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## The Domain of Subjective Economics: Between Predictive Science and Moral Philosophy

James M. Buchanan

*We . . . are in part living in a world the constituents of which we can discover, classify and act upon by rational, scientific . . . methods; but in part . . . we are immersed in a medium that . . . we do not and cannot observe as if from the outside; cannot identify, measure, and seek to manipulate; cannot even be wholly aware of, inasmuch as it . . . is itself too closely interwoven with all that we are and do to be lifted out . . . and observed with scientific detachment, as an object.* —Isaiah Berlin

### Introduction

Any discussion of the methodology of subjective economics must at once confront an elementary fact along with a necessary hypothesis. That fact is that, in any science of human behavior, the observer is himself among the observed. The hypothesis is that human beings *choose*. Without this hypothesis the activity of the observer becomes meaningless exercise. The fact and the accompanying hypothesis impose constraints or limits on any “positive economics,” if the model is taken from those sciences within which these attributes are missing. The natural scientist remains separate from the objects of his observation, and, despite the acknowledgment of the possibility of mutual influence between observer and observed, there remains the basic category differentiation. Furthermore, the simple ability to put these words together in a meaningful sentence distinguishes me, as a man, from those objects of science that most resemble me, the higher animals. By the process of writing a sentence, I am choosing what I create; I am not merely reacting to external stimuli, at least in a sense readily amenable to prediction.

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I am indebted to Pamela Brown and Karen Vaughn for helpful comments. Precursory ideas to those developed in this chapter are present in my essays, “General Implications of Subjectivism in Economics,” and “Natural and Artifactual Man,” in my book *What Should Economists Do?* (Indianapolis: Liberty Press, 1979).

In summary terms, the *subjective* elements of our discipline are defined precisely within the boundaries between the positive, predictive science of the orthodox model on the one hand and the speculative thinking of moral philosophy on the other—hence, the chapter’s title. For our purposes, I define *moral philosophy* as discourse that embodies an explicit denial of the relevance of scientific explanation. Note that this approach does not require a categorical rejection of the relevance of empirically testable, positive hypotheses concerning certain aspects of human behavior commonly labeled “economic.” Nor does the approach rule out the relevance of normative moral philosophy. The approach emphasizes, instead, the existence and the importance of the area between empirical science and moral philosophy. It denies that these categories of thought span the universe of relevance. On this point, I think that my own professor, Frank Knight, and Ludwig von Mises would have been in substantial agreement. Both would have been extremely critical of the modern economists who seek to rule out any nonempirical economics as nonscientific and, by inference, normative. Both these seminal thinkers would have been comfortable with a science of subjective economics, although they might have differed somewhat on the relevance of any other part of our discipline.

### Adam Smith and Classical Economics

Classical economics has been almost universally interpreted as an attempted, and ultimately failed, effort to derive an objective and predictive theory of the relative values of commodities. The central features are perhaps best exemplified in Adam Smith’s famous deer-beaver illustration, which I shall use here. Smith’s hypothesis was that one beaver would “naturally” exchange for two deer in that setting where two days of labor are required to kill a beaver and one day of labor to kill a deer. I want to ask the following question: Even if we grant all the required presuppositions of the Smith model, do we then derive a genuinely predictive theory of the relative values of beaver and deer? Or do there remain necessarily subjective elements in the inclusive explanatory model, even within such an extremely restricted setting?

The required presuppositions are familiar. Deer and beaver must be “goods” to all potential consumers and producers: labor must be a “bad.” Labor is the only productive resource, and units of labor are completely homogeneous. Further, each commodity must be producible at constant returns. But we must recall that Adam Smith was seeking to explain *exchange* values. The restrictions of the model, even if fully realized, do not explain the emergence of exchange, and, in the strict sense, no exchange would take place in the setting postulated. If the input ratio is two for one, precepts for rationality suggest that each behaving unit will attain an

equilibrium adjustment when the two-for-one ratio is equated to a two-for-one valuation ratio for the two goods. There is no subjective element in the analysis, as I have deliberately limited the scope for the term *subjective* here.

Adam Smith and classical economics were not, however, interested in explaining individual behavioral adjustment. Smith was interested in explaining *exchange* values. And, to explain these, he had to explain the emergence of exchange itself. To do so, he must have incorporated an additional presupposition not listed. The productivity of labor when specialized must be higher than when unspecialized. Smith's emphasis on the importance of the division of labor suggests, of course, that this presupposition was indeed central to his explanatory model. But why would exchange emerge in the first place? Here Smith resorted to man's "propensity to truck, barter, and exchange one thing for another."<sup>1</sup> The critical role of this propensity in Smith's analysis has been too much neglected in interpretations of his work. But with this propensity, Smith places a subjective element at the heart of the whole explanatory model. He quite explicitly contrasts the actions of man with the animals in this respect when he says that "nobody ever saw a dog make a fair and deliberate exchange of one bone for another with another dog."<sup>2</sup>

In some preexchange setting, the exercise of the "propensity to truck"—behavior that must necessarily have been different in kind from that which had been reflected in established patterns (and, hence, predictable scientifically, at least within stochastic limits)—allowed man to discover the advantages of specialization and to create the institutions of exchange within which relative values of commodities come to be settled. The person who initially imagines some postspecialization, postexchange state and who acts to bring such a state into existence must engage in what I shall here call "active" choice. He must do more than respond predictably to shifts in the constraints that are exogenously imposed on him.

An economy (if indeed it could be called such) in which all persons respond to constraints passively and in which no one engages in active choice could never organize itself through exchange institutions. Such an economy would require that the constraints be imposed either by nature or by beings external to the community of those participants who are the passive responders. In either case, such an economy would be comparable in kind to those whose participants are the "animal consumers" examined by John Kagel and Raymond Battalio, and their coworkers.<sup>3</sup>

Even at the level of Adam Smith's most elementary discourse, there are two interpretations that may be placed on his analysis. If Smith is read as relatively unconcerned about the emergence of exchange institutions, and if he is assumed simply to have postulated the existence of specialization, it may be argued that his aim was to present a positive, predictive theory of the relative values of commodities. On the other hand, if Smith is read as primarily or centrally concerned with explaining how exchange institutions

emerge, he becomes a thoroughgoing subjectivist in that he resorts to that particular propensity that distinguishes man from other animals. There could be no predictive science concerning the exercise of this propensity, since to predict here would imply that the direction of all future exchanges would be conceptually knowable at any point in time.

The two interpretations of Smith's basic analysis differ in their *explananda*. The first involves an explanation, or attempted explanation, of relative exchange values of commodities. The second involves an explanation of exchange institutions themselves. That which can be predicted (conceptually) can be explained with an objective or scientific theory. That which cannot be predicted can be explained (understood) only by a subjective theory. If this basic methodological duality had been accepted at the outset, much confusion in the history of economic doctrine, then and now, might have been avoided. Subjective economics, properly, even if strictly, defined, occupies an explanatory realm that is mutually exclusive with that properly occupied by positive economics. If this much is granted, however, the relative significance of the two realms of discourse for the inclusive understanding of human interaction becomes clear. Positive or predictive economics becomes largely exercise in triturating the obvious; subjective economics can offer insights into the dynamics through which a society of persons who remain free to choose in a genuine sense develops and prospers.

In subsequent parts of this chapter I shall illustrate this basic argument by reference to somewhat misguided and at least partially confused efforts to emphasize the subjective elements in economic theory, broadly defined. I shall discuss the so-called subjective-value revolution and its transformation into the modern neoclassical synthesis. I shall discuss also the dimensionality of economic theory to show that the dimensionality problem should be considered separately from that of operationality of theory. A discussion of the particular Austrian variant of neoclassical economics, as exemplified notably in the works of Mises, follows with particular emphasis on his insistence of the praxeological foundations of the discipline. The following section discusses the potential applicability of subjective and objective economic theory, and I shall offer a provisional explanation for the relative dominance of the latter in the postclassical century. Finally, I shall summarize the argument and draw some inferences for the direction of research.

### **The Subjective-Value Revolution of the 1870s and the Subsequent Neoclassical Synthesis**

As noted previously, classical economic theory was widely interpreted as an attempt to derive a predictive theory of the relative values of commodities.<sup>4</sup>

Classical economics was acknowledged to have failed in such an attempt. Emphasis came to be placed on the specific difficulties that could not be satisfactorily met with the classical models. The diamond-water paradox remained; the classical effort to explain relative exchange value by objectively measurable costs of production could not survive.

The so-called subjective-value revolution, presented in various ways in the early 1870s by Jevons, Menger, and Walras, was explicitly aimed at resolution of the prevailing difficulties in the classical explanation of exchange values. The early contributions here demonstrated that relative values depend on schedules of evaluation on both sides of the markets for goods, on demand and supply. But we must ask a question here that has not, to my knowledge, been frequently posed. To what extent does the economic theory of Jevons, Menger, and Walras, or their neoclassical successors, embody genuine subjective economics as I have defined this term? Despite its label as the subjective-value revolution in economic theory, are there any necessarily subjective elements in the inclusive explanatory models that were offered in place of the discarded classical edifice?

I suggest that the label *subjective* may be misleading in application to this theory of exchange values, notably so as the initial contributions were redeveloped and refined into the neoclassical synthesis of the twentieth century. The marginal-utility theory of the 1870s embodied the central notion that values are determined at the appropriate margins of evaluation and that the locations of the margins are relevant. The diamond-water paradox was thereby resolved satisfactorily. But there is nothing in the whole analytical framework here to suggest that the evaluation schedules (those of demand and supply), which simultaneously interact to determine the location of the margins and hence exchange values, are not, themselves, *objectively determinate*, at least in a conceptual sense. There is nothing in neoclassical economic theory that precludes the universalized existence of simple reaction patterns of behavior on the part of all persons in the economy, reaction patterns that, even if more complex, are still analogous to those that might empirically describe the behavior of rats. Once individual-utility functions are formally specified, individuals whose behavior is thereby depicted cannot choose differently. Choice, as such, cannot remain in any such formulation.

I am not suggesting here that the objectification of the solution to the problem of determining relative exchange values of goods (and bads) was necessarily central to neoclassical theory. It was not. The earlier classical effort was aimed to provide a single, and simplistic, objective measure of relative exchange values that might be both readily understood and empirically estimated. The neoclassical effort, in contrast, was primarily aimed at resolving difficulties at the level of logical coherence and rigor. There was a shift of emphasis from attempts to provide empirical bases for measure-

ment toward attempts to offer understanding of the whole logical structure of economic interaction. For the latter purpose, the issues involved in making empirical estimates or predictions about relative exchange values do not take on critical significance. These issues tend to be overshadowed by those concerning the derivations of proofs of the existence of solutions to the complex interdependencies that the economy embodies. That the empirical measurability or predictability of exchange values does not occupy center stage in orthodox neoclassical theory should not, however, be taken as evidence that, conceptually, such measurability is categorically impossible. The focus of neoclassical economic theory, in comparison with classical, is shifted from empirical estimates to analyses of structures, but there is nothing directly in neoclassical theory that implies the absence of conceptual predictability. If utility and production relationships are ascertainable, solutions exist and are determinate. It is meaningful in this context to make an attempt to compute equilibrium prices.

### **The Dimensionality and Data of Economic Theory**

My purpose in this section is to clarify possible confusion and ambiguity that may arise from my somewhat restricted definition of subjective economics and from my claim that the term subjective-value revolution as applied to the contribution of the 1870s, may be, in this context, misleading.

It is necessary to distinguish carefully between the definition of the dimensions of the space within which the operations of economic theory are performed and the operationality of the theory itself. My narrowly restricted definition of subjective economic theory is relevant only to the second of these subjects. As I have limited the term here, subjective economic theory embodies those elements of explanation of the economic process that cannot be operationalized in the orthodox sense of predictive science. For those elements of economic theory that can be operationalized, however, I have advanced no presumption whatever about the dimensionality of the space.

Confusion necessarily arises at this point between the claim that any economic theorizing must take place within a subjective-value dimension and the totally different claim that, because of the subjective dimensionality, an operational theory is not possible. The first of these claims must be accepted. Economic theory is surely concerned with evaluations, with values. It is totally misleading to think of physical dimensionality here. Goodness and badness are qualities that are assigned to physical things, to commodities or services, by personal evaluations.

The naive and simplistic efforts by the classical economists to derive a predictive theory of relative exchange values tended to obscure the value dimension and generated the absurdity that commodities may be produced

by commodities, somehow independently of the evaluation put on these by persons. In the sense that it emphasized and brought to full realization the essential value dimension, it is appropriate to label the 1870s effort as a subjective-value revolution. But, as I have noted, this corrective shift in implied dimensionality of the space for the application of economic theory carries with it no direct implication for the potential operationality of the theory itself. Indirectly, of course, there is the obvious implication that only if economic theory applies within a value dimension could there arise any issue of nonoperationality. Subjective economics could hardly be discussed in any analysis of variables in pure-commodity space. On the other hand, however, there is nothing in the value dimension itself that logically prohibits the derivation of a fully operational science. Whether or not such analysis is possible depends not on dimensionality but instead on the possible uniformity of valuations over persons.

A related source of confusion involves the informational requirements that a thoroughgoing recognition of the value dimensionality of economics places on any putative scientist who seeks to derive empirically testable hypotheses. F.A. Hayek, in particular, has emphasized the value dimensionality of economic theory and the informational implications of this attribute for the organization of society.<sup>5</sup> Markets utilize information efficiently; they do not require extensive centralization of information about individual evaluations. And, indeed, the informational requirements for a centrally planned economy may be practically insurmountable. There is nothing in the basic Hayekian insight, however, that precludes the possible derivation of a set of conceptually refutable hypotheses about the evaluations of all persons over all goods and services.

In earlier works I have stressed the subjectivity of costs, and I have tried to show how errors arise in applications of economic theory when this basic dimensionality is overlooked.<sup>6</sup> In the restricted classification scheme that I have suggested in this chapter, however, there is nothing in my analysis of cost, as such, that precludes the derivation of a set of conceptually refutable hypotheses, which is, of course, the criterion of a predictive theory. Costs are, of course, related to choices, but if there are sufficient data on the environment of a past choice and if the chooser's behavior is, in some sense, predictable on the basis of observed uniformities, choices may be judged *ex post*. Practically, the subjective-value dimension of economic behavior may make enforcement of any cost-price rule impossible, but such application of the predictive science cannot be deemed conceptually impossible.

### **Mises and Praxeology**

Mises explicitly denied that economic theory can be operational in the orthodox meaning of this term. Economic theory was, for Mises, necessarily *a priori*; it offered a pure logic of choice. In taking this extreme position

methodologically, Mises seemed to be aware that attempts to force economic theory into the straitjacket imposed by the requirements for predictive science must, at the same time, deny to persons who act the possibility of making genuine choices.

I shall confess here that I have never been able to appreciate fully the Misesian emphasis on praxeology or "the science of human action." Central to this conception is the purposefulness of all human action. Man acts always with a purpose; he seeks to replace a state of relative dissatisfaction with one of relative satisfaction. However, an observer can never get inside anyone else; he can never know what a person's purpose is. Hence, there is no way, even conceptually, to predict what action will be taken in any particular circumstance. A person chooses that which he chooses, and when he so chooses, he must anticipate that the chosen course of action will yield a net increment to his satisfaction. Although he may err, we can never infer, *ex post*, that he acted irrationally.

At its most general, this Misesian theory of choice is totally nonoperational. It can "explain" any conceivable course of action that a person might be observed to take; the obverse is, of course, that the theory can really "explain" nothing at all. Mises himself did not worry about nonoperationality as such, presumably because his reliance on introspection provided him with a basis for sorting out meaningful from meaningless explanations. To return to the Adam Smith illustration, Mises could claim to have explained why exchange institutions emerged from the vision of some person who imagined the mutual advantages of specialization and exchange. Mises could also explain the relative values of deer and beaver quite simply as those exchange ratios that emerge from the purposeful choice behavior of participants in the exchange process, whose acts of participation or nonparticipation are themselves purposeful.

Misesian economic theory becomes strictly subjective economics in my earlier definition of the term. But my basic criticism of Mises is that he claimed far too much for the subjective-economics domain. He seemed to want to preempt the whole territory when he totally rejected the existence of any relevant domain for what I have called positive or predictive objective economic theory. This somewhat overzealous extension of methodological frontiers may be at least partially responsible for the relatively limited reception that the ideas of Mises have had among economists, catholically classified.

The basic Mises conception of praxeology seems flawed in that it appears to incorporate two quite distinct sorts of human action, one of which may be analyzed scientifically and empirically in the orthodox sense. Consider two examples: (1) A man is walking along a road; he sees a car approaching; he jumps to the side of the road to avoid being run down. His action here is purposeful. It is surely aimed at removing a potential state dissatisfaction and replacing it by one that is preferred. (2) A man is walk-



ing along a road barefooted. His feet are sore. He sees some cowhide and he imagines the possibility of shoes. He acts to make the shoes from cowhide. (My thanks to Israel Kirzner for this exmple.) This action is purposeful, and it, like the first, is surely aimed at replacing a state of dissatisfaction (sore feet) with one that is preferred.

But Misesian praxeology, as I understand it, would seem to include both examples within the realm of human action that theory seeks to analyze and to explain. I submit, however, that they are categorically distinct. The first action need not reflect conscious, active, or creative choice; it can be interpreted as an animal-like response to a change in the external environment. It is reflective of behavior that might have been scientifically predicted. It is the sort of action that could describe the behavior of rats as well as men. By evident and sharp contrast, an animal could never take the second sort of creative action, which becomes uniquely human.<sup>8</sup> The Misesian praxeological umbrella that seems to encompass both sorts of action does not allow the sophisticated discrimination that must be made between the two. Indeed the Misesian emphasis on treating all human action as if it were like the second example tends to foster a critical response that involves the danger of neglect of the very type of action that subjective economics properly emphasizes.<sup>9</sup>

### **The Mutually Exclusive Domains for Economic Theory**

There are patterns of human behavior in economic interaction that are subject to conceptual prediction about which empirically testable hypotheses may be derived. There is a legitimate domain for predictive economic theory. Or, to put my point differently but somewhat more dramatically, in some aspects of their economic behavior, with appropriate qualifications, men are indeed like rats.<sup>10</sup> They are essentially passive responders to economic stimuli; they react; they do not choose. They are programmed, whether genetically or culturally, to behave in potentially predictable ways to specific modifications in the constraints that they face. The scope for this predictive theory of economic behavior is enormously extended when it is acknowledged that it is the behavior of some average or representative member of a group that is to be predicted here, not the particularized behavior of an individual.

The recognition of the domain of an operationally meaningful economic theory does not carry with it any implication concerning the practical usefulness of this theory in making predictions in the real world and/or in using such predictions to control man's behavior in that reality. There remains the awesome gap between the science that embodies conceptually refutable hypotheses and that science that embodies definitive refuta-

tion or corroboration. The familiar distinctions between the human and the nonhuman sciences involving controls on experiments arise here, along with the informational problems noted briefly earlier. Nonetheless, ultimate empirical content remains in the theory, regardless of actual testability, and the elaboration of the structure of relationships can add to our understanding of economic reality.

There are also aspects of human action that cannot be subjected to explanation in an operationally meaningful theory of economics. Any attempt to derive even conceptually refutable hypotheses about such action would amount to epistemological confusion. I have labeled this domain that of subjective economics or subjective economic theory. The objects for analysis are the *choices* of persons, which cannot be genuine choices and at the same time subject to prediction. Theory or analysis can be of explanatory value in this domain without the attribute of operability in the standard sense. Theory can add to our understanding (*verstehen*) of the process through which the economic world of values is created and transformed. Subjective economics offers a way of thinking about economic process, a means of imposing an intellectual order on apparent chaos without inferentially reducing the status of man, as a scientific object, to something that is not, in kind, different from that of animals.<sup>11</sup>

The limits of this vision of economic process must be recognized, however, along with its advantageous insights. Subjective economic theory can be of little assistance in an explanation or understanding of the allocation of values or in predicting general responses to changes in constraints imposed on actors. Since this theory advances no claim to prediction, it can, at best, suggest that any predictions made will likely prove to be wrong, indeed must be wrong to the extent that its own domain of choice is allowed operative range.

The purpose of the explanatory exercise determines the appropriate domain of economic theory to be employed. If this purpose is that of control of the economy through some manipulation of the constraints within which persons respond, the first domain of positive, predictive economic theory is the only one that holds out any scope for assistance. To the extent that this theory can isolate predicted response patterns to shifts in imposed constraints (to an increase or decrease in taxes, for example), those persons who participate in making political decisions (who may, of course, also be members of the group whose reaction behavior is being predicted by the economists) make their choices among alternative constraints on the basis of better information. The predictions of the economists have value, and this value commands a price. It is, therefore, not at all surprising that the efforts of economists shifted toward the predictive-science domain during the century-long period of increasing controls over national economies. Faith in the efficacy of such predictive science for assistance in controlling the economy perhaps reached its apogee in the 1960s, after which skepticism

emerged from its dormancy. The very failures of the predictive science of economics suggests the necessity of allowing for the existence of that domain of human action not amenable to scientistic explanation.

As the purpose of inquiry shifts toward understanding the sources of value creation with some ultimate objective of encouraging the establishment and maintenance of an environment within which human choices are allowed to take place relatively free of imposed constraints, we should expect economists to direct more of their attention to the domain of subjective economic theory.

### **Of Rats and Men**

I have found a discussion of the methodology of subjective economics impossible without first defining what I have called the “domain,” and my discussion in the chapter has been almost exclusively limited to definitional issues. After considerable intellectual floundering, my proposed classification of the two domains of economic theory emerged from a consideration of the very interesting laboratory experiments of rats and pigeons that have been conducted by Kagel, Battalio, and their colleagues. It seemed evident to me that this experimental work was scientific in a sense fully analogous with that carried out by our noneconomist peers in the natural sciences. And yet, as this work has revealed, rats have been shown to choose rationally, to respond predictably to stimuli, to react to “prices,” and in many respects to behave as true (even if simple) “economic men.” It is possible to derive demand and supply schedules for rats. That part of economic theory, therefore, that analyzes human behavior of the sort that is also evidently descriptive of rat behavior must be categorized as a genuinely predictive science.

The residual aspects of human action that are not reducible to ratlike responses to stimuli, even in the much more complex human variants, define the domain for a wholly different, and uniquely human, science—one that cannot, by its nature, be made analogous to the positive-predictive sciences of the orthodox paradigm.

There is surely room for both sciences to exist in the more inclusive rubric that we call economic theory. We must acknowledge that in many aspects of their behavior, men conform to laws of behavior such that such behavior becomes subject to scientifically testable prediction and control through the external manipulation of constraints. But we must also acknowledge that men can choose courses of action that emerge only in the choice process itself. Men create value by the imagination of alternatives that do not exist followed by the action that implements the possibilities imagined.<sup>12</sup>

Perhaps the methodology of subjective economics, once the definition

of its domain is accepted, can best be advanced by a deliberate attempt to sweep out thought patterns that are carried over from its positivist counterpart. I cannot, in this concluding section, discuss such steps in particular, but one example indicates my meaning. It has been suggested that subjective economic theory necessarily draws attention to the elementary fact that choices are made under conditions of uncertainty. Any attempt, however, to carry over the modern analysis of individual choice under uncertainty to the genuine choice making that is the subject of subjective economic theory reflects intellectual confusion. How can anything remotely resembling a probabilistic calculus be applied to choices that are among alternatives that only come into being through the act of choice itself? The human beings whose choices occupy the thoughts of G.L.S. Shackle could never be reduced to the status of rats, even superintelligent ones.<sup>13</sup> In my view, no economist other than Shackle works exclusively within the domain of subjective economic theory, as I have defined it here.

Any methodological advance must build on the work of Shackle. But as many scholars have already found, the next steps are not easy. The advances themselves will, of course, be genuine choices in the full Shackleian sense. They cannot be predicted. But there is surely some relationship between the objects of attention and the imaginative results that emerge. So long as modern economists devote their considerable intellectual energies, and imaginative skills, to the search for empirically testable regularities in human conduct, they will succeed in extending the scope of applicability for the man-as-rat metaphor to describe economic theory. To the extent that modern economists use their own imagination in efforts to understand more fully those aspects of human action that reflect man's own distinctive imaginative ability to choose his own reality, we can expect new insights about the process of economic interaction to emerge.

## Notes

1. Adam Smith, *Wealth of Nations*, Modern Library Edition (New York: 1937), p. 13.

2. Ibid.

3. See John. H. Kagel et al., "Demand Curves for Animal Consumers," *Quarterly Journal of Economics* 96 (February 1981):1-16.

4. This statement should be qualified to limit its relevance to the core problem of economic theory, that of explaining how an economy allocates resources and distributes product. Classical economics has been differently interpreted as offering a theory of economic development or growth.

5. F.A. Hayek, "Economics and Knowledge," *Economica* 4 (1937):33-54.

6. *Cost and Choice* (Chicago: Markham, 1969).

7. Thomas Nagel's fascinating review of Brian O'Shaughnessy's recently published, two-volume book suggests that at least some of the attention of modern analytic philosophers is turning to what seems to be a Miseslike a priori conception of human action. See Thomas Nagel, "The Self from Within," review of Brian O'Shaughnessy, *The Will: A Dual Aspect Theory*, vols. 1 and 2 (Cambridge: Cambridge University Press, 1980), in *London Times Literary Supplement*, 27 March 1981, pp. 327-328.

8. I ignore the tool-using action of some primates. My purpose is conceptual classification rather than ethology.

9. My criticism of the Mises-Austrian position in this section (although it was developed independently before I knew about Nozick's paper) closely parallels that taken by Robert Nozick in part 2 of his paper, "On Austrian Methodology," *Synthese* 36 (1977):353-392, especially pp. 361-369. For an informative critique that is somewhat differently directed, see, Willy Meyer, "Erkenntnistheoretische Orientierungen und der Charakter des ökonomischen Denken," in *Zur Theorie marktwirtschaftlichen Ordnungen*, E. Streissler and C. Watrin, eds. (Tübingen: Mohr, 1980), especially pp. 82-91.

10. The qualifications refer to the obvious differences in the complexity of response patterns as between man and rat. I am not saying that men are like rats in any descriptive sense. My purpose, to repeat, is conceptual classification, not accuracy in description.

11. The work of Israel Kirzner exemplifies subjective economic theory in the sense defined here. Few critics could argue that Kirzner's discussion of entrepreneurship and the role of the entrepreneur in the competitive economic process is not explanatory in the ordinary meaning of the term. See Israel Kirzner, *Competition and Entrepreneurship* (Chicago: University of Chicago Press, 1973).

12. Methodologically, it is important to insist that the two domains be treated as mutually exclusive. Unless this precept is strictly adhered to, the operational status of the predictive theory may become meaningless. Suppose that an hypothesis derived from this theory is empirically refuted. The theorist cannot be allowed to fall back on an essentially subjective-economics explanation to the effect that utility functions have shifted, that persons have exercised genuine choice. He should, instead, be forced to acknowledge the falsification of his hypothesis about behavioral reality. To resort to presumed shifts in the reality itself while holding to the central hypothesis is methodologically illegitimate.

For a fascinating discussion of a related problem that arises in the relationship between moral and predictive theory, see David Levy, "Rational Choice and Morality: Economics and Classical Philosophy," December 1979, mimeo.

13. Among Shackle's many books, see, in particular, *Epistemics and Economics* (Cambridge: Cambridge University Press, 1972) and *Imagination and the Nature of Choice* (Edinburgh: Edinburgh University Press, 1979).

# 3

## **Subjectivism, Predictability, and Creativity: Comment on Buchanan**

*Karen I. Vaughn*

James Buchanan has presented us with a stimulating chapter that raises a crucial but so far underexplored issue: the relationship between positive economics and subjectivism. He need not apologize for writing a chapter that is primarily definitional. On the contrary, the definitions he attempts are necessary before any real progress can be made in exploring the implications of subjectivism for economic theory. One simply cannot write about subjectivist methodology without first understanding what subjectivism is and what it can and cannot say or do. I agree with many of the individual pieces of Buchanan's argument and indeed have found these pieces very helpful in arriving at my own understanding of subjectivism. But I also find that the individual pieces as Buchanan presents them do not fit together into a complete argument. In chapter 3 I will attempt to locate some of the missing links in the argument and point to some sources of confusion about the methodological basis of positive economics and subjectivism that underlie not only Buchanan's chapter but also much of the existing literature on subjectivism. Second, I will briefly sketch a reconstruction of the argument that avoids some of the confusions and points up the fruitful areas of study indicated by subjectivist insights.

The gist of Buchanan's argument is that predictive, positive economics applies to those situations where humans respond passively to shifts in constraints and that subjectivism applies to situations where humans actively seek to alter their constraints. Early in chapter 2 Buchanan illustrates the distinction by contrasting Smith's beaver-deer example, a good predictive, scientific hypothesis about relative prices, with Smith's further need to supply an explanation of why there would be a division of labor and exchange in human society in the first place. If all people ever do is consume such that the ratio of the marginal utilities between all goods matched their relative prices, as the beaver-deer hypothesis requires, there would be no explanation for the emergence of markets, economic institutions—no change and no innovation. Clearly we need some explanation, as did Adam Smith, of those phenomena that reflect what Buchanan calls "active choice"; choices that involve creative, innovative thinking—what some Austrians might call entrepreneurial choice.

Although Buchanan particularly identifies the conventional, orthodox neoclassical theory as the predictive theory he means—the predictive theory that only applies to reactive behavior—he does not limit his comments to neoclassical economics. That is, Buchanan is not here talking about neoclassical economics versus Austrian economics, despite recent attempts to identify Austrian economics with subjectivism. Buchanan is distinguishing any even conceptually predictive theory, including many Austrian theories, from a Shacklesque subjectivism that denies even the possibility of theory. In chapter 2, Buchanan is trying to reconcile the possibility of theorizing about human behavior with the problem of undetermined choice. If human choice is undetermined, without cause in Shackle's sense, how can we have determinate theories about their choices? And on the other hand if we can formulate in principle testable theories about human action, does this necessarily mean that the actions are in some sense not free? Buchanan's way out of this dilemma is extreme. He argues that there are two kinds of human behavior that are conceptually different: Reactive, in some aspects almost instinctive, behavior that is predictable because it is not genuinely free—it is animal-like and somehow caused by changes in constraints—and truly free, creative behavior that is by its nature unpredictable because of its freedom.

### **On the Alleged Conflict between Predictability and Freedom**

My first comment has to do with the relationship between scientific prediction and human predictability and freedom. At the risk of sounding trite, I would point out that any theory is necessarily predictive at some level of generality. A theory is a set of hypothesized causal relationships that we use to organize and understand our world of sense experience. The tacit assumption with all theory is that if the initial conditions are met, the predicted consequences will follow. To speak of a realm of human action that is not amenable to even conceptual prediction at some level of generality is to deny the possibility of theorizing about that action. There might in fact be such a realm of human action, but that realm would have little interest for the social scientist.

Given that there is a realm about which we can theorize, to make conceptually refutable predictive statements within that realm is not to deny human freedom. That our predictions are sometimes or even often correct does not mean that human action is in any philosophical sense predetermined or genetically programmed or in any sense necessary in such a way that humans could not have chosen otherwise. (Mises, as we remember, did not rule out the possibility of determinism but only argued that if our actions are determined, we do not know or cannot know that that is the case. Certainly, there is no evidence at present for making that assumption.) It is



true that action in the Misesian sense is necessary, but no specific action is ever certain. As scientists, we start from the assumption that human beings have choices, and our theories are attempts to make sense out of the choices they do make. Because we are human, we can conceptualize the problems faced by human actors, and we can theorize about the kinds of solutions they will produce. (The framework of constrained maximization is one such attempt at theorizing about the solutions at which humans will arrive. In so far as humans attempt to get the most for the least, constrained maximization is a way of anticipating the solution—a technique that requires us to see the world as the subject himself sees it). When we are successful in predicting the actions of human beings, as scientists we have simply demonstrated our understanding of the actors' ends and the way they perceive the means possible. They are free; we must understand how they use their freedom.

The basic insight of social science, it seems to me, is that human action may be free, but it is not random. As Mises told us repeatedly, human beings act purposefully by using means to achieve ends. However, the consequences of their actions are not always what they intend. Economic science is primarily about the systematic nature of the unintended consequences of human purposeful action. Were there no predictable (or observable) regularities in the consequences of human actions, there could be no science at all. The real question Buchanan is addressing is in the nature of these regularities. He seems to want to argue that they exist only because in some aspects of life, humans are repetitive, reactive, unthinking, bordering on automata. There is an element of truth here, but it has nothing to do with the problem of undetermined action or free choice. It is much more fruitful to think of humans as willfully and rationally limiting their choices in some areas. Human beings establish habits, customs, usual methods of business dealings, and institutions, all of which limit their creative actions. They can always choose not to act within the given modes, but the fact that they do not so choose has important implications. I would argue that there are predictable aspects of human action because people, when allowed to experiment and learn from one another, often come up with similar solutions to a common problem. Once the solution is arrived at, it is repeated until some reason to change presents itself. Humans may also choose to limit their responses to minor variations in the problem they face simply because they rightfully assume that a new solution is more likely to be worse than a tried-and-true pattern of action. This does not in any way deny their freedom to make that decision.

### **Of Rats and Men: A Revisionist Interpretation**

The issue of determinism versus freedom aside, the real heart of Buchanan's view of the domains of positive and subjectivist economics is his claim that

there are two conceptually different kinds of action, reactive and active choice. Reactive behavior might be the result of a prior active choice but still can be categorically different from active choice. He presents support for this view by making brief reference to the recent experimental work of John Kagel et al., who have used economic theory to model the behavior of laboratory rats.<sup>1</sup> Buchanan observes that conventional economic theory predicts rat behavior at least as well as some aspects of human behavior and concludes that humans and rats have a lot in common, an observation that is not calculated to flatter the humans. His chain of reasoning is something like this: Rat behavior is instinctive and reactive. Instinctive, reactive behavior is modeled successfully using conventional consumer-demand theory. Conventional consumer-demand theory also applies to human action. Hence, at least in some areas, humans are instinctive and reactive in their behavior. After looking carefully at the structure and results of the rat experiments, I come to the conclusion that Buchanan's argument is demeaning to the rats. It is worth looking more closely at those rat experiments, since the implications we can draw from them are useful for understanding the uses of economic theory.

For the benefit of those who are unfamiliar with the design of these experiments, I offer a brief description. Rats were placed in laboratory cages where they could only obtain food and water by depressing levers, one for each. Prices were represented by the number of presses it took to obtain the food and water; for example, one press for a food pellet and two for a measured amount of water made water twice as expensive as food. Each rat had a budget constraint of only so many presses per day. After an initial learning period, each rat settled down into some consumption pattern that became predictable and stable. When the relative prices were changed, the rats generally substituted the cheaper for the more expensive reward and once again settled down into a predictable consumption pattern. After many of these relative-price changes were imposed on the rats, downward-sloping demand curves could be estimated for the rats.<sup>2</sup>

I call your attention to a few aspects of these experiments that may escape notice. First, the rats, without the aid of arithmetic or verbal information, had to discover their constraints. They did so through several days of experimentation on their part, during which time their behavior was not predictable in any sense. Only after they learned the best consumption bundle for the given set of constraints did they repeat the bundle. Generally, they would experiment up to three or four days before they settled on a new pattern of consumption. The second interesting aspect is that each rat had a different demand curve for food and water. Their tastes were not uniform across individuals, suggesting that they are not predestined or genetically programmed for any particular level of consumption. Certainly their tastes and actions are not nearly as simplistic and reactive as that of frogs, for

example, who are said to follow a very simple decision rule: If it moves and you can catch it, mate with it. If you cannot mate with it, eat it. By contrast, the rats were actively intelligent. They perceived changes in their environment and searched out new responses to those changes to improve their well-being. What is more pertinent, the rat behavior only became predictable in a quantitative sense after their search was complete and they had established a new consumption pattern. For those experimental rats, each point on the estimated-demand curve was an equilibrium solution to a new problem, and their repetition of the same equilibrium solution was an example of the Evenly Rotating Economy (ERE) in miniature. It should be no surprise that action in the ERE is predictable. Note, however, that this particular ERE was the result of a process of experimentation and learning about the constraints and tastes engaged in by each individual rat—an unpredictable disequilibrium process. The best the experimenter could do was to predict an inverse price-quantity relationship between equilibrium points—a qualitative prediction. Only when an equilibrium point was reached could they quantitatively predict the amount the rat would continue to consume. I conclude that the rats were indeed like people in that they were displaying a high level of intelligent, choosing behavior in what for them was a complex and unknown environment. At least in these experiments, what we might legitimately call reactive behavior was only evident once an equilibrium was reached. During the disequilibrium search, the rats were actively intelligent.

Professor Buchanan may concede my point about the rats' intelligent behavior but still argue that there are two kinds of intelligent behavior important for economics, only one of which is displayed by rats. The rats did not seek to change their constraints by their own ability to imagine an alternative future.<sup>3</sup> And remember, Adam Smith never saw a dog (or a rat) exchange bone for bone. Although they may not be reactive versus active choices, whatever you call them, are there still two different kinds of behavior that are conceptually or categorically different only one of which characterizes rat or animal behavior? And does positive economics apply to one and subjectivist economics to the other? I argue that it is not scientifically useful to distinguish sharply between reactive and active choices as if they were categorically different behaviors. I will agree, however, that there are two different kinds of conceptual problems for the economic theorist that closely parallel the distinction Buchanan wants to make between reactive and active choice.

To illustrate, assume a state government imposes an excise tax on hard liquor sold in that state (the human equivalent of changing the number of lever presses in the laboratory). Humans quite quickly react to the change in prices by buying less liquor. As time goes on even less liquor is purchased at higher prices as they predictably start driving across the state line to import

cheaper liquor from a neighboring county and as local producers start firing up their illegal stills. And as even more time goes on, some people search for a legal alternative to alcohol that is not taxed and they find it. In each case, the action taken by the individuals in our story was a reaction to the change in relative prices, and, in each case, it was predictable by economists in some sense. Yet does this mean it was all uncreative, all nonactive choice? Certainly the discovery of the legal alternative to alcoholic beverages must be considered an act of creation despite the motivation for the discovery. But if we grant the creativity of that action, do we also grant the case where the formula was already known to someone who only decided to exploit it commercially after the change in the relative price of liquor? And is not driving to the next county a manifestation of imagining an alternative future? But if that is so, so is substituting beer for liquor when the tax is imposed—one had to actively figure out that beer was an acceptable substitute and at what rate it was subjectively acceptable. What I have described is a continuum of actions all reactive in the sense of being inspired by a change in relative prices but all displaying some degree of creativity and imagination. Where are the categorical differences here?

### **Of the Dangers One Encounters While Walking Along a Road**

Although I do not think there are categorically different behaviors, I do believe there are distinct theoretical problems for economists captured by Buchanan's reference to Adam Smith. To see this, let us turn to the now familiar Buchanan-Kirzner man who walks along dusty highways barefoot. Buchanan mentions him to draw the distinction he wants to make between reactive and active choice. I will try to use him to draw a distinction between two kinds of scientific problems faced by economists. Buchanan argues that the man who jumps out of the way of an oncoming truck is being reactive and predictable, but the man who walks along the road barefooted and suddenly gets the idea to fashion a pair of shoes out of some cowhide resting on the side of the road is an example of a person engaging in active, unpredictable choice.

At the risk of straying a bit from my argument, I cannot resist pointing out that the first case is not as clear cut as it seems. Suppose the man facing the oncoming truck is despondent about his failing career and wants to end it all and alertly grasps the opportunity that presents itself to accomplish the deed while still permitting his beneficiaries to collect his insurance money. He does not jump and our predictions fail because we do not know his utility function. But, even if we did, we cannot even predict that he will not jump with complete confidence, since we do not know that he will choose this

instance to accomplish his goal. He may judge that the benefit of getting it all over with right then and there is not worth the cost of the possibility of surviving and living on in protracted and expensive pain, in which case he will deliberately jump out of the path of the oncoming truck and leave his suicide for another day. The moral seems to be that what seems like reactive behavior may well be deliberative behavior, and we cannot be too confident of our predictions about any one individual choice. This seems to be a good, subjectivist critique, although I concede it has very little to do with Buchanan's argument. My more important comment is that the two examples presented do not capture the distinction important for economic theorizing.

I believe a more useful contrast could be made between the following: In the first case, the barefoot man is walking along the road on his way to the marketplace to buy himself a pair of shoes. He thought he could get by without them, but his feet have been killing him lately and he decides to forgo repairing the pig trough and to use his limited money to acquire shoes. This man lives in a culture where shoes are worn, he knows about them, they are elements in his utility function (or his scale of values), a market for shoes exists to which he has access. This man engages in deliberative, purposeful action, weighs alternatives, and decides to acquire a pair of shoes. As economists, we can talk about the alternatives he perceives, what his eventual decision implies about his relative values, what the market price is likely to be on that particular day, how his actions will affect the market price of shoes and pig troughs, and a myriad of related consequences to his simple action. This is what neoclassical economics—including Austrian economics—has primarily concerned itself with.

Let us consider the second man, Kirzner's pure entrepreneur who walks along the rocky road and discovers shoes for the first time. What can we say about that as economists? Once he makes the discovery, we can talk about shifts in tastes and marginal rates of transformation for the individual and how he may substitute shoes for woven baskets at the margin. And, once he offers his hand-crafted shoes in the marketplace, we can talk about profits and loss and the consequences of imitation and competition. But we can say very little about the discovery process itself. Why are shoes invented when they are? If someone does invent them, will he notice their commercial potential? If he imagines their potential profitability, how does he establish or create a market for something about which other people know nothing? It seems to me that these questions are of a very different nature from those we asked in the first case. In the first case, we try to explain a set of choices and their consequences within an established culture and an established market—within a set of given institutions. Because there are established institutional parameters, we can make informed theoretical predictions about the outcome of any action. In the second case, we are asking ques-

tions about the process of market creation and institutional change brought about by the discovery of new knowledge or the perception of previously unimagined opportunities. With changing institutional parameters brought about by discovery or changing perceptions, we can predict very little even in principle, since we cannot know in advance what is going to be learned or perceived. Hence, the problem for economic theory is not so much one of the differences between reactive and active behavior as it is the difference between action without new learning and action where learning takes place. (Brian Loasby discusses this issue in chapter 10.)

### **The Domain of Subjectivist Economics**

The final question is whether, by describing these two theoretical problems, we have successfully located the respective domains of positive and subjectivist economics. I must confess I found this the most troubling aspect of my reconstruction of Buchanan's argument. In private correspondence, I accused Buchanan of veering sharply away from the schema he presented in his 1968 article, "Is Economics a Science of Choice?"<sup>4</sup> His reply, that his chapter was entirely consistent with the earlier article, I found incomprehensible until I sorted out the above problems to my satisfaction. Now I think I see better what he was trying to get at.

In the earlier article, Buchanan argued that praxeology was pure logic of choice, where wants are completely unspecified and means can be described only in the most general sense. Hence, it is a framework for making sense out of people's actions, but it is totally nonpredictive in any concrete manner. Although very important, its usefulness was tautological. Predictive science, however, is a subset of praxeology where wants and constraints are specified. Hence, it is a set of, in principle, empirically testable hypotheses about real-world events, and, because positive economics attempts to say something specific about real-world events, it must be falsifiable. Given these distinctions, we can then argue that positive economics is only likely to be possible where institutional parameters are set, because it is only in that situation that we as scientists have a stable environment about which to theorize. Where institutional parameters are changing because of real learning, we cannot even theorize about outcomes except in the tautological sense of praxeology. Hence, if we take subjectivism to be synonymous with praxeology, Buchanan's argument is sustained. That is not precisely my understanding of subjectivism, but it is certainly a reasonable one in a world where there is no consensus on the meaning of the word. To me, any definition of subjectivism should include its function of serving to point out the limitations of positive economics in all circumstances, even when there is no new learning taking place; but cer-

tainly Buchanan's thought-provoking chapter requires that we open up a dialogue on the meaning of subjectivist economics.

### Notes

1. John Kagel et al., "Demand Curves for Animal Consumers," *The Quarterly Journal of Economics* 96 (February 1981):1-15. See the bibliography in this article for a list of further experiments conducted in the same vein.

2. The experiments were really far more detailed and complex. The rats were tested for income-compensated demand curves as well as regular demand curves, luxury goods as well as necessities, and product variation (in the size of the food pellet). Ibid.

3. Even here, we must not be too hard on the rats. In the particular laboratory setting, they had very little opportunity to do anything but press levers. They were isolated from each other, so social interaction was precluded. About the best any rat might have accomplished would be to have escaped from his cage to eat the experimenter's sandwich.

4. Streissler, ed. *Roads to Freedom: Essays in Honor of Friedrich Hayek*. London: Routledge and Kegan Paul, 1969.