments, just as optimal illusions tell us about perception and forgetting el!s s about memory (Kalmeman and Tversky 1982). The same scientific heunst1c is used in other fields. The Great Depression, the stock market crash of 1987, and the savings-and-loan crisis are carefully studied for clues about the general havior of economies and markets. Engineers study bridge collapses and airplane crashes to learn how to build sturdier bridges and planes.

Whether people make judgment errors frequently or not is difficult to Judge and-to most psychologists-beside the point. Psychologists study errors because if people use simplified procedures to judge and choose, those procedures may be seen most clearly through the errors they cause, For econon_usts, the frequency of errors is important because errors might affect econolillc efficiency, and methods for removing errors could be useful policy tools.

B. Two Controversies: Methods and Implications

Since many of the psychologists' studies can be seen as direct attacks on assumptions of individual rationality,[1] the studies are sometimes hotly debated. There are two kinds of debates: methodology and implications.

The conventional *methods* used in psychological studies of decision milking are often different than the conventions established by experimental economists (detailed throughout this handbook). In the psychology experiments, subjects are often not paid according to their perfonnance, or are paid small amounts; stimuli have natural labels that may induce nonmonetary utilities; subjects do nor always make repeated choices under stationary replication; treatments are sometimes created by deceiving subjects; and so forth. As a result, many economists discount evidence from the psychologists' studies. Replication of findings using the methods of experimental economics is therefore popular and tests robustness of results.

The *implication* of evidence of irrationality is another source of controversy. Despite *the* psychological evidence, economists have been cautious about reconsidering the presumption in their work that agents maximize choices based on well-informed preferences. Their caution is often defended by a tenet of "positive economics" (Friedman 1953): the market-level predictions may be approximately right even if the model of individuals from which the predictions are derived is wrong. Thus, better models of individual decision making *may not* improve market level prediction; whether they do is fundamentally an empirical question that economics experiments help answer (e.g., Plott 1986). Experiments are helpful because_ey nat_urally give simultaneous observations of individual and aggregate act!Vlty, which are the best raw material for judging whether individual errors are present and important for aggregate behavior. There are a few studies of this sort, comparing individual and aggregate behavior within an experiment. They are reviewed below, in sections II (C,D,F.2) and III (I.3, J.1).

C. A Map and Guidebook

Th chapter is _organized in two sections.judgment (II) and choice (III). The study of Judgments is almost purely psychological, *except* for a few replications and market studies by experimental economists. Studies of choice have had more interplay between axiomatic theories (mostly, though not exclusively, generated by economists) and experimental data gathered by economists and psychologists alike.

I have tried to weave the many studies by psychologists and the relatively few by experimental economists into whole cloth depicting classes of systematic mistakes and the procedures people use that seem to create the mistakes. I say the most about ongoing debates in which several studies have cumulated knowledge-aggregation of Bayesian errors in markets, utility theory, preference reversals, buying-selling price gaps. But in many places, economists have not joined the debate because they are not familiar with psychological results, do not app-eciate their impact on economics, or think the results are unlikely to replicate. I try to remedy unfamiliarity by discussing a broad array of results in minimal detail, to encourage appreciation by providing recipes for expressing psychological findings in theoretical ternls familiar to economisls, and to provoke repliration with uurnwms suggestions for further research.

The methodological range of studies summarized in this chapter is perhaps as wide as in any chapter in the handbook. I sprinkled brief digressions about meth• odology throughout the chapter, at pomls v.here they illuminate debate and where the debate provides a context Wat adds flavor to an otherwise bland di;:u1.s1on.

Oiher soun:es include Thaler \1987), who presents much of the same evidence organize.:! as a critique of economic tenets (and sec his ***Journal of Economic Perspectives*** columns, colletted in Thaler [1992}). F.dited collectiolIB of important articles in behavioral decision !heory are Katmernan, Slovic, and Tversky (1982). Ares and Hammond ([987} and BeU. Raiffa, and Tversky (1988J. There are graduate level texlbooks by Dawes (1988) and Hogarth 0987). Texts by Dazer-mall {1990) and Rum> and Schoemaker (1989) are easier. Yates (1990) ha«kr. A series of articles in ihe ***Annu11l Review of Psydwlogy*** (most r«:ently Payne, Bettman, and Johnson [1992]) provide an authoritative duonkle of pycOOlogi-cal decision research. The chapter by Abelson and Le\i {198S) is a rough etjuiva-lent of this chapter. aimed a! psychologists. New work on models of choice is reviewed by Ma,;:hina (1987), Fishburn i 19881, Weber and Camerer (1987), and Camerer and Weber \1992).

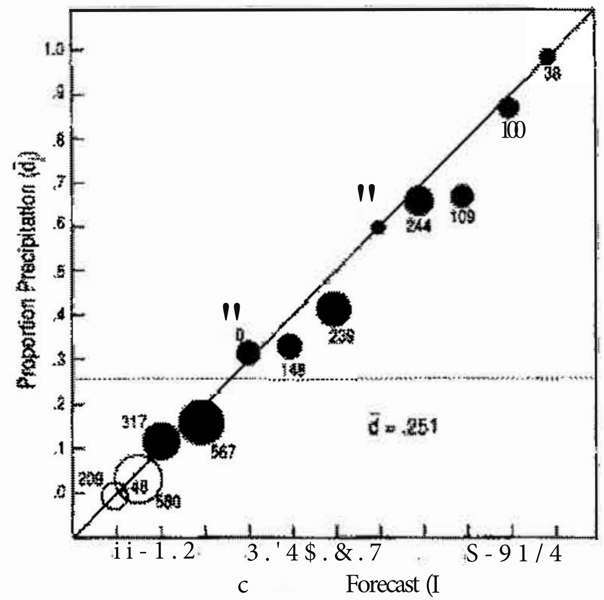
Tor psychology.economic nexus is covered by the book edited by Hogarth and Reder (1987) (reprinting a 1986 ***Journal of Busirreu*** special hMle.i, critically re-VICWed by Smith (1991). Cox and Isaac (1986) cover a small pa!ch of similar ground.

II, Judgment

A. Calibration

Good pr-0bability judgmem.s should match actual relative f$_{req}$uenclei;. The match is shown in a "calibration curve."[1] For example. in 1%5 W National Weather Servk<' began requiring in meteorologl ts to annoonce numerical judgments of the probability Ofprecipitation. Figure 8.1 shows a calibration curve for one fore-caster, using actual forecasts from several days. On !he ***Y*** axis is the relative frequency of events (proportion of days with precipitation) for each category of probability for«:ast shown oo the ***X*** a,tis. The number of events in each forecast caregory is irnlicatcd by the size of each point [and written alongside it). The forecaster shown in Figure S.l said there was a 30 percenl chance of rain nn 160 days; it actually rained slightly more than 30 percent of those days.

Accuracy of probability judgments has two distinct components, calibration and resolution (sometimes called "calibration-in-the-small" and '·caJibratioo-in-the-large"). Calibration is how weU the event forec3s:(in a particular category (all events with 3 probability) matches the actual relative frequern:y of those events (50 of 160 oci,,<1rred). In a calibration curve like die one shown in Figure lU, calibration is measured by how dose points are to the identity line (adjusting for



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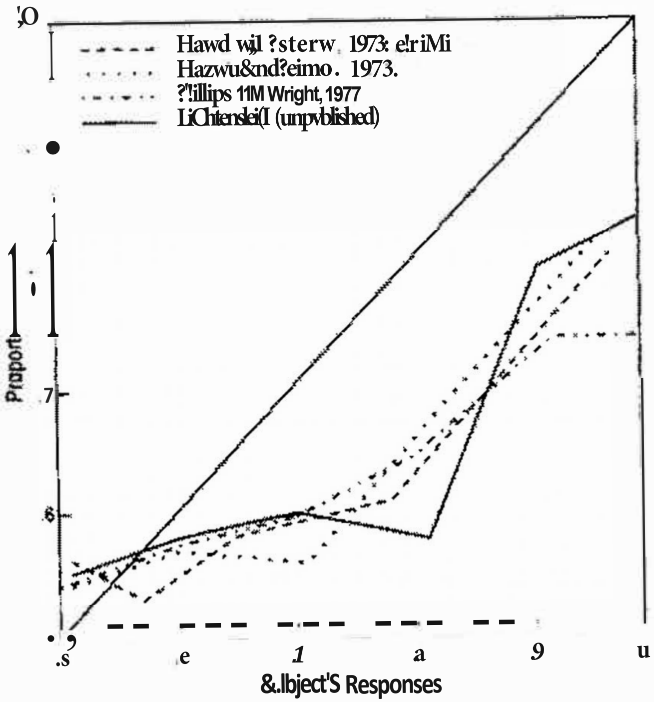
<,a,npling error). Resolution (also called "discrimin:uion") is how well probabili-ties enable one to discriminate be-tween likely and unlikely events. A high-resolu-tion forecaster will have many forecasts in the extreme categories near zero and one. 'When making predictions is difficult--in long-term economic forecasting, for example-resolution may only be achieved at W expense of calibration, by confidently makins high and low guesses !hat arc only partly righl.

The judgmeflts of the weather fureca.-;ter in Figure KI show terrific calibtation (the points are close 10 the line) and good resolution (most of the observations are between zero and .2). Calibration as good as the weather forecasters' seems to be rare (see Uchtemtein, Fischhoff, and Phillips 1982). Some empirical calibnttion curve. based on students' judgmeni'> are shown in Figure S.2, These are "hatf•• range" curves: from two possible answers to a general interest question-did potatoes come from Ireland or Peru'?-subjects pick the more likely answer and judge its probabihty (which must be at least .5).

In general subjects are overconfident. They are insufficiently regremve ill judging the likelihood of events. Events they say are certain happen only 80 percent of the time. "Full range" curves, with subjective prohabiiities from zero to om:, show overroofidenoo too. (Event$ judged to be impossible happen 20 percent nf the time,) However, subjects are often unden;onfident when ques-!Joos are- easy (i.e., when the percentage of people answering the questions cor• rectly is high).



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L Scoring Ruies

In most of the studies above, subjects were not directly rewarded according to the accuracy of their probabilities. "A proper scoring rule" is a heme that rewards 1!l'Ohabiiily judgmcms, depending on the judgment and the outcome of the event being forecaste<l, in a way that induces truthful revelation of probabilities. An example is the quadratic scoring rule: If the subject reports a probability *p*, pay her $(2p - *p^i*) if the event occurs and $n - p^i) if the event doesn·1 occur.[3] (Or defioe- ["" 1 if the event occurs and I = 0 ,f not. and pay $(1 - (p - ti).) For example, awbject who reports *p'''* JO earns cither $.19 if the event occU1li or $.99 if not; a report *p* = ,20 earns $.36 *o1* $.96. If the subject thinks the true probability is .20, then the *p* = .20 bet earns an expected value of .2 ($.36) + ,8 ($.96) = SJ;;4 and the *p* = ,10 bet eants. $.&3, so the subject should report *p* = .20

Betades being incenuve-compatible, scoring rules enable judgmems of probability to be elicited without mentioning the word "probability" or defining it.

Instead, a SUbJect expresses a probability judgment implicitly, by choosing among variom bets. (Scoring rules are sometime;; ured 10 grade s1udents for probabilist«: answers to 1nultiple-ciroice questions.)

However. soorlng rules a<Jsumc risk neul.rality; *if* a subject is r sk averse her expressed probabilities will be biased toward .5. {Allen [1987], suggem paying subjecls in lottery ticket; cf. the dillCussion of the binaty lottery procedure in chapte '.,) And the payoff fuoction has a Jlat maximum around the true subjective probability *p*, as the ex.ample above indicates, so subjects are not penalized mllCh for misreporting.

The calibration studies described in this section did *,wt* use proper sron11g roles. However, there seems w be litle difference betwen judgments motivated by scoring rules and urunol:ivated judgments (Beach and Phillips 1967; Jensen and Peterson 1973), *The* main difference is that when subjects use extreme probabilities too frequently litnd inappropriately}, scoring rules punish their mistakes verely and reduce them (Fischer 1982). When rewarded with 'improper" scoring rules that do not penalize misreports, subjects learn to misreport prnbamlitues {Nelson and Bessler 1989). For example., suppose subjects who report an event probabil:ty *P* are paid *p* if the event occurs and 1 - *p* otherwise. Then subjects quickly learn to exaggenue their beliefs. reporting: *p* = 1 if their mre belief fa abuvc 5 and reporting Ootherwise.

Scoring rules could be useful Ill a wide range *cf* e<:onomics experiments to flleaiiUte probabilistic beliefs in an 1ncentive-compatible way. For example, game theories often make sharp predictions aboul beliefs that are difficult to test indirectly (e.g., beliefs after "out-of-equilibrh1m" events that should never occw and that actually occur only rarely). Some researchers have used scoring rules to elicit beliefs of subjects in games. The only published e«momics experiment that use them, !hat I know of, is McKelvey and Page 0990f.

2. Confidence lnierval

Other st es have elicited confidence tntffVals for *quontitirs* (the length *cf* the Amazon nver, next month's spot oii price), instead *cf* probabilities for *t:wnts,* In these studies confidence intervals are typically too narrow; subjects seem to a n chor oo a point estimate, then adjust upward and downward by too little. Fifty percent intervals included the true quantity only about 30 percent of the lime; 98 percent intervals, only 60 percent of the time (Alpert and Raiffa 1982). Subjects can learn to their intervals out with intensive feedback and training, but they never get high •probability intervals quite u'ide enough.

:t.iany studies have examined the effects of expertise on overconfidence. A lot of *these* studies were motivated by the impresuve performance of weather forecasters, shown in Figure 8.1. Re&earehen then beeame curioos whether -oo'.ler experts were equally well calibrated, Profc ional ACCO!lIltants' intervals around estimated accoont balances of client firms are good <Tomassini et al 1982). Weather forecasters' intervals of high and low temperatures are precise ly the right

-width {Murphy and VIinkler 1974). But intctvals around estimat.es of physical constants, published in physics journals, are i)'S!emattcaily too narrow (Henrion and Mschhoff 1984}.'

There are mixed effects of ex.peruse in studies of even!-probabibty calibration too. Student;; and professionals for«Jsting outcomes of basltetball and baseball games are poorly calibrated (Yates 1982; Rocis .md Yates 1987;. Novices, statistical experts, and b!ackj.aci: dealers are equally well calibrated (Keien 1988) and ex.pen bridge players are better calibrated llian novices (Keren t987) at Judging lhe probability of \\<inning a hand given certain cards, Physicians are accurate in some settings and poor in others. especially diagnosing rare-diseases (Yates 1990, table 4.1; c [the discussion of base-rate faUacy in section LC.l later). Betting odds at borseracing tracks are wel.l caJibrared, with a sligh1 but persistent tendency to overestimate the chance that 1ongshots will win and underestimate the chances of favorites (Ziemba and Hausch 1986), (Curiously, the opposite betting pattern occurs at Hong Kong .racetrach; ee Busche and Hall 1988,J Forecasts by professional economists of the d1ance of economic downturn are pretty well calibrated one quarter ahead. but the calibration gets much worse as the :forecast horirou extends out to four quarters (Braun and Yaniv l992j.

There are a few cross-•cultural studies of calibration. Asians seem to have high resolution they use extreme probabilities a lot-but are very badly calibrated (Wright et al. 1978; Yates et al. 1989). Some psychologists think differences in the role of chance and bravado in Asian and Western philosophies and culture might account for the differences.

Recent studies found an important difference berween "local confidence," the appropriateness of a single confidence interval for a single quantity, and "global confidence," the fraction of several intervals that contain their true quantities. Subjects were university employees and student;; who were asked ten questions about local operation (e.g.. what is the current value of the university's land holdings?). The subjects' 90 percenl confidence intervals were too na,row (as usual) but their global confideoce was not bad: they gudSOO that about five often interv;ils conca.ined true quantities, when only three of ten actually did {Sniezek and Buckley 1991 see also May 1986). These results. suggest an important differe e between the psychological process of consuucting a judgment about asingle quantity (or event) and making a collective guess about several such judgmems. Most of us are probahly O'>"erconfident about Ille chance of publishing our next article in a leading journal or teaching a brilliant dass tomorrow, but are more level-headed about how many of our next ten articles or ten classes will be slmi• larly su..."CeSSful.

The pervasive finding that sultjects are {locally) .;.we.confident may have lm• portarn economic implications. If poopk underestimate the width of distributions of future quantities, they will underinvest in iiexihility and insurance. \\>mdh might ha\·e implications for equilibrium models of rental and owneuhip of housing, choices of mortgage terms (adjwtable vs, fixed-rate), marriage and divorce rates., managerial. investments in manufacwri$_{n\ g}$ flex.ibility, and so on, Underestimation of variation might help explain why so many :small businesses fall

of insuffidenl cash flow (stemming from overly narrow planning, perhaps; cf. Kahneman and Lovallo 1993},

Recent studies of calibration and confidence have rekindled debate along three lines. The first idea is that part of the apparent o,;.'el'Confidence coold be caused by probability judgments lhat are correct on average btu contain error (Erev, Wall sten,and Budesru 1992: Soll 1993). The second claim ls that calibration researchers may have selected sample questions nonrandomly, oversampling 'viclcy" questions in which natural cues yield the wrong answer (1mdh as the Peru-Ireland potato question), and hence producing more overconfidence than is present in natural settings (Gigerenzer, Hoffr,age, and Kleinbolting 1991; Justin. in press;). Some new studies i;arnple questions differently and reduce apparent overconfidence, but Griffin andTversky (1992)and Soll (1993) sampled randomly and still observe <'Yerconfidence.

Ttnrd, Griffin and Tversky (1992) sugge:;t a framework to organize many empirical results on oo:nfidcnce results. They point out that evidence has both strength (or extremeness) and wight. In several studies they find that judgments of confidence overemphwiize the strength of evidence (compared to a Bayesian probability benchmark) and underemphasize its weight. Their framework can exptam tbc observed difference in calibration for hard and easy questions (people underweigh the strong weight of evidence in easy questions}, conflicting results on expert calibration (experts will be highly overconfident in unpredictable environments. when they overweight weak evidence), and predicts some other phenomena.

B. Perception and Memory Biases

Machines are namral metaphors and benchmarks for human perception and thinking. The metaphor of man as an information-processor now dom.inate.s cognitive psychology (e.g.. La.chman, Lachman, and Butterfield 1979). (c has proved fruitful by suggesting coherent theory and many empirical tests. Can people nx-ord events as cnmerru. do'! Are memories stored like films in a library? Doe5 information-proccss.\ng proceed in steps like a computer program?

However, much evidence suggests that human perception deviates syMernaticalty from the camera benchmark and memory deviates from the computer benchmark, (My goal in ihis very brief section is to inform readers about some shreds of evidence, to whet their appetites, and to suggest ways the data might matter for oronomk;s.) For ex.amp.le, Bruner, Pi,strnan, and Rodrigues (1951) showed Stlbjecu; glimpses • f playi$_{n\ g}$ cards in which c;;;lors and shapes were deliberately mismrud1ed--hearts were black in.stead of the familiar re,t Subjects thought they saw the familiar cards (red heart$}. Eron of this sort are systematic, not random: people more oftefl err by mistaking unfamiliar patterns for familiar ones than vice versa. Put more formally, errors ill ab&orbing information appear to be correlated with how tmUSUal !he information is. Mispereeption of surprising events implies that a,gcms will misperceive outliers that signal regime switches or turning points in a time series. Tiwir expectations will not be rational (in the sense of efficiently using available information) because the processi of new infomiatioo depends on the stock of old information, or familiar 1mages.6

There are many biases in memory too. When guessi$_{n\ g}$ which cancer claims more Jives or which journal to submit an artide to, people ru.mple their memories. Sampling memories is a natural and reaS<mable heurintic because our memories are a sample of life, But even if our life sample is random, !he sample we retrie\-t from memory will not he raru:lom because memories are nOI equally retrievable or "available" (fvendcy arui Kahneman 1973). For example, Jhe most pleasant and t pleasant memories are more easily remembered, which creates illusory nos• Wgia {Holmes 1970). Personal and concrete cx:periences are ofren overwcighed (:Sisbett et al. 1976). For example, Kunreuther e! aL (1978} found that the purchase of earthquake insurance rose after a quake (though the probability of a subsequent 1arge quake actually falls, because sl.ress oo the fault line is :relieved). The availability of personal experiences is thought to create "$_{e\ g}$ocentnc" biases in judgmenr.s of fault {boffi pooses think they are responsible for more than half of their household chores, or argurnenl:S (RoBS and Sicoly 19821; or two sides in an experimentid dispute boll! think a judge's settlement with favor them [see Babcock et al, io press]). Memorable media reports cause biases in judgments because media coverage i,i not nmdom[7] (e.g., Greenberg et a t 1989}. For eumple, Combs and Slovic (1979) found that newspapers vastly overreport accidents compared to diseases, and pe<iple think deaths from disease and IIC(:idents are equally common. (In fact, deaths from diseaseh are *15* times more common.)

Availability can limil imagination and make theories, lists of words, or "faull trees" appear more complete than t.My really are. In a study by Fischhoff, Slovk, and Lichtenst.ein (1978), students and automei:bani:cs undtresllmated the probability of "other causes" in an incomplete fault tree listing reasons why a car would not start. Similar biases in imagining contract contingencies. might lead contracts to appear overly incomplete.

C Bayejian Updating and Representativenr.u

\Viten rhe probabilities people judge are corulition«i. as tu updating belief in X after learning M, they should follow too prescription of Bayes' rule:

$$P(XI_M) a \frac{/'\backslash MIX)P(X)}{i'\backslash M)}$$

Com$_{p\ u}$ting probabilities using Bayes' rule ls complicated. People seem to use simple heuristics iliSlead: th$_{e\ y}$ aocltor oo P(X) IUd adjusi it to reiJeci M; or they judge Pi:XIM> by how "representmve" X is of M (Tvmky and Kahneman 1982),

Representativeness will be a useful heuristic because representative values are generally more common than unrepre5elllative ones. (Eagles are less. represema.-tive of the set of bird., than robins, and less common,) But judging likcli according to representativeness neglects some features that are normatively im. portant acrordi$_{n\ g}$ to Bayes' rule---irn:luding the base rates PO[) and P{M). sam,

piing properties. and regression effects, Other features thIIt are not nonnatively important loom large in representativnness-based trunking, Representative, ness therefore creates several systematic departutell from Bayesian judgment, or biases.

L Underweigbting of Base Rates

A frunous problem used to 1Udy Bayesian judgment was introduced by Kalme man and Tversky (1972):

> A cab was involved in a hit and run accident at night. Two cab companies, the Green and the Blue, operate in die city. Yon are given the following data:
>
> (a} 85 percent of the cabs in the city are Green and 15 percent are Blue,
> (b) a witness identified the cab as Blue.
>
> The court teS!ed reliability of the witness under the same drcumstanc:e& th.at existed on the night of the accident and concluded 1hal the witness correctly identified each one of the two colors 80% of the time and failed 20% of the time. What is the probability that the cab involved in the accident was Blue rather than Green?

In the experiments. subjects are given the problem exactly as written above. often as part of a package of problems. Their probability judgments are recorded, and they are paid a small sum for participating (or course credit, in some cases).

The median and modal response is .80. It appears that subjects think the wit ness' judgment i,; representative of the actual color of dle cab, and its repreuntr tiveness leads them to confuse P(identify Blue lBlue) "",8 (from the court's l:e!lt) with the probability 1hat is a ed for, P{Blue I.identify Blue), According ro Bayes rule. the posterior probability that is asked for, P(Blue/i:dtntify BJue). shoo.ld reftect the base rate P(Blue} = .15 also; but the base rate plays no role in the logic of reprei,eritativeness. When the base mte is. included too correct pm:lerior proba bilily is .41.g

In these problems. and others like them, base rates arc usually underweighted and often entirely neglected. Studies show that when attention is drawn to base rates. by varying the base rate in several versions of the problem or presenting them in causal forms (15 percent of the cab accidents in the city involve Blue cabs). rubje<:1s take base rates into aa:ount but still underweigh them {AJzen 1977; Bar-Hillel 1980a; cf. Koehler 1989).

The cab question is typical of stimwi nsed by psychologists to study judgment Word problems describing narural events are used to escape from the limits oi earlier traditions that emphasized mote abstract stimuli, oo the sensible presumption that psychological processes people use in everyday life could be bertes understood by asking people ques1ioos drawn from everyday lift:, The nse of the word problems r;tl$eS some methodological concerns for both economists: and psychologists. For example, economists might wonder whether base -rne neglect affects asset prices in a market; some srudies: answering this question are reported ip section ll.G below. We return to the methodological concerns after describing a replication of the base rate studies.

Grether (1980) studied base rate neglect in an abstract setting wJth thre.e bingo cages, A draw from the first cage (whose contents were knownl determined whether state A or state B had occurred. The 'itate A cage had four N balls and two Gs; the B cage bad three Ns and three Gs, Subjttts observed a sample of six dra'M> from whichever cage had been cboseu (A or B} and were asJred to del.'ide which cage was more likely. FOt example, a subject might observe a sample of one N and five Gs. then choose whether ro bet that the draws came from the A or B cage, The proces was repe,rted several times, with fresh samples ea.ell time. At the end of the one trial was picked and a subjecl earned $10 if they had picked the righl cage on lhat trial.

Using logit estimation, Grether found that subjects weighted e b 'lt rates P(A) and P(B} less than the likelihoods P{samplc | A) and P(sample IBJ, as representativeness pre iicts, but they did not ignore the base rates entirely, Subjects also thought P[A isample) was especially high when the sample was four Ns and two Gs, exactly matching the contents of the A cage {and similarly for the sample matching the B cage). Previous experienc.e with a particular sample, or experience CQmbined with monetary incentives for accuracy, reduced representativeness bias slightly but did not eliminate iL Concerned that Grether's subjects were not properly motivated, Harrison (I 989b) replicated Grether's experiment with a variety of financial incentives. He found little evidence of representativeness among sub· jects with experience or financial incentive. There is no obvious way to reconcile the disagreement between his results and Grether's.

Grether (1991) extended his. earlier work in three ways. In one experiment he was able ro bound the degrees of belief in random events by having subjects clloose between a bet on the most-likely cage and a bet on a chance device (so that choices reveakd whether beliefs were higher [> .75} and lower [<,25j). In a SC(:-ond experiment he elicited probability judgments with a variant of the incentive• compatible Becker, DeGroot, and Marschak (1964) procedure (_see chapter l for a description). Choi<:es in both experimettts were affected by representativeness. Probabilities elicited with the BDM procedure in the serund experiment were <>fien far too low or too high, but oo average they were fairly close to Baye;,ian {within J)5 to .10).

In a third experiment Ille A and B cages coch had ten balls and samples of four b.uffi were drawn. Assuming four ball samples cannot be representative of ten ball cages, representativeness should not affect judgments, ln this experiment, jm:lgments were quite different man in the first two experiments-Sample information was underwcighted rather tban overn."Cighted (see the next section on conserva-- tism), Grether rooCluded; 4'his [difference] suggests that in making judgments under ty individuals use different dcci:sion rules in different decision sitw.tions." a "contingent-judgment" hypothesis espoused by many psychoki-gists, (e.g., Payne. Bettman. and Johnson 1992}.

A DigreS$i<m <m Metlwdclogy: Psychology and Ecmu,m;c.r

Gretber's experiments are designed to address many criticisms some economisfi have of methoos used by some P5:tchologis.ts.. The Bayesian judgment problem was operationalized using pbyskal devices (bingo cages) rather than a vignette like the cab problem, Subjects made choices rather than simply reporting probabiliiC11; they were paid $1Oi fooe of !heir choices, randomly selected, was OOttiCt. (In Grether [1991 l, a typicai error cost 5 to 20¢.) Subjects made repeared choices, with an opportunity to learn; in the psychology studies, su ecu often answer each question once because the purpose of the-experiment is to study initial inw-itioos., .not leanung. The existence of SOmE e m m wa reasonabiy robust to all these changes in conditions in Gremer's data, but not in Harrison' Incentives airo reduced the number of incoherent and outlying responses (Grether 1981; cf. Smith and Walker 1993).

The differenct between p$yclJ.ofogical and economic e riments should not be ovemated. In lhe I960s, long bef Grether's wort. psychologbts and others used random devices to siudy judgment (Edwards 1968) and used the BDM -procedure to study valuations (Lichtensrein and Slov:ic 1971). Even recently, there is substantial overlap across disciplines in methods., and substantial varia1km within disciplines. However, the typical differences in methods .art worth analyzing because they usually follow from different background presumptions about humart nature and different target domains investigators hope to generalize to. It is pre, sumptuous to argue that either general method is superior.

For example, many psychologists are curious whether people can recognize and apply statistical rules to everyday situations, like the cab problem in which s1atistical structure is not transparent. They often use vignettes or problems drawn from natural setting (rather than problems based exclusively on random devices) because (1) they want lo learn how people reason about natural events and (2) they think people may reason differently about events and about random devices. Given these interests and presumptions, word problems are well suited to doing their research and bingo cages are not, Economists are interested in differ+ cot questions {nOt how people reason but whether people violace Bayes's rule) and are also more inclined to presume that. reasoning about bingo cages and taxi-cabs il, -.Jmilar. For these purposes, cages and dice are better because they lay bare the statistical structure (making detection of a Bayesian error clear) and are pre .wmed to he good sultihtutes for word problems.

Another area of typkal difference is financial motivation of subjects. J>s.yclwio-gists do not alway-" motivate subjec s financially !bough many have and a few are adamant aboot doing so—be<:anse incentives usually complicate instructions and psycllok,gists presume suhjects are cooperative and intrinsically motivated to perfonn well (Nawrnl stimuli are also thought to ke:eps.ubjects mentally involved and raise their instrinsic motivation, which substitutes for financial motivation.)

Repetition is another area of typical difference, The psychologists' ta&ks are often not repeated, with stationary replication, because psychologists are often most curious about initial heh;ivior in a complicated environment. In addition, many psychologists think stationary replication overstates the frequency, speed, and clarity of feedback the world actually provides. Economists tend to think oppositely: they are mostly curious about equilibrium behavior-the last period, not the first-and they think extensive laboratory feedback is the best time-compressed imitation of the strong learning forces present in natural settings.

To reiterate, there is substantial overlap in the way psychologists and economists do experiments. When their methods do differ, very roughly speaking, psychologists use natural stimuli, do not pay subjects, and do not repeat tasks. Economists pay subjects, prefer blandly labeled random devices as stimuli, and insist on repeating tasks. My view is that these different methods are preferred by different investigators because they effectively produce answers to different questions. Broad-minded students of individual decision making should have a healthy tolerance for variety in methods. (And variation in methods is essential to gathering data, to determine whether different methods do affect behavior substantially.)

It is worth noting that judgment errors, like those revealed in the cab problem, have been a lively topic of research within psychology too (e.g., Cohen 1981). Many of the arguments made in that literature are like those economists have made about methods or interpretations of results. For example, Gigerenzer, Hell, and Blank (1988) used physical devices to operationalize base rates and found some reduction in base rate neglect (though Grether, and others mentioned later, also found substantial base rate neglect using physical devices).

A more interesting argument is that some apparent biases might occur because the specific words used, or linguistic convention subjects assume the experimenter is following, convey more information than the experimenter intends.[10] An example is the famous "Linda problem" (Tversky and Kahneman 1983). Subjects are told the following:

> Linda is 31 years old, single, outspoken, and very bright. She majored in philosophy. As a student, she was deeply concerned with issues of discrimination and social justice, and also participated in anti-nuclear demonstrations.
>
> Then they are asked to rank several statements about Linda by their probability:
>
> Linda is a teacher in elementary school.
> Linda works in a bookstore and takes Yoga classes.
> Linda is active in the feminist movement (F).
> Linda is a psychiatric social worker.
> Linda is a member of the League of Women Voters.
> Linda is a bank teller (T).
> Linda is an insurance salesperson.
> Linda is a bank teller and is active in the feminist movement (F&T).

Any ranking of probability should satisfy the conjunction law: Linda is less likely to be a feminist bank teller (marked F&T), than to be a bank teller (T) or a feminist (F), since the event F&T is a conjunction of the events F and T. In fact,

about 90 percent of subjects exhibit a conjunction fallacy, ranking the event F&T as more likely than one (or both) of the events F and T, usually T. (In a sample of well-trained Stanford decision sciences doctoral students, 85 percent made the same mistake.) The standard psychological explanation is that the description of Linda is more *representative* of a feminist bank teller than of a bank teller; subjects mistakenly think it is therefore more *likely* that Linda is a feminist bank teller.

The potential linguistic problem is this: in the presence of the statement "Linda is a feminist bank teller," subjects might think that the statement "Linda is a bank teller" tacitly excludes feminists; they might think it actually means "Linda is a bank teller (and is not a feminist)." If subjects interpret the wording this way, none of the statements are conjunctions of others and no probability rankings are wrong.

The linguistic interpretation can be tested in several ways. For example, use a between-subjects design in which some subjects rate the T statement without seeing the F&T statement (and vice versa); or replace "Linda is a bank teller" with the clearly comprehensive "Linda is a bank teller, who may or may not be a feminist," or with the more specific "Linda is a bank teller (and is not a feminist)" and see whether conjunction errors persist

In fact, the purely linguistic interpretation appears to be wrong. Tversky and Kahneman (1983) tried both the between-subjects and the clearly-comprehensive variations and still found persistent conjunction fallacies. Others manipulated subtle details of wording and found no substantial changes in some conjunction problems (Morier and Borgida 1984) and some error reduction in others (Krosnick, Fan, and Lehman 1990).

2. Underweighting of Likelihood Information (Conservatism)

A second bias is underweighting of likelihood information, or "conservatism." Conservatism has been observed in Bayesian updating tasks like the one Grether studied. Consider two bingo cages, A and B. Bingo cage A contains seven red and three blue balls; B contains three red and seven blues. Suppose each cage is equally likely. Suppose a sample of eight reds and four blues is drawn (with replacement, of course), which clearly favors the A cage. What is P(A | 8 red, 4 blue)? The typical response is between .7 and .8 but the Bayesian posterior is actually .97. Subjects are far too conservative in drawing conclusions from samples like these. One estimate derived from experimental data suggests that it takes two to five observations to produce a perceived diagnostic impact equal to the Bayesian impact of one observation (Edwards 1968).

McKelvey and Page (1990) ran a study in which subjects observed different parts of a full sample, then reported probability estimates to each other. After hearing the estimates of others, people reported new estimates (taking into account the estimates of others), and so on for several rounds. (This iterative process resembles the aggregation of information through polls and other processes; see McKelvey and Ordeshook [1985]). They observed some conservatism in updating of probabilities. Eger and Dickhaut (1962) found some conservatism when accounting students simply reported probabilities, but the effects were substantially reduced when stimuli were described an accounting context. The conservatism also disappeared when subjects revealed probabilities by betting against an experimenterin a way that pena.lium Bayesian errors. Sy contrast, Sanders (i96&) found oonservatism u:lllJl! a proper &eoring rule with no financial incentives.

At fim glance, tire ooniUct between evidence of base rate neglect (reviewed in the last section} and 1--m1servatism s to indicate that people use Bayes·s rule oo average, but sometimes they weigh base rate,; too little and sometime.s too much. This is a weak justification for adhering to Bayes·s rule .as a descriptive principle in all circumstances, if one can predict the situations in which the two erron; 1,ccur. {By analogy, lIltgbt jacket is the wrong thing to wear on a trip with stops in Alaska and Taipei, even if it ts appropriate for the average temperature of the two places). \\'hethererron; are prcdktable across situations is then the-crucial empirical que tion.

There seem to be veral reasons why base raks are underweighted in some settings (the taxicab problem) but sample information is underweighted in othets {conservatism experiments), Base rates are incorporated when they are salient or interpreted causally, a$ they are likely to be in the conservatism experiments (partly be.:ause judgments are repeated). Al o, sample infonnation may be underweighted in the conservatism tasks because it is not highly representative (as witness accuracy is in the taxicab problem): for instance, no sample of draws exactly matches lhe contents of the bingo cages. Furthennore, Griffin and Tversky (1992) argue that conservatism and base rate neglect are opposite sides of the ame coin. They argue that both phenolll(',m1 resl,lil from people overweighting the strength of evidence and underemphasinng its weight Conservati m is a kind of undcrconfidenee that results when people unde:remphasire the large size (or weight) of a tample nf weak evidence. Base rate neglect occurs becau,<se people overemphasize strong evidence.

3. The Law of Small Numbers and 1isperceptions of Rando=

In the logic of the representativenef;S. there is m, pl for sampk size: a small sample can represent the population or procesl. that generates it as well as a large sample cun (for example, a »ample of two coin fii ps. one head and one tail, represents the Bemoulh trial very uicely). The he.lief that all samples will closely resemble the process.es or populations !hat generated them is an intuitive extension of the law of large numb«:$. to small samples. facetiously called "the law of small numbern·' ('fvenky and Kahneman 1971). The lawl)f small numbers predicts that agentS wiU gather too little data and wiU OYergeneralize from small samples to distributions. In economic appiicatlmu,. they will search too little (see the evidern:e in secnon III.K) and foam ti,Q quickly, oompared to utodefa of optimal sampling and inference.

'The law of small numbers alro causes biases tike the '·gambler's fallacy": when people arc asked lo generate or identify random seq11ences their sequence:; often

haw negative autocorrelation (Wagenaar 1972), because the prorotypicaJ repre-Jcntative random series repeatedly seJf-cormcts to keep the sample proportion close to the poputruion proportion (see Bar-Hillel and Wagenaar [in press} for a recent review!. For example. lottery betting on a given number actually drops off shatply••--OY nearly half-in the several days after that number wins (Ootfelter and Cook 1993}.

Mathematlcally sophisticat:ed subjects are better at generating truly random 1nmtbers, but so are cluidren who have not yet learned the law of small numbers (Ross and Uwy 1958; Chapanls 1.953). People can also be taught to choose randomly after several hour,; of training with excellent feedback (e.g.. several measures of the randomness in the previous block of rnspon-.es, N\':uringeT [19-86]. and see Edwards (1961bji. Tbese training data suggest that experienced ageo in some settings might be able to learn to choose randomly. Whethcr they do in o!her &ettmgs, under natural conditions, is an empirical question.

Truly random sequences will snow no negative autocorrelation. Obsencn. who expect negative 1u.11ocorre\ation in a tandom series will be swprised by the number of kmg fUllS !hey see and will come to believe the series is positively autocorrelated. This misconception appears to be the origin of the unshakable belief among basketball fans and players that outcomes of shots are positively autocorrelated•···•players have "hot hands"-even though both field data and experiments show hits and misses are remarkably close to independent (Gilovich, Vallone, and Tversk:y 1985). Cameret· (1989b) found that mis.taken belief in winning streaks-tearnwwide hot ha reated errors in betting odds on professional basketball games of about one point (The- error is small because professional teams score roughly 100 points a game.) And Brown and Sauer (1993) question whether streak beliefs. are mistakes-, but their tests are inconclusive. Regardless, betting markets are active and patterns like the pe«:elved hot hand are very easy ro ob• serve and profit from; mch markets might be the worst place to find bias. The modest one point error suggests that larger effects might exist in markets that are lcs welt policed.[11]

Mispereeptions of random sequences are important in game theory because mixed strategy play in repeated: games as.sumes s.Wjects can generate independent random draws (or appeal to independent privately--Observed hunches that others do nm observe). O'Neill I1987) reported tha! average play corresponded to mixed strategy proportions in a zero-sum game with a unique mixed strategy equilibrium, Skeptical of the slreng!h of O'Neill's conclusions. Brown and Rosernhal {l990J reanalyzed h.ls data, Their reanalysis and subsequent work by odlers are a good ca,e study iJlu.strating how careful critique Qf an imaginative experiment can lead to further designs and a cmnulation of knowledge.. Brffi\n and Rosenthal first pointed om that some rest statistics O'Neill used, hke the percentage of tunes the row piayer won, had little statistical power to distinguish equililrrium mixed s1r.negy play from vanou,; diSl'qUilibrium altema:tl\-'eS Oike random choices with equal probabilities fnr all strategies), Then tlrey showed that despite the: hick of power to detect deviations: from mixed strategy predk'tions. about d third nf tbe players did deviate significantly, in different directions.

Furthermore, choices were not lndepcndcut across plays; choices often depended on one's own previous plays a.rufon the opponent's previous play {and sometimes on the interaction).

The work on randomization is now cxtcnsiw and suggeSats an area of genuine eollab<iration and cross-fertilization between economiwi and p:,.ychologists. 1 b e conclusions of O'Kcill, and Brown and Rosenthal, have been largely replieated by Rapoport and Boebel {1992) and Mookherjce and Sopher (1994). In these games., many players seemed to believe in the law of small numbe: after they won by making a particular choice. they were less likely to try rile same ch-Oicc. Whether players can detect the predictable aonranJornncs-;; that other players ex· hibit is an interesting open question,

Rapoport: and Budescu (1992) compared the randomness of sequences subjeets generated in two conditions: (I) a game condition in which they played a two strategy game wi!h a unique mixed strategy equilibrium fwith each strategy equally likely) 150 times and were paid for each choice according m the out- comes; and {2) a choice condition in which the subjects were simply told to choose randomly ISO times (and were not paid for their choices). Formally the two tasks are the same, But in the game condition, subjects created seqnences that were more random than Ulose they produced by random choices: for example, subjects reversed thtir previous choice 59,1 percent of the time in the choice condition (exhibtt.ing "negative recency"), but they only reversed 53.4 percent of the time in the game condition. (A true randomizer would reverse 50 percent of the time.) One explanation for the difference is that subjects played the game more seriously because money was at suike. Rapoport and Budescu suggest a second, psychological explanation: remembering previous choices is essential for choosing nonr.mdmuly: perhaps playing the more complex game inhibited mem• ory of previous choices, making it more difficult not to randomiw.

Mookherjee and Sopher (1994) compared behavior in two conditions, in which subjects learned their own payoffs but the choices and payoffs of others were either known or unknown. In many "routine-learning" models, knowing the choki:'.& or payoffs of -Others is inessential because players arc assumed to simply choose strategies that yielded high payoffs in the past. These models predict that behavior in the known and unknown conditions should he the same. Behavior was not the same: convergence toward equilibrium mixtures was more rapid when the odterplayers' choices and payoffs were known, suggesting a sophistica, lion the routin leaming models do not capture.

MnJwdologitol Digrnsion: Trillning

Several psychotO.gists have tried to train subjects to avoid judgment errors {"de- biasmg"}. Fischhoff 0982) reports some soccessful training exercises, mostly using large amounts of well-structured feedback. For example, overconfidence and hindsight bias !_discussed in section IT.F below) can be reduced by having peopie generate reasons why their predictions or recollections might be "Wrong. (Groups and organi?.ations might debias individual judgment if several people generate such rea:;ons, questioning each ochen' judgment. But the opposite could occur too-group:. could inflame bias----lf groups geuerate lillpporting arguments or if overconfidence is taken as a signal of knowledge.}

Extens-ive studies suggcs1 reasons to be pessimistic about how well traillng transfers across. time or task,,, When subjects adapt to a setting and optlmize in it, it is often the case that they have not learned a general rule they can recognize co apply to a structurally identical tall that ha;, different surface features. For exam- ple. Kagel and Le,.in's {1986) subject;. learned to avoid the winner's curse in three bidder markets. bur overbid when three bid.kn; were udded; compare with chapter 7.

Nisbet! et al. (1987) and Larrick, Morgan, and :.lisbett (1990} tralned &Ubjects co u ;,imple statistical rules and ignore sunk co:-ts.. For example, 45 per.:ent of their trained subjec gave a correct response on a sunk cost problem, compared to 29 percent of untrained subjects. A month later, the trained subj.x:t» reported Uley had boughc and *!WI* used 1.14 objects m actiYitie-&•••e.g., they returned a rented videotape without watching it---compared to 0.84 by untrained subjects. These are modest victories for training, but the breadth of the rules people have learned to use is subject to debate.

4. Market Level Tests of Representativeness

A central question for economics is whether individual judgment errors aggregate 10 create errors in market prices, aUocatio1ls, and effidendes {and wh<:ther ern;i aggregate in groups, 11 firms, and societies). 1be aggregation question has been addressed theoretically by Haltiwanger and Waldman (1985), Russell and Thaler (1985), Akerlof and Yellen (1985), and many others. Wb.etber individual errors affect market behavior depends on the answers to many deep questions (see Camerer 1992b); do rational agents have more impact than irrational agents? (Do the more rational agents know who they are? Can they get more capital? Is it always optimal to behave rationaJ\y when others are not?) Do irrationaJ agents learn from others? Can they buy arlvke? Cat\ they go bankrupt? (Will 1hey he replaced by other irrational agents if they do?}.

The answers to these questions will undoubtedly vary across markets (cf. Z.eck- hauser 1986, table I). They are fundamentally empirical questii,n. To answer them, some evidence ofjudgment error in market behavior has been gathered (see Thaler 1992). But the evidence is inevila.bly controversial because it is easy to construct rationalizations Df apparent market anomalies based on risk aversion, transaction costs, unobserved variables, or-the current fashion-information asymmetries, To test whether indivhlual errors affect markets, it is therefore help,, ful lo conduct market experiments in whkh .;:ompeting explanations rnn be ruled OUL The next section describes some studies of whe;!her errors ill Ba)'t\siau Judg" ment, caused hy representativeness, affect prices and allocations in markets.

Duh and Sunder (1986) pohlished the first market study. They tested whether underwcighting of base rates lsee section ILD.l above) affected prices in a market for ()Mwperlod assets. Their design and those use.I by rue (Crunerer 1987,1990) were both inspired by Grether f1980t 1 will describe my own de;ign in rome detail.

Asset values depended on wllich of two state£, X and Y, OC<urred, States were physically represented by bingo draws. If X had-OL-turrod (f;,,,,6) three balls were drnwn with replaceu:tent from an X bingo cage. hidden in a box., containing one red ball and two bl.act balls:, If Y occurred the cage had two reds and one black.

SubJects were given rwo shares of an asset and loaned experimental currency (fmncst each period. SuhJeC!S ea.med a state-dependent dividend for each share they held at the end of the period. (There were lwo dividend schedules, crealing type I and type II trooers.} The value o{ the assets to subjects therefore depended on their suh1¢ctive prot,abilities of X and Y, which depem.led on the sample of balls drawn from the X or Y bingo cage. A sample of one red rand two biatls) indicates the state is likely to be X. A Baye.Wltl subjru:t would calculate

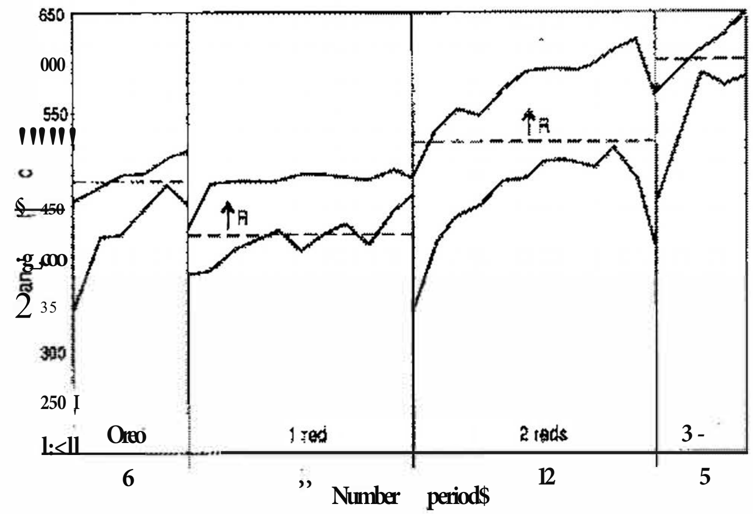
$$P(X \mid 1 \text{ red}) = \frac{P(1 \text{ red} \mid X)P(X)}{P(1 \text{ red} \mid X)P(X)+P(1 \text{ red} \mid Y)P(Y)} = .75$$

The judgment literature suggests several alternative hypothe5es about how subjects es-rim.ate P(X I 1 red). One could interpret represemativeness. to imply PfX 11 red}"''" 1 (Duh and Sunder's J\.'BRI model), An interpretation more fmthful to representativt'ness is P(X | I red} = P(Y, 2 red), which followt if the base rates P(X) and P(Y) are ignored. Both of those theories are rejeered hy the data. An hypothesis that fits- better is "exact repre1lentativeness," in wfilch Pi.X: 1 red) > .75 because a sample of one red and two blacks exactly matches the contents of the X bingo cage. Given any such assumption about PfX | sample), predictions about prices and allocations can be derived by astuming risk neutrality and competitive equilibrium.

&ch experuncnt had thirty to forty trading periods with lltationary replication. using different samples each period. Since the equilibrium price varied among four possible prices from period to period {one price for each different sample), prices were volatile and convergence was slow. Carner-er (1990) reports the time wries of mean prices in all sessions. Camerer t1987) condenses rhe daUi in a compact way, shown in Figure 8.3, giving a time series of -.:onfidence intervals around the mean price across several sessions with the Sanle paramewn., n Bayesian predictions for each sampie are shown by horizontal lines.

Figure 8.3 shows that menn prices vary across sessions (the confidence intervals are wide), Across periods with !he same san1plt. prices converge roughly to the Bayesian predictions, An "R" denows the dite(:tion of price deviations ex• pected by {exact) representativeness. Prices in zero-red and three-red periods are remarkably close to BayC$ian (though the Bayesian probabilities are also close to O and 1 in those cases. so there is less room for error). Prices in the one-red and two-red periods begin well below the Bayesian prediction. and converge above it iu the direction of representativeness.

Dividend parameters were caiefully cbosc-n i-; the ex Oment so that final allo-



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cations of shares, as well as prices, would distinguish the Bayesian and represe-n-tativt!lleS$ explanations. When !he sample had onNed, the Bayesian expected values were equal for type I and type II traders. so the Bayesian tberuy predi(:ts an equal allocation of shares among the two types (even if subjects are risk averse or risk preferring}. The representativeness theory predicts that type I traders will hold more /iham, In fact. iUX]Ut 80 percent nf the shares were held by type I traders {90 percent after traders were once). supporting the reprnsen tativeness theruy.

The price biases are modest in probability tenru,[11] but small in cost---about a nickel per trade, or a couple of dollars over an experiment. However, paying subjects five times a,s much nwie little difference.

Experience reduces price bias but does not eliminate it. Experience also re• duces noise in prices and trading, which makes biases more statistically significant and cin.1c allocations to rtjcct !be Bayesian &$SUmptmn even more strongly.

Duh and Sunder (1986) ran &imilar experirne.,rts using a deslgn wuh several interesting variations (including a wider range of prior state probabilities and a labeling differeru:.e), Their results are runilar, Subjects were dost to Bayesian, OOt t y erred in the direction of an e:xtrenle representativeness: theory that pre diets that a &amp]e of ooe draw is taken as perfectly diagnostic. AUocatl-Ofls were strongly supportive of representativeness.

Anders<m and Sunder i_1989) report fuUI experimental ses-sions comparing SUI- dents with professional securities and commodity traders, in a destgn similar to Duh and Sunder (]986) ;mt! Cameror tt987). In three of the 1-0\lr sessions, the prices predicted by the Bayesian theory were below the price& predicted by npre-

sentativeness theories. Prices were far from Bayesian in the smdent sessions, and closer ID Bayesian in the sess.fons with traden;. (However, in their design representativeness pushes prices below Bayesian levels, just as ri\k aversion does. It is possible the professionals are simply les.., risk averse, and therefore *appear* more Bayesian.) Allocations did not :fuvor either the Ba:,estan or representativeness theories, though there is some movement toward Bayesian allocations in the professional trader sessions. The professionai traders alw iiliowed less underweighting of base rates in a word problem similar to the cab problem dis.,-u&Se<l in section II.Cl. While these data are limited. they do suggest professional traders are less prone to neglect base rates, and more likely to trade assets closer to Bayesian prices, than students,

Ganguly, Kagel, and Moser (in press} ran market e periments using an asset wh.oi;e value depended on the success of a hypothetical firm. A baM: r&tc of success and likelihood information were given, making the fiml's sm;ce.,s. isomorphic to !he cab problem. which elicits large underweighting of ba.'le rates. Their study has two innovations: a word problem is used to test for representativeness (the studie.s above used bin.go cages); and both individual judgments and market prices m measured each period, so one can measure whether market prk:es reduce mrlividual error. In two sessions, judgments and prices were much cki er to the bar.e rate neglect prediction than to the Bayesian predictio11. There ls oo apparent convergence to Bayesian price levels across sn!een periods.

Plott and Wilde (1982) studied product mark.els in which agent give advice to buyers about producr quality, based on samples of data that agents can r,ee but buyetefJ cannot. They report some evidence that agents \!Sed representativeness, rather than Bayes's rule, in drawing inferencei. from the samples.

This small collection of work on market level effeclS of Bayesian Judgment errors :ruggests that errors caused by representativeness m the rule, not che exception, in experimental market prkes and allocatioos. 1be errors are relatively small when uncertainty is generated by chance devices (bingo cages). Errors are quite large when uncertainly is generated by word problem,i like the taxicab prob• !em. Market experience appears to reduce error. though not eliminate it (Camerer 1987); professional trading experience does appear. tentatively. to reduce error iAnderson and Sunder 1990). Future re.search could profitably continue to map oot the boundaries of the influence of judgment error. COOlplDe individualll and their market behavior more carefully, and replicate whethec experience and education reduce error (as they appear to, m some studies). Srudie:; should also begin to disentangle the influences of incentives, competition. task repetition, learning from 001;;;ome feedback. and le<lrrung from actions of others, wtrich are currently confounded in market treatments.

D. Confirmation Bias mul Obstacle. to Leaming

An :important source of disagreement between psychologists and ICQnOIIUMs concerns learning. Psycholog1sts often suspect lha! the immedia1e, frequent. dear, exogeneous teedhack subj«-ts receive in e.conomics e,ipcrimenls overstates how well people learn in natural economic settings. Economist;,. in contrast, think lha! experiments undem.ate rhe rate of natural learning: be.cause context, ao;:e:.-s to advice, higher incentives, and added time to retket or calrulan:: are absent from experiments, and probably improve perf-0rmaru:;e in natural mungs.

Much of the psychologists' pessimism arises from evidence of heuristic tendenci in judgment that present obstacles to learning {e.g., Einhorn and Hogarth 1978). I mention only two obstacles here. One tendency is called "confirmation bias:· A tricky problem due to Wason 0968) can be use.d t0 illustrate this:

YQUare shown four cards. marked E, K, 4, and 7, Each card has. a letter on one side and a nnmberon the other. Yoo are given tile following rule: Every card with a vowel on one side has an even number on the other side, Which cards muSt you turn over to test whether the I'\lle is true or false?

In lhe four-card problem, most subJects answer !mil E must be tume-tl over. nu- E and 4. They think you should check the one card with a vowel (E you should-and perhaps check the even-number card {4) too. The oorrect an wer is E and 7. Few subjeCl'i think rotum over the 7, but they should becauaetberule is falsified if !he 7 card has a vowel on the other side, The four-ca«t problem sug. gests that in testing an hypothesis, people instinctively seek evidence that could confirm the hypothesis: for example, findin.Ji! a vowel on the other side of 4 woold provide support for the rule, but that evidence could actuaHy ne•;er test whether !he rule is always true (as turning over the 7 can). Confinnation bias ls one force that may inhibit learning.

A related problem is the production of treatment effects (ot seIM:uJtilling prophecies). When people believe an hypothesis is true. their actions often produce a biased sample of evidence that reinforces their belief. t A busy waitel' who thinks poorly-dressed patrons tip badly will give them poor service and receive a bad tip, reinforcing his Uleory. Treatment effeds inhibit learning whether one's 1.mdertying belief is false. (The only way Io tes-t the belief is by experimenting, giving good sen1ce to a poorly-dressed patron,) fupectations of a bank run. or a bubble in asset prices., can be self-fulfilling in a simiJar way.

E. Expectations Forma.tion

There is a large literature oo expectations formation, and a smaller literature about expectations people fottn about variables generated endt:igeneously by their own collective activity-future prices, .for instance.

There are many studies of whether price expectations are rational, Most of the slUdies use published forecasts by coosum.ern. busi1iessmen, or professional coonomists (*see* Lovell [1986], and Williams (1987}, for reviews). GeneraUy, they find that fore.casts are biased: fure<:ast mots (forecasts minus actual results) have a nonzero mean. Forecasts usually violate rationality of expectations: they are correlated widl ohservable variables (typically past forecast errors and current forecast levels), implying U'at wme available information is ignored when forecasts are made. Forecasts also usually follow an adaptive process in which forecast changes are related to past fonx:ast erron (Nerlove 1958, and see below).

Apparent violations *uf* rationality of natura1ly-occuring forecasts coo1d be due to Bayesian learning in an economy where the statistical process generating out-com keeps changing (Caskey 1985; Lewis l9R9}. For example, the surveys show that blliinessmen were consistently s.nrprised by !he persistence of price inflation ill the 1970s (their forecast: errors had a negative mean), li „;:oold be argued that cheir forecasts were rational. éK ant.e, but the stochastic inflation pro• ces,s chllilged during the period and it took forecasters some tune to learn whether the change was temporary OT perm.anent.

To cootrol for changes in the statistical process generating outcomes, several experiments examined forecasts of outoomes of a statiStical process that 1s un• known to sul,jecti; but fixed throughout the experiment, and known to be fixed tSchma.lansee- 1976; Garner 1982; Solle 1983}, Their results are generally fo. consistent with rationality of expectations too, but suggest 5 0 m e learning and ra- tionality in special settings (a random walk with no drift, in Dwyer et a t [1993J·;t

Several researchers have gathered forecasts that subjects in an experimental mark,et make IJf the future prices that they themselves generate {Carlson 1967; Knez. Smith, and Wt.lliams 1985; Wiihams 1987; Daniels and Plott 1988: Smith, Suchanek, and Williams 1988; Wellford 1989; Carucrerand Weigelt 1990; Pet.etc son 1993). I will describe the Williams (1987) study in detail. He conducted experiments with a series of five doubie auctions fur single period goods. Starting after ihe first period, subjects forecasted the mean price in the next period, The person with the lowei;t cumulative absohlte forecast error eamed $ L The forecasts were generally remarkably accurate, but the small deviations from rationality were smistically signiiicant. Nearly half the forecast errors were zero (the mode), but on average prices were about a penny too high. {Since goods cost about $5, a penny deviation is tiny.) Forecast errors ·were modestly autocorrelated (r = .15, $p <$ .Olj. Expe<:tations were estimated w be .idaptive[17] if the adaptation coeffi• ..:ient b was positive in the specification:

$$E(P_t) - E(P_{t-1}) = b[P_{t-1} - E(P_{t-1})] + e_t \tag{1}$$

(where E(PiHienotes the forecast of prices in period rand P_{t-1} is the actual price in period r - Jl. Williams eslimaled b ''' .86. Forecasts of experienced subjr-.-ts-, who had participated in other auction experimenl , were less biased and less error prone. bul not by much, Peterson (in press) found that the estimated bin equarion (1) converged to one across periods of an experimem. and changes were Juge.'-t for the least experienced subjects.

Besides contributing to the debate about rationality of forecasts, th studies by William and others allayed methodological fears. that simply gathering forecast:. might affect market behavior. One concern was that asking subjects to forecast prices hefore each period might increase or decrease tbeir attention to market behavior and affect convergence. But partems of convergence looked like those in previous double auction experiments (see chapter 5 for examples). suggesting there was no such effect. Another concern is that subjects who are rewarded tor making accurate forecasts. may samfice trading profit to cotloct the best-fore- caster bonus. There was no evidence of such an ffect either.

Williams's methods and result:,. are typical of tn()!;t OU!ef studies. Forecasts are usually slightly biased (too low if prices att rising: too high if p r f f l are falling).

Forecast CITO!'$ are <1utocorrelatetl and correlate.ct with some ob:.ervables (previous price dumges or current forecast levels). And forecasts generally adaptive; estimates of the adaptiv.:ness oefficient h are remarkably con5tant across a wide variety of studie"-. benveen ,6 aud .8.

A notable except:Ion is Daniel and Plott (1988). They studied forecasts in goods markets \\-'itb price inflation that was induced by shifting supply and de· mand curve.s upward by 15 percent each period (until lhe last few periods). Prices adjusted 10 the iliflatiilll a bit sluggishly. Graphs of average forecasts and prices suggest that forec.isJ,; were biased and .autocorrelated (they were too low during .inflalion, and overshot when the inflation stopped). But regressions indicated that subjects' forecasts were rational rather than adaptive. It is not clear why lhe ex- pe;:tations of their subjects (Cal Tech students) are not adaptive, as they are in mmt ot:bcr studies,

Price forecasts are easy to gather. Perhaps experimenters should collect them routinely, So far, the forecasts have not generally been put co much use to inform either psychology or economics. but they could be. A good example is Smith. Sochanek, and Williams (1988> who use evidence of systematic fore.:ast error to explain why price bubbles persist in experimental asset rnarkelli (see chapter ti).

Psychofo-gica! Siu.dies e f Expectaiio1 1s

P$yc001ogiHs have dooe two kinds of studies germane to understanding rational- ity of expectations. One kind is studies of "multi-cue probability learning"' (MCPL} (e,g., Castellan I977i. In MCPL studies subjects try to predict a depen- dent variable from given values of predictor \'ariables, in a series of JOO or so u1als. The studie-. indkalf- that learning is very diffiwlt except In simple. deter- ministic situations (e.g., when dependent variables are a linear combination of independent variables; Brehmer 1980). Leaming stochastic rules is especially difficult.

A second body of literature concerns judgments made repeatedly by people (mauy of them e,.;pem) ln natural settings where stochastic outcomes depend on some observable predictors (e.g., te5t scores} and some unobservables, faamples include medkal or psychiatric diagnosis (severity of Hodgkins' disease, sclrizo- phrenla), predictions of re.;idlvism w parole violation by criminals. ratings of marital happiness, arul bankruptcy of finns. About 100 careful studies have been documented so far, The remarkable finding in altnost all these s is that weighted" linear combinations of obterVables predict outromes better than indi- vidual experts can (Meehl 1954; Dawes. Faust, and Meehl 1989), In a typical study (Dawes 1971). it was Wscovered that academic success of doctoral students could be predicted better by a sum of three measures----GRE scores, a r,ating of the quality of the studenfs undergraduate school. and her undergradua,;e grade.,._ than hy ratings Qf a faculty admissions committee. (Put bluntly, the faculty's deliberation jm;t added noise to the three me;uure iode-x,.) The -0nl, documerited exceptioos to the general conclusion tha! modeis ootpredict experts are a few kind; of esoteric medical oia,goosis,

Jn these studies, experts routinely violate rn.tional expei..—tation by using observable information inefficiently (worse than simple models do). The violations have two common forms: (1) experts often add error to predictions by using complicated interactions of variables (weighting grades from low-quality schools more heavily, for example), rather than more robust linear combinations of variables; (2) experts pay attention to observable variables that they should ignore because the variables are not highly predictive of outcomes (personal interviews, for example). These psychological tendencies can be traced to some of the judgment biases discussed above (e.g., Camerer and Johnson 1991).

F. *Iterated Expectations and the Curse of Knowledge*

In many economic settings, agents must guess what others think. These guesses are "iterated expectations," or expectations of expectations. We can express these formally as follows: suppose agent i and j have information sets I_i and I_j and agent i is guessing j's expectation about a variable X. Then j forms the expectation $E(X \mid I_j)$ and i forms an iterated expectation, $E(E(X \mid I_j) \mid I_i)$.

Most asymmetric-information settings are modeled by assuming one agent knows strictly more than another ($I_{less} \subset I_{more}$). These models usually revolve around the less-informed agent's attempt to learn what the more-informed agent knows, perhaps by observing a signal (cf. the asset market examples in chapter 6). A hidden assumption in the models is that the more-informed agent has an accurate mental model of the less-informed. The psychology of memory and imagination suggests that assumption may be wrong: it is hard for the more-informed agent to forget what she knows and imagine what the less-informed agent is thinking, because her extra information is available in memory.

Normatively, $E(E(X \mid I_{less}) \mid I_{more})$ should equal $E(X \mid I_{less})$. If the extra information in I_{more} is hard to forget, empirical estimates of $E(E(X \mid I_{less}) \mid I_{more})$ will be biased away from $E(X \mid I_{less})$ toward $E(X \mid I_{more})$. This bias is called the "curse of knowledge" (Camerer, Loewenstein, and Weber 1989). It seems common: teaching is made difficult by knowing too much; after solving a problem it seems obvious that others should see the solution too[19] (Nickerson, Baddeley, and Freeman 1987); and what writer of computer manuals—an expert, usually—has ever written one that novices can understand?[20]

1. False Consensus and Hindsight Bias

Two brands of curse of knowledge have been studied in some depth. One brand is called "false consensus" (an unfortunate misnomer): people use their own tastes and beliefs as information in guessing what others like and believe (Ross, Greene, and House 1977). In one study, students were asked whether they would walk around a campus for thirty minutes wearing a sign saying "Eat At Joe's." Some did, others refused. The interesting finding is that both kinds of subjects thought others were likely to make the same choice they made. Those who wore the sign estimated 62 percent of others would; those who refused thought 67 percent would refuse. Using one's own tastes or beliefs as information is not a mistake

unless that information is overweighted. (It is reasonable to use one's own tastes as a single draw from the population distribution of tastes. Then two Bayesians with different tastes will have different posterior beliefs about the population—as the two groups of sign-wearers and refusers did—but a difference in posterior beliefs is not necessarily an error. See Dawes [1990].) I suspect overweighting one's own tastes might contribute to the high failure rate of small businesses: owners think more consumers share their tastes than actually do and either underinvest in market research or ignore its result.

A second kind of curse of knowledge is "hindsight bias": current recollections of past judgments tend to be biased by what actually happened since then (see Fischhoff 1975; Hawkins and Hastie 1990; Christensen-Szalanski and Willham 1991). Fischhoff and Beyth (1975) asked subjects about the likelihood of various events occurring before Nixon's historic trip to China (Will Nixon meet Mao?). Several months later, after the trip was over, subjects were asked to recall what probabilities they gave before the trip. They remembered having given higher probabilities than they actually had for events that happened, and lower probabilities for events that didn't happen. Subjects were not paid for accurate recollection but I bet the hindsight bias persists even with financial reward (assuming subjects cannot record their initial answers and look them up afterwards). Hindsight bias is often modest in magnitude but robust, and affects events with low ex ante probabilities most strongly. Hindsight bias appears to create second-guessing in firms, courts, and political institutions, which may create added employment risk when good ex ante decisions results in bad ex post outcomes (cf. Baron and Hershey 1988).

2. Market Level Test of Curse of Knowledge

Camerer, Loewenstein, and Weber (1989) tested whether the curse of knowledge affected prices in experimental markets.

Before the markets began, one group of "uninformed" subjects guessed the 1980 earnings-per-share (EPS) of several actual companies, based on accounting data from 1970–1979 and a Value Line profile of the firm's 1980 prospects. Call the uninformed subjects' average estimate $E(EPS \mid data)$.

Traders in asset markets then traded a one period asset that paid a dividend equal to the average estimate of uninformed subjects, $E(EPS \mid data)$. To value the asset correctly, market traders had to make the best possible guess of what uninformed subjects thought 1980 earnings would be. Market subjects knew the *actual* 1980 earnings per share. Their guess about the uninformed subjects' average estimate is therefore an iterated expectation, $E[E(EPS \mid data) \mid data + EPS]$. If market subjects suffer from the curse of knowledge, asset prices will be closer to true EPS than they should be. (It will be hard for traders to imagine that subjects could not have guessed the true EPS.) A separate control group of subjects did not trade in any markets, but simply made judgments about what the asset value would be (knowing the 1980 EPS); they were rewarded for accurate forecasts just as market traders were.

Titls setting is similar to underwriting, in which a group of expert buyers must purchase goods that are resold to a group of less-expert consumers. The consumers' opinions establish the value of the goods, which the expert buyers must anticipate. Financial underwriting or buying of clothes, art, or wine for retail sale are examples. The empirical question is whether the experts will let their extra knowledge get in the way when figuring out what nonexperts will buy.

Traders traded assets based on eight different companies, for two trading periods each. (We used two trading periods to measure the change in forecasts and prices between periods. We used only two periods so we would have time to trade several companies because we suspected---correctly, as it turned out-that lhere might be inherent variation in the degree of curse of knowledge across companies, which we hoped to average out by using eight companies.) The degree of curse of knowledge, or hindsight bias, was estimated by having traders give forecasts of the asset value three times, before each of the two trading periods and after the last period. We compared their forecasts to the actual asset value (0 percent bias) and the true 1980 earnings (100 percent bias). Figure 8.4 illustrates the degree of bias across the eight companies. The mean degree of bias in the forecasts of market subjects after two trading periods is shown by a regular line. The mean bias in the control-group individual judgments is shown in Figure 8.4 with a dotted line.

The market traders' forecasts exhibited roughly 30 percent bias, whereas the individual subjects' forecasts exhibited roughly 60 percent bias. Thus, market forces reduced the curse of knowledge in traders' judgments by about half, compared to the control group subjects, but did not eliminate the curse entirely.

A closer look at individual behavior suggests why market forces had an effect. Less biased traders were slightly more active in the markets (56 percent of bids, offers, and acceptances) than more biased traders (44 percent). Prices generally began between the Opercent bias and 100 percent bias levels. Of the price changes that moved toward one benchmark and away from the other, 63 percent of the changes were toward the O percent bias benchmark. In these experiments, the market is actually a bundle of forces that could be separated in future work. Compared to the individual-subject control group, traders in the market made three forecasts (rather than one); spent more chronological time thinking about each company; and had the opportunity to learn from bids, asks, and acceptances by others. We suspect the third force is most important. The double auction market is a specialized communication mechanism that allows people to express their opilllons, and learn from others, in a limited form. It could be usefully compared to other opinion-aggregation schemes (e.g., open group discussion) and other exchange institutions for bias-reducing properties.

The curse of knowledge implies that more information might hurt those who are trying to guess what people without the information think. If a harmful good is freely disposable----toxic waste, or curse producing information-it should have a price of zero; but information is *not* freely disposable if it is hard tO forget or ignore. We ran two market experiments, with eight traders in each, to see whether traders would bid zero for curse-producing information (Camerer

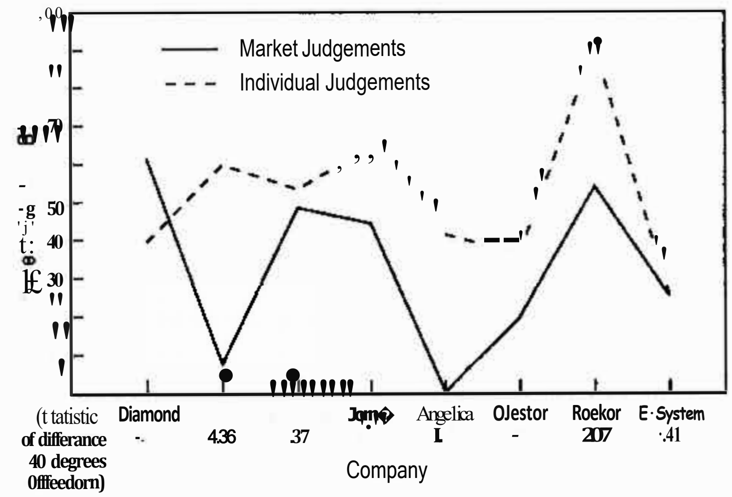


Figure 8.4. Degree of hiru;h;ight bias (or cum: of knowledge) in individuals (dotted liru,J and nwket traders (solid line). Source: Cam=, Lowenstein, and Weber 1989.

1992b). We auctioned off the actual 1980 EPS to four traders in a uniform price auction where the price was detennined by the fifth highest bidder (cf. chapter 7). lnfonnation was auctioned off once for each of the eight companies.

Bids began at very high levels, close to the asset value itself (around $3), probably because some subjects had no idea what the information wa.; worth. Others bid zero immediately. After two or three company auctions, the market price converged to zero. Some subjects even made small negative bids-they wanted to be *paid* to know something worthless, perhaps because they knew it was not freely disposable and might hurt their judgments.

G. *The Illusion of Control*

People sometimes act as if tasks that involve only chance have an element of skill too. Psychologists call this belief the "illusion of control." Gamblers throw dice hard to produce high numbers. Lottery bettors buy "dream books" that explain what numbers to bet after a certain dream. People wait longer or pay more for specific numbers in a lottery than for randomly-assigned numbers (Langer 1975).

The illusion of control is one kind of "magical thinking," a misunderstanding of causal relation, akin to rain dances and superstitions![1] There is some remarkable evidence that control illusion improves mental health. In one experiment, subjects were allowed to bet varying amounts of money, with varying expressions of confidence, on a chance device. Subjects who *did not* suffer from the illusion of control-they bet less money, less confidently-were more likely to be clinically depressed than others. (They are "sadder but wiser," Alloy and Abramson 1979.) Taylor and Brown (1988) review a wide range of evidence that suggests that unrealistic illusions of optimism and control, rather than realism, are associated with mental health

Control ill:usl:on might be important in agency relationships and compensation. Jn the standard economic model of agency, output is assumed to be a funcllon v(e, 0) of an agent's effort e and a random variable (L The illusion of COtltrol implies that agents and emplnyw; overestimate the effeci of effort vtvlde). In equilibrium, they will tie cotnpenS<ation too ckr.:!ely to output, and reward or punish more than is optimal.

In an experimental study of agency oontracts, Berg (\995) found that comrol appears to matter in an interesting way. In her M!tting, agents choose an effort leve:L Output is correlated with effon: and with an observable signal. In the "control" condition agents: have \,"Olltrol over the signal because their effort is- correlated with the signal's value. In the "no-control" condition, their effort is not tly correlated with the signal (so they have no contr(ll over i.t}; but the signal is infonnative to principals because the joint distrihulion of output and signal is correlated with effort n Optimal conttaet& should use lhc signal to determine an agent's compensation in both conditions, since the signal is always informative about the agent's effort (when coupled with observed output).

Subjects did use the signal in the control condition. but not in the no-control condition. They acted as if penalizing an agent for outcomes (signal values) beyond her control was pointless or unfair. The result jibes with evidence from natural settings that sh11reJ.housing contracts and executive compensation does not depend trongly on variables that are uncontrollable but informative, as it should (Wolfson 1985: Antle and Smith 1986).

H. Judgmen.r: Summary and New Directions

The studies 1-eviewed in this section suggest a variety of beuristic rules people use to make complex judgments: they rely on what's available in memory, and similarity, to judge likelihoods and correlations; they are overconfident when they st.ate probabilities of events or forecast numbers; their expectations are adaptive, responding to observed marketwide behavior rather than expressing a rational undentmding of the market (but see Lucas 1986); iterated expecuuions xpectations about the expectations of others--are jncorrectly influenced by memory; and people overestimate the influence of personal control.

Only a few of these findings have been replicated with the methods of experimental C('-OlLOfUists. More replications would test robustn= of !tie findings.

Only a few of the replications rook place within economic Umimtions (!llarketst The market eJtperimcnts., onenms iu Bayesian updating and iterated expectations, suggest markets reduce simple judgmcnterrorn but do nor eliminate chem. (Experience and expertise seem to reduceenms too.) More tests in which individual errors might be manifested in economic settings, including games or markets, woold be usefut Tests oould introduce instirutional feanires such as overlapping generatio . b.anktuptcy, access to capital, and advice market,;., to carefully dissect precisely how markets reduce errcr.

m. Choice under Risk and Uncertainty

M econollllsts are familiar with theories !hat represent choice& b}' numerical function.s. 1_e.g., a utility function). Sometimes functinnal forms are simply posited, bul Ujtially theorists sean:h fur primitive axioms oo preferences that impty a ;;peclfic functional form (e.g,, expe.:ted utilityt A le$$ familiar tortn of chn1ce theory is a process. model thai: ex,pre:,ses the procedure a person uses to make choices., in an algoritbm. {Expected utility ITlllXimizatioo is an example of an algorithm.) Process models will generally oot obey the axioms of utility the· ory, so the preferences they generate cannot be neatly sllIIlJillIriud hy a utility function.

Within economics there is a vast amount of work Ol .uiomatic vtihty rtpresen• tations and a little work on process models. I will try to summarize both.

A. Expected Utility

During the development of statistical reasoning. it was taken for grouted that proper choice meant maximizing expected monetary value. Pmvoked by the SL Petersburg paradox, in 1738. Daniel Bernoulli proposed matimizing some concave function of money (he suggested logarithmic), lo reflect diminishing marginal value of dollars. Expected utility wns born.

Almost two hundred years later von Neumann and Morgenstern (1944J showed, en route to game theory, that if preferences obeyed a particular set of axioms then those preferences could be represented by the expectation of some utility function.

The discovery of underlying axioms was important because it is easier to judge the intuitive plausibility of specific axioms than to judge the appeal of lhc utility representation they imply. (Establishing the a1doms also laid the groundwork for modem thwrists to weaken specific axioms and generate surprising alternative thcorie.) The utilily representation can .also discipline preferences by pohl.1ing out inconsistencies and violations of appealing properties (Strotz [1953, 3921 gives an example). Expected utility also provided a natural way to establish "measurable utility" (cf. Zeuthen 1937}, which was in great demand at the tilne.

L Notation and a Diagram

Some nouuion is useful. Denote lotteries by X. Y. Z, and probabilistic mixtures of lotteries by pX (I- p)Y. {Spedfic outcomes are just degenerate lotteries with fill outcome probability of one,) DenoteXpreferred to Yby X > f, and X inditferen! toYbyX·-Y,

Predictions and data can be uwfully di.splayed in the triangle diagram devel• oped by Mam;halc {1950) and put to good use by Macltina (1982) and othetS,,; Fix three gambles X_l, X_m, X_h (the subscripts represent low, medium, high) such th.u $X_h > X_m$, $X_h > X_l$, and $X_m > X_l$' {In most ex;perimems, the gambles are

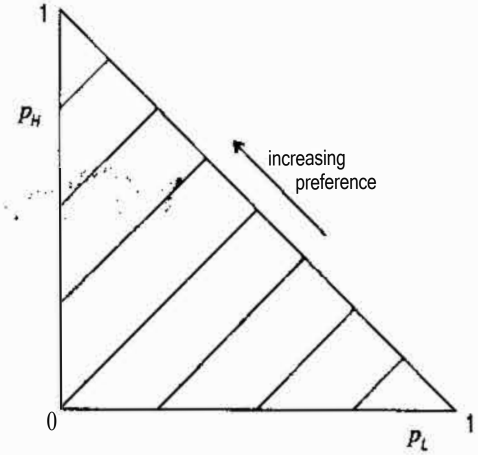


Figure 8.5. The Marschak-Machina triangle diagram (showing EU indifference curves). *Sources:* Marschak 1950; Machina 1982.

degenerate gambles with certain outcomes, such as 0, $5, or $10). Take the set of compound gambles which are probability mixtures in which each of the three gambles occurs with (objective) probabilities P_L, P_M, P_H. If we assume that two stage compound lotteries are equally preferred to single stage gambles with the two stage probabilities multiplied together (an important assumption we return to later) then this set of gambles can be represented in two dimensions, in P_l - P_H space, as in Figure 8.5. (The third dimension, P_M, is implicit in the graph because $P_M = 1 - P_L - P_w$.) Since the sum of the probabilities cannot be greater than one, the set of feasible probabilities is a triangle bounded by the lines $P_L = 0$ (the left edge), $P_M = 0$ (the hypotenuse), and $p_H = 0$ (the lower edge). A utility theory makes a specific prediction about the shape of indifference curves that connect equally preferred gambles in the triangle diagrams.

2. The Axioms

The axiom system von Neumann and Morgenstern used to derive EU got refined by others (e.g., Marschak 1950; Herstein and Milnor 1953). The crucial axioms are as follows:

1. Ordering. Preferences are complete (either $X > Y$, $Y > X$, or $X - Y$) and transitive ($X > Y$, $Y > Z$ $X > Z$). Graphically, completeness guarantees that any two points in the triangle are either on the same indifference curve or on two different curves; transitivity guarantees that indifference curves do not cross *within* the triangle (e.g., Fishburn 1984).
2. Continuity. For all $X > Y > Z$, there exists a unique p such that $pX + (1 - p)Z - Y$. Continuity guarantees that there are no open spaces in the indifference map; uniqueness of p guarantees that indifference curves are not "thick."
3. Independence. If $X > Y$, then $pX + (1 - p)Z > pY + (1 - p)Z$ for all Z and $p \in (0, 1)$. Independence implies indifference curves are parallel straight lines.

The axioms imply that preferences can be represented by a numerical utility index, and the utility of a gamble is the expected utility of its possible outcomes. For a discrete lottery with several outcomes x_i, each with a p_i chance (denoted $\Sigma p_i X_i$) the functional form for EU is:

(2) $$U(\Sigma p_i X_i) = \Sigma p_i u(x_i)$$

In expected utility, probabilities of outcomes are assumed to be objective and known; choices are made under "risk." But in most natural settings probabilities are not well known or agreed upon; choices are made under "uncertainty." In subjective expected utility (SEU) (Ramsey 1931; Savage 1954), people take acts that yield consequences in uncertain states (see section III.F later). If act preferences obey several axioms like those in EU, preferences can be represented by an expected utility of consequences weighted by beliefs about states (their subjective, or "personal" probabilities). Anscombe and Aumann (1963) fused EU and SEU by allowing outcomes with objective probabilities ("roulette lotteries") *and* uncertain states with subjective probabilities ("horse lotteries"). Since most of the debate about EU holds for SEU too, and most experiments test only EU, I defer further discussion of SEU until below.

B. *Some History and Early Evidence*

The publication of von Neumann and Morgenstern's book in 1944 caused quite a stir. Economists had just become satisfied with ordinal utilities, unique only up to monotone transformations, and knew how much analysis could be done using preferences that reveal only ordinal utility (Hicks and Allen 1934[24]). Then, just as economists became convinced that cardinal utility was unnecessary, von Neumann and Morgenstern discovered a simple way to derive utility cardinally: $u(X) = p$ when X is judged to be indifferent to $pH + (1-p)L$ (and $u(H) = 1$, $u(L) = 0$ arbitrarily).

1. Three Controversies

The first of three immediate controversies was mathematical. In their book, *von* Neumann and Morgenstern said nothing about an outcome set, indifference, or an independence axiom. In brief symposium papers in *Econometrica*, Samuelson (1952) and Malinvaud (1952) solved these mysteries and showed how the now-familiar independence axiom followed from von Neumann and Morgenstern's axioms. The second controversy was confusion over whether a von Neumann-Morgenstern utility function was a riskless value function too (à la Bernoulli), and could either be derived from preferences over lotteries *or* by directly comparing differences in lottery outcomes (Ellsberg 1954).[25] It's *not* a riskless value function.

The third, and greatest controversy came at a symposium in Paris in 1952, where Maurice Allais presented two papers[26] critical of the descriptive power of the theory of the "American school" (including Friedman, Savage, de Finetti, Mruuhak, and oth«s died above) and introduce<l llis fmnous paradoxes (Allais 1953). {More about them below.)

After von Neumann and Mol'.geruitern's book was publi.'lhed. empirical 1em began to trickle in. Excellent reviews of early v.-0rk rue Edwards i 1954c, 196la} and Luce and Suppes (!965!. The collection edited by Thrall, Coombs, ;md Davis (1954) wfle,.-tt; the spirit of grop with the new model& in an hlstoricnlly fasdw nating way.

2. Initial Tests

Preston and Baratta {1948) did the first tesL They auction«! off chances to vil1 r points with probability p, for 42 ti:, p) pairs.:" Bids were approximalely linear in outcome x and nonlinear in probability p: Low-probability gambles (p = .01, .05) were sold for ,:everal times e ted value, high-probability gambles for slightly lt$S than expected \<alue. (Indeed, the probabH.ity weight function they estimated, shown in Figure 8.6, looks strikingly like the "decision weight" function hy-polhe.sized thirty years later by Kahncman and Tversky fI9791, but the interpre-tation is a bit d:ifferent?i) The modern reader will suspect the quality of their e-vldence because 100 method& are casual and unorthodox, the sample is small, and hypotheses ant not tested. But similar findings of nonlinear probability weights were reported by Griffith (1949) using rru:clr.lek betting (la1er. McG\oth-lin 1956), Attneave (1953) using guessing games, and Yaari (1965) using indif-ference judgments.

Mosteller and Nogee ( )951) estimated utility curves by offerin_g subjects com-plicated bets on three-die outcomes. Subjects played all bets they chose, and earned about $1 per hour. Since the same bets were offered repeatedly, <etl:ainty-equivalents were estimated by observing which bets subjects took half the time they were offered.

Student subjects were -Hghtly ri averse: National Guardsmen were rather rii,k seeking.[79] Using utilities estimated from ooe sample to predkt fre h choices, EU got about 70 pen:-ent right {compared to 50 percent right for expecte.d value). There was strong evidence of nonlinear probability weighting by National Guardsmen (mudi as in Preston and Baratta"s study) but not by s!udents. as shown in Figure 8.6.

'There were several complaints about Mosteller artd Nogee's design. One was that tbe bets had complicated probabilities. Edwards began to test EU with an eye toward measuring subjective weight,; of probabilities, using simpler stimuli.'° In choices aJJlO!g gambles that were played out he discovered coosistent "probabil-ity preferences" {overweighting of specific probn.hllities), notably a preference for .5 {Edwards 1953, I954aJ Replkmiou in a military context (hypo1hetical choices of attack targets) yielded ;,,lightly different result; (Edwards I954bt Nobody bas found quire these probm;ihty preference" since. Edwards's data also show that probability weights for potential gains and losses differ, .indicatmg a k)nd of "'wishful thinking" (cl. Irwin 1953[31]).

In the early 1950s the psycbologrn1 Clyde Coom began lry.ing to measure subjective probability and ulllity simultaneously. (Edwards showed that th! was

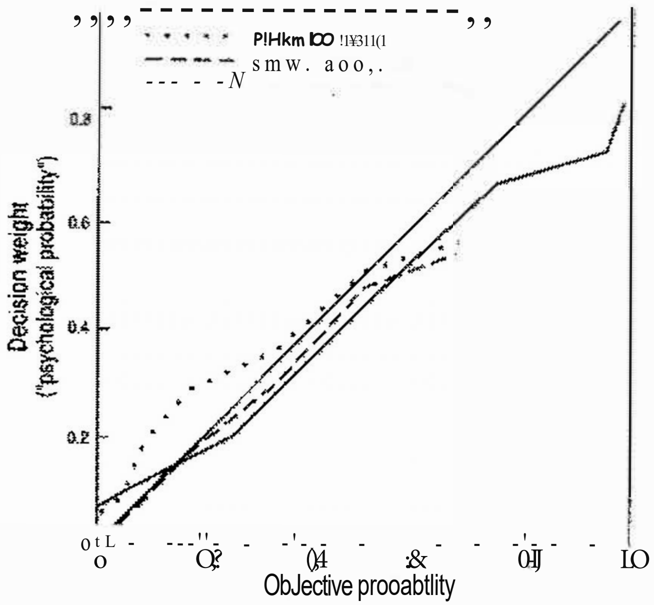


Figure S.6. Empirical decision wdghw <t;ive;I fmm !W<early studies. Saune;: Preston and Bnmtta (1948): Mosteller and Nosee (1951).

important to do, since nonlinearities in subjective probability were observed when utility was assumed to be linear, bu! he did not show precisely Jlow to do it.) Some -of Coombs's early results were supportive of expecled utility. But Coombs inihally scorned gambles over money (thinking subjects could not avmd respond-ing to numerical money vaJues ralhcr than psychological utilities), so it is im-possible 10 compare the predictive accuracy of EU to the lltl.tural benchmark. expected value. l!sing money, Coomhs and Komorita (1958) fmmd lha! utili• ties did satisfy a kind of addi1ivityY Hum and Siegel (1956) found tha; an "or-dered metric" utility model ompcedicted expe,cted value, in an experiment where pris.ooer chose bets over cigarettes (a common setting for early p,-,ychology experiments).

Da\-idson, Suppes, and Siegel (1957) untangled probability weights and utility most carefully. Their technique depends on finding an event that J;; perceived to be exactly as attractive ro bet on as !0 bet against. After rejecting coins and reguW dice, they ch(& a si:i:"sided die with two nonsense syllables (ZEV and ZOV, shown by otbers to have few almost oo mertial associations) on three sides each. Since svbjccts did not care whether bets prud off on ZEV or WV, their subjective probabilities could be taken ro equal .5. Usmg choices over bets on !he special die, bounds on utilities could be determined while holding subjective probability fix.ed.;;

11ie estimated utility fuuction.s were remarkahfy ronsii.tent l:ict'ffil sessions. Twelve of fifteen subjects had nonlinear Cllrves. typically showing nsk preference for gains .trl(! nsk aversion for losses. An experiment betting on one side of a fouMided die ihowed that people gave an event with objective probability :25 a decision weight of abom .25.

In later studies Tversky (1967a,1%7b) and olhe:rs were able 10 opermkmalize tOOindependence of probability lmd utility (the rrucia! katu.re of EU) a,; a kind of additivity that was easy to check experimentally. Tversky experimented with prisoners, playing gambles fm money, candy, aOO cigarettes using the Bec:ker, DeG:root, Mid Mars.ebak (1964) procedure. Their choices generally obeyed additivity•••i,e., indeperulence-but showed either a utility for gambling {when riskless vallte functions and rislcy utility functions were compared) or rubadditive weighted probabilities (sec also Edwards 1962). Experiments by Wallsten (1971 l supported independence too.

In the 1950s people also began exploring stochastic choice models (Debreu 1958; Luce 1958, 1959: Luce and Suppes 1965). wtikh allow subjects to choose differently when facing the same t:hoice several times. In these model a gamJ;,le's probabilit, of being chosen out of a pair increases with its utility[31] (in EU, the probability increases in a step, from O to 1). Many experiments (e.g.. Mosteller and Nogee 1951) were designed with stochllstic choice models io 1llind, which meant long repeated sessions in which subjects made the same choice many times. (Wallsien l 1971 j had four subjects, each making choices for thirty hours.) This technique is largely out of fashion now except in some domains of mathematical and experimental psychology,

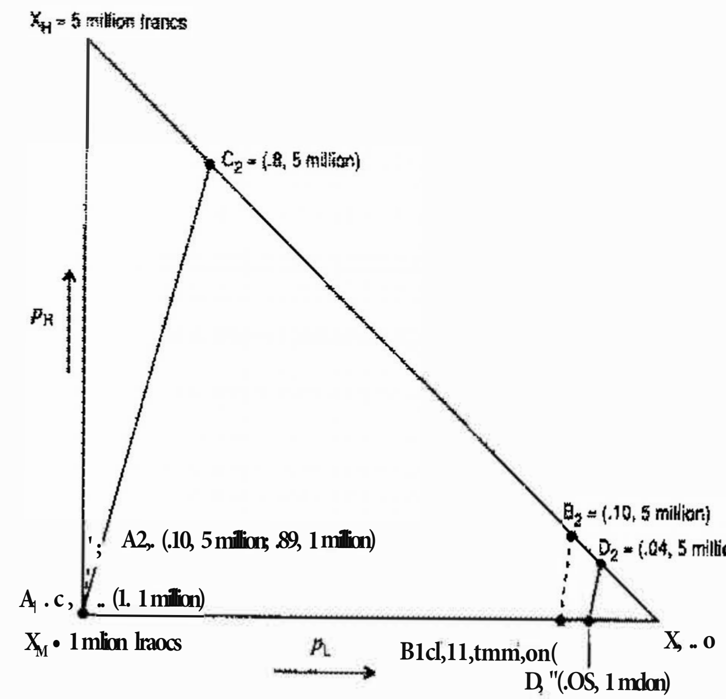
C. *Moun.ting Evidence of EU Violation (1965-1986.J*

Except for some evidence that probabilities were weighted nonlinearly, and the simmering impact of A.llais's paradoxes, EU emerged relatively unS<:af.hed from the flrs.t waves of tr:sts, Elicitation yielded reasonable utility funuions: expected utility predicted choices better than expected value did; indCf!Cndence of probability and utility wa& generally satisfied, Then evidence of paradox began to mount.

1. The AUais Paradoxes

Many felt All:m-'s examples uwd such extreme sums that they did liule damage to everyday application of EU, But the examples were provoc.ative and were rep• Heated repeatedly in the 19606 and later \\-ith smaller sums (and paid sl,1,bjects).

The most famous Allais example illustrates a "conunon oonsequence effect-" Subjects choose between Al and A2, where A l = 0 million fraocs) arn\A2 = JO chance of 5 million francs, ,89 chance of t million fraocs, and .01 chaoce of o. denoted *A2* = (.JO. 5 millioo francs; .89, 1 millioo frarn::s: .01, O). They also choose between Bl = {JI, 1 mmion francs: .S.9, 0) and82 = (.10, 5 million francs; .90, 0). It is easy to shrrw thut the frequent choice pawm,.Al > *A2* and *82* > Bl violates expecW'.rl utility. The choices are shown in a triangle diagram ln Figure iL7. m; requires that indif'furnlce curves be parallel lines. Since the chords connecting !.he choices Alw;\2 and the ch.oic BI-B2 are par.¼llel, Mlbjects with paraJ.lel indiffel"C'llce curves must choose either Al and fl1, or A2 and B2.



Figll!'C8.7.Allais'sconnrumcom;equeoce(A >A,,B.>B):uu.lconunontatiofC1> C,D , > O, effecls.

The Aflais. paradox. attacked EU in a fundamem.ally different way than the painstaking empirical tes of the 1950s did. The paradox circumvents elicitation of. utilities and directly tests consisrency requited by the axioms, using two pairwise c?°ices. Most recent tests have followed this route 100. An important drawback ls that the *degree* of inconsistency and differences among individuals are hard lo measure with this method.

The first Allais replication, in a dissertation by M.acCrimmoo (1965). reported a ut 40 pelCent EU „iolations. Morri!.nn (1967) used _gambles over- aero.al grade potms, and found 30 percent violatioo. (Open di ussion among subjects improve co sistency with EU slightly.j Skwic and Tve:csky t1974) found 60 percent violation and presented subjeci.8 „,,ith ·written arguments pro and con Ell. After reading both arguments. slightly *more* subjects switched their choices to be.come inconsistent with Ell than became coru;istent.

MacCrimmoo and Larsson (1979j revle\\>"e-0 w. ral axiom »)11tell5 and MJme empirical evidence, and reported new data resting robustnes.,. of the paradoxes. They found fairly robust common consequence effects with d f ferem parameters (though the effects are .wongest for extreme payoffs and prob.abilities). They also studied a = o n d "common ratio" problem due to Atlais (see Figure 8.7): choose either Cl = (1 million francs) or C2 = C80, 5 million francs; .20.0). and eith:e; Dl = C05, 1 million francs; .95,0) or D2 = (.04, 5 Jll.lllion francs; .%,0), Notice that the payoffs in Cl and C2 have the same ratm of probabili.ti.es, li.S, as in DI and D2 {.05/J)4): hence the tenn "common ratio effect" People often <'hoose Cl > C2 and m > Di, violating EU.

MacCrimrnon and Larsson (1979) found !he common ratio efreci was less robust than the common consequence effect: A majority violated it with large runot.mlli „,nen the winning probabilitie.s in the C gambles were much different man those in the *D* gambles, but the rate of violation fell to a third or less as the and probability differential felL "nieir evidence was the brruuiest indication that the proportion of subjects violating EU might vacy dramatically with C001ce parameters, suggesting a direction for further tests and ri-pe opportunity for .alternative theories.

2. Process Violation._

As Ail 's paradox-es continued to provoke debate, a second wave of pSj<cholog• kal eviJeuce began to rise, even mvre deeply critical of EO m; a descriptive theory. In experiment after experiment, subjects appeared to use procedures or process which were much simpler than EU (or even EV), For instance, in one study the value of gambles was better predicted by an addiriw combination of probability and outcomes ("risk dimensions") than by their product (Slovic and Lichtenstein 1968), even when subjects were told the gambles' expected values tLichtenstein, Slovic, and Zink 1969).

In some experiments subjects compared the probabilities of winning in two lotleries, and their outcomes, in a way that led to intransitive cycles (Tversky 1969). Loomes, Starmer, and Sugden (1991) give a recent illustration. In their study, many subjects chose {.6, £8) > (.3, £18) and (I, £4) > (,6, £8-) but also chose (.3, £18) > (1, £4). (These cycles accounted for about 17 percent of the patterns resulting from the three pairwise chokes.) An intuitive explanation is that these subje<:ts chose the gamble with the larger probability as long as !he payoffs were close, but chose (.3, £18) > (1, £4) because the payoff £.18 is Mifficl1mtly greater than £4.J In some experiments on multiattribute choice under certainty (finding an apartment), subjects were not told the attribute values for each choice, like the rent on apartmenl 1 or the size of aparttnent 3, unless they asked ю see those vables. {Forcing subjects to ask for information is a prilllltive way to measure their information search and draw mferern::es about their thinking pt'IXCSSes.) Utility•maximizing subjects should ask to see all the information but tnOSt subjects did not (Payne 1976}. Im,tead, 1'.ubjects often chose a single attribute, such as rent, then eliminated an apamnents with rent above some threshold and never asked to see the other attribute values for those eliminated apartments.>'

3. -- Throry

SweepjPg evidence and an alternative ""prospect tbcoey" was offered by Kahne• man and Tvenky (1979). They replicated Allais's common ratio paradox and introduced others. Prospect theocy has four imponanr elements: an editing stage

in which rul.es either dictate choices or transform gambles before !hey are ev.aluatixl; choice of a reference point {from which gains and losses are measured): a nskleS" valoe fum-i:i;in ;;ivcr gains and losses; and a funclion that weights probabilit.es oonlinearl:;- and applies the rern1ting "decision weights" lo outoomes. to evaluate grunbles.. Each clement in the theory is derived from experimental evidence.

The idea that people value changes from a reference point, rather than wealth posid<irtS, i an old one \e.g., Markowitz 1952), It extends to !he financial domain the w1despread evidence that in makfog psycbophyskal judgments, hk.e bri_ght•ness and heat, people are more sensitive to changes from adapted levels than to absolute leveh (Helson I964J.'l1i As many people have note.ct, there is no axiom in EU implying that wealth _positions are valued rather than changes in we!Uth, but it follows from the integratmn of assets and could be viewed as a basi.:: principle of rational choice {like "description-invariance" and some other principles described later). Furthermore, it ls easy to construct examples in which an EU maximizer will make consecuuve choices wluch are suboptimal (compared to simultaneous chokes} if she values changes rather lhan wealth positions (e.g., Tversky and Kah:neman 1986, S255-256).

Kahneman and Tversky presented new data sugges-ting the val.ue function has two itnportanl properties: (Ii it i steeper around the reference point for losses than for gains ("loss-avert.ion")[19]; and (2) risk attitudes "reflect" around the reference ps,inl--lhe value function is concave for gains (risk averse) and convex for losses (risk seeking). Fishburn and Kochenberger (1979) also reviewed published studies showing reflection. Like the existence of a reference point, reflection can be interpreted 88 a psychophysical phenomenon, diminishing marginal sensitivity (marginal gain.-. feel less and less good, marginal losses feel less and less bad).

The decision weight function in prospect theory is akin to earlier measurements of subjective weights- of probabilities, by Edwards, Preston, Baratta, Mosteller, and Nogec, et al. The data suggest low probabilities are overweighted and high probabilities are underwe-ighted, as shown in Figure 8.6. with a cro;;sover point roughly between .1 ;md .3, Underweighting of high probabilities implies a "certainty effect," in which special wcigh1 ls given to certain outcomes compared to slightly uncertain ones (i.e., the decision weight function is convex and steep near ooe).

4. Elicitation Biases

Many researchers discovered systematic bia$Cs Jo elicitation of utility functions. In the chained certainty-equivalence teehnique, a value of p is chosen and people are asked for X'. such that X' ~pl1+\l - p}L(HandLare high and low amounts), x:" such that K' ~ p H + (1 - p)X', etc. Karmatkat 0974} and McCord and de Neufvllk- !1983) found that using higher values of p in a chained procedure yielded more concave utlhty functions. Hershey and Schoemaker (1980) found that ,-ubstantiaily more :mhject. preferred a l0Sof10to agamble(.01, -$1,000) when I! was cailed an insurance premium than when it was uol®eled. {Lypny f199t] corroborated this finding in an interesting experimental study ofne<lging-) Hershey and S.::hoemaker (1985) also found that utility functions elicited using probabmt)' and certainty equivalents were systematically different !cf. Johnson and Schkade 1989), violating the presumption that utility is invariant to the pt-Oee-dure used to elicit it (see section UIJ later), Hershey, Kunreuthcc, and Schoemaker (1982) i,,ummarited many of these elicitation biases and others.

Wolf and Pohlman (1983) elidtcd parameters of a specific kind of utility function from a Treasury bin dealer, by di<:'iting cenainty equivalents for several hypothetical gamb over hlll wealth. lhen they timated the same parameters: using the dealer's actual bids (combined wlrh the dealer's forecasts of the resale price of the bids}, The utility functions: derived in these two ways were similar in fomI (decreasing abwlute. roughly constant proportional risk ave.rsion) but the degree of risk aversion evldem in btds <was much larger than in the hypothetical choice&. (His bids would have been four times as large as Lhey actually were if he had bid according to the utility function derived from hypo!het!cal choice.5.) The study is. oot condruive evidence of a hypothetica.l: real difference. because that difference was confounded with the method by which utility was elicited {certainty•equivaleuts s. actual bids). However, the differeo.ce 51.1ggests caution in extrapolating from a utility function measured one way. to its application in another domain.

D. *Generalizations of Expected Utility and Recent Tests*

By the mid- !970 or iiO, ooveral developmoots !\ad convinced many hers that it was time to take altcmru:tves to EU s.eriou.sly. lmportanl milestones were grudging acceptance of the power of Allai.s's exarnplei.: IJle ubiquity of EU violations in choices, process data, and elicitation procedures: the elegaoce of Katmeman and Tversky·s batch of new paradoxes: and Machin.a's (1982) assimihltion of some of the empirical eviOOnce against EU and imrodoction of sophistk.JJed tools for doing economic theory without the independence axiom:'[0]

The anomalies motivated theorists to propose genetalitations of expected utility in which axioms are weakened or replaced. Most of the theories weaken in - pendence, but theorists have explored generalizations of other axioms too.4'

Several recent empirical studies test these generaliz.ations of Et::. Tv.ill first describe some of the theories, then describe tesl,;. of various theories in some delall (see also Camerer 1992a). Table 8.1 Mimmarizes several theories and their predictions about the shape of indifference curves in the Marschak-Maehina triangle diagram,

Before reviewing !he various theories, a note about the modern influeoce of Allais and his European colleagues is appropriate (see Hagen l9'Jl). Allais (1979) himself felt there were two main sources of EU violations: a certainty effect, and the fact !hat EU expressed aversion to risk only indirectly, through curvature of the utility function. He proposed a "neo-Bemoullian" model that presumes a car• dinal utility function of outcomes. obeys stochastic dominance, and assumes people choose gambles according to both the expectation and the variance of the

gamble's utilitiQ. (Higher expectation, and lower variance, are preferred,) Hagen (1%9) proposed a similar model in which positive skewness of utility is preferred as well.

These contributions got rela1ively linle attention in the United States and England, for both sociological and scientific reasons, Their articles are bluntJv ,,;riti• cal of EU {and of some other alternative theories); I suspect many Arirn11 readers are pl!t oft by the critical tone. Most of the work is published in book -chapters or in journals like Theory rul Decisicm and Journal of &·onomic Psy• chology, which are more widely read in Europe than ill the United States. Mo<it unponam!y, !he Allais and Hagen formulations have free functions thal seem to be especially difficult to measure and test. FOTexample, one cannm easily concoct a paradox like Allais's to telit Allais's own theory, by using pairwise chokes that hold c00-stant the influence of statiBtiral moments of tlu: utiliry of gambles, without knowing the under!y1ng utility function. (And the utility function cannot be easily measured using eertainty-equlvalents. as in EU, because choices are u • sumed to depend on expectation and on higher moments.)

I. Predictions of Generalized EV Theories

Wt>ighled wility theory (Chew and MacCrimmon 1979a; Chew 1983) aSSl.lmes a weakened form of independence. As above, denote gambles by capital letters (X, Y, Z), compound gambles. by pX + (I - p)Z, and preference for X over Y by X > . Their axiom is:

> Weak Independence. If X > Y, then for all p in (0, 1) there exists a unique q in (0, l) such that pX + 0 - p;Z > qY + (I - q'il for all Z.

Weak independence in combination with the other EU iuioms implies a representation of the form

$$U(1_{P_i} x_i) = \frac{1P_i w(x M t')}{!p;>v(x,J} \tag{3}$$

In weighted utility, indifference curves are straight lines that meet at a point out- ,-J the triangle. (If w{XM) < l, for instance, in the dorru\in of three outcome gambles where X_M denotes the middle outcome, then the curves will look like those in Figure 8.8.}

There is a generalization of weighted utility called "'skew-symmetru: bilinear" (SSB! utility theory (Fishburn 1982, 1983, 1988}. In SSB utility. preferences are represented by a function of both lotteries, (X. Y). X > Y if and only if ¢/(X, Y} > O, 4,\X, Π = 0 implies X ~ Y.

SSB re.wlu fr1,)t11 replacing EU with a weakened fmm of independence calle<i ··symmetry." adding a betweenness axiom {sec below), retaining ,;umple!.mess and oontimmy, and abandoning transitivity, When transitivity ls added back in, surprisingly, SSB reduces to weighted utility,

Regret lh£ary (Bell l9S2; Loomes and Sugden 1982, J987a) gencraiu:es SSB further by exiending it lo droi_:e,;; between lotteries with oorrelated ootcofi;es

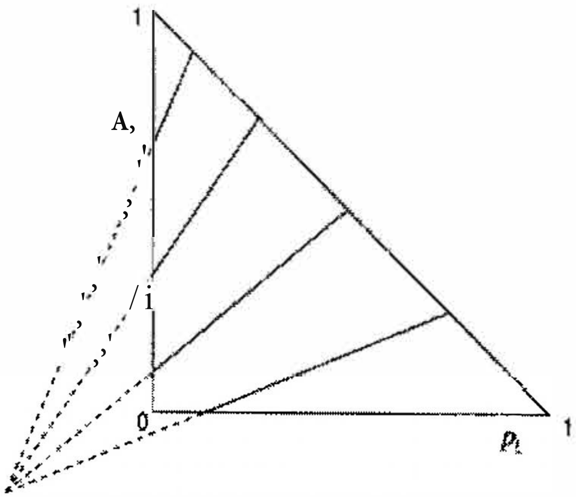


figure 1!.8. h:tdiffeiwt:e curve& that *fan* OHL

(i.e., where the outcome from one lottery depends statistically on what the other lottery's outcome was). Some tests are described in „ection HI H.2 below,

lmplici1 EU. A weakened form of SSB or weighted utility, called "lmplicil weighted utility" hy Chew (1989), or "implicit E1f" by Dekel (19S6), depends oo a weakened form of independence called "betweenne;;;;."

Betweennes$.. If $X > Y$, then $X > pX + (1 - p,r > Y$ for all p in {O, l) (orX~ YimpliesX- $pX+(l$ --pJY ~ YforaJlp in{O. ll).

Betweenness implies. neutrality Inward randomization among equally-good outcomes. It yields an implicit utility -representation of the form

(4) $$U^{*} = U(\Sigma\, p_i x_i) = \Sigma\, p_i u(x_i, U^{*})$$

The utility function u\x;, *(1'!* denotes !he uliHly of an outrome x„ but the util ity ftmction used to value x depends on $I/$, (Utility is therefore defined implicitly: U' is an expected utility that is calculated using a utility function that depends on i/,} In implicit utiiity, indifference curves are straight lines (which arc posittvely-:s.Joped, and don't cross over), but they *are* not nocessarily parallel as in EU.

Betweenness can be violated in either of two ways. ff preferences are slrictly "quasi.convex," then X -- Y implies X pX + it - p}Y (people are averse [o rru:u:iomiuuion}. If preferences are strictly "quasi-concave," then X ~ f imp-lies pX + (I - p}Y > X {people prefer randomization)_ Jn the triangle diagram, quasi-corrvex. (-concave} preferern:es imply concave (convex) indifference curves.

Chew, Epstein, and &!gal {1991} propose a weakened fonn of betweenness, called "mixture symmetry": If X ~ Y then pX + (I - $p)Y$ -- (1 - $p)X + pY$, Together wilh other a:tioms, mixture symmetry imphes that preferences switch from quasi-convex to quasi-concave, or vke ver-;„a, as gambles impmve (in the

set1se of stochastic dominance). (Curves then switch from concave to convex. or vice versa, as one moves northwest in the rrianglc diagram.)

In weighted utility theory, indifference curves moy "fan out," getting steeper as one moves from the lower right-hand com.er {or southeast) tQ !he upper left-hand corner for northwest), as in Figure 8,K (Ibey can also "fan :in,'' getting less steep to the northwest.) Stttpe.r indifference curves correspond to more risk a\l'erse behavior: the steeper the curve. the more P_u a per-son demands in order to accept a unit increase in PL Therefore, fanning out occurs if people are more risk averse toward gambles. that are better {in the sense of stochastic dominance).•:: Machina {1982) showed that several empirical anomalies could be explained by the *fan• ning our* hypothesis {without requiring the further assumptions of weighted utility lheo.-y),

GuJ {199n proposes a theory inoorporating *disappointment.* The intuition is that the probabilities of outootnes below and above a ga.mble's certainty-equiva-lelll are weighted differently to reflect the additional satisfaction from having "bearen the odds," or unhappiness from having lost to them, His theo$_{ry}$ is a special case of implicit EU that satisfies bctweeruiess and allOW3 curves to fan in fOf better gambles i in the northwest part of the t:riangle) and fan out for worse gambles (in the southew;t).

Jn *expeatd utility wdi'l ronk-dependent probability weights* cumutarive proba• bilities arc weighted, and the utilities. Qf outcomes am weighted by the differential in the weigh!Cd cumulative probability (Quiggin 1982; Segal 1987b, 1989; Yaari 1987; Green and JuUien 1988). {This _procedure ensun:s that stochutic dominance is never violated, which can happen if probabilities are weighted sepa rately.) T u weighl *of* an outcome depends on its probability and its rank order in the set of possible outcomes. Suppoae we rank outconu.:i from high to low, x_1 '> x_l > . , , > x,. Then the functional form for rank-dependent utility is

(5) $$U(\pounds p,x_i) = \underline{i}u\{x\text{-}f\ [g(p. + p_i + , .. p,) - g(p, + P, + \cdots P_{;-1})]$$

Note rhat if g{p),.,. p, the brack.ered expression reduces to P_i and equation (5) reduces to E11 [n rank-dependent lheory, indifference curves will not be straight lines unless: the probability transfonnation function $gf.p) = p$; otherwise.. they are curved in a way whkb depends on g(p) (Roell 191$7; Camcrer 1989a}. *A* convex g(p) expresses rime avemon in a novel way, by underwe.ighting lhe probabilities of the highesN·anked outcomes and (since weights sum to one) overweighting the lowest-ranktd outcomes; similarly, concave $g(p)$ expresses risk preference.

Lottery-deptmdent utility theory (Becker and &ltin 1987) a:>s-umes only SIO cha!ltic dominance, ordering, and continuity, The theory is quire general:

(6} $$U(p,x_i) = \Sigma\, p_i u(x_i, c_F)$$

whe!'C c_F =-!- $h(x_i)p_i$. B«:ker and Sarin (1987} suggest an exponential fonn for the utility function $u(x_i, c_p)$ which makes the theory moreprcdse and useful. Irnliffer-ence curves fan out in the exponential form and lottery...::lependent preferences are quasi-eonvex (vurves are concave) if h(x) *is* i;:oncave (Camerer 1989a, 73), In prospect theory (}(ahneman and Tversky 1979), indifference curves may vary wllh the choice of reference point, but for testing purposes it is usef\1l to consider a variant of prospect theory which assumes that the reference poi n t is cuttent wealth. Th:en the "1ilue of a gamble is simply

(7) V/(p ,.t: + P,?) = 'ITTp,Mx,) + 71{p,)Il{x,!

In general. p, + P, s; 1 (and 1 - p, - P, is the probability of gettmg oothlng). If p' + p, "' 1 and both x, have the same sign, then the v;i,lue is

(7') V(p X +Pi<) = 11(x1) + 'IT(p)(">'(.t) - v(:i)) = (1 - 'I'f(p,)}Vlx,} + 'I'f{p)v tx)

The shape of the indiffereoce curves derermioo:l by equations (7) ,md (7') depends on 'I'f(p}, but they will certainly be noolinear unless ,r(p)"" p. If 1rtp) ls most nonlinear near O and 1 (as originally proixised by Kahneman and TverskyJ the curves will change slope and shape dramatically at the edge-s. As a rewlt, dtoices between gambles inside the triangle will violate EU less than chokes involving gambles on the edges.

I'rospect theory was original!} restricted to gambles with one or two nonzero outcomes. In "cumulative prospect theory," Tversky and Kahneman (1992) extend the theory to gambles with many outcomes {including continuous distribu" tions) using a rank dependent form like equation (5) (cf. Starmer and Sugden 1989a, 99-100). Toe difference is that, in their formulation, probabilities of gains and losses can be weighted differently. They also suggest a parsimonious, one parameter probability weighting function[43]:

(8) g(p) = p'"flp'Y + (1 – p)'Y)11γ

Other Theorie$

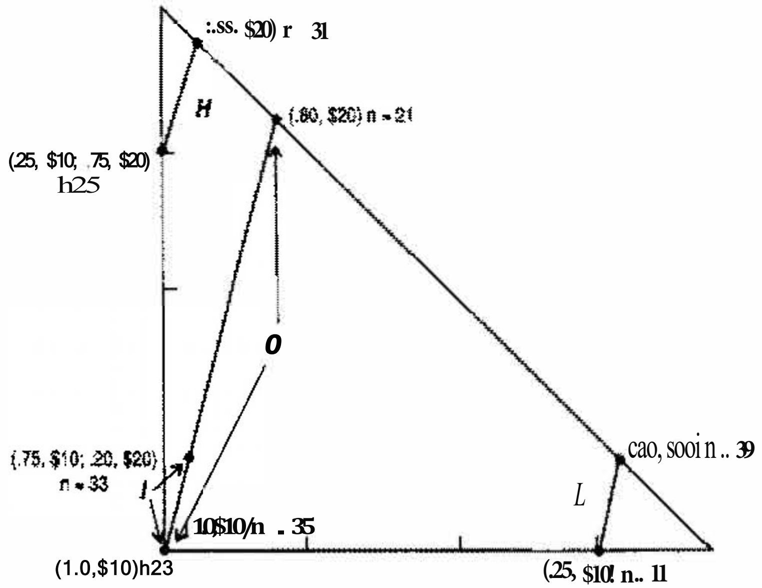
Handa (1977) proposed a precursor to prospect theory in which probabilities were weighted oonlinearly (see, much earlier, Edwards 1954<:). Kannarkar (1978) and Viscusi (1989) -propose specific functional forms for probability weights 'IT(p). Leland (1991) suggests an "ap proximate" EU in which people lump ootcomes (and possibly probabilities) into di categories, making their utility functions discontinuous (cf. Rubinstein 198'8). One special feature of this a_pproach is that it allows convergence to EU with experience, since experi ce permits finer grained categorit.ation of outcomes (cf. Friedman 1989).

2. F.ropirical Studies Using Pairwise Choices

Thete are many recent studies UBing pairs of choices to test EU and its generali.za tioos. Each of the theories descnoed above pre.diets different indifference curve shapes in some pan. of the triangle. 'The predictions are summaril.ed Jn Table S J. By choosing pairs carefuUy from throughout !he tri.mgle, each theory can be rested againSI the others. The tint srudy of this kind was done by (..'hew and Waller (1986). 1 wm describe their design because it is typical and raises basic methodological questioos.

Table 8.1. Predictions of Competing Theories about Properties of Indifference Curves

	Functional Form for U*			Properties of Curves			
Theory	Continuous, $U^*(F(x))$	Discrete, $U^*(\Sigma p_i x_i)$	Restrictions	Straight Lines?	Fanning Out?	Fanning In?	Miscellaneous
Expected utility	$\int u(x)dF(x)$	$\Sigma p_i u(x_i)$		Yes	No	No	Curves parallel
Weighted utility	$\frac{\int u(x)w(x)dF(x)}{\int w(x)dF(x)}$	$\frac{\Sigma p_i w(x_i)u(x_i)}{\Sigma p_i w(x_i)}$	$w(X_m) < 1$ $w(X_m) > 1$	Yes Yes	Yes No	No Yes	Curves meet in a point
Implicit expected utility	$\int u(x,U^*)dF(x)$	$\Sigma p_i u(x_i,U^*)$		Yes	Maybe	Maybe	Only testable property is betweenness
Fanning-out hypothesis	$\frac{-U''(x;F)}{U'(x;F)} \geq \frac{-U''(x;G)}{U'(x;G)}$ if $F(x) \leq G(x)$ for all x			Maybe	Yes	No	Movements to northwest cause steeper slopes
Lottery-dependent utility	$\int u(x,c_F)dF(x)$ $c_F = \int h(x)dF(x)$	$\Sigma p_i u(x_i,c_F)$ $c_F = \Sigma h(x_i)p_i$	h concave h convex	No No	Yes Yes	No No	Curves concave Curves convex
Prospect		$\pi(p_x)v(x) + \pi(p_y)v(y)$ $p_x + p_y < 1$ or $x < 0 < y$ $(1 - \pi(p_y))v(x) + \pi(p_y)v(y)$ $p_x + p_y = 1$ and $0 < x < y$ or $y < x < 0$		No	Lower edge	Left edge, hypotenuse	Parallel along $P_H = (1 - P_L)/2$
Rank-dependent utility	$\int u(x)d[g(F(x))]$	$\sum_{i=1}^{n} u(x_i)[g(\sum_{j=1}^{i} p_j) - g(\sum_{j=1}^{i-1} p_j)]$	g concave g convex	No No	Lower edge Left edge	Left edge Lower edge	Parallel along hypotenuse



Figwe 8.9. Chew and Waller's HILO structure and results. 0, J, I and H denotll the four gamble paln. u;;;I 'II their elpeniment. The number of subjects (total 56) choosing each gamble in eah pair is Mown in parentheses. Dollar amounts are in thousands. Sowrc:- Chew 1lDdWillier 1986.

Chew and Waller used an ingenious, compact set of choicCll (originally developed by Chew and MacCrimmon 1979b) called the "'illLO structure:• Figure 8.9 shows one set of four *HILO* pairs they used {their context 2b), drawn in a rrianglc diagram. The three outcomes in the gambles, in thousands, are X_n = $20, X_u = $10, and X_L = 0. Every subject chose one gamble from each of the four pairs, four choicu in all

The pairs were picked to efficiently deteet ..everal different effects. with only four choices: Ule common ratio effe.:t (pairs *0-L* and *0-H),* ib€ common conse• quence effect (l-L and 1-lf). and a teSl of the betweenness axiom (/-01. Figure 8.9' ghflws the Illlmber of subjeru. {out of fifty sixl who chose the gambles in each pair, S-ubjects were not allov. to e;,;press indifference and didn"t play any gambles.

The ComtlIQU ratio effect occurred because thirty-fiv-e subjects dt06e risk-aversely in the O pair (lhiny-five picked \l.0,$10} and twenty-one picked t80.$20}} but only seventeen did so in the L pair. A weak common consequence effect occurs because twenty-three subjects chose risk avenely 'in the/ pair and venteen did in the L pair.

'The faruring ont hypothesis predicti; that people bet.ome more risk averse, and indifference curves fan out, with movement toward the northwest comet, 'The prediction appears false in the Chew and Waller data: fewer subjects chose risk

.IVURel)' in the *R* pair (twenty-five} than in the *O* pair (thirty-five),[44] and about tbe SMI¢ number chMft rii,k aversely in the/ pair (twenty-three).

Pairs land O rest whether the betw n s axiom holds (since the inner gamble {.75.$10: .20,$20) in pair / is aprobabili.ty mixture of the outer gambles (LO,SlO) and (,80,$20).) Betweenness is violated because 35 subjects chose (1.0,$10) in the 0 pair but only 23 chose it in the l pair.

A MMhfXWWgica/ T>igresmm:
Bn>Altr1l• vs. Wiimn• cts Arwlysis

P hologisll> call the sort of analysis expresiu,d by Figure 8.9 "between sub-jecr:S·..': the fractions of subjects behaving in a particular way in two different settings are compared (but the subjects may be different in the two settings). In a "within subjects" analysis, lt!e fact that a single subject made choices in two or more tting exploited, Within-subjects tests are always more statistically powerful, but they run a certain risk: presenting several stimuli to a single subject could conceivably induce her to behave more consisteQtly than she would if she saw the stimuli one at a time. For example, in tests of whether two different descriptions of an equivalent problem elicit the same choices ("framing effects"), presenting 1he two problems one after the other might cause a subject who recog-nizes the equivalence to guess that the experiment tests for consistency, and respond w;cordingly. (The same issue arises in some of the judgment research reported io section U above.)

Many peopk think within-subjects analysii; is the only proper analysis in chc,ice experimen1s, bet'ause BU requires consistency of individual preferences. But, of course, belween subjects tests are equally legitimate (though less pow-erful) if the subjects in different groups can be presumed to have the same distri-bution of ta tes, up to sampling error, because they were drawII from a single population,

Chew and Waller (1986) did a within-subjects analysis by counting how many sub-jeers exhibited a twticular pattern across choices. For instance, in the O and L pairs (the common ratio test) twelve su e(:!S chose tbe Jess risky gamble in both pairs and sixteen subjects chose the more risky gamble in both p m , so twenty-eight of the fifty-six. subjects satisfied m;, Of the twenty-eight who chose incon-, s..istently, twenty-three (82 percent) chose risk a.vmely in O and risl. preferringly in L, manifesting che standard common-ratio pattern..

Ahofiter Methodningkai Ofgrenion:
Judging Vialmion Rala

ls a 50 percent rate of EU violation (twenty•eight of fifty-six chose incons.istently) large or small? If we allow random error in expression of preferences. then tbe appropriate benchmark fot Vlolation rates shoold be the fraction of people who swttch their choices when making ihe same c!wice twice (called "reliability" in psy<:homctrics).«. Cbew and Waller did not measure the fraction of random swirehing hut other studies sugge.'.l that percentage is about 25 to 35 pen:ent for

.-hokes like these tStarmer and Sugden 1987b: Camerer 1989a). Using that ndlmark. a „;-test shows that the fraction of inconsistent subjecls in the 0-L pairs (28/56i is much too high to be a chance deviation from random switching. Another way w te$t whether violations are systematic is to compare the fraction of inconsistenci s in both directions. In the *0-L* pairs. twenty-tllree of twenty-eight swHched in one direction and five in another, an asymmetry unlikely to oct:ur by chance.

Of course, the violation rate i likely io be sensitive to the gamble pair coo:.en. 1Wo f.imilar gambles will have a violation rate close lo 50 percent. That simply tn¢MS that if your goal is to reject theories. using i.uch a pair of gambles will require a large sample to detect true systematic violation statistically.

A Thinl Merhodoiogfca! O/gr,rssien: lncetW l's

In Chew and Walle(s study, choices. were entirely h:ypothetic.d. In most other studies sub;ł ets played one of the gambles. they chose.47 The common procedure of randomly choosing one of several gambles tJ play has been tested by Camerer (1989a, 82) and Starmer and Sugden {1991a): it elicits roughly the same pre!er-i:mces as when subjects. make ooly one choice i and play the gamble they pkked}.

Several smdies have compared hYJ!')thetical .:boices with real choices (in which one choice was played). They found either no effect or a $.light tendency for playing gambles to yield more rlsk aversion {.Edv„;mJs 1953; Becker, DeGroot, and Marschak 1963; Camerer 1989a; Battalio, Kagel, and Jiranyakul 1990; Ho-garth and Emhont 1990: Schoemaker 1990; cf. Slovie 1969).

Harrison ti990) reported the only evidence that actually playing a gamble sub-stanliaUy reduced the rate of EL' violatto (BatmlJo et at [1990] and Camerer [19S9aJ repon no such effectsj. ln his common-ratio e,;periment, seven of twenty subjects violuted EU when choices. were hyp<,thetical and three of IWeOty {differ-ent) su jecis v1olated EC when choices were real. The difference is not significant at conventional levels (p = .15).

In a contrasting study, KAchelmeier and Shehata 0992) elidted certainty-equivalents from Chinese students (using the BDM procedme), fot gambles with high or low outcomes. Subjects played every gamble, and payoffs rnngt;d from 1 !o lfJ yuan, which are sutmantiai amounts for students. who spend about 60 yuan per month. Certainly-lXjtllvalents exhibited dramatic overweighting of low proba-bilities ut both high and low payoff levels (e.g., the certainty equivalent of *(p,* X) was about twice pX fot p = .05 or. !OJ; aiffillar patterns were observed with Cana-dian sutdents playing hypothetical gambles. Risk aversion amOQg the Chinese ;.tuderu;. was also greater for high payoffs than for low payoffs. Thus., the over-wetghllng of fow probability observed in so many experiments (which leads to many EU viola!lom,) is present when very large payoffs are used, but large pay" offs also induce some differences in risk averm:m.

In experimems where gambles are played, the amount of expected value (EV) subjectt; lme by violati ng EU ts usually small. Unfortnflately, there h oo simple way to design experiments that have large e:;tpeeted value penalties for violating

EC but that can also sharply distinguish EU from eompeting tboocies {mduding EV). Since the effect of dominant payments is difficult to determine empirica!ly, ıı pragmatic way to approach the problem is to spell out and test whatever theory or intuition underlks the claim that higher payoffs would reduce EU violations {s.uch as Smith and Walker's [1993] analysis of "decision cost"). The intuition seems to be !hat subjects. will calculate more, thinl; harder, or oomebow see the appeal of axioms when they are faced with latger stakes, But in experiments where subjectS< were !old expected values (Lrohtenstein, S!OVIC. and Zink 1969J or given written arguments explaining the independence axi ng it easy for them to think barder----EU violations were not reduced {Slovi.<: and Tversky 1974). Indeed. some studies suggest ttw the main effect of paying subjects is a reduction in variance of !heir responses, which increases the statistical signifi-canc:e of EU violations (Harless and Camerer 1994),

The effect of paying subjects is likely to depend on the w k they perform. ln many domains, paid subjects prob.ibly ® exert extra mental effort, wltich im-proves Lheir perfx:umance, but in my view choice over money garubles is not likely to be a domain in which effort will .improve adherence to :ratiooal axioml>< Subjects with well-formed preferences are likely ro express them t:ruthfulty, whether they are paid or not. lf !heir preferences are not well formed. it seems unJikely W t subjects would be both sophisticated and Jazy enough to make au expected utility calculation when they are pitld, but not when choices are hypo-theticaL (Furthermore, if payment does induce more formal reasotlJng, it is likely to be expected value maximization. not EU maximization,}

Other Srudies of Common C;;„uequr:ru:£ and Comnwn Ratio Effects

Chew and Waller's data replicate the common ratio .and common consequence effects in the southeast comet. but !hey show tittle fanning out in the northwest comer. Camerer (1989a} found the same general pattetn. ln ıı replication using tlte Allais payoffs, Conlisk (1989) found fanning in in the northwest corner and Pre-lee (1990) found dramatic fanning in (55 percent of subjects) close to the fowel' edge and southeast comer. {Their llul!jects did not play gambles.)

Starmer and Sugden (1989a,1989b) $tlldied a wide variety of comm<m ratio problems. The rate of EU violations was only significant in three of fifteen pairs. Thœy found strong fa.Ming out along the lower edge, and some fanning in along the left edge. Starmer (in press) found common consequence effects, showing fanning *jn* rather than the more typical fanning out, especially along the lower edge.

Battalio, Kagel, and Jirnnyakul {1990) observed fanning in along rhe lower edge, in common ratio problems where gambles had strictly positive payoffs.. Fanning in also appeared in the northwest comer with gains and the southeast comer with losses.

The pa1tems in these studie,; are complicated. Most of lhe differences in remlts can probably be traced to small differences in the particular gamble£ being studied. It would be useful to have a composite picture of the indifference <UI'ICS revealed by all the studies, butliObody knows how to create such a compositir (but see Harless and Cainera 1994, discussedbelow). ltdoesseem !hal any composite picture is likely ro cast doubt on the generality of fanning ouc For .!xample, the fanning out that is observed in the Allais problems aloog the tower edge of the triangle is generally reduced or reversed-sometimet1 fanning in oce ong the left edge. The fanning out hypothesis was sugge«ed by Macliina (1982) to explain evidence that had come primarily from the lower right rorner of the triangle {e.g., the Allais paradoKes illustrated in Figure 8.7), His inference from t00$e data 10 the entire triangie was ingenious, but apparently no! quite right

Two classes of theories can account for most of the mixed fanning evidence from common ratio and common consequence studies. One class of theories posits mixtures of fanning in and tanning out (Neil soŋ in press) or derives them from axioms (Gui [1991J, which adds only one parameter to EC), Another class weights probabilities nonlinearly, which generates indifference curves that fan out or in depending on the weighting function.

Evidence of /J,m,eel1rles Violation

Camerer and Ho (1994) reviewed nine studies in which the betweenness axiom was tested. (Tests of betweenness efficiently test several theories that as ume if.•• weighted and implicit EU, disappointment, SSB, and regret-in one fell swoop.) In these studies, betweenness is frequently violated bul the pauem of violation is complicated. Gambles with gains generally show quasi-convexity (concave indif• ferencl;'. curves) except close to the lower and left edges (CL Bernasconi 1994}; loss gambles show the opposite.. Toe fact thal cmvature: reflects fot gains and los implies people seem to weight gain and lQSS probabilities differently {See Tversky and Kahneman 1992}. Evans (1992) found comparable degrees of betweenness violation when gamble valuations were elicited with the BDM procedure. and with second- and fifth-price sealed-bid auctions (see chapter 7J,

We fitted data kom all the studies to a variant of EU with nonline:ar rank· dependent probability weigh(s (using the weighting function (8)). The maximum-likelihood coefficient estimates 'h� around y = .60, which implies·a nonlinear 1,veighting roughly like that pictured in Figure 8.6, with probabilities. under .30 overweighted and probabilities above .30 underweighled. The data therefore reject betweenness (which requires -y = 1) and corroborate both guess from the earliest studies from the 1940s and sharper estimates from studies al:mo.;;. fifty yearn later (e.g., Tversky and Kahneman [1992] estimate -y = ,61 for gains and 'I = ,69 for lOlISCS). But the very latest evidence (Wu 1994) shows some violauona of Ute raok-<Jependent approach which should renew interest in prech.oice edit· i:ng" rules.

The mm!. promineru theory that can account for !he mixed fanning rcponed in the last section, Gui's {1991} disappointment theory, assumes betweenness. The fact that betweenness is often violated casts doubt on Gul's theory and !eaves the nonlinear-wcighting theories as the best available account of both comvlex fan· ffing out patterns and betweenness violation.

Evideri« of Better Co,iformity to EU huidf the Tri£mgfe

One of the most interesting and robust effects in the new wave of rests is that EV violations are much smaller (though still statistically significant) when subjects choose between gambles that all lie inside the triangle. Conlisk (1989), Gigliotti and Sopher {1990}, Camerer (1992b), and Harl= (1992b) all discovered this phenomenon independently,

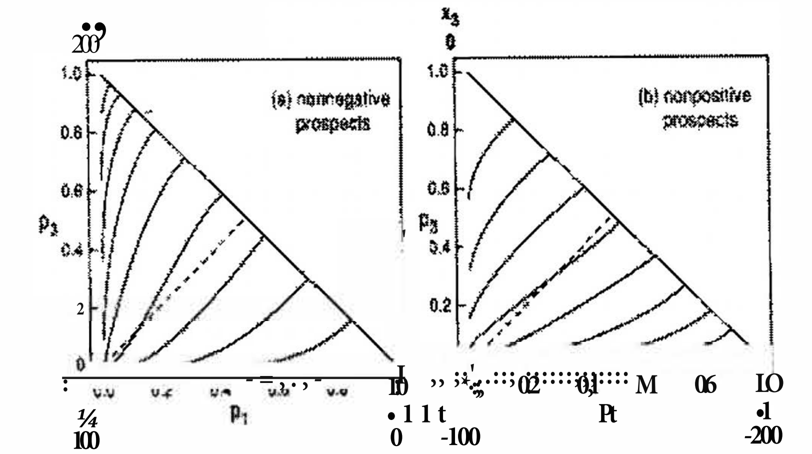
The shrink.age of EU violations imiide the triangle does not vindicate EU. Inside ,gambles have pro bilities P,, P,, and p_H which are all nonzero; edge gambles have at least one of l:1'e three probabilities egual to zero. The fact that violalions disappear when moving from the edge of the triangle inside suggests they are probably due to nonlinear weighting of probabilities near zero (as the rank-dependent weighting theories and prospect theory predici).[41] Moch as Newtonian mechanics is an adequate approximation ac low velocities, but relativistic mechanisms is accurate at all velocities, the linear weighting of probabilities imposed by EU may be an adequate approximation when outcome probabilities are not too low or high Morgenstern (1979) drew the analogy with mechanics, and anticipated such a conclusion; "(TJhe domain of our axioms on utility theory is also restricwd.. , . For example, the l)I'OOabilities used must be within certain plausible ranges and not go to .01 or even less to .001, then be compared to other equallv tiny n1lmbers s1lch as ,02, etc." (178).

Whether nonlinear weighting of low probabilities affect choices among natural garn les witll man} outoomes is a fundamental empirical question. In any case, theotie. dhat assume nonlinear probability weights are here to stay: they ate the only theones- that can explain evidence of mixed fanning, violation of betweenness, and appn:1X.imate EU maxillltz.ation iQ&lde the triangle.

Fnfermat und Formal $.wmmarif's £!ffaidence

There are at lea1 two way ω summarize aH this evidericc on experimental testing of various. utility theories. Camet:er {1992.a) summarizes the evidence informally in a list of "stylized facts." Many of the facls can be expressed as shapes of lndiffenmce cnrves in the Marschak Mach.ina triangk. Tbt crucial facts appear to be that indifference curv« vary in (local) slope from risk averse to risk seeking; indifference curves fan in and out in a systematic, complex pattern (strict fanning out can he reje.ctcd); betweeru:ss violations imply that indifference curves are not straight; indifference curves ere more n ly parallel inside the triangle than on the interi and curves fur gains and losses reffect around a 45 degree line.

The right kind of oonlinear probability weigl;ting furu:ti<;n can have an these properties. Figure 8,10 shows curves plotted by Tve.rsky and K.ahneman {1992), for gambles over gaiM and losses, for cumulative prospect theory using their one parameter weighting function. The dotted line shows pairs with equal expected value. The plotted indifference curves have all the properties melltioned in the prevroJJS paragraph: They are soe times flatter (more risk preferring) and sometimes steeper {more risk averse): they both fan in and om; they are not straight; they are moo curved near the triangle boundaries; and they reflect from gains (panel a of Figure 8JO) to lD&ses (panel b),



figun> 8 IQ Empirically derived indi i:l.ll'"IU of cumur..ii-¾ prospect lheory for f/UII O'\, (a) gains and (b) ! - . Sm,1r,;es: Tn,n;ky lltd Kalm,llliill 19YL

A S1atis1ical Summary

Harless and Camercr (1994) summarize the evidence more fonnally. lu studiell-v,ith ueveral pairwise choices, like thotle discussed above, theories are usually judged by what fraction of the choice patterns they preilictoorre.:tly. For example, in the Chew and Wallet {1986} study people made one choice from each of fo!lt' pairs. There are 2' = 16 possible patterns of choices. EU predict$ only two of thos-e patterns, and excludes fourteen of them. Most statistical tests compare the *pet·* cent.age of patterns that are prerucied-two of sixteen in the EU example. or 12.5 percent-with the percentage of subjects actually choo&ing those pa«gms {Z5 percent in their data} and perform some hypothesis test. .Harless. and I show that this method ignores useful information. If one adds the po.-.sibility that people choose erroneously with some probability, then EC predicts that of the fourteen excluded patterns are more likely than others. Considering all fourteen patterns equally likely mistakes ignores information in !hie relative frequency of the fourteen pauems thal could be used to judge the thcm:y.

We developed a method that uses all the data and generntes a chi squared statistic testing each ,of several al1emarive theories. Since chi-squared statistics can be added across 1ndependem experiments, the results of several studies can be easily aggregated using our method. The result is an "efficient fronner" of theories that are most a<:eurate (best-fining), given the number of patterns they allow (most parsimonlOUS}. A compilation of twenty-tlrree data set$, from a total of 2.000 subjects making 8,000 choices, slHwl1 that the following theories are on *the* efficient frontier: mixed fanning, prospeci theory. EU, and EV.

The difference between boundary gamb1es and imerior gambles emerges strikingly from the compiled dal:a. For interior gambles, then: is a broad range of

parsimony-accuracy rradeoffs for which EU is the best theory. But for boundary gambles, EL is rpreferred. (1f one *is* willing to trade off enormous predictive accuni.cy for parsimony, preferring EU ro a generalization, then one should go even further and choose EV over EU.)

A Field Obserwl!i,;m and E:q,,,riment

Marshall, Rkhard, and Zarkin (1992) studied an int stmg field situation, They begin with a faci: in t983 in the United States, twice as many accidents (per trip) occurred during evening rush hours (4-7 P.M.) than occurred during morning rush hours {7–10 A.M.). The choiee of whether to wear a seat belt is a choice between two risky accident distributions: wearing a seat belt is, roughly speaking, a riskw averse choice: it reduces the probability of having a serious accident but, curl• ously. slightly *raises* the probability of having *some* kind of accident. Since ac.ci dent rates are lower in the morning, the safer morning choice is a better pair of gambles dıan the evening chOKe (in the sense of stochastic dominance}, so *the* fanning out hypothesis predicts that people will behave more riskwa.versely-weMJ\g their seat bclts----in the mmning than *in* the evening. They do: more people wear seat belts in the morning than in the afternoon (15.4 percenJ. vs. 13.9 percent). Marshall et at show that observanons of increased seat belt use in the morning, when accident rates are lower. reje(t the EU hypothesis and support fanning: out.

Their conclusion is debatable because strong. IJ.I'lfealistic assumptions must be made to conduct the statistical. tests and an experiment with students, in which choices and accidenct rates were ptefflllted abstractly, did not corroborate the field observation. (Studoots ovCIWhebn:ingly made the choice corresponding to weal'-ing seat belts in bolh time periods.) Nonetheless, !heir method is a unique illustra-1ioo of bow field data could test alternative choil;e theories.

Cicchetti and Dubin (1994) did a similar study (but with no experimental component): they fit an EU model to consumer decisions to buy insurance against the cost of repairing wires in their phones. The EU model fits reasonably well. They cannot rejecr the hYJ)Othesis that COl1$Umets estimate repair probability accurately, but dle data also show some modem support for nonhnear w;;ighting of probabilities..,,

3. Fmpirical Studie_\ Measuring Indifference Curves

The mosi: direct approach to testing generalized utility theories through their predictions about the shape of indifference curves, *is* by directly aS3eSSill!I' indiffer-ence -i:urves. Hey and Strazzera {1989) estimated curves by eliciting a set of equally good gambles, using a serie8 of "lottery-equivakn t" (McCord and De Neufville 1986) indifference judgments. Some curves crossed (reflecting inu:ansitivity) or had negative slopes (reflectmg stochastic dominance violations). Ltne$ fit to the elicited curves generally obeyed BU (parn11elism coold not be reject«l). but the statistical powe.r to reject EU was limited by the number of curves and the number of points on each curvt.

Abdellaoui and Munier (1994) did a similar curve-fitting exercise. They found evidence of fanning om using common-ratio type choices, and some fanning out along the lower and left edges of the triangle. Curves were roughly parallel in the triangle interior.

Hey and Di Cagno (1990) used choices scattered throughout the triangle, rather than indifference judgments, then fitted probit models to the choices. They strongly reject a special form of regret theory, but accept a more general fonn.

4. Empirical Studies Fitting Functions to Individuals

A few researchers have estimated functional forms or parameter values for individual subjects (Currim and Sarin [1989] for prospect theory, and Currim and Sarin [1990] and Daniels and Keller (19901 for lottery-dependent utility). In a typical experiment, a subject makes a series of choices or states certainty-equivalents for several gambles. To fit EU, for example, a particular form of utility function is assumed, such as u(x) = l - e-cx, and a best-fitting value of c is estimated (by minimizing squared deviations from the stated certainty-equivalents, for example). Alternative theories have more degrees of freedom and require estimation of more parameters. Then the fitted parameter values are used to predict choices in a hold-out sample of fresh choices. The method in Hey and Orme (1994) is similar except gocxlness-of-fit tests are used (EU is tested as a restriction on non-EU theories) instead of malting predictions for a hold-out sample.

The results of these studies are somewhat discouraging for new theories. Alternative theories always fit initial choices better than EU, since they have extra degrees of freedom, but they do equally as poorly as EU predicting new choices, getting only about 60 percent right. (In the Hey and Onne study, the EU restrictions cannot be rejected for about half the subjects; for the other half, prospective reference theory or the one-parameter version of cumulative prospect theory appear to fit best.) These early results serve as a reminder that many subjects obey EU, and the lean functional form in EU is more statistically robust to estimation error than more complex functional forms are (e.g., Carbone and Hey [1994]). Of course, these are initial efforts. Refined techniques and larger samples might work better and enable more precise estimation of non-EU functions and parameters, and better predictions of hold-out sample choices.

Lattimore, Baker, and Witte (1992) also fit variants of EU with nonlinear probability weights to choices by students and prisoners (involving gambles over crimes and sentences). They assumed a power utility function u(x) = x, and a two-parameter probability weighting funclion of the form:

(9) $$n(p;) = \frac{o.pf}{(o.p_r + ip!_{ji} I_J)}$$

The parameter u expresses additivity of weights (weights add to one if α = 1) and 13 expresses the degree of overweighting of certain probabilities.

Lattimore et al. found reflection effects between gain and loss gambles, and nonlinear weighting of probabilitie (roughly similar to Figure 8.6; Camerer and Ho [1994]; and Tversky and Kahneman [1992]). Their daia indicate weighling of losses and gains are slightly different: loss weights are more likely to be subadditive. They did ***not*** fit parameters to one set of choices then predict new choices.

5. Cross-Species Robustness: Experiments with Animals

Besides the many animal experiments studying risk aversion, "consumer" choice, optimal foraging, etc., there are a few experiments testing whether animals obey expected utility. Rats exhibit the common ratio effect like people do (Battalio, Kagel, and MacDonald 1985). (The cross-species replication is especially notable because the rats respond only sluggishly to incentives--e.g., they only choose a stochastically dominant lever 55 to 90 percent of the time.) Rats also fan out and fan in, just as people do, in the northwest and southeast comers of the triangle, when they choose between levers that give "losses" (delays in dispensing focxl) (Kagel, MacDonald, Batialio 1990). Rats also fan in over gains (cups of water) in some parts of the triangle, and violate the betweenness axiom (MacDonald, Kagel, and Battalio 1991), though no attempt has been made to check whether people exhibit those patterns for comparable gambles.

So ***all*** the available evidence to date indicates rats exhibit the same EU violations people do.[51] This cross-species generalizability is profound; it encourages the search for theories that can explain both human and non-human behavior in one fell swoop. My own view (which may be biologically naive), is that rats and humans are more likely to share misperceptions of probabilities than to share feelings of regret and disappointment. So if one prefers a common theory across species, the animal dala shifts a bit more support to theories with nonlinear probability weights.

6. Some Conclusions from Recent Studies

The evidence collected in the last five years is as voluminous as the evidence gathered at any stage of testing EU. There are important lessons in the data.

Common ratio and common consequence effects are easy to replicate, but their strength varies across probabilities and outcomes. Fanning out is systematically violated for several kinds of gambles. Risk attitudes, and weighting of probabilities, appear to reflect around a reference point. There is an interaction between the degree of EU violations and the size and sign of outcomes (cf. Edwards 1954a)--i.e., violations are more frequent when outcomes are larger. Some models can account for the interaction by abandoning separability of outcomes and probability weights (e.g., Hogarth and Einhorn 1990; Becker and Sarin 1987), but the loss in parsimony may be be too high a price to pay for better fit.

Many studies (beginning with Preston and Baratta 1948) are consistent with overweighting of low probabilities (below .2 or so) and underweighting of higher probabilities. Mixed fanning, betweenness violations, disappearance of EU violations inside the triangle, and replication of human results with animal subjects, might all be accounted for by nonlinear weighting. Some other studies, not mentioned above, also found evidence that probabilities between .3 and .8 are underweighted (Cohen, Jaffray, and Said 1985: Cohen Md Jaffray 1988; de Neufville and Delquie 1988).

The few attempts to lit models forindi\'idual wb.tecis suggest that more general theories fit better than EU {since the)' have more degrees of freedom} but arc no better in predi,;ting new choices. More studies of Uris sort are crucial. for estab• lishiog whether the new thoories can actually make bettet' prediction; lhan EU.

Among classes of theories., we can declare a few winners and losers in the empmeal s . Theories that incorporate nonlineatUy in probabi1tty-such as the rank-dependent approaches, particularly, and cumulative prospect the-ory to have the necessary empiricru properties (Figure 8.10) and wili, I think, prove relatively easy to work with formally. Betweennes.,..based theories (including implicit and weighted EU. and disappointment theory) have elegant foonal properties but cannot accumodare apparent nonlinearity in probability adeqwilel;.

Finally, the continued use of EC can only be justified in two fairly narrow w s: first, EU is nm so badly viola.ted in choosing over gambles with the same set of possible outcomes (triangle interior), or with probabilities well above Oand below I-though it is still statisticaJly rejecrable. Sttond, EU might be preferred in an application where :parsimony is very highly valued compared to predictive accuracy (but even then, EV is often just as good)<

7. Investments in Risky Assets

Loomes (1991a) conducted a novel experiment. He gave subjects lotteries over three events, denoted A, B, and O(a zero payoff), which occurred with probabili• ties $p_{,,}$ p_{II}, and 1 - P., - P_w (A wa>. always more likely than 8.) Subjects coold allocate £20 between the events A and B, in any proportion they liked. If an event occurred, they earned the amount of money they allocated to that event. (Subjects played one lottery,} For example, suppme p_A = .6 and P_n "' A A person who ls risk averse miglil allocate A' = £to and B" = £10 {then is certain to earn £10); a risk seeker might allocate A· = £20. creating a .6 chance of winning £20. The tn0ney"1;p}itting w k resembles allocation of wealth in a portfolio Of risky as s; making A' andB• close to£10islik.e buying bonds; making A* closeto£20 is like buying stocks.

An EU maximiz.er will divide the money to solve t'<A')lu'{£20 - ,4') = Pi/P_A. A risk neutral or rlsk preferring petWll will put all £20 in Lhe more likely out• oome, A. A suffidently ri$k avm:e person will put less than £20 in A.

This simple problem provides a remarkably powerful lest of several .altemative choice theories. Under EU, asp" and$p_8$fall, the amount A' should stay the same lf the ratio p_elP_1 is held oonstant. (Regret should not filfect A' either, in the display that was used.) Fanning out predicts the amount A· will rise, and a restrict'ed form ofrank-dependeru EU predicts it willfall.n V.'henp_1 = .6 andp_8 = A subjects put £13,15 in A' on average, Half the subjects chose amounts proportional to tl\e probabilities {A'"' 12, B' = 8). As p_8 and p_A fell, to .2 and .3, a third chose A' = £10, and the average A' fell to£11.58. The drop in A' violates EU and fanning out.

No cocrent theory explain.s- all the observed behavior, Curren!. theorie£. aS£Ume the choices of A· and II are induced from underlying prefereru::es between gambles-i.e., a person chooses K = 12 and B' = &if and only if (P.,,, 12;p_s, 8) > (P_A• A'; P_{IP} 20 - A') for all other values of A'. Maybe sub}Ccts do not lnduee divi&ions from preferences; instead,, !hey regard mone5H>plitting as akin to problem-solving and use a simple heuristic {such as A'/11' = PiP$_1$) th.at generates allocations that are inconsistent with complete pairwise pn:fen:nces.

The money-splitting task vaguely resemble. investment in risky assets. Kroll, Levy. and Rapoport (1988a,l988b} studied asset portfolio problems more di-re.;:tJy. Their experiment&! design operauonali,res portfolio theory (Tobi.o 1958; 1'.iarkowitz 1959}: Subjects can invest in iwo risky stocks with nonnally-distrib-uted returns (one has a higher mean and variance than !he other}, They can also il'TV!$t in risk free bonds or borrow, up to a limit, by issuing bonds. Portfolio theory makes a remarkably counterintuitive prediction, called "portfolio separa-tion"; difference» in risk tastes determine how much is invested in ruktess bonds (or is borrowed_) and how much ls invested in stocks, but everyone will hold the same proportions of the rwo stocks in their stock portfolio, regardless of risk tatites, (For example, an optimal portfolio might place 60 percent of the stock investment in stock l and 40 r:,erumt in stoek 2; those proportions should be the same for all investors, regardless of the overall amount they have invested in stocks instead of bonds.)

Relative to the theoretical prediction, subjects invested too heavily in the high-retum, high-variance stock and did not issue enough bonds. (About a quarter of their portfolio choices were stochastically dominaied by portfoliru. io which more funds are borrowed and invested in the low•retum stock) Since different subjects diose- different stock mix.cures, portfolio separation was badly violated.

Kroll et al. paid wme subjects ten times as much money, to see how behavior would change. Highly paid subjects invested more heavily in the klw-rerurn, low-variance si:ook, bringing their portfolios Closer to the optimal mixture, (They also searched more for information about stock returns in previous periods, even though !hey were told returns were independent each period.) However, my guess is that the iocreased incentive brooglu their portfolios closer to the optimum sim, ply by increas.ing risk aversion and reducing investment ln the high-variance stock, which coincidentally moved allocations toward the optimum predicted by portfolio theory.

A third study by Kroll and U!vy (1992/ varied several features of the earlier studies: MBA students participated in weekly sessions over a semester, competed for grade points in a tournament StnJcture, and coold see the publicly poste4 decislOIIS and performance of others. The students' portfubru; were much closer to those predicted by portfolio theory than in the two earlier studies; however, they did not reallocate portfolIDs in response to changes in between-sroek inter• cocrelatiQfl as sharply as the theory prescribes (but subjects in the earJier studies did not respond at all). Students also tended to rnimick the decisions of high-performing students, and the tournament payment structure appeared to crente a borrowing frenzy at the end of the experiment.

£ *Subjective Expected Utility*

In "subjective expected utility" (SEU), probabilities are not objectively known as they are assumed to be in EU. The events over which people have subjective (or "personal") probabilities are called "states."" Decision makers .choose acts X which yield oonsequences x(.r} thal depend 00 which of several states (s E SJ oci;urs. The SEU axioms show when preferences can be represcn1:ed by subjective expected utility, with utilities u(x(:r)) and (subjective) state probabilities p(s) both derived from preferences., as follows:

{IO) SEU(X) = !. p(s}u{x{s))
,•,

SEU was inspired by Ramsey (1931) but made dear by Savage (1954), who combined tbe von Neumann and Morgenstern (1944) EC approach with de Pinet.ti's (1937} calculus of subjective probabilities. Since probabilities: -in SEU are derived from preferences, rather than assumed (as in EU), SEU applies more widely. See Fishburn (1988, 1989) and Kami and Schmeidler (1990) for technical revie , or Camerer and Weber (1992).

Mosf of the empirical evidence specifically critical of SEO concerns precisely the distinction between whether probability is known or unknown, This basic distinction goes by many names: risk versus uncertainty (Knight 1921); un ambiguous versus ambiguous probability (EJlsberg 1961): precise or sharp versus vague or fuzzy probabilities. In SEU the distinction between known and unknown probability is pointless because subjective probabilities are never unknown-they are always known to decision makers (and inferrablefrom choices). But empirical evidence suggests people do make such a distinction.

1. The Ellsberg Paradox

The first serious challenge to SEU wru; posed by the paradoxes of Ellsberg 0961). Two similar problems were posed in his remarkable paper. Here is one of them:

A decision maker chooses from an um that contains thirty ret!- balls and sixty balls in some combination of black and yellow. There are two pairs of !'Ct&, X and r, and J i and Y" Al-U have wnsequence,; of W or 0, defined ln Table 8.2. For example, the act X pays W if a red ball is drawn: Y pays W if a black ball is drawn.

Many people choose X > Y and f' > X', T h number o.fbl.ack balls that yield a win if act Y ii; chosen is unknown (or amhiglIOUli); people prefer the less ambiguous act X. The same principle. applied ro the second choice, favors Y' beeause exactly sixty halts yield W. (The is true fur losses., W < 0.) In ttns example, people prefer act& with a known probability of winning. That is, they take oon• tidcnce in estimates of Mlbjective probability into account when making choices,

Such a pattern is inconsistent with the sure-thing principle of SEU, Suppose p(r), p(b), and p{y) are the subjective probabilities of drawing a red, black, or yellow ball. Under SEU, X > Y if and only if p(r)u(W) > p(b}u(Wj (selling u(O) = 0), or p(r) > p(_b). Similarly. Y' > X implies p(b v y) > p(r u y), Uweassume probabilities are additive. thenp(b u y} = p(b) + p(y} (sincep(b n y) = 0). Then Y > X lmplies p(_b) > p{r}, whlcb conflicts with the earlier inequality.

Table 8.2. The Elkberg Paradox

Act	30 fulls R«l	60 Balls	
		Black	Yellow
X	w	0	0
y	0	W	0
X'	W	0	w
!"	0	W	w

Source: Ellsberg 1961.

There are several reactions to the paradox, One is to deny it (e.g., de Finetti 1977; Howard 1992)-wbether there are thirty balls, or some number between zero and sixty, shouidn't matter-but dt:nial does not explain the evidence. Another reaclfon i red1.1ctionist (Marschak 1975): even if probability is not sharply known, there may be a sharp •·second order" distribution of probability (SOP), or probability of various probabilities, that restores the usefulness of SEU. For example, in the Ellsberg case a person might not know the number of black balls, but might thlnk that each possible number of balls from zero to sixty is equally likely (i.e., p[k black balls]= 1/61 for O.5k 5 60), But there is no guarantee that a sharp second-order probability e'(ist:s, or that it captures subjects· intuitions about ambiguity. A more constructive reaction is tQ study the paradox empirically.

2. Conceptions of Ambiguity

Defining ambiguity is a popular pastime in decision theory. Bllsberg's (1%1, 657) definition is typical, if messy: amhiguJty is the "quali(y depending on the amount, type, reliability, and 'unanimity' of infomlarioo, giving rise to one's degree of 'oonfideoce' in an estimate of relative likelihoods." I favor a slightly pithier definition: ambiguity is kMW!l•fo•be•missing infermatio11, or oot kn<JWing relevant 'information !ha! C(ll)ld be known (Frisch and Baron 1988; cl. Heath and TvenJ:.y l!J91).

The missing infurmation definition includes other kinds of ambiguity as special cases. The compositmn of the ambiguous EUsberg um is ml.$Sing infonnation, which is relevant and could be known bi.it is not. Doubts about the credibility of sources and disagreements among experts create missing information (namely. whether a source or expert can be bclie:ved). Keynes (1921} proposed that the weight of evidence be taken into account along with its implications. For instance, in Scottish law there are three verdicts: guihy. innocent, and unproven. While evidence might imply guill, if there is too little evidenc¢ it has low weight and the verdiu will be '"unproven." If the weight of evldew:e is defuted as the fraction of available infonnation, then missing information lowers evidential weight,

3, Empirical Tests

EUsberg did not nm any fonnal experiments,[51] but his thought ex.µeriments frequently replicated and cxlended. Becker and Brownson 0964) did the fim careful study. Their subjects clrose between urns rontami:ng 100 red and black balls. Drawing a red ball[,4] paid $t. The numbret of red halls felt wtthin a different range for each um. For example, one urn had exactly fifty red balls; another had between fifteen and eighty-five balls, The urns with unknown contents were rov-ered; they did not report how many balls were actually used in each, Sultjects chose between pairs of urns, differing in the range of red balls, and said how much they would pay to draw from their pref,crred um,

Subject;. al.ways picked the less runbiguous um and paid high tnnounts to avoid ambiguity (which increased with the degree of ambiguity). For example, they paid an average of $.36 to choose from an urn with 50 red balls instead of an ambiguDl.lS um with.Oto 100 red balls. (The expected value of a draw was $.50!)

There were several othcr studies in the 1970s and 1980s, all using similar _paradigms. In most of the studies ane choice was picked randomly and played for money. Some stylized facts emerged from these studies.

Ambiguity aversion is found consistently in variants of the Ellsberg problems (many of them using small actual payoffs). Ambiguity avetters are typically im• mune to written arguments against their paradoxical choices (e.g., Stovic and Tversky 1974), and pay substantial premiums to avoid ambiguity-around 1O to 20 percent of expected value or probability (MacCrimmon and Larsson 1979; Curley and Yates I989; Bernasconi and Loomes, 1992). Risk attitudes and ambi• guity attitudes are uncorrelated (Cohen, Jaffray, and Said 1985; Hogarth and Ein" horn 1990). Subjects would rather bet on known probabilities. p than on known probability distributions of probability (compound lotteries) with a mean of p (Yates and Zukowski 1979; Ull'son 1980). Increasing the range of pos!>ible proba-bilities increases ambigtlity aven;ion {Curley and Yates 1985). There is some twidence of ambiguity preference for betting on gains with low aml>igoous prob-ability, or betting on losses Wlth high probability (e.g., Kahn and Sarin 1988). (This may be due to perceived skewness in ambiguous distributions of low and high probability. which makes their means higher and Iower than ls aSSUJt!Cd,)

Curley, Yates, and Abrams (1986) tested several psychological explanations fur ambiguity aversion. Subjewi wito said the urn could not be biased against them were ambiguity averse too, sugge.stffi8 ambiguity at;-mioo is not due to an expressed belief in "hostile" gt"neration of outcomes. Subjttts we.re ambiguity averse when indifference was allowed (cf, Roberts 1963). Suhjcru: were more ambiguity averse when they knew the contents of the ambiguous um would he revealed afterward to others.

Competence

Ambiguity aversion implies there may be a gap between subjects• beliefs about an event's likelihood and their willingness to bet on the event {In SEtJ !here can be no such gap. since beliefs are derived from betting prefetenee$,) Heath and Tversk.y (1991) suggest that competence-knowledge, skill, comprehension-is what causes !he gap, They ran one set of C;(periments in which subjects gave probability assessments for natural event?; they knew a little or a lot aboJJt_ (They were rewarded with a scoring rule.) The subjects were then w.ked whether they would like to bet on the event. or on a chance device comtructcd to have the same probability as the event. If people ate ambiguity av they should always preter to bet on chance devices since events are inherently ambiguous, Bm subject:. who knew a lo! alxm! a domain of events preferred hetting on events: those who knew little preferred betting oo chance, People preferred betting on event'i they knew a Jot about holding btlkfs conslwft.

The competence hy is broadem the study of Choice anomalies in SEU by suggesting tbat ambiguity about probability is ju_ one of many forces that makes people reluctant to bet, by undemll;iing competence. Heath and Tversky uggest that competence. influences betting because personal and social assignments of credit and blame are asymmetric; competent people can take credit for wirm:ing in a way that incompetent people cannot, and. incompetent people may suffer more blame.

Ambigwty in Markets

Camerer and Kunreuther (1988) studied ambiguity in fill experimental market for hazards that incurred a loss; subjects could pay "ins.urance" to get rid of them. A second-ordcr probl1bilily distribution was used to induce ambiguity in loss proba-bility (Le., the probability had three possible values, which were equally likely). Ambiguity had little effect on market prices or volume. but it did incteal!e the variance in the distribution of sales across sellers (Le., some selkrn sold more insurance policies and others sold none).

Sarin and Weber {1993) studied ambiguity in market settings.. They auctioned off ambiguous and unambiguous lotteries in double-oral and sealed bid auctions with German sub:jec.ts. An unambiguffilS lottery was a draw of a ball from an open 1111\ with live winning and five losing balls (a winning draw paid 1O marks). An ambiguous lottery was a draw from a hidden um v.ith an unknown compositioo of batls, They found p,ersiste111 ambiguity aversion around p = .5 (hut not around p = .OS}. even under stationary replication. and even when the. ambiguous and Ullambiguous lottery markets operated simultane0usly.

While these two studies explicitly tesce-d the effec[of ambiguity in an experi-mental mruk:et, other studies may provide indirect evidence of ambiguity effect;. In moot mar t e ments subjecis are not fully informed about the market's structure (they know only their uwn valuations), so Lhey face some ambiguity due to missing information. In game&. they usually know the entire :structure, but their opponent's rai:ionalily is ambiguous. Anomalous bcha,.ior in markets and game; might lherefore he explained by ambiguity aversion (Camuer and Karjala.men, 1994}. FOf example. subjects in market eJ1periments often behave, very conserva-tively early in the experiment (e.g.. failing to trade when it is optimal): their behavior is usually labelled "risk aversion" or "confusion." The apparent con-fusion may he a manifestation of ambiguity aversion: subjects would rather do nothing, foregoing profits, than take action in an ambiguous environment where information is missing and they feel incompetent. As the experiment progresses, subjects learn and the amount of missing information shrinks, reducing their ambiguity aversion and conservative behavior. Thus, notions like competence, missing information, and ambiguity aversion might help us make sense of disequilibrium behavior early in experiments, which we currently call "confusion" and largely ignore.

4. Fonnal Models

Several kinds of formal models have been proposed to accomodate ambiguity effects. Some of the models are axiomatic generalizations of SEU. Others invoke psychological principles or propose ad hoc decision rules. I mention just a few (see Camerer and Weber 1992, for details).

There are several ways of modifying SEU without abandoning the expectations principle. Utilities can depend directly on ambiguity (Smith 1969; Sarin and Winkler 1989). Expected probabilities can be undetweighted (Fellner 1961; Einhorn and Hogarth 1985), or possible probabilities in a second order distribution can be weighted nonlinearly before taking their expectation (Segal 1987a; Kahn and Sarin 1988; Becker and Sarin 1990). The nonlinear weights might depend on outcomes (Hazen 1987).

Another way to modify SEU is to replace the expectation principle with more general decision rules that combine expected SEU with the minimum SEU over a set of possible probabilities (this was Ellsberg's proposal; cf. Hodges and Lehmann 1952, and Giirdenfors and Sahlin 1982). A special case of the combined model is maximizing the minimum SEU over some set of possible probabilities (Gilboa and Schmeidler 1989), perhaps weighting possible probabilities by a person's willingess to bet on them (Nau 1989). (Allowing preferences over ambiguous acts to be incomplete can yield a constrained maximin representation too; Bewley 1986.)

More radically, subjective probabilities may be precise but nonadditive: that is, $p(A \cup B) \neq p(A) + p(B) - p(A \cap B)$ (Luce and Narens 1985; Gilboa 1987; Schmeidler 1989; Wakker 1989; Tversky and Kahneman 1992). The idea is that unwillingness to bet on ambiguous states can be expressed by attaching lower subjective probability to those states. Nonadditive SEU must be calculated with a special summation or integral, first discovered by Choquet (1955). If states are ranked by the utilities of their consequences $f(s_i)$ for a particular act f, from $u(f(s_1)) > \ldots > u(f(s_n))$, then the finite Choquet integral is

$$u(f(s_1))p(s_1) + \sum_{i=2}^{n} u(f(s_i))\,[p(\bigcup_{j=1}^{i} s_j) - p(\bigcup_{j=1}^{i-1} s_j)] \tag{11}$$

Note that if $p(.)$ is additive and the states s_i are mutually exclusive, then the term in brackets reduces to $p(s_i)$ and equation (11) reduces to SEU. There is obviously a close kinship between nonadditive probability in SEU and rank-dependent probability weights in EU (compare equations (11) and (5)).

5. Applications to Economics

Theoretical and applied work on variants of SEU is several years behind the work on EU reviewed in section 111.D. For example, there are virtually no empirical tests pitting the formal models against each other.

There are several applications to economics. Dow and Werlang (1992) apply SEU with nonadditive probability to financial markets. They show that in theory increased ambiguity creates wider bid-ask spreads. Blank (1991) reports a large *American Economic Review* experiment comparing single-blind refereeing, when a paper's author is known to the referee, with double-blind refereeing, when. the author is unknown. (Not knowing the author of a paper, and knowing that you could, creates ambiguity.) Incoming papers were randomly allocated to the two reviewing conditions. Single-blind papers are rated more highly by referees (3.47 vs. 3.37, on a five-point scale), and accepted more frequently (14.1 percent vs. 10.6 percent) than double blind papers; one could ascribe the difference to ambiguity aversion. French and Poterba (1991) document a global preference for home-country investments, which costs investors the equivalent of about 3 percent in annual returns in foregone diversification. The preference for home-country investment and the revealed preference for publishing known-author papers are consistent with the Heath and Tversky (1991) finding that people prefer to bet on events they know more about (holding likelihood constant).

In smveys using hypothetical vignettes (based on naturally occurring risks), Hogarth and Kunreuther (1985, 1989) found that pricing decisions by professional actuaries and insurance undeiwriters reflect ambiguity aversion. (Indeed, ambiguity premiums are ***required*** by many of the pricing rules used in insurance companies; see Hogarth and Kunreuther 1990.) Knight (1921) and Bewley (1986) suggest entrepreneurship can be understood as ambiguity neutrality. Bewley also sketched some applications of his theory to labor contracting.

The idea that missing information creates uncomfortable ambiguity implies that agents will demand information simply to reduce ambiguity (and discomfort), even if the information does not help them make better decisions. (Demand for information could then be modeled as a primitive, instead of deriving it from preferences over outcomes that result from informed decisions.) Demand for ambiguity-reducing information may help explain potential anomalies like uninformative advertising and alleged medical overtesting.

F. Choice over nme

In traditional models of choice over time, simple axioms imply that preferences over consumption streams $X = (x(1), x(2), \ldots, x(n), \ldots)$ can be represented by an additive discounted utility representation (e.g., Fishburn and Rubinstein 1982):

$$X > Y \Leftrightarrow \sum d(t)x(t) > \sum d(t)y(t) \tag{12}$$

where $d(t)$ represents the present value of consumption at time t. Further axioms imply the familiar exponential forms $d(t)$ = JI and $d(t) = e^{-dt}$.

Considering the importance of intertemporal choice in economic theory, there have been relatively few experimental tests of choices over time. The tests conducted so far indicate several systematic violations that are remarkably similar to violations of EU. (1) Implicit discount rates, derived from choices over income or consumption streams, decline with time horizon: people are much more impatient about immediate delays than about future delays of the same length. Put formally, discount rates seem to be hyperbolic, $d(t) = (1 + \alpha t)^{-\beta/\alpha}$, rather than exponential ($d(t) = e^{-\beta t}$). (The exponential form is the limit of the hyperbolic form as α goes to zero.) (2) Discount rates are larger for smaller amounts of income or consumption, and are larger for gains than for losses of equal magnitude. (3) People demand more to delay consumption than they will pay to speed it up. Loewenstein and Prelec (1992) give a generalization of discounted utility, using a value function akin to the one in prospect theory, which can explain all these anomalies.

Some of these findings, especially hyperbolic discounting, have been replicated by Thaler (1981), Loewenstein (1988), Benzion, Rapoport, and Yagil (1989), and Shelley (in press). Hyperbolic discount rates are also commonly observed in experiments with animals (e.g., Ainslie 1975).

Human subjects were not financially rewarded in many of these experiments. Indeed, economic experiments on choices over time are extremely difficult to run because subjects cannot be paid immediately. Even if experimenters intend to pay, subjects may not trust them. Making subjects return to collect their money also imposes a cost that must be accounted for. A few experimenters have used monetary payments and observed the same anomalies observed in hypothetical choices (Loewenstein 1988; Holcomb and Nelson 1989; Horowitz 1991; Carlson and Johnson 1992).

Time affects choices in some other interesting ways (see Loewenstein and Elster 1992). Temptation arises if immediate discount rates are too high, creating a problem of self-control (examples include smoking, overeating, procrastination, and using credit cards; see Ausubel 1991). Many people have modeled self-control as a neo-Freudian conflict between multiple selves: a myopic self greedily consumes immediately while a foresightful self tries to restrict the myopic self's binges (Thaler and Shefrin 1981; Schelling 1984; Laibson 1994). The neo-Freudian view helps explain the existence of institutions that make self-control easier-Christmas clubs, "forced saving" by overwithholding income taxes from wages, voluntary diet plans that ration food, and so on.

Ex ante anticipation of consumption, or cherishing of ex post memories of consumption, may affect timing of choices too. Loewenstein (1987) asked college students when they would prefer to kiss their favorite movie star. (Kisses were hypothetical.) The median subject preferred to wait three days, exhibiting a negative discount rate, to savor the consumption before it occurred. Savoring and its negative counterpart, dread, are probably important parts of the explanation for why people buy insurance and gamble.

Varey and Kahneman (1992) explore judgments of the overall quality of temporally extended episodes (such as watching a movie or taking a week-long vacation). A natural model is that people judge overall utility by cumulating or integrating momentary utilities experienced across the episode. This model appears

to be wrong in interesting ways. For example, the cumulation model predicts that episode length will be correlated with overall quality, but in experiments using short and long films, length is only a weak correlate. People also appear to dramatically overweigh the momentary utilities at the peak and end of the episode (Fredrickson and Kahneman in press). It is easy to generate dominance violations as a result of this overweighting: in one study, subjects preferred long. mildly painful colonoscopies that ended with a gradual easing of pain to shorter ones that had less overall discomfort but ended painfully (Redelmeier and Kahneman 1993).

G. Process Theories and Tests

There is a large body of work, mostly by psychologists, spelling out procedures people use to make choices. I will mention only a few germane contributions.

When the number of alternatives is large, people often use conjunctive and disjunctive rules to reject alternatives, especially in complicated settings with many alternatives having several dimensions or attributes (e.g., Einhorn 1970). These rules specify cutoff levels for all attributes. With a conjunctive rule, if an alternative falls below any cutoff it is rejected. In a disjunctive rule, an alternative that satisfies any cutoff is not rejected. Tversky (1972) proposed and tested a related rule called "elimination by aspects" (EBA): subjects pick one attribute with a probability that depends on its importance, eliminate alternatives without the attribute (or below a cutoff level), then choose another attribute and repeat the procedure.

Grether and Wilde (1984) studied conjunctive rules experimentally. Subjects were forced to use conjunctive rules to make choices. "Inspecting" an attribute had a cost; subjects could choose cutoffs and the order of inspection. Subjects tended to inspect the lowest-inspection-cost attributes. They often violated normative predictions of optimal cutoffs and inspection order (Wilde 1982) because they ignored the influence that a drop in cost of inspecting one attribute should have on inspection order and the level of other attributes' cutoffs. (Roughly speaking, subjects used a partial-equilibrium mental model and ignored important general equilibrium effects.)

Using simulations, Johnson and Payne (1985) showed that in some choice settings simple decision rules. like conjunctive rules, are almost as accurate as more complex rules, like EV-maximization, and take much less effort. (Estimates of effort come from psychological studies of how long it takes to perform various mental operations such as adding, remembering, multiplying.) Johnson and Payne's simulations give an underpinning to the "labor theory of cognition" proposed by Smith and Walker (1993) (cf. Marschak 1968; Wilcox, in press). In the labor theory of cognition, mental effort is like manual labor: People dislike thinking hard; more effort is supplied in response to higher incentives (except when constrained at computational limits); and greater effort reduces the variance of responses around an optimum.

The labor theory is a natural way for economists to comprehend thinking, but in some ways it represents a psychological step backward. Some history is needed ю explain my point. From about 1930 to 1960, psychoklgy was dominated by "behaviorism," the study of the response of animals (including people) to stunulus, Behaviorism igooi:ed !he dctaits of cognitive process and treated the brain as a black box. In the 1960s a лe',l;' way to model cognition came around---t.he brain is like a computer-··.and the informatiw pm;:essing parailigm was born. Behaviorism wm; largely abandoned (except for some domams of animal learning) because the computer metaphor was so appealing and because of mounting evidence, such as transfer ofkarning across tasks, which was anomalous for behavioris.m and cried out for a cognitive ex.p!anation.

Tilt labor theory of cognition is a partial return to behaviorism because it concentrates on the relationship of stimulus (incentives) to respon..e (choices), compressing the details of cognitive processing: into the catch-all category. "effort. The empirical question is whether researcl1 chat incorporates more detail of think ing processes, like that discussed in section JI and in pans of this section, generates better predictions than the labor theory. l think it docs in many cases, However, the 'Simpler labor theory may still be useful in sorue ocooontic applications (espe<:htlly in formal theorizing) because of its parsimony and rough accuracy.

H. Descn.j>tion invariance

Utility theories makes several invisible background assumptions that are usually considered too innocuous to spell out. Two crucial assumptions are "description invariance'• and "procedure invariance·•; different representations of the same choice problem, and different elicitation procedures, should yield the same prefer, ence (Tversky and Kahneman I986V Money illusion is an example: doubling all wages and prices, or denominating them in Irish pounds rather than dollars, shouldn't make anynne feel richer.

Both principles are sometimes violated. Invariance violations are especially troublesome for utility theories (including generalizations of EU and SEU) and provide the strongest indications th-at preferences are constructed from proce<lural rules.

Luce and von Wmterfeldt (1994) offer a similar decomposition of axioms. They distmgwsh between axioms of "structural rationality" {or "accmmting equivalences"), which prescribe indifference between formally equivalent descriptions of a gamble, axioms of "preference rationality" (like independence), and axioms of ..quasi4ationality," which prescribe how consequences are coded as gains and iosses.

I, Framing Effecl;,

Th.e most famous violations of description invaria.nre are "ftamirtg effects," revcnmls of preference induced by changes in reference points. For example, McNeil, Pauker, and Tversky {1988) gave some American doctors and lsraeli medical students data on survival rates after treatment for hmg cancer by radiation therapy and surgery, as shown in Tobie 8,3, (Survival nu:es are the- percentage of

Table 8.3. Snrvir.al and Modality Frummg of Lung Treatments

	Sllf'i"ival Frame (% alive)		Mortalily Frame (% dead)		Both Frames Presented	
	Radiation	Surgery	Radiation	Surg«y	Radtation	Surgery
Af;er treatmeru	JOO	90	0	JO		
Afier one year	77	68	23	32		
After fr;-e years	22	34	,•	66		
Petcentage cltoosing each:						
American docton. and medical b'tudents	16	84	50	50	44	50
	(117)		(80)		t2:13)	
Israeli medical and science student,	2(J	80	4'	56	34	66
	(126-)		(132)		(144)	

S;;l(_cec McNeil et a!. 1988.
Note; Nurntxers in paret.!he,111a are n.

patients surviving a given length of time.) Others were given the same data, phrased as mortality rates (percentage of patients who died before a given lecgtli of time). A third group got both frames-, In the survival frame, 16 to 20 pero::nt favored radiation therapy, since smgery only reduces immediate survival from 100 to 90 percent and keeps more patients alive in the long-run. But in the monal• ity frame the lO percent death rate afler surgery looms large; then nearly half favored radiation. Given both frames, 40 percent favored radiation (suggesting the mortality frame is more natural or potent).

The most pressing question is whether framiog effects are systematic and predictable. The evidence ii; mixed. Fi-schhoff (1983) used a simple pairwise choice that could be framed several ways. Subjects' ratings of which frames seemed most natural were unciirrelated with the frames they appeared to m;e in making choices. Van Schie and Van OM Pligt (1990} found similar re stilts: expre:,;scd frame preferences were only weakly corre\ared with risk aversion in choices (replicating Fischhoff's discouraging rei>UIl), but the initial description of the choice did affect risk aversion. (If the problem was initally posed lo terms of losses, rather than neutrally, frames emphasizing losses were- preferred and choices were more risk seeking.)

Gertner (1993) reports fill interesting framing finding u.sing data. from actual bets (averaging $3,200i on a television game show, He re-port. tbul. when the cash stake available fur betting :increase,;! by $1, bets i:acreased by about $.60 {cf. the "house money" effect in Thali,U' and Johnson [1990)). But when the amount of cash winnings that cooldn'tbe bel increased (or when contestant had won a car) by $1, bet,; increased by only a penny. The data are clearly lnconsistem with the theory that contesta.nL'> integrat.e assets (bettable cash, unbettable crush, and car) then bet based on their :integrated assets.

Thaler (1985) proposed a "hedonic editing" rule for choosing reference points. For example, people :.hould segregate two $50 gains (resetting their reference point to absorb the first gain before valuing the second one) becaui.e v{SO) + v(SO) > v0OO) if v(.) is com;;ave for gain:,. But Thaler and Johnson (t990) found that hedonk editing did not explain cholc in two- age senin where prior gain or loss was followed by a cbotce. People use a varie1y of rules instead.

Consider a prior !uss of -S7.50 followed by a :recond ;,tage choke between $0 and (.5,$2.25; .5,-$2.25). If subjects integrate the loss and the choice (and obey prospect 1heory), they c!toose bcrween -$7 :0 and (.5,-$5.25; .5.--$9.75), and lake the gamble, because they are risk seekmg over looses. But if subjects segre• gate the loss they will lake **$0** over the gamble (which is unappealing because of Josi. aven,ion). Sixty percent of subjects rejected the gamb-Jc, which sugge»ts they are segtegating the prior lo;s.

Now com>ider a prior loss of -$7.50 followed by a choice between $2.50 and (.33,$7.50; ,67,$0). Uoder .integration. the choice i between -$5.00 and i.33,0: ,67.-$7.50); subjeets should gamble. Under segregation, subjects shoold lake the sure $2.50. In fact, 71 percent of the subjects preferred the gamble, which sug, gests they are inregrating the. prior Joss. Integrating the Joss is appealing becau.se the integrated gamble gives. a .33 chance uf breaking even by winning $7.50 aJhl recouping the prior loss,

In the fim situation, the prior Joss appears to be segregated from the ga.mble choi<:e. In the second situation the loss appears to be integrated (when breaking even is possible). Framing principles appear- to be a long list of rules like. these dictating whether people integrate or segregate dependiog on contextual details (cf. Luce and von Winte1feldt's, (1994] discussion of "joint receipt"/.

2. Lottery CiJtTelatlon. Regret, and Display Effec1s

The correlation between outcomes of two lotteries is an element of choice de- scription which, according to EU. should not affect preferences. Consider the top two <:hoices in Table 8.4 {see Loomes 1988a_). The probability distributions of A and B out.:omes are the same in both choice'l-A. is (.3,101 and Bis (.7,5)-,---hut tbeIr correlation is different. In choke I, the payoffs arc negatively correlated and in choice 2 they are positively corrdated/ Under any utility theory that assigns a number to outcome distributions, u(A) and u{Bi should be the same in both chokes, But regret theories assign a nu ber ro lt{A: B), .w the correlation -between A and IJ outcomes can matter.

Loomes and Sugden (]987.a) suggest ;i "convexity" (o; "regret aversion") hy- polhe is to make the theory testable. Corivex.ity implies that the comparative util- ity from getting Oand foregcmg 10 is worse than the sum of comparative utilities from getting O im.tcad of 5. and getting 5 instead of m **{That** is. u(0,10)< u(0,5) + .u(5,10).) Then people might switch preference fromJ\I > Bl in choice 1 to 82 >- A2 in choke 2 **(but** they should not sw.itcn the opposite way).

In experiments with choice displayed graphically such as the top two choices in Table 8.4 (whh one gamble actually pl.ayed), about a rhird of subjects switch

Table 8.4. Choices in Regffl E

		Probability of Stale		
		.3	.7	
Choice I	Al	JO	0	
	Bl	0		
		.3	A	3
Choice 2	**A2**	J0	0	**0**
	B2			0
		3	.7	
Cboice3 (collapsed table)	A3	JO	**0**	
		7		.3
	B3			**0**

preferences between the two displays. 80 percent of them in tbe direction pre dieted by convexity (Loomes 1988a,198Sb, 1989a; Loomes and Sugden 1987b; Starmer and Sugden 1989c). Regret-aversion also predicts that if payoffs are jux taposed properly people will choose a MOC:hastically dominated lottery (e.g.. (.38,$3; .2,$10) will be preferred to (.42,.$3; .2,S 10)). About half of subjects did violate dominance in this way in studies by Tversky and Kahneman (1986) and Loomes, Starmer, and Sugden (1992).

Battalio, Kagel, and Jiranyakul (1990) found much less switching when the A and B payoffs were shown horizontally, rather than vertieally as in Table SA, In tests with several different displays, Harless (1992a) only found systematic regret effects-switching in the direction predicted by regret aversion-in the vertically aligned Table 8.4 display. He also experimented witb displays in which states yielding the same payoff were collapsed for each act, as shown in choice 3 in Table 8.4. Regret effects presumably arise from comparing two acts' outcomes in the same state (i.e., comparin_g two row entries within a column). The choice 3 display might weaken rc_grel effects by making it more difficult to compare out• comes within a column, Indood. Harless found no regret effects when choice 3 displays were used.

StarinN and Sugden (1993b) found no regret effecu usil1_g choice 3 ilisplays ("strip displays") either. They dis,::overed wroething else even more subtle, and astonishing; Compare choice B2, (.J,5; A,5; .3,0), with choice B3, (.7,5; ,3,0). Bolh -,kscribe the same lottery (a 70 percent chance of winning 5), bul Ihe B2 cboicehas two stales that yield the prize of 5, one with .3 probability and one with .4 probability, while choke B3 has a single wiruring state, with probability, 7, Yet they found that about tO pei,:ent more subjects preferred. the two 'Jtate choice B2 over a third gamble than preferred the one state choice over the third gamble.

Simply splitting one state into two states with the same outcome, and the same total probability, increased preference for an act noticeably. (A similar preference--for-splitting can occur in theories with nonlinear weights, since w(.1) + w(.1) can be greater than w(.2). Indeed, Tversky and Kahneman [1986] exploited this property to construct violations of stochastic dominance in "opaque" choices.)

The regret studies show the interplay of experimental studies, and the cumulation of discoveries, at its best. Several studies by Loomes, Starmer, and Sugden established an important anomaly, the effect of lottery correlation on choices, which violated description invariance but could be neatly explained by regret aversion. Then a small test by Battalio et al. hinted that regret aversion could be sensitive to display effects. Harless landed a second, harder blow. Then Starmer and Sugden confirmed that regret aversion largely disappeared when displays were altered. In so doing, they (re)discovered the remarkable fact that splitting states made an act substantially more attractive. This is a story of successful detective work. The effects are small in magnitude. The studies used large samples of subjects (40--200 or so) for statistical power and a wide variety of gambles for robustness. Less methodical investigators might have falsely accepted the null hypothesis that there were no regret effects in the first place or underestimated their sensitivity to display.

Earlier studies tested the effect of using different lottery displays-trees, matrices, verbal descriptions-:m the rate of Allais paradox choices (Moskowitz 1974; Keller 1985a). Display effects were generally too small to eliminate violations, but large enough that more work on display effects could prove fruitful.

3. Compound Lottery Reduction

Most utility theories contain an explicit or implicit reduction axiom stating that whether a lottery is described as a compound gamble with several probabilistic stages or as a single-stage gamble should not affect preference.

The reduction axiom has great normative appeal but is often violated in direct comparisons between one-stage choices and multiple-stage choices (Bar-Hillel 1973; Ronen 1973; Kahneman and Tversky 1979; Keller 1985b; Bernasconi and Loomes, 1992; Conlisk 1989). Most violations seem to be caused by the tendency to "isolate" the uncommon elements of two gambles, by cancelling common first-stage probabilities or common final-stage payoffs. For example (Kahneman and Tversky 1979), gamble 1 is a two-stage gamble in which there is a .75 chance of winning nothing at the first stage and a .25 chance of moving to a second stage gamble that pays a certain 3000. Denote gamble 1 by (.75,0;.25[1,3000]). Gamble 2 is a .75 chance of winning nothing at the first stage and a .25 chance of moving to a second-stage gamble that pays 4000 with probability .8, or nothing with probability .2. Gamble 2 is denoted (.75,0;.25[.8,4000;.2,0]). In reduced form, these compound lotteries are equivalent to (.25,3000) and (.2,4000); most people pick the riskier gamble 2, (.2,4000). The isolation effect refers to the tendency to ignore the common (.75,0) stage and choose gamble 1 or gamble 2 by isolating

their uncommon second-stage elements. Then people usually choose gamble 1 because they prefer (1,3000) to (.8,4000;.2,0).

This cloud of violation has a silver lining, however. Since isolation is simply an application of the independence axiom, the independence axiom (and the betweenness axiom) is less often violated when gambles are presented in compound form (see Conlisk 1989; Segal 1990; Luce 1990; Brothers 1990; Camerer and Ho 1994; Bernasconi 1992; cf. von Winterfeldt, Chung, Luce, and Cho 1992). In the example above, if people prefer (1,3000) to (.8,4000;.2,0), then they will prefer the compound gamble (.75,0;.25[1,3000]) to (.75,0;.25[.8,4000;.2,0]).

Reduction is important for some procedures that are widely used in experimental economics. An example is the "random lottery" procedure, in which subjects make many choices but only one choice is picked, at random, and played out for money. If subjects violate independence and the random lottery procedure is used, then if they obey reduction they will not always choose as if they made only one choice. (Whether they will do so depends on the set of choices and the nature of their independence violations.) But suppose subjects isolate each pair while they are choosing, rather than multiplying its probabilities by the chance of that pair being randomly picked.[57] Then they will choose as if they were making only one choice, even if they violate independence. In fact, people *do* seem to isolate each pair (Starmer and Sugden 1991a; cf. Camerer 1989a).

Reduction plays a similar role in the Becker, DeGroot and Marschak (1964) procedure for revealing preferences and in the Vickrey auction for gambles (see chapter 1). If subjects obey reduction and violate independence, these mechanisms will not be preference-revealing for gambles (Chew 1985; Karni and Safra 1987). But if subjects violate reduction by isolating, both techniques are preference-revealing. Ironically, the violation of reduction that is implied by the isolation effect works to the experimenter's advantage, *ensuring* the usefulness of the random-lottery procedure and the BDM procedure.

Reduction is also assumed in attempts to induce risk neutrality by paying subjects in lottery tickets (Smith 1961; Roth and Malouf 1979; Berg et al. 1986). Evidence on the effect of the ticket procedure is mixed (see chapter 1 and chapter 7). In the ticket mechanism, reduction violations work against the experimenter.

I. Procedure Invariance

Elicited preferences should be invariant to the procedures used to elicit them, but they seem not to be. The elicitation biases mentioned above in section III.C.4, such as differences in utility functions derived by probability- and certainty-equivalence, violate procedure invariance.

Shafir (1991) discovered a violation of procedure invariance that is both illustrative and startling. Consider two lotteries, (.5, $50) and (.8, $150; .2, -$10). Asked which lottery they would choose if they had neither, 75 percent of subjects (n = 279) picked the riskier one, (.8, $150; .2, -$10). Asked which they would give up if they had both, 50 percent preferred to give up (.8, $150; .2, -$10). The pattern is surprising because complete preferences, and invariance of expressed preference lo whether the choil.11' is an acceptance or a rejection, imply that !he two percentages should add 10 t00 percent Shafir's explanation is that the serond gamble is ""enriched" there is a good reason to pick it {it has a higher "winning payoff) *and* a good reason to reje,;t it (it has a ch:mceoflosIng). If people choose gambles with more positive features, and reject gambles wilh more negative features, then enriched gambles with both kinds of features ill both be chosen and n,:Jecied more often, a dramatic violation of the principle that the procedure for eliciting choice should not affect choices. :"(otice that this violation, while not substlUltivt'l:,y rational, seems to have a dear procedural explanation that is amenabk: to formal modeling.

Aoolher violation of procedure invarianre with a long Jusrory i& preference reversal (e.g., Tversky and Thaler 1990; Liomes forthcoming). The reversal literature began when Slovic and Lichtenstein (1968) noticed that the prices subject$ gave for bets were highly correlated with bel payoffs, but choices were more highly correlated with probabilities, They conjectured that if subjects were offered two bets. one with a high probability and low payoff (a "P-bct") and another with a low pmbebility and hlgh payoff (a ·'$-bet"), they might ch sc !he high-probability *P-* but price the high-payoff $-bet higher.

They were right. Lichtenstein and Slovic {1971, 1973) observed nsstematic, widespread preference r-eversals (see also Lindman)971). The rev ls attracted relatively little attenti,m in psychology; perhaps there were plenty of other demonstrations that how a question is asked fofluefIces its answer (cf. opinion polls), and p,;ychologists were busy discovering other anomalks, Then Grether and Plott (1979) replicated the earlier findings, using the Becker, De.Groot, and '\iarschak (BDM) (1964) procedure to elicit UITT:ntive-compatihle selling prices. Their replication auracted much attention within e('.Ooo:mics, The early debate is described in chapter L

L New Evidence of Preference Revenal

Recent evidence has e,;tablished SO!lle new facts. Revmals disappear when choosing over portfolios of gambles played repeatedly (Wedell ;md Bodrenboh forthcoming); this is unsurprising because repeated play reduces the difference in probabilities and payoffs of the two bets that generate reversals. Irwin et al. (1993J e. i!.hed large reversals in choosing vs, pricing consumet goods and environmental values (such as cleaner air). In gambles over real losses from a cash endowment, subjects exhibited reversals in the opposite direction-when they chose $-bets *they* usually priced *P-bet;* higher (less negauvely) (McDonald, Huth, and Taube 1991), Casey (1991) also found oppositerever,,als using buying prices aOO high4takes gambles (replICated with real payoffs in Casey [1994}),

Most other recent evidence addresses three general explanation;. for wversals (laid out bj' Tversky, Slovic, and Kahnerrum 1990). RcVC.rsals are either {1) an • artifact of the method used to elicit bet prices; (2) violations of transitivity; or (3) violations of procedure invariance,

The artifa.:t explanation (1) has attracted by far the moM .attention from econo"

mists. The idea is that the Becker, De{)rool, and Marschak (BDMJ H964) proce• dure dues *not* elicit rruthful relling if the Indepcndcnce axiom is nolalr.d and reduction Js obeyed {see Holt t986; Kami and Sarra 1987; Segal 198S). Apparent revenals might tben be due to systemruic misreports of true sel]ing pnces.

Cox and Epstein (1989j avoided problems with the BDM procedure by using a different method. Tbt-y asked subjecL< m value a P-bct and S•bet concurrently, !hen compared the *ronk* of the vah.lationa with subjects' pairwise choi=. Subjects were somewhat motivated to give accurately ranke4 valuations be<:ause the llighcNanked gamble in *one* randomly chosen pair was played for money (bnt they were not penalized for inaccurate valuations so long as the preferred gamble had a higher valuation). Since the BDM procedure was not used, any reversals in preftrence could not he due to BDM distortions. They observed fewer asymmet• ric reversals {P-bet > $-bet, $-bet priced nigher), but many symmetric reversals, The rate of symmetric reversals is roughly &imilar to the 15 to 25 percent rate oi reverW obseived in some studies cited earlier, in section III.D..?, but Is sub&tantially lower than would be expected from purely random switching,

There are ng theoretical, philosophical, and empirical counternrgUmenls to the l\t'Ufactual explanation, which blames the BDM procedure for apparent rev*er*, sais. First, the BDM procedure only faih if independence is violated *and* reduction is obeyed. If subjects exhibit an isolation effect and violate reduction (as they appear to do; ;;ee section III.H.3) !hen *the* BDM procedure works properly.

Second, virtually identical patterns of reversals are observed when prices were elicited *withou!* the BDM procedure (in earlier studies); w how can the BDM procedure be blamed for those reversals? (Are they a coincidence?)

Third, Safra, Segal, and Spivak (l990a,1990b) showed that the artifactual ex• plar,ation has two testable implications: {I) revernlng preference goes hand in hand with viola1ing mdep endence by faunmg out; and (2) a gamble's selling price SP (derived using BDM) and certainty-equivalent CE (derived from intro8pecrion by subjects) need not be equal, bu! SP and CE should lie on the same side of the gamble's expected value. New eX:periments suggest both implications are false. McDonald. Huth, and Tuubc (199{) discovered that implication (1) u; false-subjects: wh0 exhibited f.anning out were no more likely to reverse preferences than others who did not fan out. Keller, Segal, and Wang (1993) discovered that implication (2) appears to be false ---although SP and CE iie on the same side of EV nearly two-thirds of the time. dlC violation.-; of this same-side property are strikingly asymmetrk: 22 percent of subjects show $SP > EV > CE$ white only 9 pe:rcent show $CE > EV > SP$. (If SP-CE differences are due to independence violations in the BDM procedure, the two patterns are random errors and should he roughly equal in number.)

Given ihe 1.trength of these counterru:gurnents--the second one of which has been known for twenty years-it appears that the artifactual explanation may have received too much attentiou from WCnted researchers with better things todo.

Tversky, Slov1c, and Kahneman (1990) conducted an experiment to separate the tram:itivity (2) and procedure invarim::e explanations (3) of reversals." Con-

sider a P-bet (35/36,$4) and a $-bet (11/36,$16}. lf a subject chose the P-bet, then stated prices of $3.50 for the P-bet and $5.00 for the $-bet. she reversed preference. in the usual way. SubjlX'.ts then chose betwren the P-bet and a pred..otemtincd certain amount ($3.&5, in this easel and between the S-bet and $3J 5. Choo;.ing $3.85 instead -0f the $-bet (for which the subject stated a price of $5) would indicate the $-bet was: overpriced in the pricing task compared to ii:, value in choice, indicating that choice-based preference and value-based preference are different (i.e., procedure invariance is '<iolated). If procedure invariance holds but lransitivity is violated, then P-bet > $-bcl but P-bet < $'.U5 and $3.85 < $-bet. fo their data, most revwmts (66 percent) wt'fl! due solely to overpricing of S-bets, Only 1O percent reflected intr.msitivity. Cox and Grether (1991) closely replicated this result (10.5 percent intransitivlty).

Loomes, Starmer. and Sugden 0989, 1991) di putetl these results (see also Loomes 199lb). They think the method understates the de s ree of intransitivity and overstates the importance of 1msprîcing:s They used a :simiiar dei,ign and fotmd more evidence of intransitivity. In one experiffii'nl. they compared lhe fre• quency of reversal,; measured two ways. First. subje,.u stared valuations and then chose betwe_en certain amounts and gamb:les {as in Tversk}• et al.). For example, a subject who said the ***P-bet*** was worth $3.50 would later be asked to choose between the P-bet and $4,00. Set'ond, a subject's preferen.:es in the choice between the P-bet and $4,00 were automatically determined ("impL1ted") from their earlier valuation. iln dre example, the subjecl would be forced to choose the $4Jl0, since \he £aid the- P-bet was wonh $3.50.) The second method (imputed-choiee) forces valuations and choices to be consistenr: the first Illethod (actual-choke) does not. If choice vah.llition discrepancies generate reversals there shoukl be substaruia!ly more reversals with Um actual-choke method (because the- imputed-choice method does not allow discrepancies). In fact. there were slightly more reversals using a<itual choicc:s \ 11_75 vs. 14i, In another experiment using only choice Loornes., Starmer. and Sugden \ !991 l got about *W* percent intransi• tive cycles, Loomes and Taylor (1992) got 25 percent cyck-s using gambles over tosses, These data show about twice- as many imcan itivities (in a pure-ch ce tting) as were reported by TverM;y et a l Md Cox and Grether, Regardless of the precise "market share" of inrra.nsitivity. it seemi; dear that both inmmsitivity and procedural variance play a role in explaining reversals. Both phenomena are well worth studying further,

Bostic, Hermsiein. and Luce (1990) and Loomes \ 199leJ elicited prices using an iterated choice procedure m which subjects made choices between a bet and a series *of* ctmain amounts !hat varied up and down until indifference was reached, establishing a price, Replacing pricing (a judgment task) with iterated choice should reduce the number of reven,aL-. if they are c by procedure variance. Reversals ***were*** reduced.

Recent proce&S evidence .is informative too. Schkade and Johnson (1989) used a c-Ompnte-r display, with probability and payoff infonnatfrm hidden in boxes that opened when the subject mowd a cursor into them. to rrace what information a sub-ct was looking at (and for bow long). Subjects lonked at payoffs a larger

fraccion of the time when setting prices (55 percent) than when choosing {45 percent!.

JohnSQ!l, Payne, and Bettimin (1988) made biliues more difficult to process simply by multiplying them {e.g., 9/10 became 513/570). The change made e1q:1ectation•ba&ed strategies more computationally difficult and doubled the number of reversals.

Much of the new evidence corroborates the original Slov.ic and Lichtenstein interpretation -0f the cause of reversals. Their interpreta.tion. is now known as "contingent weighting": the -weight attached to a ilimens10n tOCreases when the dimension is psychologically "compatible" with the ***response*** e {payoffs and price-setting, for instance). When oo dimension is psychologically com 1bl, the most "prominent'" dimenskm is weighted more hi_ghly (e.g., probalnlity_ ts weighted highly in choosing}. Tversky, Sattath, and Slovic (1988) showed con n• g(int weighting effects in several settings, of whic preference reversals are Just ***one*** example. Contingent w-eighting cafi al.so explam the oppOSite pa ms of reversals for gambles over losses (loss amount ls weighted more hlg.11y m fonming valuations lhan ifi choosing, leading people to prefer $-bets but ':"ce m m re negalively}. However, explaining opposite reversals using buymg es with high4,takes gambles (Casey 1991) require& a more complicated theory with framing features.

2. Arbitrage and Incentives

Sevenil experimental economists have studied the effect5' of arbitrage and incentives on preference reversals.

Chu and cnu (1990) money-pumped Chinese students who exhîbîted reversals (&S did Betg etal. 1985). Their subjects stated prices for two gambles an ked one of the gambles. lf they exhibited a reversal, choosing the P-bet hut pn:1-ng the $-bet higher {denoted c(S) > c(P)), an experimenrer would 11 the subj the $-bet (collecting c[$l), make her switch the $-bet fur the ***P-bet*** (tn accord with her choice). then buy back the f'-bet for, the price c(P). The subject ended where she began, with no bets, but was ***c(P}*** - c($) poorer.

For most subjects who expressed reversals, il took roughly two arbitrage cycles to eliminate the reversals, Money-pumping a subject on the first of three gamble pain reduced reversals in the second and third pair DO. Berg et al. {19-85) nd that the magnitude of reversals, but not their frequency, was reduced by a mn1lar money•pump procedure. These results suggest that iu an environment where preference reversal is a recognizable, costly mistake that outsiders can spot and exploit, then peopie can leam to switch their expressed preferences (redl)(:e the size of any discrepancy), But there is no e1,,JdetKe of whether S Jeets w o are disciplined this way then leam ro express preferencer. more conststently m the future. or whether reversals actuaily persist in natural settings.

Bohm (1990) oond\lcted a highly original experiment., recruiting tweni:, su Swedish students to choose between and bid for two used car:;, a Volvo m d an Opel. (The cars were actually sold!) His goal was to test for reversals of preference in a high-stakes, natural setting with eager (self-selected) consumers. Most subjects chose the Volvo, and bid higher for it too. *No* subjects reversed preference (although four reversed weakly, choosing one car but making equal bids). Bohm concluded that reversals of the usual kind may be uncommon in natural settings. The problem is that there is no strong a priori reason to expect reversals in a choice between a Volvo and Opel. So it is difficult to tell whether reversals disappeared because of the extraordinary incentive in a natural setting with eager conswners, or because of the poor correspondence between the car choice and the P-bet/$-bet paradigm.[60]

Harrison (1990) criticized preference reversal experiments for providing inadequate incentive, because small misreports of prices cost subjects very little when the BDM mechanism is used. He showed that making the scale of price reports more coarse (e.g., forcing prices to be rounded the nearest $.25 or $.50), or increasing the difference in expected values of the P- and $-bet, reduced the number of reversals substantially. The effect of coarseness is not surprising (in the reductio ad absurdum case, if subjects could report only one price then no reversals would occur). Increasing the expected value, like the effect of money-pumping, makes expressed reversals more costly and does reduce them. The results point out that large differences in expression of preference are less frequent than small ones, but they do little to answer the pressing question of how large such differences are likely to be in natural settings.

Berg and Dickhaut (1990) studied incentives too. Their work uses the two-error-rate model (Lichtenstein and Slovic 1971). That model assumes a fraction q of subjects are truly risk averse and prefer the *P-bet*. Subjects make errors in expressing choices with probability r, and rank the prices of the two bets backwards with probability s. Certain values of q, r, and s lead to certain fractions of subjects choosing *P-* or $-bets and setting higher prices for P-bets or $-bets. The observed fraction of reversals can therefore be used to estimate q, r, and s. The error rate model provides a clearer view of how subjects respond to incentives than overall reversal rates, because incentives can reduce error but *increase* the number of reversals.[61]

Berg and Dickhaut showed that experiments that used hypothetical choices or interdetenninate incentives were best fit by a three-error-rate model in which pricing errors by risk averters (who prefer P-bets) were more common than pricing errors by risk seekers. Experiments with incentives for truthful revelation of prices (using the BDM procedure) were best fit by the two-error-rate model. Experiments with arbitrage (Berg et al. 1985) reduced error rates substantially. Curiously, the error rates r and s inferred from the data were mostly greater than .5. This casts doubt on their interpretation as errors in expression of preference, and poses an important puzzle for work using the error rate models.

Berg and Dickhaut also ran experiments in which subjects earned payoffs in points that were convertible into lottery tickets, rather than in dollars. Risk aversion and preference were induced by converting points into tickets with concave and convex functions (a la Berg et al. 1986). Since prices were stated in points, just as payoffs were, the contingent weighting explanation of Tversky et al. (1990) predicts the same rate of reversals as in experiments with dollars (unless compatability of a familiar dimension like money is different than an unfamiliar dimension like points). But reversal rates (and error rates) were actually much lower using points, and most reversals occurred when risk-seeking subjects chose the $-bet but priced the P-bet higher. Their paper suggests error rates are sensitive to incentives and raises a new puzzle, since opposite reversals were observed with point payoffs (as in Casey's reverse reversals with buying prices for high-stakes gambles).

3. Reversals and Markets

Knez and Smith (1987) studied market trading and preference reversal. In their experiments, subjects hypothetically chose between a P-bet and $-bet and hypothetically valued both bets (giving selling or buying prices) four times. Between each choice-pricing iteration, they traded the bets in a separate trading period for each bet. The market trading experience reduced the incidence of preference reversal across the four hypothetical choice-pricing iterations, from about 60 to 40 percent. Subjects also sold or offered below their stated minimum selling price, or bid or bought above their maximum buying price, about a third of the time. (The average violation was substantial, about $1 in bets with expected values of $3.85.)

Cox and Grether (1991) studied the influence of markets more thoroughly. They compared selling prices elicited three ways: with the BDM procedure, with sealed bid second-price auctions (see chapter 7), and with an English clock auction in which prices fell steadily until only one person remained willing to sell. Notice that the sealed bid auction requires subjects to state prices directly, while the English clock auction requires a series of choices (i.e., whether to sell at the current price or not). The two markets therefore compare the choice and pricing modes of expressing preference.

Cox and Grether observed a typical rate of predicted reversals (around 60 percent) in the BDM and sealed bid conditions. Reversals in the English auction were much more symmetric; pricing the P-bet higher while choosing the $-bet was more common than the opposite, familiar reversal. Since the English auction is like a series of choices, disappearance of asymmetric reversals is strong evidence that choice-pricing discrepancy underlies reversals. When all three tasks were repeated the rate of predicted reversals declined slightly using BDM (though here they have few data) and both declined substantially and became more symmetric in the two markets. In the markets, bids were highly correlated with the last market price, which suggests that markets reduce reversals by giving traders an observable price to anchor on when generating bids.

Cox and Grether varied incentives too. Some subjects earned a fixed payment of $10 while others played all their choices and earned large amounts (an average of $59) or small amounts ($36). Incentives made little difference in the BDM procedure. In the market tasks, however, fixed-payment subjects behaved in the opposite way to the others, exhibiting no systematic reversals in sealed bid auctions and strong reversals in English auctions. Their data provide a striking example of how incentive effe.cts can vary in predictable ways across domains. In the English auction, watching the price fall is dull. (The fall is larger for the high-payoff $-bet, since the clock starts at each bet's maximum payoff.) Fixed-payment subjects often dropped out of the auction quickly so they could ignore the computer screen and read newspapers or daydream, thereby establishing high selling prices-especially for $-bets, because the fall is slower and more boring-and a high reversal rate.

4. Social Comparison and Reversals

Loewenstein, Blount, and Bazerman (in press) report a novel type of reversal that springs from the tendency of people to compare their outcomes with others'. They went to a class to recruit subjects for experiments. They recruited students in one of three conditions. In one condition, subjects could earn $7 for forty minutes of work; 72 percent of the students (n = 39) agreed to participate. In another condition, subjects could earn either $8 or $10, depending randomly on the last digit of their social security number. (No convincing explanation was given for the disparity in wages, a weak point in the study.) Of the students who would have earned $8, only 54 percent (n = 44) agreed to participate. In a thrird condition, students could choose to participate in either experiment. Of those who chose to participate at all, 22 percent chose the $7 experiment and 78 percent chose the $8 to $10 experiment (when they would earn $8). A preference reversal occurs because *fewer* students participated for $8 than participated for $7 when they considered the experiments separately (54 percent versus 72 percent), but many more students elected the better-paying $8 experiment over the $7 experiment (78 percent versus 22 percent) in a direct choice between the two. Students appear to compare their wages with others when rating any one activity (see also Bazennan, Loewenstein, and White 1992), but weight their own wage more highly when choosing among different jobs. This intriguing finding (which is only marginally significant) deseives further exploration.

5. Some Conclusions about Preference Reversals

The discovery of systematic preference reversals is now about twenty-five years old. Economists have concentrated on the role of incentives and incentive mechanisms used to elicit preference. Increased incentive appears to lower implicit error rates, though reversal rates are not always lowered (Berg and Dickhaut 1990; Harrison 1990). Arbitrage reduces the magnitude of reversals, and sometimes their frequency (Berg et al. 1985; Chu and Chu 1990). Experience trading bets in markets appears to reduce reversals, perhaps by giving subjects a way to establish a gamble's worth (Knez and Smith 1987; Cox and Grether 1991). 1\vo studies in new domains-repeated gambles (Wedell and Bockenholt forthcoming) and used car auctions (Bohrn 1990)-did not find reversals, which simply shows that not all pairs of choice objects are prone to systematic reversals. Perhaps the clearest

result from the last five years of research by economists is that theories that trace reversals solely to problems using the Becker, DeGroot and Marschak mechanism are wrong (see Cox and Epstein 1988; McDonald, Huth, and Taube 1989; Berg and Dickhaut 1990).

Psychologists have been less interested in the roles of incentive and experience, since the replication with casino gamblers by Lichtenstein and Slovic (1973) suggested neither variable was important. Instead, they suspect reversals are caused by a difference in revealed preference that results from different procedures used to elicit preference (Tversky, Slovic and Kahneman 1990; Mellers, Ord6fiez, and Birnbaum 1992). lntransitivity appears to play some role too (Loomes, Starmer, and Sugden 1989, 1991).

There are at least three obvious directions for further research. First, since errors underlying reversals, and the dollar size of reversals, can be pounded down by enough incentive and arbitrage, an open question is how much discipline economic settings actually provide. (Knez and Smith [1987] start in this direction.)

lt would also be useful to take the psychologists' explanation for reversals-that preferences are procedure-dependent-more seriously. One direction aims at the individual level: for example, Luce, Mellers, and Chang (1993) give a theory for how certainty-equivalents could be constructed for a set of gambles, and yield a preference order systematically different than would be obseived in pairwise choices.

Another direction is the market level. (A start down thris path is long overdue.) 1\vo examples spring to mind. First, in choice among commodity bundles, if one commodity is the numeraire then the marginal rates of substitution will value that commodity more highly, ceteris paribus. Similarly, in negotiations over alternative multi-attribute settlements, the attributes being adjusted to make a settlement acceptable will seem most valuable (cf. Tversky, Sattath, and Slovic 1990). Second, different exchange institutions correspond to different response modes or procedures for eliciting preference (Machina 1987, 140--41). Buyers in posted-offer markets make choices; bidders set prices. If preferences depend on response modes, prices and allocations should differ systematically across institutions (familiar territory for experimental economists; see Cox and Grether 1991). Chapter 7 offers a related interpretation of the discrepancy between sealed bid and open outcry auctions.

J. Endowment Effects and Buying-Selling Price Gaps

Economic theory predicts that the prices a person will pay to buy and sell an object should be about the same. But a wide variety of studies indicate a large gap between buying prices (measuring "willingness to pay," or WfP) and selling prices ("willingness to accept," or WTA). See Kahneman, Knetsch, and Thaler (1991) and Hoffman and Spitzer (1993) for reviews.

The buying-selling price gap, or WTA-WTP gap, was discovered by environmental economists in the 1970s. For example, Hammack and Brown (1974) found that duck hunters would pay $247 each to maintain a wetland suitable for ducks, but lliked $1,044 to give up the wetland. Many other studies reported similar large gaps (see Cummings, Brookshire and Schulre 1986), ratios of median ViTA-WTP around two or more. "Contingent valuations" like these[6] are useful foT doing cost-benefil analyses to make governmental allocations of non-market goods, and the gap between buying and selling prices raises the difficult question of whkh price is more appopriate. Knetsch and Sinden {1984) studied price gaps for lottery tickets (with cash or a gift certificate as the prize). In a typical experiment, nineteen Qf thirty-eight subjects would pay $2 for a ticket, but only nine of iliiny-eight would sell at that price.

I. Market Experiments

An immediate concern Wa5 whether these gaps would persist in repeated market settings, Coursey, Hovis, and Schulze (1987) studied an unusual "bad." the obligation to hold a harmless bitter-tasting liquid called SOA in one·s mouth for twenty seconds. The buying (selling) price was the amount one would pay (oc-.;ept) to get rid of (assume) the obligation. Prices were bids ill a uniform price Vickrey auction in which the four high bidders paid the fifth-highest price. There were large gaps in hYf)OtheticaJ valuations made before the series of auctions, but repeated auctions reduced the gap substantially, to a ratio of 1.5/2.5. Hov..,r, their conclusions have been disputed (Knetsch and Sinden 1987; Gregory and Forby 1987) because they are especially sensitive to outliers and skewness in WTA values.[1]

Boyce et al. (1992) auctioned off houseplants. that resemble pine trees (called Norfolk Island pine>'). They used the *BDM* procedure to elicit prices. The prices were then used as bids in an auction amoosubjects. (Their procedure adds an intermediate step to the usual procedure of having subjects bid directly.) Mean buying and selling prices w{!I'c $4,81 and $KOO. Prices were sub$tantial!y higher, $7.81 and $18.43, when subjects knew that any trees they did not keep or buy would be destroyed by d'le experimenters. (One subject was drafted as a witness, for credibility; some squearnl$h QOOSrefused.) They suggest the incruse in prices captures the "eltistence value" people ptw;e on mere existence of the trees.

Kahneman, Knetsch, and Thal« (1990) ran several market experiments. They first conducted a choice-bas sealed off«-hid auction" with tokens of known value, to le$t whether subjects bid their tn.ie values. ('Otey did.) The token market established confideru;e ilia!: the auction mechanism elicited good approximations to true value&. Then they cond.lcted auctions with coffee mugs, pens, and other con!IUltlel' goods. The median selling price for a mug was $7.1.2; the median buying price was S2.87. Ptices did not converge much ai;rosi, fuur trials (one of which was clloseo and played afterward). Since mugs were randomJy allocated to begin with, if buying and selling prices were !he same roughly half the mugs showd be traded, but only alxiut a quarter were.

Franciosi « al. {1993) replicated these experiments by changing the instructions to remove terms such as "buyer' and ·'seller" which, they thought, might be overstating the buying-selling price gap. They observed signific:Mtly lower selling•price values {$5.36 versus a mean of $6.89 in the KKT data) but the gap between buying and selting price& was still large. They also replicated the finding of rnug-undertrailing using a unifornvprice double auction {in contrut to the sealed bid mechlsm used by Kahneman et al.).

Several experiments studied markets for lottery ockets with money prizes. P. Knez, SIIDth, and Williams (1985) lMd M, Knei and Sm.lth (1987) t;'(lfl)J),lrnid hypotheth:fil buying selling prices wllh actual trading in maikeis. SubJ«ts routinely paid more in the t than their slated buying price, or sold for less than iheir rutted mirumum selling price. Trading volume wns only slightly lower thlll1 expected if \VTA = WTP.

McClelland, Sdrulze, and Courney (1991} ran Vickrey auctimns for lottery tick-Cb over gains am! lOsre,,. Elicited buying and selling prices were dos(o together for gain tickets, but sailing prices for insurance on kiss tickets were roughly bimodal {either zero or several times expected value) and were larger than buying prkes,

Harless (19&9) measured buying and selling prices using a uniform price Vick•rey auction with a within-subjectli design. Each subject gave a buying price, im•mediately followed by a ,;eUing price (or vice versa;, and paid or received money immediately after each lottery, The within-subjects design enables a buying-selling price ratio to be caknlared for ea<.h subject, The modal ratio was one, but several ratios were very Large. Thc median and mean of the ratios were 1.3 and 2,7. The low ratios Harless observed &how the greater consistency that can some•times result from a within-subjects design, especially when two tasks follow immediately in tirueM Kachelmeler and Shehata (1992) also measured buying and selling prices within-subjects. The ratio nf median prices in their study was about two, and me difference in prices was highly significant.

Overall, the data suggest that competition or learning in markets reduces the buying-selling price gap somewhat, Iii some settings, but does not eliminate it. The gap is large for environmental and oonswner goods (like wetlands and mugs) and small for lottery tickets. Prcdisp:rnitions of the investigators may also play a role, peihaps through e differences in the designs or domains of application they choose.

2. fa.planm:ions Based on Experimental Artifacts

The experiments have been careful to eliminate several artifactual explanations for the buying-selling price gap. First, it is possible that subjects do not make hypothetical valuations of duck wetlands or pine trees very carefully, or &trategi cally state high selling prices and law buying prices. Direct comparisons indicate there is some misrepresentation, especially in hypothetii.':al seUing prices te.g.,, P. Knez, Stnitb, and Williams 1985: M. Knez and Smith 1987). But in most of the experiments subjects were paid, or got to keep goods they bought or didn't sell, and price gaps persi...ted.

Second, sellers are wealthier than buyers because they own the object being sold, Wealth effects cause a legitimate buying-selling price gap that can be large under very special conditions.[66] Studies show that wealth effects explain essentially none of the observed price gap. Coursey et al. (1987) controlled for wealth effects by endowing buyers with $10 (but see Knetsch and Sinden 1987). Franciosi et al. (1992) regressed prices given by a subject against the wealth accumulated by that subject earlier in the experiment and found no apparent wealth effect. Kahneman et al. (1990) controlled for wealth effects by allowing some subjects to choose between a mug and a sum of money, for several possible sums of money. These "choosers" are in exactly the same wealth position as sellers endowed with mugs, but their median valuation was only $3.12, close to the median buying price ($2.87) and much lower than the median selling price of $7.12.

3. Endowment Effects: Some Psychology and Implications

The leading psychological explanation for the buying-selling price gap is called the "endowment effect": people prefer the things they own, ceteris paribus. Endowment effects are thought to arise from the "loss aversion" assumption in prospect theory-losses are more painful than equally sized gains are pleasurable (see Tversky and Kahneman 1991). The stylized fact that buying-selling price gaps are larger for environmental and consumer goods than for lottery tickets (in most experiments) suggests that the gap is larger for goods bought for use, and smaller for goods, gambles, or securities that are routinely sold or easily valued.

Endowment effects are related to at least five other psychological effects. These phenomena are conceptually distinct, in principle, but are empirically entangled ("confounded") in some experiments. All of them follow from two principles: valuation relative to a reference point, and loss aversion (Tversky and Kahneman 1991).

1. "Status quo bias" is an endowment effect in which having a current choice, or default option, enhances preference for it (see Samuelson and Zeckhauser 1988; Knetsch 1989; Hartman, Doane, and Woo 1991). For example, New Jersey drivers now get cheaper insurance, restricting their right to sue, unless they pay extra. Only 17 percent of drivers paid extra in 1988. Pennsylvania drivers make the same choice, but their default option is the more expensive, unrestricted insurance. More of them chose the default option, paying extra for the right to sue, than in New Jersey (Johnson et al. 1993). (Part of the difference could be due to transactions cost, of course-the cost of filling out a form and sending it in-but experiments in which subjects must choose, forcing them to pay the transaction cost, show a comparable status quo bias.)

2. Buying-selling price gaps can result if people are more sensitive to overpaying (which incurs an out-of-pocket cost) than to selling too cheaply (an opportunity cost), as they appear to be in other domains (Thaler 1980).

3. Ritov and Baron (1991) show that people treat errors of commission, or action, as more blameworthy than errors of omission, or inaction. The reluctance to pay too much is an action error; passing up opportunities to sell is an inaction error. Greater fear of action errors will make buying prices too low; ignorance of inaction errors will keep selling prices too high. Schweitzer (in press) showed that

status quo bias is largely due to a bias in favor of inaction. (Subjects preferred a default option, which would be chosen if no action was taken, even if it differed from the current status quo option.)

4. There is some evidence that the purchase price of assets matters in financial decisions, creating a "disposition effect," because people are reluctant to take actions that create an irreversible loss and are eager to take actions that create gains. For example, trading volume is lower for stocks that have fallen in price (Ferris, Haugen, and Makhija 1988; Weber and Camerer 1992). Casual observation suggests the volume of houses sold falls when housing prices fall. In experimental asset markets, volume appears to thin when bubbles burst (Smith, Suchanek and Williams 1988, see chapter 6).

5. Marshall, Knetsch, and Sinden (1986) asked people whether they would buy or sell objects at certain prices. and how they would advise others. The advice people gave others revealed no buying-selling price gap—the gap disappeared because they urged buyers to pay more-which suggests endowment effects are not recognized or encouraged in giving advice.

An important question is how endowment effects change economic predictions. Tversky and Kahneman (1991) show how theories of choice and exchange can be altered to accomodate endowment effects (which they call "reference-dependence"). Bowman, Minehart, and Rabin (1993) showed how a prospect-theoretic kind of loss aversion could explain empirical anomalies (and predict some new surprises) in economic theories of optimal consumption, savings, and asset pricing. Hardie, Johnson, and Fader (1993) found that an expanded logit choice model, incorporating reference-dependence, fit within-household time series data on orange juice purchases better than a conventional model.[67] Kahneman, Knetsch, and Thaler (1990) showed that trading volume in goods markets was substantially reduced by endowment effects. Hoffman and Spitzer (1993) point out that if marginal buyers and sellers in a market have no endowment effects-if sellers are firms, for instance-then market prices and volumes may not be affected. Endowment effects are therefore likely to have their largest impact when individuals buy and sell on both sides of the market-residential housing, for instance, or some kinds of labor.

Rietz (1991) conducted market experiments that exhibit a surprising endowment effect[68] (and deserves further study). His subjects trade state-dependent contingent claims which pay 1,000 francs (= $.50). A Blue ticket pays off if the Blue state (p = .3) occurs. A Green ticket pays off if the Green state (p = .7) occurs. The states are mutually exclusive and exhaustive, so a portfolio of one Blue ticket and one Green ticket pays 1,000 francs with certainty. Subjects are initially endowed with at least two portfolios of tickets (i.e., two Blues and two Greens) in each period. In addition, each subject was endowed, in alternating periods, with either four extra Blue tickets or four extra Green tickets.

Behavior in the double auction markets for tickets tests various theories of prices and allocations. After sixteen trading periods, mean prices were around 350 for Blues, and 850 for Greens, well above the expected values of 300 and 700. The sum of the two prices was often 1,200 or more. Prices this high are bizarre because suhjeru always began with at least two pairs of Blue and Green tickers. They could sell a pair of ticlrel;,:, which are worth e:<actly 1.000 together, for more than 1.000, but they did not do so frequently enough to drive prices down. Furthermore, rational subj ts should end a trading peri-Od with an allocation ofti_:k. ets that depends on their risk tastes, bot does not de-pend on their initial endowment. They did not: in periods when they start with four extra Blues tlley ended the period with more Blues than when they S(arted with four extra Greens and no extra Bll.le$, (Carlson and Johnson [1992] observe a similar endowment effect in expenmcntal bond auctions,}

The puzzling prices and allocations Rietz observed can both be explained by endowment effects: suppose people value whichever tickets. they s w t whh *more* highly. 'Then Blue-holders wiil ask a high price for Blues and Green-holders will ruk a high price for Greens, which pushes: prices above expocted value. Because people caimot sen many tick.els: at the high prices lhey BSt, their final allocations depend on their initial endowments.

Endowment effects: have a natural application to law (Hoffman and Spitzer 1993). The Coa.se theorem presumes that the valuation of a property right is independent of who owns the right, an assumptton that is questioned by observed buying-selling price gaps. Cohen and Knetsch {1990) suggest that the law frequently recognizes the special losses that result from reducing one's endowment, and assigns property rights so as to minimize those los:.es (though see Hoffman and Spitzer 1993).

K. Search

1. Search for Wages and Prices.

One setting in which individual decisions have direct marker oonse.quences is search. There is a large theoretical foernrure on search (e.g., Lippman and McCall 1976). In a typical model, a person !ooh for work each period. With soiiK' probability, she turns lip li job offer drawn from a distribution. She can accept !he offer or search more. If she decides to take the old offer after <,earching more, it is available with some probability that depends on how much time hes passed. The models can apply to consumer shopping and -Other settings too.

The optimal strategy is usually to set a reservation wage and ru.x:eptany job that pays more. The optimal reservation wage comes from a diflkult recursi-On lot a series of recursions, if the time hori.7.on is finite). There is little empiric.al evidence about whether people search as the models predict. So experimental evidence is useful.

Schotter and Bratinucin (1981) and Braunstein and Schotter (1982) studied several variants of the 1 - i c model with an "infinite" horizon {Le., subjects could se.arch as long as they liked}. Subjects stated reservation Wl\ge$ but were not bound by them. (they could accept lower offer-s}, ln a baseline c.onditioo mean reservation wages were amazingly dose to optimal (134.5 vs. i33) but subjects

searched too little (3.7 .actual periods when 4.5 was <iptimal}, When parameten: were changed-whether rejected offers could be recalled, the cost of ,;ean;:h, the dispersion in the offer distribution-staled !'C$Ctvation wages and the actual acceptance of offers changed in the correct direction but differences were not always significant. 'w'hen risk aversion was induced by paying subjects a concave function of points they earned (anticip;:1ting the method *of* Berg et al. 1986), their reM;tvation wage wwı mnch too low { UO when 1.30 was optimal).

Kogut (I 99tl) studied search in infinite-horizon settings too. Subjects paid a search cost each period {typically $_08) and drew price offers from a known umform distribution, In each trial they paid the price they accepted, and search costs, and earned a known value, Their reservation price should be the s.une each period, which implies that -Optimal searchen; should never reject an offer, then go back and accept it later. They *did* accept old offers, about a third of the time. They often stopped searching too early (even assumıng a large degree of risk a-.ersion). Most of the early stops occurred just before the *lOtal* search cost was large enough to make their profit negative, suggesting some subjects are sensitive ro sunk costi; and not IC marginal cost,; and benefits of search,

Cox and Oaxaca 0989) w.idied search in finite-horizon experiments with twenty periods of search. (Tht:y ch{)!le finite horizons to estabhsh more control than was exerted in the infinite-oorizon seuing of Braunsteiu and Schotter.) Assuming they were risk neutrai, subjects quit searclring at the optimal point 80 percent of the time. Searches ended an average of half a trial too early. As in the Braunstein and Schotrer experiments, -search duration responded in the right direc(ion to s.everal parameter changes, including search t--ost and offer dispersion, but tbe size and significance of the changes was. not optimal.

Cox and Oaxaca tl990J replicated their earlier results but asked subjects to precommit by stating reservation wages that were used to automatically accept or reject offers.,r (Precmnnutment made little difference except for a slight increase in n!ik avers-ion and some learning effects that diMippeared after several trials; Cox and Oaxaca fin pres,,].) Stated reservation wag fo1lowed the optimal path reasonably well toptimaJit:y under risk aversion could not be rejected), except Ibey were too low at the start and too high at the end; overall. subjects seerched too little, compared to a risk neutral benchmark.

Hey ! 1982) studied search in a shopping setting where the price distribution was unknown. (Subjects were not paid.) Recorded statements surtiects made dut• lng the experiment ("verbal protocols," to psychologists) suggested sl rules of thumb. One rule was a reservation wage strategy. Other rules prescribed stopping points dependıng: on the previous sequences of price offers (e.g., stop if the current price il' .above the previous price), These rules might be manifestations of judgment bla<teS discussed in sectioo H, smce many of them try 10 c talize on apparent noorandomne55 in the price series. Hey found some subjec1s u;;mg each of the six roles, or mixtures of tbem.

ln Hey (1987} subjccis were paid and half of them knew the price distrii,utiou. Knowing the distribution increased subjects' use of the optimal reservation price strategy, but financial incentives dld DOI. The ability to rccaJl rejected offers actually hurt slightly (reducing overall profiUJ, whkh is puzzling. And they searched too liUle (perhaps due to risk avenion).

Moon and Martin (1990J extended Hey's work. They spelled out several more alternative heuristics subjects might use. Jn their data, culof:f rules 81.)(h as "'wait for Ilprice k standard deviations above the mean" explain decisior1s roughly m; welt as the optimal theory, Simulations show that beuri9tic rules can be very dose to optimal (only I percent worse).

Harrison .and Morgan (1990) studied several search problems. Ill variable-Sam• pie trials iliei:r subjects could buy a sample of n_t offer;s in each period k. In sequential trials subjects could only sample one price at a time (n_t = I), In fixed-sample trials subjects .could only sample for one period (k = 1), In theory, the extra free. dom in tlte variable-silrnple method should enable subjects to earn about 10 percent more profits than in the sequential or fixed-sample triaJs,

Subjects did exploit the freedom in voo:iable•sample trials. They chose bigger samples than in the n_k = I sequential trials and searched longer than m the k = 1 fixed-sample trials. They earned substantially higher profits too, but lhe increases were not significant by nonparametric tests, The direction and raw i.lze uf devia• tions from optimal sampling are not reported (a result of the authors' obsession with the cost of deviations.), but the fraction of subjects who mooe errors of a certain cosr are reporu:d. SubjeclS are apparently good at deciding whether to keep searching, but not as good at choosing the number of offers to sample each period (cf. judgment errors in section n above. espe.cially ll.D.3).

Since the optimal strategies in these &earch problems are difficult to derive. the subjects· approximation of them ln manj• of lhe expenments ls generally impres• sive. But there are some anomalies, especially in resporu;es 10 parameter changes, And in general pe-0ple search too little. compared to the amount of search recommended under risk neutrality. It would be useful to induce risk neutrality ;;,r · sure the degre-e of risk aversion to tesl whether the observed underseareh can be rationalized by risk aversion, It would also be helpful to koow wbelher heuristic rules could produce such an impressive approximation to optimality {siruuiations by Moon and Martin [1990]. suggest they can), Heuristw rule$ mighl also explam the persistent teodeocy to undemmtch and the relative mabtlity to choose optimal &ample Mzei..

A natural extension is to experimental mlll'kets where sellers choose prices whlle buyers shop arourul Sellers must undersumd how buyen. search to set prices optimally, A variety of theoretical models predict endogeneous price dispersion that depends heaviiy on shopping habit!.!. Grether, Schwartz, and Wilde (198&) report experimental evidence supportive of some modtk

Another intetesting direction ls to reproduce apparent search anomalies from natural setting6, For instance, Pratt, Wise, and Zock:hauser (1979) found that across categories of consumer goods, price dispcrs.ton (standard deviation) was a roughly linεM function of mean price, This finding is rational if iCMCh oosts are higher fur more expensive goods. A competing behnvioral explanation is that

people calculate the marginal benefit from shopping as a percentage. not a dol]ar amount; they search longer for a $5 saving on a $20 calculator than fOf a $50 saving on a $400 washing machine.

2. Search for tnfonnatioo

There are many psychological studiea on the purchase of information that is used for making decisions. The rewl are much like those for search over wages and prices: people are insufficiently seflSltive to factors such as accuracy of infonna• iion and: cost. which should affect search, and overly &eMitive ro factors that should be irrelevant, such as the soun:e of information or the total information available (e.g., ConflOity and Gilani [1982J and references !hey dte}, One study found that providing subjects with a decision aid, which coovetred infurmation into decisioll5 optimally, reduced mistakes in information purchase by about half (Connolly and Thom 1987).

Il would be useful to extend the psychologists' results to economic domains in which the value of information is derived from it'I use in making decisions., such as information markets coupled with asset markets (e,j., Sunder 1991; Copeland and Friedmao 1992; and see chapter 6}.

L Choice: Summary and New Directions

The studiel. reviewed in thfa section sugge:Af a variety of bro.ad classes of anomalies of the m.rtdard utility theories under risk and uncertainty. Many of the anomalies can be traeed to the ideas that values are judged relative to a reference point, probabilities are nor weighied linearly, and decision weightti are not the same as beliefs. Preferences also seem to depend on the way choice objects are described (creating framing; effects), the pt<Jeedure by which they are elicited {creating preference reversals), and oo one's current endowment (creating a buying·selling price gap). The5e phenomena. and anomalies in portfolio choice and the purchase of infommion. suggest people use simple procedures to make cboices, constructing their preferences from procedural rules rather than Jru1xi mizing over weli·formed pretCrences.

At the same time, studies of sean:h and market trading of risky as ts (cf. chapter 6) suggest that models based on maximization are not badly violated (Plott 1986; Smith 1991). FutI.il't research should concentrate on three dasses of explanation (and exploration) for the disagreement across StI.ldles: (1) experience, lflcentive, and discipline in markets combine to create stable preferences which are well approximated by normative models (contrary to the individual choice results); (2) anomalies in the models, which loom large in a large sample of individual choices, are too small to see in markets; or {3) studies of markets and search have not looked at the settings in which anomalies are likely to be large and common. (For example, buying-selling price gaps arc largest with ooruumer goods and smaJlest with money gambles.)

IV. Conclusions, and Research Directions

My perspective in this chapter is unapologetically behavioral. I think the search for systematic deviations from normative models of individual decision making has been extremely fruitful

Economists have had two reactions to data on individual decision making and have made two kinds of contributions, which might be called "destructive" and "constructive." Destructive tests, often motivated by skepticism, are designed to check whether apparent anomalies are replicable, robust across settings, or might be due to flaws in experimental design. My opinion is that some occasional tests of this sort are essential, but too much energy has been devoted to destructive testing with very little payoff. Not a single major recent (post 1970) anomaly has been "destroyed" by hostile replication of this sort.

Constructive reactions of economists to decision research have taken at least two forms. One reaction is the construction of alternative theories to explain anomalies. For example, Kanodia, Bushman, and Dickhaut (1989) show that the failure to ignore sunk costs-managers sticking with projects after learning they are bad investments-can be privately rational, for the managers, if there is information asymmetry about their talent. Theories of this sort are easy to construct (probably too easy); most of the theories posit information asymmetries, then show that an apparently irrational action-sticking with bad projects, following the herd (Scharfstein and Stein 1990), sticking with an inefficient status quo (Fernandez and Rodrik 1991)-is actually rational because the action conveys information. The main problem with this class of theories is that most posit a highly stylized economic setting much different than the setting created in the original experiments demonstrating the anomaly: at best they are sufficient explanations for an anomaly, but they are hardly necessary. Of course, in principle these explanations can be pitted against behavioral accounts and tested (e.g., Berg, Dickhaut, and Kanodia, in press). (I fear many readers do the opposite, exhibiting a "sufficiency bias" by taking sufficient explanations as eliminating the need to explore a behavioral phenomena further.) Experiments can play a special role because one can test theories of individual behavior directly and simultaneously test their implications in markets, rather than testing only market implications.

A nd oonstroctive reaction is expressed by Plott (1986) and Smith (1991). They frame the basic issue as a puzzle of aggregation: why do models that assume individuals behave rationally perform so well describing behavior in market experiments, if individuals behave irrationally in psychology experiments? There are three possible answers: (1) It does not take much rationality to behave nearly optimally in an experimental market; (2) traders learn in market experiments; and (3) market experiments overstate the degree of rationality in naturally occurring markets.

Gode and Sunder (1993) explore the first answer. They show, using simulations, that double auctions can be highly efficient even when simulated traders have very limited rationality.

Learning is a second answer, and has been insufficiently explored. For example, in Cox and Grether's preference reversal experiments, subjects' bids in a period are highly correlated with the previous period's winning bid. That correlation suggests markets are helping traders construct (or "discover") a preference by watching others, rather than simply revealing their well-formed preferences.

The third answer is that experimental markets overstate the ability of natural markets to erase individual irrationality (The best answer along these lines can only come from further studies of behavioral phenomena in naturally occurring markets, which lie outside the scope of this handbook.) Two experimental approaches have been taken to exploring the boundaries of market magic. One approach begins with individual errors and constructs an experimental market in which they might persist, to search for domains in which experimental markets might fail. The other approach begins with market level anomalies and searches for explanations based on individual errors.

Several studies reviewed above took the first approach, creating experimental settings in which individual errors were likely, and studying whether errors were reduced by market forces (Duh and Sunder 1986; Camerer 1987, 1990, 1992b; Anderson and Sunder 1989; Camerer, Loewenstein, and Weber 1990; Ganguly, Kagel, and Moser, in press). These studies mostly show a tendency for prices to converge toward, but not to, Bayesian predictions. Psychologically predictable deviations persist. The data suggest the market glass is both half-full of deviations and half-empty because some deviations were drained away by learning.

The second approach tests whether anomalies originally observed in aggregate experimental data can be explained by behavioral models of individual choice. I mention three examples. Lind and Plott's (1991) alternative specifications of nonrational bidding behavior in low-bid common cost auctions with "seller's curse" is one. Cox, Smith, and Walker (1983) is another (cf. chapter 7 for a longer discussion): they give two behavioral models to explain the observed difference between Dutch auction and first price auction prices, and run experiments to test the models. A model in which bidders violate Bayes' rule, by underestimating the risk of losing the auction if time passes, appears to be the better of the two. Guler, Plott, and Vuong (1987) is an especially sophisticated example. They ran experiments based on "zero-out" auctions for airport landing slots, in which a government authority rebates all bidding revenue according to a known formula. Because of the rebates, airlines could bid much more than their reservation prices. Indeed, bids increased explosively over repeated auctions in one set of experiments and converged in another set. The data present a puzzle for traditional analysis: competitive equilibrium predicted badly (because the rebate formulas meant that sim valuations depended on the bid of others) and Nash equilibrium predicted badly too. They then considered two classes of alternative models of bidder decision making: game-theoretic models in which decision rules are derived from system equilibrium conditions (e.g., winning bidders bid slightly more than losing bidders); and decision-theoretic models in which bidders form beliefs over important parameters (e.g., what the losing bid will be) and update their beliefs. Models of the latter sort presume no game-theoretic sophistication and violate rationality because infonnation is not used efficiently, but they predicted the path of actual bids better than the game-theoretic models.

Theorists, listen up: If a fundamental question for economics is whether individual decision ma.king deviations persist at various levels of economic aggregation (households, groups, firms, markets, societies), then the aggregation question should certainly be studied theoretically as well as experimentally (and in the field too, of course). Theoretical progress is not impossible. In fact, much progress has been made. We now have a rather good underst.anding of some of the procedures people use instead of Bayesian judgment and utility maximization.[11] In the realm of choice, Machina (1982, 1989) has shown that some basic results in economics can be derived without expected utility maximization; many applications have followed (Epstein {in press] reviews some). Gilboa and Schmeidler (in press) describe choice models grounded in the vast psychological literature on analogical "case-based" reasoning. Models of heuristic probability judgment, adaptive expectations, limited memory and costly cognition, preferences that are reference-, context-, endowment-, and procedure-dependent, time preference with hyperbolic discounting, overweighting of low probabilities, and utility maximization with nonadditive probability are now well developed enough to prove tractable for many kinds of economic analysis. For example, variants of subjective expected utility could prove fruitful in explaining demand for information (to reduce ambiguity) and timid behavior that looks like risk aversion but disappears over time (it is probably ambiguity aversion instead).

Naturally, there is a tradeoff between analytical tractability of assumptions-the observed procedures will be less tractable-and their predictive accuracy. But it is hard to know how much tractability is lost in the new generation of behavioral models without trying them out for a decade or two.

Notes

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1. For example, Barro and Fischer (1976) wrote: "A fundamental difficulty with theories of expectations that are not based on the predictions of the relevant economic model [i.e., rational expectations] ... is that they require a theory of systematic mistakes" (163). (They go on to sugges1 such a theory is impossible.) In fact, the "heuristics and biases" paradigm in decision making is a theory of systematic mistakes.
2. The original work, much of it in meteorology, is by Brier (1950). Murphy (1973), Yates (1982), and many others in between. Yates (1990, chaps. 2-3) is a rec.en! review.
3. Call r_j the reported probability of event i = I ... 1I. Then if eve11tj occurs, the q adratic scoring rule pays (I + 2r- - Ir/YI, the logarithmic rule pays log(r), and the sphencal rule pays rj(Ir,1)'. All three rides are "proper" (incentive-compatible). The quadratic scoring rule was first discovered by Brier (1950). The quadratic and logarithmic proper scoring rules were independently discovered by Toda in the early 1950s and reported in an unpublished 1963 paper. Van Naerssen (1962) and de Fmetti (1962) rediscovered them. Roby (1965) discovered the spherical rule. None of the latter seemed to know of Brier's earlier paper (in a meteorology JOurnai).
4. The spherical scoring rule punishes misreports of probabilities slightly more than th., quadratic rule, and both are substantially more punishing than the logarithmic rule (except for misreports near one) (Murphy and Winkler 1970). However. the logarithmic rule requires an infinire penalty if an event that is called impossible occurs anyway. Fischer (1982) fourid that using a large penalty instead worked well.
5. That is, the standard error bars which are reported in early journal articles announcing estilllllles of physical constants, like the speed of light, tend to not include very recent estimates.
6. Lightman and Gingerich (1992) suggest a revised version of Kuhn's hypothesis about "paradigm shifts" in science, along these lines. Kuhn's idea was that the weight of anomaly would eventually topple any theory, when it could be replaced by a new theory that could explain the anomalies. Lightman and Gingerich suggest, instead, that many obseivations are not even recognized as important anomalies--because of misperceptions like the red-hearts phenomenon-until, in a kind of "retrorecognition," a new theory arises that can explain them and that makes clear how anomalous they are for the old theory.
7. In economic rerms, media infonnation provides entertainment and creau.s an extemality by affecting one's stock of knowledge about the world. If consumers do not recognize the knowledge externality, distortions will result For example, people may overinvest in prevl':nting accidents-avoiding airplanes---while underinvesting in nutrition and exercise to prevent disease.
8.
$$P(\text{Blue} \mid \text{id Blue}) = \frac{P(\text{id Blue} \mid \text{Blue})P(\text{Blue})}{P(\text{id Blue} \mid \text{Blue})P(\text{Blue}) + P(\text{id Blue} \mid \text{Green})P(\text{Green})}$$
$$= \frac{(.8)(.15)}{(.8)(.15) + (.2)(.85)} = .41.$$
9. For example, in memory studies subjects are often asked to remember nonsense syl)ables (such as ZEV and ZOV) to deliberately eliminate the influence of prior memory.
10. The informational-wording argument draws on the work of linguists, who show that listeners expect a kind of cooperation from speakers (e.g., Grice 1975). Therefore, subjects may read between the lines of a word problem and respond appropriately to a perceived problem different than the one actually posed.
11. For example, hot hand effects might be important in labor markets for managers who do separable projects where luck plays a role-like movie producers or studio heads.
12. Argote et al. (1986, 1990) and Sniezek and Henry (1989) studied group judgment in Bayesian tasks like those described in lhis section. They found that aggregation of opinions in groups did not reduce errors much, but there are many Opet) questions about group gregalion. Furthennore, group judgment fonns a nlllllllli comparison for marke1-level rests and bridges the gap between traditional domains of economics and psychology.
13. Take all the periods with the same sample (say, O-black) from a particular experimental session, and arrange them in chronological order. Do the same for all the experimental sessions. Now form a 90 percent confidence interval using the mean prices in each of the first O-red periods from all sessions. Fonn a second interval using the second O-red periods from all sessions, and so forth. St.op at N when the number of sessions with N O-red periods falls to three or less. (Note that the sample of mean prices shrinks as one goes along, because l!Ome sessions had very few O-red periods, raising the standard error across sessions and widening the confidence intervals.) Do the same for each of the four possible samples. The procedure is conservative. It treats each session period's mean price as a single data point and draws inferences across sessions (which we can presume to be statistically independent).
14. For instance, inexperienc.ed subjects regan:l a sample of I red (Bayesian p = .75) as being almOllt as indicative of the X state as a 0-red sample (estimated P = .85 for I red v. P"' .923 for O reds). If the mistakes were any bigger they would violate a kind of monotonicity condition that requires P(X I k reds) to decrease in k.

15, Robyn Daw , e.ted >lIOdrer example: d11m.p1sts who believe that t:luld Jbuser>- nev_er Rtmp without thenipeubc treatment h1tvt thmr beliefs reinforced every time 1ln !WJLer vi,rts their dink. The abusers who white the theory never CQ£C to the clime!

1(1, The f<mX'Olll' ,.f i\ raixioru walk ,:.ollected by Dwyer ct al. (1993) MC the nlo&t lndieativ of rationallry of expectanons. But even their data am mi,md: when fore,;.,asv; are pooied th\ir variance is greater than the variance from a naive f = a s t (which is m optimal f = s t for a Wldom walk); and (w(r,tep-llhe d foR"CS\$,hould he exactl, equal to one- ahead fortcasts if subjects know the proce-,s is a random walk, but t:M)' are not.

17. Adapovene. couflicl.\ wilh ra\Jonality of expe;.'tbtion bec•w,e it lmplies 1 hal ?WP use information about a preview; forecast error lo alter their n.-it foreca,'1:. Bm if foroc.ast5 att rational w begm wlth andmnly diotrlbmed around the corre,;:t price—!.hen the previl)lls for«ru.1. error contams no information and ohould bf ignored (leading to b"' zero in. Il.J). Adaptiveness and bitt- in forecast errors are dlerefore Ukly to g<i_hand in h!llld, ut ii is pc,,sible that forecasts could IX' unbiw;ed :md-oot r liring that-torncastcr exhtbil Illtap-tivencs,.

HI. lt rmmeo little whether weights are derived by regre,; ins; pw!(outioomes on ob\<ervlddn; {"acruarial" model6), by randardl:cing oos«Vablea for vari aml wmghting them eqwdly, Of by regres ing expert judpµient, on ob;.;1-.'®lo ("boomrappmg" modcis); they inipmv.i accuracy by dimuding (oowy teg.n:oolQn residunls).

19. After discovering tbeir M.iom of ex utili,y, 'Morgenstern (1970. 809} repons that von Neumann "called out in awmislunent '8l.11 didn': anyoru, . u mat?',,

20. Norman (19 U) mllke> a sint\lar poiul ab<,ut pmduct design. M tedmolr,sy i:n;,rove.. it h easy to dei;1gn produm with more function . TM tricky p.lrt ls \XIIIV<'ying the. f nctioru. to conMlmetli ,o they can be quickly WJJert.tood IOO !.I>ed_ y father bauglu. a VCR dlac can record up to eigbl pt()grt\l'/1& a ytw: in arlvancc. He cannol figure oot how ro r<>{;l]U >:ven ow: program one day m advance.

2L Magical tl!inkmg is akin 10 tWQ false "luv.'8" andtropo}:o11ist$ tudy: the law o f smruhm,y (sitnihw object, &hare propertttt:) and the- Jaw of contagmn (OO_Jem transfer by touching). For iMtiUltt'. some primnive poop!e. even civitiied ehi n. are afraid of pho-tographs or dotl resembling tigen; because trunk tb-Omn>i>j<t !IF a dan n; hgw,; !IF Inrui.te hehefs. like ilurnc !IF importanl even m nmdern uul1nres. Many species o f are endangtmi-the black rhinocem;, Me:w:an sea turtles, some s e ml· nous people desire w eat the aui , or weai; pm1, of them. to !mpl'O"< tm:Jr he:dth nr i.e:ural potency (Gilovkh 1991,cilap, lJ,

22, A» example of m 1U1Cont:rol!able, inf<JI7Mt• uigrutl is lhe pn{omtilnrn 1if other tifltlli m llll induat:ry, Thll effort of oompllly X', IW!la!leTS doo\ oot aft&t othef firm,: p,;rf \tiy much). bul th¢ perfonmnce of other firms ia mfotmativ,; aboul c<mIMJO industty,wide s h o c b that oompany X faC<'d, and hence- 1; in j!.Jdgl.ag x · s pe-rfunnance.

23. E.g., M0r.hina (SS,112, 1987); Sugdea (1986), and Weber and Ciutwm {1987),

24, As Ed.ards 0954c, 3$6) ;,oinwd ow;, the Hieb and Aile» paper "WL; fur ooonoraics some-thing l:!kc the bchlll'iorist revol!.Kloo in psyclwilogy." It wwed the seem; fur modem erooo· misi ' dimlVlt W 1Ul'ley evidence or mtrospi,ct1oa_ and alm(x;:t etthmve relian.;e oo choices a dato for C<)ru!ln!cting theorica of ilchavtor,

25. That J;, crmld theeeffillntyequivilletuX' that wlve5X' p l ! • (1 ~ p;L alrobe got by w!vi/1$ for X: in the dil'ference--cowparnKmu<H,- u{X'l e:'4X')-lt(LJ1 lfso. then thevoo Nffi1Ill111lir Mrugenst.em utility funi::tk,n j,; a rillldc$$ value funcuon (h la 8emouili) loo. While ,nme piwage, Jn V-On Nmum!IOU and M<.qell$ttffi's h o o k an equ!v;,leare, ii is slrilltgly dw.ted in othtt pla,;es, See Fmlhum (in pres;) for w.l ,;,,-egesk.

26. AJ.lus felt that a nc(I-Bcmo1Jlliatt' value ftiar:tioa should uadvrly the thtt:iey of clv;iets imdei:" nllk and rhk avmioa C{IU}d he captllrod hy INW$10'I to ¥.uiance tn vaiue o. di.'ltOftlon of probability. The: Ja;t two hum.lmd o f Allrut and Hag;,,1 (!979) ail ln;; views.

27, They induced v:..!U¢ :oy the player with d;c moo. pomu: If! the end tltoosc ,;aruiy. cigmt:tte$, or cigar, as a prized). lt IS WJJj I!.I)LJll;le !ha! wV<rai $1Jttloct\ were f3CW.ty ir.t:mbl,n

("in many cases !hey were OOv,ave;1 making active nre of !his i l !he&y" 11116});ʼ fucir respoos.es were no iliffetenl

ii_ The Prestoo-Saraua curve, and thore dmved by many othen, CIWIOlbe Y!E11 io re&ci: pm, probability we.gilt,. beca!Jie did not control for t.OnlIINITTty of utilif:y in deriving i t Fwl:hernKm:., they interpreted !hcir weights: ,u psydrok,gkal U'l!M!ations with !he funnnl properties of pmhibiUries (satisfy111g additivity. etcJ which Kaimeilllll! and T\1HU.y'a d«i-.ion ght3 need not satisfy.

29. The Guardsmen were mud! closnr to risk neutral when insuuttirtg ..ge11tt how to bet fₒᵣ tllem. which wggests wmc wilily fur participation as fflk pre{trt:noo'4-bllttthey madebels .

30, Edwa!Uli \' 1954¢) pointed out that bo<h oatcomc,; and probability could be wcigbtro li.nearly or nonlinearly and oomhi.ned, resulting in four po$liiblr. models, fu'poc!ed -IUIIC !inearity of both, e!lpeel.00 utility &.ffllnled linearity of pri;,bmlity only, IIB modtl U$UmOO linearity of GUtcomes ooly {until Hinda 1977 and Yaari 1987}. and Edwards proposed an ei.;tem,ron of expoc1Cd utility in whlch both were OOl!lll\Ut He used !he tetm " utrjective expected utility to refer TOhis 11JIdtsmTI, bur J will r e f a to "nonlintarptob.1:bility Wfll1.lht" in.stead since Savage (1954) defined suhjl!ctive d utility diff¢ffill:!y tlw1 EtWards.

31. m their inflation experiment. Daniels and Ploo {198S) tested whether OOYffi fon;c:med i m - - prices than sellen, reflecting 1lh a d of wishful thinkmg (or rirlm:: of al belief drat forecasting low prices woukl make prices low), lo forty-eight of sevtmy--ti£hl periods :.vera buye;- and £Ell« f WM' differem (p''' .02, 000-Wledi. Thi I a good ex.ample of lhe kind of behaviorally proro<:Wve finding tluu emMget by fl:atheri11g rich& kind;; o f data, and asking more unorthodox quemtrns. th.a!! experimenral eroooru;;tS olWI (lo_

32. That is..$upposea,b,c, anddarezll meli&ltedutilitm;. a >h a n d c > d s h o n t d imply a + c > b + ,L It did, in twenty•nine oflhirty dlSe'£.

33. Tire exeruciating care in lheu"tectwiques is remarkable, Dl !itJ.g: sl!bjn.;;tive prob:nbil· iiy and utility wa& cmwderod me crucial empmcal question in decision ma.kmg iil the 19:50!. (aft-er Ramsey showed how to d o l t in principle !ti 1931) 1111d no effoo wi,s ;;pnred 10 ,Jo if properly, All c h o i .- - operati.maliud with random de-'10e&; Ubjet!s played oome gam• hles, fot" fillhMmttial stakes: instructioos and II lm: of lli'" data IIIC pubu.sltt\d ill !heir mono-graph; subjects were run in up to tbret:repeated r,essioru;; ubjects .u:nu!]y played gambles a: me end, etc. Their msttuctrom reJld (52): "Tiu:scJic:ehav-' b u n made especially for 1», and they an, as fair as dice can be. Ia fact, they h been ground 10 ii:pecifications IICUI''Tm to l/IO,OOOth of an incl!.n

34. This design another =mplaint abou! M!'.1$tdler and Nngee. !hat utility of money Wll confounded wJth util:it:, of gwnblin,g btxau<le subjeci: compared ttrtain !ums (no 1.1tillty of gamMi11g) with g;unbkt. (utility o f gauibling).. ln !he Davidsoneul. design llley oornpared two bets, rather than stming-certaitlty-aiuivalents, S¢ utility Qf gllllllblfog was hcld ti:xed.

3S- This approach is furu!ame11!ally different than that taken by MAchina (t985) 1lnd Crawford (1988} who 800w that with qullfil-oonca'ie preferences peoplepreft'r to tliltd<>mite m choosing bclweea t1lt;, gamble,:, generating die appeamoce of l'tocli&stic choice.

36. The datt. are also crmsiste11t with their regret-b!lSed theory {se., rection 11.H.2 later) which issume5 a-different lo_gic than the altnlmte-compwison process described here. However. the onginal Tversky (1969) data are irn:omistenl with regret aversion. Furthermore, those dobi and the Loomes l!lld Sugden cyele$ can both be eJJ.plailled by an additive-difference model di cu .ed by Tvcrsky.

37. 1lti$ behavior is consistent wilh a nwltiattribure uti!i model in which eath llttnhute has a separable utility, tuid the attribute utilities are added to der.er:minc the alkrnalive's overall ut.ilicy. But cliruinati11g high-rem a.partments enlitely implies that the dj5yti.Jjty from ren1 above ₛₒ₁₁₁₄ threshold is negatively infinite,

JS • .fut YOUI'id!: hlmd in bot wate:r and your right hand in oold water for a minnte. Bolh will adapt ro 1he tempt,a;tures of the water in which they sit. Then put both hands in lhe sarm !Uh of w u m Jl.1ller. Siner, your bands are sensitive to dumges from the temperature !.;:-vol to whk:h.

Ibey have adafXN. the hot left himd will feel i:oW.:c; in\O cold kft hwid will tee! warmer, even tiw11gh the water te111p,mirure (hey both feel ii; procisiel:y the same. Pt ocy as&LllLeS a !llmilar k>rul of adaption WQll'i in judging utisia<.;fioo w'1h ,nonew,ry (.i\00 o!herJ om«ime,.

39. For example, sobjec15 are roughly md:Jff¢Ts'fit betwten getting nothinz_ lli!d accepting a coin !Up -$JO Md $XwhenX 11 around 25 tTvm:k) and Kal:meml!fl 1991,1.

40. The bretidrti of M.ai::hina's tectmical cootriburion rn.itht be questioned <n empiri,;al gtoood... His t m - f<J<"eumple, shJwed Ural if pOOple irnegra.e asseI.S Md ll!e "globally" 11,k a,;erse-duitcach of the "local 11tillty furu::Mni" use<! w vlllue different gamble exhlbrt risk n-Uii:m many tions of EU lllDdcls as.urning a single m k an:rw 110lily func-tion wculd lwld. But the u.cl'ulness of his rem.llOOlbk proof is underan hy empirical ev1deflla: that \(CS_ un!ity functions an: nm unifotmly ml< averse Of risk seeking tsi::e eope<:ially sec• li00$ IDD and lll.H later}; m tiw; u-· pan of his "if,tlit({- proof appean; q11eruon&ble.

41. The implication of weakrning other pi<:uru; have bffln lhoroughly worked oot 100. Au· UWP (1962) iliowtd that weakening oompietcnei.s to a,;yclkity of preferen= (X > Y. Y > Z i m p lie5 Z ;·; X- l : 15 not preferrtd to XJ yickls a unidirectional p.uuat ocder in which u(X} > ;;(l') i f X > Y!b\it ii(X) > ..(Y) does not alw imply X > Y, because prtferences ;;;an be inoompkllt) Hausner (1954) and! Chiprtlan (19601 showffl that weakening coolinnity yields a ve.::ror ntihry i::epresentat:1/In over whi<:h preferences att le!!:i&Ol);l'aphic, mt.her ilnm a wigle real-v"11.md l'lmct!oo.

42. Muchina \ 1982) iletined fanning Otl formally as followil: S u w - a gwnble X tocbasticaHy domma Y (ThenXliel lu the northwefll of Y- bigher P n)om:,r P,,-in a triangle diagf1iln 1 lnditferen,,—e curve& fan out if the slope of their tangent hl1-: at X u gtt:.i!W Ulan me slope at Y' Fanning in ts the oppo .e: too t;U;pe .u Xis smaller itraa 111 Y;

43. In genernl, welgbting funcooni ha'>!! two 1mµortarn femmes, the location of lhe ;;n>>!}<>Ver point where M1" " p, and the gr« of CW"latture 1be <me p!,ll'!JUCter form In equaoon (S) d<l<S not allow t,om ieatureli to mmie indepew.iePtly; the N<>AfOVU poi.nl falls as c w v - bcrotne!l greater, Addi!lS a woond paramettr dcwuvks th!liC t-..o graphieal propenies.

44. The R-O compariwn ;.ices nQt quilt te$l farming out, because all We gambln lyrng along lhe O-pair chord are not 11tocIta3tically dominated l:;ji the R-cbooi gamblffl;, Thu,,,. om: .:an con• &l'Ut ml 1llllm!al pkttem of JOCH utihty functions that flm iNt and g¢Mf!ltt, the observed data. This is a good enmple of how a small dlfferelWtl in the de½igo of Pll,irs {pulling the Ptt in r.lle riskier 0-pl!lrgamble down beli;;w the PH in the l-ess-0.Sky H•piur gamble) mak¢5 a big diffffl:m;:e betlVeen a d w i te;,t of fanning out Mid an appro;,:i(llat(, Wit.

45. Many of the judgment Clperiments u,ed between-.ubjc,,'t dei;ign becaut¢ the)' search bx moon, ie; in j11dgtMIlU1 in two settings. The optimal choice of defilgn ;;k.p,;nds on !he netld for statistical powi:f (wtthln i1 more powerful thm between). the nb.)Ccis· toleranre fur repeated 1f18h {high tolerance permib ;;,Wml), ind !he ern:in from overstatiug.rtn<l undentat• Ug; ,;:onsisten...j' ntlmve to natural Mttmga \the conventional view ia !hat within ovtmt111e:„ betwun understales. w t f <ll n0l: know of much cvWence on preci,;ely th!• point}.

46. Earlier !!l!.Jdies (e.g., M<»lcllef !Ind Nogee 1951} gadiered &uch data by giving ubjt :ts ldenti.:al cilciCt$. many times. The danger in that making so many (:hoo:e,0 may ov,;rwhelm subje..''U and mflY mduu them to use simple ro\ei; that m;;sqw:;&oo as conformrty to thwiy \al. tlala in Slovic, Llc!rtt:nswin, w:td Edwatds {!965] sugp).

47. Wb!lther t:!l.pcrinw have subject/\ play gambles i$ not oorire!y a product of background or modem rllS'N c=ioosnt'li. Siegel, Coomb. Edwards, Slovic, Tverslcy, and oilier psyeh,:,loJ.i&ts ofteu had illh_ioon; play gilmblts in lhl'- 1950. lfill'.l 1960!,. Some felt lhnt playing gambkij mad!: littk differem:e and quit dmng it; othtn think playtng gam is important.

48. Neilson (1989) wggests- a c\('Ner alre!'Nidve UeQry in wh.ieh l<tiliti,n rise when gamblet hll.\l! fewer powble oull:Ome&, His lhwfy ,ivoids. nonlinear wei$hting of ptobabtlities en-tirely. This theoiy violates cootinwty. and cannot e;,;piJin all the,:; d1ua, but di:ser>!K further elqlloration.

49. Theit modd use, ,i Taylor sllrie5 i:;xplD.l5iQn tu lm/!1.f.expected utility by 5<lYerai terms

wlm"< coefftc1eots can be ¢.ilimared. 14 !ht EU approach, \h,; o;ietfu::iem of the variance of repa.i; prot,uhilicy {in thcir now.ion, (p - p/) should be :tUQ. The coo.ffi.cient llppeaf$ to be negative (with a modc,n t-statistil: of 1.57), whith ru a wei.glltlf18 fwtaiong(p) that is concave aromtd the low reparr probability of .00:5 (as pm.ikfed by prospect theocy and !lOtlle othn account&).

50. Subjects were ;;bown an i!litiul gamble on ettht(the left or !tlW¢t triangle edge then b d to name a gamble 0l1 the hyp;xenme, wherep,,, "'0. which was tqUivatrn(in preferenc.l':{J.e.. a point nn lht wne rndiffetM(e curve). Subjects then namud anc=ive mkriof)lOints tblll. wen- ua.Hy preferred ur1til t.hrte m five poims on ll singJe i.ndiff'crencc CUlVe were fuund. (SI dki .not play any :gamble'J.)

51. Real (1991) ,:tmlied too behavior of bees "isiting" urtifidal flowert widt differmt probll-btlil)I di tribution, of fltClw: {controllt>i by the experim,mler). He diseoveml thal bee apt'lelU" ro \Wdoo::snmate tiW probabilities of getting rew\mlei.l. "with nectar, and overestimate high probabilities. which hurts at m important cro,s— difft.rtl:l.tc in pmbabilit)' perception between and people.

52, As the twlJ probabilities foU (m;limainmga «mBtant ratio), iffllllllling out is OlY people will 1:«ume more T1$ seeking and choose value. of A.

53. Elisbta\l alluded ro "A large number of niws.. undet absnlute!y ial coodi-li-Om" w.ggesting ambiguity av=ion ls the majority ! of-choice.

54. The -OD!oc red was cbn!len by a ooin fl.ip, after "the group was llSkKd if there was aoy obj«OOll 10 flipping a coin ro determint the. winning cllWf fur the game to be playt:d" (67), ln other experiment$ wbjects could «wose a color 10 bet oo, oc were Mlked their certainty• equivalents fot bth on both t;:.),:m;.

5;t A third ptfocip.lt i$ «mteM invariance: pref= for ao ohjfl:'t not dep(>nd (Ut the set uf clloices from wh.ieh u CM be picked. There are some illtere!;lfuf: Yl{>lations of th!! pnncipllnoo \Huber, Payne., and Puto 1982: Tversky and Simonwn 1993), The lottery com:• lation 111:fuct Pbsen-ed in H1.1dle'i of regret, dm:ribed in s«:tion HI.H.2 later, afi: llilOtha-eu.m• pk, because they snow th!ll: preferem:e flJT a gamble in a pairwise depcnd:s in uysu:m-att;:: way on the gamble if is paired withs

:56. In illflllY c.hmce e:tperiments, the curwh1tion of outeomes i, not e;qilid!ly fu\00. Regret may cause EU vi.olatiom if h.lve particular «>m.':iaoons in mind.

Baruilio, Kagel. and Jiumyakul (1990), Harless (!Wu}. Camerer (1989a. 199"2a}. Stamter and Sugdun {1987b} u!i<I di playi mat rontruUeii fur regret effects and still found EU viola· lions; Tversi:y and Klll!Mman (1992) cootrolb.t for regret and fmmd SEU Vlo.latiMS {using bets on IIIIW!al evenlll) flow-ever. it h !mt that the mm;t stri ng: EU .ioiatilll'J$ Appe!TTW 111 SlUrdie$!hat did not ::.ootrol for rtgtef (e.g., Prelt(1990. and Conltsk 1939). Loomt:s Md Sugden (1987b), LOOUteS {198&!.l, and Starmer and Sugden 11989a) foomt WJ Rgrel eff«ul llllCllllll for purt of Lht, eommoo ratio (llld ommoo eo:n e effr(.,•t„ but not all.

:Yl Notice that violating reduction in mndom-garuble $CUing5 by ioolating eadt pair ;:cmo-m:iw; on mootiil effort: ro obey teductioo reqllmr, mukiplying pmboo.il.ities and dloo,ing & portfolio of gambles to e !he ,::b(,ice o{ llfl e:,;trelll<!;l)' (;:Qfflpl <:Ollipound lm.tery.

58. Si®larly, llll .enrichcJ" pernrui like thtl emerwoot Madonna is likety to ht both married freqHendy !d,osen) and di freqnc:ndy (rej; a bu&ine&!l with m.any Ul'f!Wlal feature:i rnigh! be a likely cilndldate fur both !ie4ll1Sition am! diV{tttment.

59. T'v-eNky et al. also !Md a pro,,;.eoon: 10 1lV<lid eritkism of BDM, like Co:,; and Eps(dn't, proce,:1.ure, ie wlooh sut;,j«ts sf.a.led that were nl!td oozy lo rank hew. All dlffe«mce is thru 'Albjeci:s priced h bet in a pair $Cparatdy {rattw Ullll ly, /l5 in Co!i anti Bpsr.cin, which to make pricing m:w:h like choice). TYfflky ct aL a],o ilbm,,-e,1 a dramatic rate of reveNa!s iu u novel inrcrternpoml choiCl:' coolext. I"eof,le much II'I(II+ impatieru when dlom:ing itraa when pricing; fl>t ¢,<ample, 57 pt,n:tnt nf 8Ullojem preferred $1.600 in eighteen mornhs to Si,$00 in five y-cm, hut only 12 percem pnced the S1.600 bond lnghe'I-.

60. 1.klhm's experimMI strikes 1M as an txmup!e ofoo.lt the best and mt wol'M of experimental econwucs methods brought to beat on a y-inspired phenomenon. The ecoon.

mists' intuition that people might behave differently if they are highly motivated drove hi, extraordinary design, creating a test of robustness to incentives that few psychologists would ever conduct. At the same time, his odd chmce of objects (Volvo and Opel) violates the basic recipe for producing reversals that has been well-known since the early 1970s: that is, have people choose between two objects that are high and low on opposite attributes, then "price" both objects (or adjust one to indifference) using a rating scale that matches one of the two attributes. A poor choice of objects meant the ingenuity of his design was largely wasted.

61. A lower value of *r* implies less error in choosing, so more P-bettors will actually choose the P-bet. Since reversals occur when P-bettors price the $-bet higher, an increased number of P-bettors can raise the overall reversal rate.
62. Such valuations are often called "contingent valuations" because they are designed to measure how consumers value goods *as if-or* contingent on the assumption that-there is a market for the good.
63. When means are used, WTP = $3.45 and WTA = $4.71 in the final auction; when medians are used. WTP = $1.33 and WTA= $3.49.
64. In their auction, bids were not stated directly. Instead, buyers (sellers) were asked whether they would prefer to buy (sell) a mug or not. al each price between 0 and $10 (in $.25 imef\lals). Their responses were used to construct demand and supply cuf\leS; their intersection determined a market price and who would !rade. Thus, subjects' bids were constructed from choices between thl' good and a series of poteniial market prices. (In psychological terms, bidding is a choice task rather than a "production"' or valuation task.) The procedure seems to make bidding one's true valuation more transparent; conversely, it makes strategic underbidding to affect prices more opaque. (Strategic underbidding can be optimal because the risk of failing to buy at a profitable price, because of underbidding, is offset by the potential gain from being the price-setter and lowering the price.)
65. As discussed in a section III.D methodological digression, the within-subjects design may overstate subjects' consistency (compared to the degree obsef\led in between-subjects designs) if subjects think consistency is demanded of them, or if the equivalence of buying and selling is more transparent and if the buying and selling tasks are conducted right after one another.
66. Hanemann (1991) poims out that the wealth effect can be large if income elasticity is high and the cross-elasticity of otber goods with the good being valued is low. These conditions might apply to some environmental goods (such as glorious beachfront property or mountaintop views) but they only apply in experiments with more mundane goods such as mugs under !be absurd presumptions that (1) there are no good substitutes for mugs and (2) sellers prefer to spend most of their mug-wealth on mugs. (The condition (2) is empirically indistinguishable from the endowment effect, and can be considered a formal restatement of it.)
67. The reference-dependent model also explains the finding that cross-price elasticities appear to be asymmetric: high-quality brands gain more market share from low-quality brands by cutting prices than vice versa (because consumers do not want to give up the high quality 'in their endowment').
68. Kahneman, Knetsch, and Thaler (1990) suggest that endowment effects are *,wt* particularly common in markets where goods are specifically bought for exchange, but Rietz's results suggest O{herwise.
69. Call lhe optimal reservation price in period *t* P_t. If llll observed price P' is rejected one period, then P' < P_t. Since the resef\lation price should be *P* in all periods, if P' < *P*, in one period it is less than P, in all periods (since P_1 = P) and should never be recalled.
70. One motive for eliciting reservation wages directly is that the percentage of searches ending ai the right time is a weak test of whether behavior is optimal. Cox and Oaxaca (1990) calculate that a naive subject who used the mean of the offer distribution as a reservati.on wage would end searches optimally 75 percent of the time. Compared to this benchmark, the fact that 80 percent of searches ended at the right time is unimpressive.
71. I am always surprised that economist do not eagerly or routinely tum to the psychological models as viable alternatives, or at least to psychological facts for theoretical inspiration. Of cow-se, the psychological ideas usually do not come packaged as economists need them. Psychological models are often parameter-heavy or expressed verbally, since models are not usually constrained by severe analytical or econometric demands. Refitting the models is a chore economists must do for themselves. I do not see how psychologists can offer more help than they have already, in papers such as Tvenky and Kalmeman (1991).

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