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Economics and Psychology: Lessons For Our Own Day From the Early Twentieth Century

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I. *Introduction*

A. *Sen's Paradox*

IN HIS 1973 ESSAY "Behavior and the Concept of Preference," Amartya Sen points out the paradoxical attitude of economists toward psychology. Economists tend to believe that economics can be independent of psychological assumptions. They are suspicious of attempts to analyze economic behavior by asking people about the motivations for their actions, but prefer to look solely at behavior (Paul Samuelson 1938; Ian Little 1949), or, for those who follow Milton Friedman (1953), solely at aggregate data like prices and quantities. This antipsychological attitude underlies the perceived importance of the theory of revealed preference, which reduces preference theory to a set of behavioral propositions.

Yet it is so obvious that the assumptions of economics rely on psychological reasoning for their plausibility. Economists are, for example, very attached to the notion of rational choice, and, as Sen (1993) argues, rationality is a nonsensical concept if it is not motive-related, but only behavioral. Rationality is, by its very nature, a *psychological interpretation* which we place on behavior that we observe. In the rational-choice approach, when we observe an action, we interpret that action as the result of some motive. The source of this interpretation is *external* to the behavior we observe. In fact, in many circumstances, observations of behavior are quite poor (or even misleading) data for determining which preferences an individual possesses. Nonbehavioral information, such as verbal communication, may be much more revealing of individual motivation, especially when moral considerations dominate choice.

The inconsistent coexistence of both psychological and antipsychological ideas within the economics discipline is somewhat puzzling, and begs some explanation. I call this quandary *Sen's paradox*. In recent years, this paradox has become more visible. We are all familiar with the recent controversy over rational choice, with the criticisms of (mostly experimental) psychologists (cf. Sarah Lichtenstein and Paul Slovic 1971, 1973; David Grether and Charles Plott 1979; Daniel Kahneman and Amos Tversky 1979; William Samuelson and Richard Zeckhauser 1988; Tversky, Shmuel Sattath, and Slovic 1988), and the resulting soul-searching among economists.[1] To what extent, we are asking, do microeconomic assumptions need to be psychologically realistic? Can we really be independent of psychology? Economics now faces the daunting task of discovering an effective solution to today's methodological confusion.

This confusion is difficult to resolve (I submit) partly because today's discussions occur in a temporal vacuum, and participants generally lack a good understanding of the historical roots of today's disagreements. This paper attempts to provide this much needed historical context by looking back to the birth of Sen's paradox. It studies a controversy very similar to today's debate, the crisis of hedonism, which occurred near the turn of the century. By studying this earlier historical episode, we learn that the debate over psychological assumptions is only a small piece of a much larger intellectual debate that concerns the relationship between economics and the other human sciences, most particularly, with sociology. Only when we come to terms with this broader context will we be able to understand the real lessons which the economics and psychology debate has to teach us. Sociological influences deeply affect the psychology underlying economic behavior. Therefore, any serious reevaluation of the psychological underpinnings of economics requires that careful attention be payed to sociological analyses in our economic investigations.

B. *Historical Synopsis*

The early twentieth century was a period of severe tumult in the economics community.[2] Marginal utility theory, then only in its infancy, found itself under severe attack, as psychologists questioned its scientific integrity. As in today's controversy, some economists joined psychologists in their attack, and attempted to integrate advances in psychology into economic theory. These institutionalists used psychological critiques as ammunition for their more fundamental criticisms of the perceived narrowness of orthodox theory. Today's economic sociologists are the intellectual descendants of this movement. Mainstream economists responded mostly by arguing that the criticisms of psychologists were irrelevant. Economics was independent of psychological assumptions; it spoke only about behavior, which could always be rationalized by some preferences, whatever the actual psychological cause.

A behaviorist movement arose in economics, as theorists attempted to free economics of all psychological elements.

[1] See for example *The Journal of Business* 59(4S), (reprinted as Robin Hogarth and Melvin Reder 1987) which includes the proceedings of a 1985 conference on the behavioral foundations of economic theory. The focus was largely on the different attempts by economists to come to terms with experimental results which appeared to falsify utility theory. For a more recent analysis see Vernon Smith (1991). Some economists have resolved the issue by formulating more psychologically realistic theories (cf. Richard Thaler 1991).

[2] By the "economics community," I mean here those economists centered in Britain and America. This paper does not attempt to cover the broader economics community, which would include such countries as Austria.

This movement contributed to the replacement of the older theory of cardinal utility, with the new notion of ordinal preferences. Later, the theory of revealed preference eliminated the need to interpret even ordinal preferences psychologically. Preferences were transformed from "metaphysical" entities into scientifically valid, truly empirical objects derived solely from behavior.

Despite its bold claims, behaviorist economics failed in two major respects. First, Sen's paradox became increasingly evident as more traditional economists pointed out the folly and impracticability of nonpsychological preference theory. For nonpsychological preference theory to have any scientific rationale, it had to be possible to derive preferences from behavior; only behavior could be observed with scientific objectivity, and therefore, an intuitive, psychological foundation for preference theory could play no role. However, actually deriving preferences from behavior turned out to be very difficult,[3] and thus, from a behaviorist perspective, preference theory had little practical use. Second, the evasive "declaration of independence" from psychology failed to address the substantive and sociological criticisms of psychologists and institutionalists; that is, that economics ignored real-world phenomena and focused on an overly narrow range of questions with an overly narrow theory of human behavior. Behaviorism in this extreme form eventually lost favor among economists. However, the "declaration of independence" from psychology remained, and it haunts economics to this day.

C. *Primary Theses*

This paper attempts to accomplish three main goals.

[3] For more on this point, see the discussion of Houthakker (1961) in Section V.C. below.

1. *Interdisciplinary Pathology Thesis.* First, this paper is a study of the way in which the economics discipline interacts with the other human sciences; in particular, with sociology and psychology. Economics has always absorbed ideas from other disciplines. For example, Francis Edgeworth's book *Mathematical Psychics* definitely dealt with psychological and sociological questions. However, such interdisciplinary dialogue has not always been as productive as it might be.

This paper uncovers a pathological pattern in the relationship between economics and the other human sciences. It appears that throughout this century, economists have been reluctant to acknowledge the interdependence between economics and its sister disciplines, particularly sociology and psychology. Only under pressure will we acknowledge our dependence, and even then, our attention focuses almost exclusively on the psychological shortcomings of economics, rather than on the sociological shortcomings which are much more fundamental and difficult to address.

Why economists deemphasize sociological issues in their work is a complex question that this paper does not attempt to answer. The strained relationship between economics and sociology has deep historical roots (Richard Swedberg 1987, pp. 31–35) and it continues to this day (James Baron and Michael Hannan 1994). I will also not attempt to argue at length for the relevance to sociology to economics. Good arguments for this position can be found in Swedberg (1987, 1990) and Baron and Hannan (1994). Whenever two disciplines are related as closely as economics and sociology are, opportunities for synergy are bound to exist. When we ignore these opportunities, scientific progress is thwarted. When we seize them, we make scientific progress, as have Herbert Simon (1947),

Becker (1976), George Akerlof (1984), and other economists who have recognized the potential of interdisciplinary research.

Rather than lingering on the benefits or drawbacks of a sociological economics, this paper begins with the presumption that interdisciplinary research is good for science. It then draws attention to the pathology which has prevented this sort of research from flourishing in economics, in the hope that if we understand this pathology, we may be able to escape it. Our thesis is the following: Economists tend to downplay the relevance (to economics) of the other human sciences. In particular, we tend to ignore outside criticism. When we do take note of it, we pay attention solely to that coming from psychology. The critics of economics encourage this pattern by focusing on psychological issues, even when their main points of contention are sociological.[4] As Mark Granovetter (1985, p. 75) writes,

> The notion that rational choice is derailed by social influences had long discouraged detailed sociological analysis of economic life and led revisionist economists to reform economic theory by focusing on its naive psychology. My claim here is that however naive that psychology may be, this is not where the main difficulty lies—it is rather in the neglect of social structure.

The result of this interaction is counterproductive. Exclusive attention to psychology distracts us from social scientific issues. Worse yet, it may tie the hands of those economists who wish to take on the sociological challenge. For example, in the heyday of behaviorism, economists were discouraged from responding to their institutionalist critics by formulating more sociologically realistic theories, because such theorizing necessitated exactly the sort of psychological language which these very critics so avidly attacked. Uncovering a past expression of this pathology might help us to avoid a repetition of past mistakes, so that the relationship between economics, and sociology and psychology, might become less pathological and more productive.

2. *Antibehaviorist Thesis.* Second, this paper explores the history behind Sen's paradox, described above. It describes how behaviorist ideas first entered economics, and it uses this period of intellectual history to illustrate the folly of renouncing verstehen (defined in Section II.B.2) and attempting to rid economics of psychology. In contrast with those economists who reject verstehen as an unscientific remnant of the past, we argue that, in fact, verstehen is an essential foundation for social scientific endeavor, especially for the theory of preference, or rational choice. The story that follows illustrates the wisdom of embracing the differences between the human and physical sciences, rather than attempting, naively, to apply a single mechanistic methodology to both.

3. *An Historical Journey.* Finally, this paper relates an exciting piece of intellectual history which is interesting in its own right. We alternate between telling the story, and discussing its relevance to the two main theses described above.

II. *Historical Background*

The mushrooming of criticism against economics near the turn of the century was a product of two historical developments: the rise of marginal utility theory, and the rise of instinct theory in psychology, which we discuss later in Section III.A. Section II.A below reviews the marginal utility revolution. Then Section II.B describes the methodological con-

[4] A notable recent example is Amitai Etzioni's 1988 book *The Moral Dimension*, which aims to be a sociological critique of economics, but which devotes most of its energy to a discussion of the work of such psychologists as Tversky and Kahneman.

text in which this revolution occurred, and sets the stage for the imminent attack from psychology.

A. *The Marginal Utilitarians*

Prior to the late nineteenth century, economics had been dominated by the Ricardian school of classical economics, which explained price determination by emphasizing *objective* factors such as technological costs. A close relation of classical economics was the Benthamite school of social theory, which emphasized, by contrast, the importance of *subjective* factors in determining social phenomena. Late in the nineteenth century, the classical school lost favor, and a new group of economists, among them William Stanley Jevons, Léon Walras, and Francis Edgeworth, incorporated the Benthamite emphasis into their new marginal utility approach to economics, uniting the two British schools.

Jeremy Bentham himself used the concept of a *hedonic calculus* as the foundation of his social theory. He wrote that "Nature has placed mankind under the governance of two sovereign masters, *pain* and *pleasure*." This calculus of pain and pleasure was all-inclusive. "It is for them alone to point out what we ought to do, as well as what we shall do . . . They govern us in all we do, in all we say, in all we think" (Bentham 1789, p. 1). Bentham even recognized that people might be nonselfish if they were "to some extent interested in the happiness of others" (Jacob Viner 1949, p. 312). It must be emphasized that for Bentham as for his followers, utility was a psychological (or physiological) magnitude which measured an individual's inner happiness; it was not, as it is in many modern texts, simply a proxy for the degree to which an individual has reached whatever goals he seeks.

Jevons, Walras, Edgeworth, and the other marginal utilitarians (as Wesley Clair Mitchell was wont to call them) reformulated Bentham's approach in mathematical terms, so that utility became an explicit quantity. Jevons, in particular, emphasized his debt to Bentham. For their mathematics, the marginal utilitarians drew from physics, and in fact, they formulated their theory explicitly in emulation of concurrent developments in energetics (Philip Mirowski 1988, pp. 13–15). Utility, marginal utility (motive), and consumption corresponded respectively to energy, force, and position. Literally like a force, marginal utility drew people toward more appropriate consumption options, just as a gravitational or magnetic forcefield drew a particle from one position to another. It must be emphasized that, like Bentham, Jevons (1879) and Edgeworth (1881) considered utility to be a real psychological (or physiological) substance, and Jevons (1879, pp. 63ff) actually devotes some space to an elucidation of the units in which utility and marginal utility ought to be measured. From the perspective of the marginal utilitarians, the theory of utility transformed economics into a science by making it possible to derive economic laws from a single fundamental law of human nature—the law of maximum utility.

This transition to a new, subjectivist economics occurred over the objections of classical economists such as J. S. Mill (Neil de Marchi 1973, p. 84), who argued that the notation of marginal utility theory implied "the existence of greater precision in the data than the questions would admit of" and who preferred to restrict quantitative analysis to quantities and prices, which could actually be measured.

B. *The Old Methodology*

To understand the debate that ensued, one must understand the methodological

context in which marginal utility theory was born.

1. *Verificationism.* In the late nineteenth century, the reigning methodology in economics was that of a priori-ism and verificationism (both defined below). John Neville Keynes' 1890 book *The Scope and Method of Political Economy* is perhaps the best summary of this approach. For Keynes, economics was a deductive science, rather than an inductive one. Its fundamental principles were a priori; i.e., they were derived prior to and independently of empirical observation. Economics derived its laws from "a few simple and indisputable facts of human nature" (p. 14) such as the desire for wealth. Because they arose from deduction, these laws did not have the certainty of laws of physics, but were "only true hypothetically, that is, in the absence of counteracting agencies" (p. 16). Therefore, it was impossible in practice to test economic theory empirically. If one found a counter-example, this was merely an indication that the law did not apply to the situation at hand. Instead of testing one's theory (making it vulnerable to *falsification*), one sought empirical instances that *verified* the theory, thus the term *verificationism.* William Baumol (1985, p. 1) notes that the economists of the late nineteenth century (with the important exception of the historical school) made "virtually no systematic use of history" or statistics.

The economics of this period did not aspire to be a universal social science. However desirable such a science would be, wrote Keynes, we are unfortunately forced "to leave out many circumstances, which are of importance in individual cases, but are nevertheless unimportant when instances are taken in the mass" (1890, p. 15). Alfred Marshall (1890, p. 20) makes a similar statement; for particular applications, economists had to use their common sense and take into account numerous factors which the theory itself ignored. Even Jevons, who, unlike Keynes, thought that economics should emulate physics, "was prepared to hand over to others the study of social and institutional change in order to free pure economic science from such entanglements" (Donald Winch 1973, p. 66). Sociological economics found no place in this framework. The narrow scope of late nineteenth century economics contrasted starkly with the universalism that Benthamite social science had once claimed.[5]

2. *Verstehen.* Finally, Keynes, like many social scientists of his time, emphasized the distinction between mechanical behavior and human action. The "facts of human nature" on which economics was based were to be derived, not from direct observation of human behavior, but from "an *introspective* survey of the operation of those motives by which men are mainly influenced in their economic activities" (Keynes 1890, p. 173, italics added).

This notion is closely related to what Max Weber called verstehen.[6] Weber argued that, in the social sciences, we cannot perform the controlled experiments that are possible in the physical sciences. However, the social sciences possess something better than experiments: In studying human beings, we have a significant advantage because we are ourselves human beings, and we can therefore comprehend the motives behind human behavior directly through our own introspection. This verstehen, or intuitive understanding of human motivation, is what distinguishes the human sciences from the physical sciences (Weber

[5] This narrow scope also contrasts with more recent developments in economics (Becker 1976; Akerlof 1984).

[6] Actually, the idea of verstehen goes back farther than Weber, but Weber's exposition is the most famous. A full discussion of the history of this concept is beyond the scope of this paper.

1922, pp. 18–19). Verstehen is of no use in deriving the laws of physics, because we cannot introspect into inanimate objects; we introspect only into ourselves.

Like Weber, Keynes also emphasized the importance of motives, but he focused on one particular motive: goal fulfillment. (Not all motives are teleological.[7]) "The differentia of economic laws, as contrasted with purely physical laws, consists in the fact that the former imply voluntary human action" (Keynes 1890, p. 86). Thus, economics was explicitly and unembarrassedly teleological, and this teleology was its hallmark.

III. *The Attack*

While economists were increasingly emphasizing the role of conscious rational choices in their treatises, other human scientists were doing just the opposite, as "several psychologists and biologists, notably William James and Jacques Loeb, began to attach much greater significance to . . . unlearned acts and reflexes as keys to psychological explanation" (Zenas Clark Dickinson 1919, p. 395). Instinct theory psychologists emphasized the role of habit and of several fundamental human instincts in determining behavior. Section III.A discusses this challenge to economic theory. Then in Section III.B, we see how institutionalist economists joined in the attack.

The focus of this section is on our interdisciplinary pathology thesis. We see how the institutionalists focused on psychological issues when criticizing mainstream economics, in the hope that attention to psychological issues would lead to progress on the institutional (or sociological) front. These hopes would be disappointed, as mainstream economists noticed only the psychological criticisms, and addressed even these inadequately.

A. *The Birth of Modern Psychology*

Although psychological hedonism "had been under the fire of ethical writers for centuries" (Dickinson 1919, p. 395), the rise of instinct theory psychology engendered an explosion of attack of such magnitude that economists found it hard to ignore.

In his famous 1890 textbook on *The Principles of Psychology*, William James criticizes psychological hedonism on the following grounds. Psychological hedonists such as Alexander Bain (1859) attempt to use only one human motive to explain all of human activity (see Keynes, above). Bain does admit that other motives are important, but

> he prefers to give to that part of the activity exclusively which these feelings [pleasure and pain] prompt the name of "*regular* outgoings" and "*genuine* impulses" of the will, and to treat all the rest as mere paradoxes and anomalies, of which nothing rational can be said . . . At bottom, this is only verbal play. (James 1890, p. 555)

To James, such an approach is unscientific. Instead, scientists ought to take into account all possible "springs of action" (motives) in their theories, leaving the (teleological) hedonic motive as merely one example (p. 555).

Psychological hedonists, James argues, "obey a curiously narrow teleological superstition" (p. 551) for they assume without foundation that behavior always aims at the *goal* of maximum pleasure and minimum pain; but behavior is often impulsive, not goal-oriented. Later, William McDougall (1910) echoed James' earlier criticisms. In an oft-quoted quip, he says that "It would be a libel, not altogether devoid of truth, to say that classical political economy was a tissue of false conclusions drawn from false psychologi-

[7] The word teleological means here "concerned with goals, final ends, or purposes." The term is often used as a pejorative, partly because of the influence of the ideas discussed in this paper. No pejorative meaning is intended here.

cal assumptions" (McDougall 1910, p. 11). Note that, although these criticisms were always directed at psychological hedonism, psychologists were offended not so much by hedonism as by the (narrow) teleology that accompanied it. Even if pleasure and pain were well-defined magnitudes, it would still be unrealistic to model people as so goal-oriented that they always sought the hedonic optimum.

Economists were hard-pressed to ignore this attack from psychology, for their own professed methodology admitted the truth of the criticisms. Economists like J. N. Keynes and Marshall acknowledged that economics left out many important motives, but protested that such an omission was unavoidable. It was simply not practicable to include in economic theory the myriad of motives underlying behavior. At this time, there was little discussion of the issue of hedonism among mainstream economists. However, they did quietly tone down their language, eliminating explicitly hedonistic language that might spark criticism. As Mitchell (1916) notes,

> In the later editions of his *Principles*,[8] Dr. Marshall changed utility "or pleasure" to utility "or benefit," defined consumer's rent as "surplus satisfaction" instead of "surplus pleasure," dropped his reference to Bentham's treatment of the propinquity and certainty of pleasures, and inserted a note contesting "the belief that economists are adherents of the philosophical system of Hedonism or Utilitarianism." (pp. 144–45)

B. *The Institutionalist Critique*

Although most late nineteenth century economists rejected the critiques of psychologists, a small group avidly joined in the attack and used it as a foundation for their own institutionalist, empirical brand of economics.[9] Institutionalists such as Simon Patten (1889, 1893), Arthur Hadley (1894), Thorstein Veblen (1898, 1909, 1914), Ezekiel Henry Downey (1910), Mitchell (1910), Frank Fetter (1916), Carleton Parker (1920), and Rexford Tugwell (1922) all emphasized the importance of instinct theory psychology for economics and developed the critiques of James and McDougall. Some writers, most notably Patten (1889), Veblen (1904, 1914), and Parker (1920), actually wrote treatises in which they used instinct theory to explain economic phenomena.

The mission of this school went far beyond the application of instinct theory. In fact, we do well to distinguish between two different critiques: (1) institutionalist and (2) psychological. Veblen saw the psychological critique of standard economics as a springboard for his more important campaign for the increased study of economic institutions and evolutionary change, rather than the formulation of more and more (as he saw it) metaphysical, static economic theories with no empirical content. Thus, in Hadley (1894) and Veblen (1898, 1909), and especially in Downey (1910) and Mitchell (1910), we see an argument for the application of instinct theory to economics, and simultaneously one for a greater emphasis on the evolution of institutions and the social embeddedness of economic activity. Downey (1910), in particular, argues that utility theory has taught us nothing about price determination.

> Elementary human nature may (or may not) be uniform, but it functions through institutions, and these are not uniform. The behav-

[8] These changes occurred in the third (1895) and fifth (1907) editions.

[9] In this paper, I use the word "institutionalist" quite broadly, to denote a school of thought within economics. Members of this school were unified by their emphasis on economic institutions and the embeddedness of human behavior in society. They also tended to view instinct theory favorably, so that economists who used instinct theory felt an allegiance to this school, along with those whose main focus was more literally institutionalist.

> ior of men can be neither predicted nor understood apart from their habitual modes of thought and from the institutional situation in which they act. It is not surprising, therefore, that a century and a quarter of diligent research into "labor-pain," "abstinence," "marginal utility," and the like, should have contributed substantially nothing to "the increase and diffusion of knowledge among men." (p. 268)

Even the notably noninstitutionalist economist George Stigler agreed with Downey that turn of the century utility theory had failed to find applications. Stigler (1972) writes,

> utility was not a part of the working equipment of economists during this period [1893–1923]. An economist working on taxes or trade or labor or the like did not introduce utility functions into his analysis and use them as a method of developing his subject. This absence of utility theory from theoretical work devoted to other subjects persisted for another two decades: not a single article in the *American Economic Review* of 1940 used utility theory in any fashion. (pp. 82–83)

Like many current critics of preference theory (cf. Etzioni and Paul Lawrence 1991; Albert Hirschman 1984; Kahneman, Jack Knetsch, and Richard Thaler 1986; Simon 1986; Michael Slote 1989), institutionalists argued that by focusing solely on the marginal utility calculus, economics had become overly narrow and now neglected many important causes of behavior (cf. Veblen 1909; Mitchell 1910; John Maurice Clark 1918). It had lost touch with the world, for it ignored the institutional, social context in which economic behavior occurred. By contrast, institutionalists approached real, sociologically complex economic questions with vigor. For example, in his 1917 paper "The I.W.W." (Industrial Workers of the World), Parker examines the causes of radical syndicalism, a movement which was having a damaging effect on American industry at the time. Rather than taking workers' motivations as given and unchangeable as a mainstream economists would have done, Parker explores the socioeconomic conditions which contributed to the feelings of disenchantment and disenfranchisement that characterized I.W.W. members. He suggests that, to alleviate industrial unrest, society must attack its root cause: the deplorable social conditions of day laborers.

Mainstream economists generally considered the arguments of institutionalists like Downey to be irrelevant. Even Marshall, who was more sympathetic to institutionalist concerns than most of his mainstream contemporaries, argued that economists can ignore most institutional issues because, in the long run, they are not important. Although customs often seem to be the underlying cause of economic behavior, this appearance is misleading because these customs owe their very continuity to their efficiency:

> even in such a country as India no custom retains its hold long after the relative positions of the motives of supply and demand have so changed, that the values, which would bring them into stable equilibrium, are far removed from those which the custom sanctions. (Marshall 1885, p. 48)

The second, psychological critique did, however, elicit a more substantial (though problematic) response from the mainstream. It was argued that the teleological nature of utility theory rendered it unscientific. Veblen's (1909) argument is best said in his own words. Speaking of utility theory, he notes that

> the theory is confined to the ground of sufficient reason instead of proceeding on the ground of sufficient cause . . . the immediate consequence is that the resulting economic theory is of a teleological character . . . instead of being drawn in terms of cause and effect. . . . The relation of sufficient reason runs only from the (apprehended) future into the present, and it is solely of an intellectual, subjective, personal, teleological character and force; while the relation of cause and ef-

> fect runs only in the contrary direction, and it is solely of an objective, impersonal, materialistic character and force. The modern scheme of knowledge [Science], on the whole, rests, for its definitive ground, on the relation of cause and effect; the relation of sufficient reason being admitted only provisionally and as a proximate factor in the analysis, always with the unambiguous reservation that the analysis must ultimately come to rest in terms of cause and effect. (p. 624)

Veblen admits that "the relation of sufficient reason enters very substantially into human conduct" (p. 625). However, "it is at the same time no less true that human conduct, economic or otherwise, is subject to the sequence of cause and effect, by force of such elements as habituation and conventional requirements" (p. 626). These factors, rather than the teleological factor, are more important in scientific inquiry, argues Veblen, and by limiting itself to sufficient reason alone, economics makes such inquiry impossible. Veblen apparently viewed teleology as the root cause behind the narrowness of standard economics, which was his primary concern. If economists gave up teleology, he mistakenly hoped, they would pay more attention to real, institutional questions.

Like their counterparts today,[10] the mainstream economists of the early twentieth century reacted to this sort of criticism by arguing that economic theory was not so narrow as its critics had claimed. Consider, for example, Henry Stuart's 1895 article on the "Hedonistic Interpretation of Subjective Value." Stuart agrees with Hadley (1894) and others that psychological hedonism is flawed. However, it does not follow that economic theory is itself faulty, for, he argues, this theory does not rely in any essential way on psychological hedonism (or on teleology). Human behavior, Stuart agrees, is not determined by the desire for pleasure. Instead, people seek objects; they have goals, which they pursue both consciously and unconsciously. When a person achieves a particular goal, such as food to satisfy hunger, her desire for a particular good becomes satiated, and she demands no more. This, he argues, is the behavioral foundation for the principle of diminishing utility; psychological hedonism itself plays no role. Furthermore, economics does not claim to say anything about the nature or origin of the goals which people seek, and it does not assume teleologically that these goals are rational and yield individuals maximal utility.

> In many cases the minimum of consumption may in a manner be prescribed by society, or may be fixed for the individual by custom, the real extent of whose influence upon him the individual may be utterly unconscious of; but the fact remains that increments of the commodity in question, in excess of the minimum, suffer, in these as in all cases, a progressive depreciation. Our desire for a certain thing may be from the point of view of some one else, utterly irrational. But it is none the less a desire because it is irrational or even habitual, and, as a desire, it is subject to the same inevitable law which all other desires obey. (Stuart 1895, p. 78)

Over time, more and more economists expressed similar positions. Joseph Schumpeter (1908), Philip Wicksteed (1910, pp. 434–35), Sydney Chapman (1911), Herbert Davenport (1902, 1913), and Hubert Henderson (1922) all declared that economics was independent of psychological assumptions, that it was interested only in facts of choice and

[10] For example, Stigler and Becker (1977) argue that, when applied creatively, preference theory can explain a surprisingly wide range of phenomena. To support this view, they provide models of addiction, advertising, and fashion, all of which are often used as evidence against economic theory, and as evidence for the malleability of tastes. They claim that by applying preference theory in this way, economists can free themselves of any need to turn to other disciplines (such as psychology, anthropology, or sociology) for guidance on the causes of preference formation and change.

made no assumptions as to the exact motivation behind these choices. Ironically, Veblen's criticism that economic theory was teleological would become the basis for arguments from the mainstream that there was really nothing wrong with economic theory, and that economists did not need to change their research agenda as Veblen had argued they should. The second institutionalist critique would divert attention away from the first, more fundamental one.

Institutionalists reacted to this perceived cooption of their movement with irritation. Economists were simply generalizing their theory so as to make it meaningless and irrefutable.[11] Downey (1910, p. 259) writes

> Some adherents of the marginal-utility school insist that the whole issue between hedonists and anti-hedonists is irrelevant to value theory. Admitting that marginal utilitarians usually have been hedonists they deny that the marginal-utility doctrines stand or fall with hedonism. Those who take this position . . . assert that economics is concerned only with the fact of choice between goods or between alternative activities, and not with the basis of choice.

However, Downey argues, this "disavowal of hedonism [deprives] marginal-utility economics of its whole content." The theory becomes reduced to a statement that people choose what they want, and want what they choose. Such circularities do not in any way advance our understanding of the economy. It would be much more useful to replace the entire theory with one that possessed "a psychologically tenable analysis of the process of individual valuation" (p. 253). For, Downey argues,

> if choices are really made between goods and not between the "utilities" represented by the goods, why talk of "utility" at all? And if it be admitted that economic choice is more frequently the outcome of habit, suggestion, and the like, than of a rationalistic weighing of alternative gratifications, the marginal-utility analysis of price loses all its significance.

Davenport (1913, pp. 97–102) responds to this criticism, but not very satisfactorily. Whatever an individual does, this behavior will be consistent with some preferences because, whatever an individual's motives are, rational or impulsive as they may be, "it is still true that men do choose" (p. 100). Thus, Davenport explicitly acknowledges that choice-based utility theory is tautological (as Downey asserts it is). "Rightly understood,—utility meaning merely the fact that a thing is wanted,—the marginal utility doctrine is almost an axiom" (p. 102).

The claim that utility theory was independent of teleology appears particularly weak when we notice the purely rhetorical nature of most of these claims. They were not accompanied by any substantive changes in theoretical approach, but were simply used to defend existing theory, unrevised. (Cardinal utility continued to be used.) In fact, the same people who argued for the independence of the principle of diminishing utility from psychological hedonism would often also, in the same work, use explicitly hedonistic language in their nonmathematical, heuristic expositions (cf. Wicksteed 1910, ch. 1; Marshall 1920). As Downey intimates, giving up psychological hedonism was not so easy, for in doing so, economists had to give up the very reasoning that made their choice-based analyses intuitively plausible.

More importantly, economists who professedly adopted a choice-based approach hardly bothered to attempt any explanations of any real choices that people actually made. Moreover, they did not attempt to answer the instutitionalist critique, by using the choice-based approach to model sociologically complex

[11] For similar recent arguments, see James March (1978, p. 155) and Simon (1986, p. 222).

economic behavior. Even if these economists claimed that the new approach could take into account diverse motives, they did not provide any concrete evidence for this claim. As Stigler (1950) notes, economists resisted attempts to add new elements to utility theory, in spite of the ability of these elements to make the theory more realistic. Rather than actively exploring the world they lived in, they theorized from an armchair. Economists, argues Stigler, should have insisted "that theories be examined for their implications for observable behavior, and these specific implications compared with observable behavior" (p. 395); but they did not do so.

> Not only were such specific implications ["The implication of the diminishing marginal utility of money, that people will not gamble. . ."] not sought [out] and tested, but there was a tendency, when there appeared to be the threat of an empirical test, to reformulate the theory to make the test ineffective. Thus, when it was suggested that there might be increasing marginal utility from good music, as one acquired a taste for it, this was interpreted as a change in the utility function[12] . . . . they did not anxiously seek the challenge of the facts. (p. 395)[13]

As a result, utility theory remained almost useless to economists who analyzed actual data.

IV. *The Rise of Behaviorism*

Like the mainstream economists mentioned above, some institutionalists also argued that economics should be founded on behavior rather than on (teleological) psychological hedonism; but these institutionalists argued that independence of psychological hedonism in no way implies independence of psychology proper. Rather, economics should employ the methods of behaviorist psychology, which was just then beginning its rise to prominence.

Behaviorism was part of a general philosophical movement to eliminate the "soft science" characteristics which had heretofore distinguished the human sciences from the physical sciences. One casualty of this movement would be verstehen. Thus, our focus shifts to the second, antibehaviorist thesis of this paper, which concerns the negative consequences of attempting to transform economics into a mechanistic, nonpsychological science. Section IV.A explains how behaviorist methodology introduced a new element into the debate over psychological hedonism and teleology. Then in Section IV.B, we study the influence of this movement on mainstream economic theory.

A. *The New Methodology*

Prior to this time, psychology, like economics, had relied a great deal on introspection. One must remember that hedonistic psychology was, after all, a movement in psychology as well as an element in economics. Even instinct theorists, who were so critical of the perceived methodological crimes of economists, themselves employed introspection when they enumerated the basic instincts that determined behavior. Theirs was merely a more inclusive introspection. Early twentieth century psy-

[12] In 1977, Stigler would take on this issue. Stigler and Becker (1977) provide a model of "addiction" to good music. The model uses the concept of "consumption capital" to explain increasing marginal utility from good music using a fixed utility function.

[13] Note the ironic similarity between Stigler's comments and Simon (1986, p. S222), who writes, in criticism of economists such as Stigler, "Neoclassical economists . . . are prepared to make whatever auxiliary empirical assumptions are necessary in order to preserve the utility-maximization postulate, even when the empirical assumptions are unverified. When verification is demanded, they tend to look for evidence that the theory makes correct predictions and resist advice that they should look instead directly at the decision mechanisms and processes."

chologists reacted against this a priori method. In his famous 1913 paper, John Watson laments the current state of psychology.

> Psychology, as it is generally thought of, has something esoteric in its methods. If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but it is due to the fact that your introspection is untrained. (p. 163)

Disputes, Watson argues, are irresolvable unless scientists can base their theories on *objective* rather than *subjective* data. Therefore, psychology must be reformulated so that it simply studies the laws of behavior, without discussing such vague concepts as "consciousness." By becoming behaviorist, psychology can finally attain the empirical rigor of the natural sciences.

By emulating the empirical methods of physics, behaviorists were proposing something quite radical. Prior to this time, the predominant methodological approach in the human sciences had been verstehen, which emphasized the differences between the physical and human sciences. By contrast, behaviorists argued that there was only one correct method of science, which should be employed universally. McDougall, an adherent of the older, instinct school, opposed this philosophy and defended verstehen. He notes (1926, p. 370) that due to the success of physical science,

> there obtains very widely at the present time the opinion that we understand mechanical process in some more intimate sense than we can understand appetitive process; and that therefore, it is the business of all science to explain its facts in terms of the laws of mechanism, and that all appetitive processes can only be rendered intelligible if they can be reduced to the mechanical type. But this is a delusion. Of the two types of process, we certainly understand the appetitive more intimately than the mechanical; for we directly experience appetition, we have an inside acquaintance with it . . .

Watson's methodological attitude was representative of the logical positivist methodological atmosphere which prevailed among the scientists of the early twentieth century. According to logical positivism, science does not seek to help us to *understand* the nature of reality; that is the domain of metaphysics (a pejorative term). *Understanding* is, according to this view, a meaningless pursuit. The world simply consists of observable empirical regularities, and science should therefore restrict itself to *describing* these, in the form of objective, falsifiable propositions. Every scientific concept and theory should have a clear empirical meaning; no ambiguity can be tolerated. Therefore, psychology should restrict itself to describing the behavior of individuals under different conditions. Such concepts as consciousness, or thought (rational or irrational), are too vague to be of scientific value.

Among economists, the foremost proponent of logical positivism was Terence Hutchison. In his widely read 1938 book *The Significance and Basic Postulates of Economic Theory*, he argued against the verificationist methodology that had been prevalent in economics, and formally introduced Popper's concept of falsificationism into economics. Hutchison makes a fundamental distinction between propositions of pure theory and scientific propositions. Propositions of pure theory are generally tautological; they are derived logically from definitions, and as such are analogous to mathematical theorems. However, in order for a proposition to qualify as scientific, it must say something about the real world, and be falsifiable by some conceivable observation of this world. Therefore, if economics is to be a science, then we must transform the tautological propositions of pure theory into falsifiable scientific propositions, by as-

signing precise empirical meanings to all theoretical concepts.

Hutchison criticizes the tendency of economists to evade tests of their theories. If an observation seems to contradict the proposition, say, that demand declines when the price of a good rises, then economists usually blame some extraneous cause.[14] When the price rises, demand declines, but only ceteris paribus—assuming that nothing else important changes and thus reverses the causality. By making such arguments, economists render their propositions tautological.

> no attempt is made, usually, to indicate under what conditions they are true and under what false, and the meaning of the vital qualification "ceteris paribus" is left hopelessly imprecise. The ceteris paribus assumption, just as much as any other, must be precisely formulated if the propositions it qualifies are to have any clear meaning. The *intention* of the assumption obviously is to lessen the falsifiability of the too often falsified generalization "If the price of a good rises, the amount sold declines." But exactly *how far* is its falsifiability thus lessened, and **if it remains an empirical proposition, what conceivable possibilities of falsification remain**? (p. 41, boldface added)

From this perspective, Hutchison criticizes the antiempirical nature of marginal utility theory. The law of maximum utility and Gossen's law of diminishing utility are generally derived from a priori assumption or from introspection. These assumptions have come under attack. He cites Benham, who wrote in 1930 that Gossen's law was "at best an unproved hypothesis, obtained by an amateur incursion into the domain of psychology" (Hutchison 1938, p. 134). Economists are at a loss to respond, Hutchison argues; they cannot appeal to any empirical data, for utility theory has no empirical content. If economists want their propositions to qualify as science, and if they wish not to be vulnerable to attack, then they must reformulate their theories so that they say something about "how people do in fact behave" (p. 134). Hutchison emphasizes, in contrast to more extreme behaviorists, that concepts such as utility, or even social utility, are not inherently unscientific; they have merely been so in practice. They will become scientific if and only if we choose to make them so, by giving them precise empirical (behavioral) meanings.

Many institutionalists adopted the logical positivist philosophy, and it proved an effective weapon in their attack on economic orthodoxy (although, in the long run, it proved counterproductive). Hadley (1894), Downey (1910), Tugwell (1922, 1924b), Sargant Florence (1927), and Morris Copeland (1931) all criticize the antiempirical nature of mainstream economics. Thus, Hadley (1894) argues that "Nearly all the conclusions of the Austrian [marginal utility] school of economics are framed in such language that nobody could ever find out, by observation of prices, whether they were right or wrong" (p. 260). Florence (1927, pp. 86–89) laments the decline in the use of induction since the time of Malthus. Downey (1910, p. 268) uses stronger language. "Marginal-utility economics is an admirable body of dialectics—scarcely surpassed for subtlety, reach, and want of content by the finest products of medieval scholasticism . . ."

At the root of the problem was the tendency of economists to explain phenomena by appealing to unobservable psychological magnitudes such as utility. Hadley (1894) argues (echoing J. S. Mill) that by speaking of utility as an exact mathematical quantity, economics "gives an appearance of definiteness to things which are extremely uncertain" (p. 254).

[14] Note the similarity to Watson's disdain for psychologists who, when faced with results that fail to reproduce their own findings, blame the results on untrained introspection.

However, Hadley argues (citing Patten), the utility concept does not possess the definiteness assigned to it by theory, for all sorts of problems plague any attempt to pin this utility down, or to verify that people in fact maximize this quantity.

Rather than playing metaphysical games, Copeland (1931) suggests, economists should formulate "specific sociohistorical theories" (p. 70) which can be tested empirically. They should adopt a realistic, sociological approach and study the economy as it is: embedded in social institutions. As an example of this approach, he cites Veblen's *Theory of the Leisure Class*. He recognizes that economists want to do more than enumerate empirical regularities; they want to *understand* the underlying cause of a phenomenon. However, Copeland argues, "Physicists long ago left the search for ultimate causes to metaphysics. Economists would do well to follow suit" (p. 70).

Given this background, we return to the main issue, the critique of declarations of independence from psychology. Tugwell (1922) notes that economists have become increasingly aware of the criticisms that have been launched against such concepts as utility. "There has resulted a searching of souls among teachers and writers" (p. 321). The most common response has been "to deny that economics necessarily has foundations in human nature at all" (p. 321). However, as Florence (1927) points out, such declarations of independence have been insincere. Citing Henderson (1922) as an example, Florence notes that

> Henderson does not practice what he preaches, in fact cannot practice what he preaches. Throughout his book the reasoning is deductive, and he deduces his conclusions from psychological assumptions; he cannot therefore argue without assuming some sort of psychological theory. (p. 54)

If economists think that they can be independent of psychology and also independent of a priori assumptions, then they are fooling themselves. They must get their assumptions from somewhere.

Tugwell (1922) argues that the attitude of economists toward psychology results from a misunderstanding. If it is true, as some people think, that psychology is a subjective science, then in order to free itself of subjective concepts, economics must free itself from psychology. However, "this is precisely the weakness behaviorists object to and the root of their departure from the older forms of psychology" (p. 330). If economists are to formulate a new economics, free of the a priori assumptions that have plagued previous theory, then what but behaviorist psychology can provide a sound foundation for the new theory? Perhaps economists can avoid psychology and at the same time be scientific, but only by limiting themselves to describing the regularities of prices and quantities (p. 322). Most economists aspire to do more; they want to explain where these prices come from, and for this task, behaviorist psychology is indispensable.

B. *"Behaviorist" Mainstream Economics*

Until now, the mainstream economic arguments we have considered have been relatively straightforward and even familiar. However, the mainstream response to behaviorism and logical positivism was far more complex.

Near the turn of the century, many economists did at least nominally renounce psychological hedonism and argue that utility theory could be based on choice behavior alone. However, as we saw above, the arguments of Hadley, Davenport, Marshall, and others indicate that most of these economists (ostensibly) rejected psychological hedonism, not out of any inner conviction that

choice-based analysis was more scientific, but simply in order to defend existing theory from attack. It appears that, at some point, the situation changed and the logical positivist critique of marginal utility theory truly became internalized by the profession. Slowly, the hedonistic discussions that had once been so common in textbooks disappeared, as Marshall was supplanted by the likes of John Hicks and Paul Samuelson. In a 1938 article called "The End of the Marginal Utility Theory?," Harro Bernadelli notes that more and more people have come to believe that the marginal utility concept is unscientific;

> whereas originally this opinion has been voiced only by writers who did not belong to the camp of marginal utility theorists, by outsiders so to speak whose anti-mathematical bias made them blind to the success the marginal utility theory could claim in all branches of economic inquiry, lately a complete change in the situation has taken place. *The argument has found its way into the friend's camp itself* . . . Weighty mathematical reasons have made them doubt that marginal utility is a legitimate concept. No longer can the argument be brushed aside as irrelevant prejudice, *the crisis has developed from within.* (p. 192, italics added)

1. *Ordinalism.* This movement "from within" dates back to the efforts of Irving Fisher (1892) and Vilfredo Pareto (1900, 1901, 1906, 1911) to replace the concept of measurable (cardinal) utility with that of ordinal preferences.[15] They realized that, if we take as our data only the choice behavior of individuals, then the cardinal utility function is not well defined. In other words, the behavior of an individual does not determine that individual's utility *function* uniquely, not even up to a linear transformation, because any monotone transformation of a given utility function predicts an individual's choices just as well. Only the *order* in which options are ranked is determined uniquely. Pareto proposed that a choice-based ordinal utility function be adopted precisely because he was concerned that one could not *measure* utility as a precise psychological quantity. In a letter to Benedetto Croce, he writes (1900, p. 183) "I was worried about that *pleasure* and that *pain* which had to be measured, because in reality, nobody is capable of measuring pleasure. Who can say what pleasure is double another pleasure?" Thus, in his 1906 *Manuale*, although Pareto does talk of "pleasure," he does so in such a way as to avoid implying that this pleasure is a well-defined numerical quantity.

Just as Watson hoped that behaviorism would bring psychology into the natural sciences, Pareto hoped that ordinalism would forge economics into a science on a par with physics.

> This entire theory . . . rests on no more than a fact of experience, that is, on the determination of the quantities of goods which constitute combinations between which the individual is indifferent . . . The theory of economic science thus acquires the rigor of

[15] By "measurable" utility, I mean here the proposition that the utility function can be determined uniquely up to a linear transformation. Thus, measurability and cardinal utility were viewed as equivalent concepts. In modern usage, the terms are somewhat different. Today, "measurable" utility is sometimes used to mean utility that can be measured psychologically. Cardinality of a utility function, however, is simply the mathematical property of uniqueness up to a linear transformation. Von Neumann-Morgenstern utility functions and time-discounted utility functions are two examples of this cardinality. When the von Neumann-Morgenstern axiomatization of expected utility was published, there was some discussion about whether the authors had resurrected measurability from its prior disrepute. A consensus was then reached that cardinality and measurability (in the above sense) were different concepts, and that the former in no way implied the latter. In the period that we are studying in this paper, this clear distinction did not yet exist, and cardinality was believed to be equivalent to measurability. The approach of these older economists was not necessarily wrong. Whether today's sharp distinction between cardinality and measurability makes psychological sense remains debatable.

> rational mechanics; it deduces its results from experience, without bringing in any metaphysical entity. (1906, p. 113)

For Pareto, ordinal preferences did not require any psychological interpretation; they merely summarized empirical regularities of choice behavior.

We noted earlier that the marginal utilitarians viewed utility and marginal utility (motive) as the analogs of energy and force in physics. Logical positivist physicists argued that the concept of a force was metaphysical, and that physical laws should enumerate only the equations of motion of bodies. Pareto saw his break with hedonism as analogous to this movement in physics. "[I]n pure economics there was something which corresponds to forces in mechanics. Given the fact of choice, that something is no longer there. However, mechanics can be studied leaving aside the concept of forces" (1900, p. 185), and so, Pareto implies, can economics be studied leaving aside the concept of motives. Pareto does not mind losing the teleological rationale for consumer theory. "Let others concern themselves with the *nature*, with the *essence* of 'value'. I am interested only in seeing whether I can discover which regularities are presented by prices" (1901, p. 204). Thus, a new analogy to physics arose. Physicists would discuss equations of motion, and economists would discuss individual behavioral patterns and demand functions. By arguing in this manner, Pareto went farther than other economists, who renounced psychological hedonism and argued that economics makes no assumptions about which particular motives move people to act. Instead, Pareto suggested that the very discussion of motives was unnecessary and metaphysical.

At first, most of the profession ignored Pareto's critique, and his *Manuale* was the only textbook in the first decades of the century to use the ordinal approach. However, a series of influential papers by Eugene Slutsky, Hicks, Roy Allen, and Paul Samuelson eventually established ordinalism as the mainstream approach to consumer theory. Like Pareto's *Manuale*, these papers differ from other declarations of independence in that they propose actual changes in the theory of the consumer rather than mere changes in vocabulary (although even these changes failed to address the first institutionalist critique), and in that the authors are motivated by logical positivist rather than rhetorical concerns.

First, Slutsky (1915) reformulated utility theory so as to express its implications in terms of prices and quantities, which can be observed. Thus, he provided a theoretical foundation for the transition (discussed above) from a theory based on psychological motives, to one based on individual demand functions. The new formulation would hopefully be more conducive to empirical testing. He motivates his paper by arguing that "to place economic science upon a solid basis, we must make it completely independent of psychological assumptions and philosophical hypotheses" (p. 28). However, Slutsky was not an extreme behaviorist, and in his paper he does not rule out the possibility that at some point in the future, the connection between the objective laws of consumer demand and the phenomena of the mind might be explored experimentally.

Then, in 1934, Hicks and Allen published their famous paper, "A Reconsideration of the Theory of Value." In this paper they fill in some of the gaps left behind by Pareto's *Manuale*. Although he believed strongly in ordinalism, Pareto unwittingly allowed cardinalism to sneak its way back into his analysis. First, he made assumptions about the signs of the second derivatives of the utility function and thus retained Gossen's law of diminishing marginal

utility; in fact, these signs are not well defined in a choice-based approach.[16] Second, his "second postulate" assumed that people could compare the magnitudes of differences between utility levels; this postulate is actually closely related to measurability of the utility function, and Oscar Lange (1934) believed that the two concepts were equivalent.[17]

To solve this problem and to assure a complete eradication of cardinal utility (and therefore of psychological hedonism), Hicks and Allen (1934) eliminate all discussion of either marginal utilities or second derivatives of the utility function—both of which are not well defined in an ordinalist framework.[18] In place of marginal utility they introduce the now standard concept of the "marginal rate of substitution," which is equivalent to the ratio of two marginal utilities in the old cardinalist notation. In the place of Gossen's law they introduce the increasing rate of marginal substitution, which basically states that indifference curves are convex to the origin. This new terminology quickly became standard in the textbooks of the 1930s (Richard Howey 1973, p. 36).

2. *The Difference Between Ordinalism and Behaviorism.* Treated *historically* (in the sense of its place in the history of economic thought) or rhetorically, Hicks and Allen (1934) was clearly part of the antipsychological movement within mainstream economics, and it brought economic theory one step closer to eliminating all psychological elements.[19] However, in a purely positive sense, their work was in fact perfectly consistent with what we could call a *psychological ordinalist* (rather than psychological hedonist) perspective. This is the view, commonly held by today's economists (I submit), that one cannot measure utility as a (cardinal) quantity of pleasure, but nevertheless, people do act purposefully and therefore they do have (ordinal) preferences that really mean something psychologically (rather than being ex post rationalizations of behavior). The psychological meaning of preferences does not involve pleasure, as in psychological hedonism; but this psychological meaning does exist nevertheless. In effect, Hicks and Allen had provided a good foundation for a genuine declaration of independence from psychological hedonism, but not from psychology proper. The very plausibility of their theoretical framework still rested on the inherently psychological concept of (ordinal) utility.

For precisely this reason, Paul Samuelson (1938) argued that Hicks and Allen (1934) did not go far enough in freeing economics from psychology. Perhaps preference functions were now

[16] In a choice-based approach, the signs of the first derivatives of a utility function are defined by whether an individual does or does not desire more of any given good. However, second derivatives are not uniquely specified. For example, if there is only one good and an individual desires more of that good, the first derivative of utility is positive for any utility function u, because u must be increasing. However, u'' can have any sign at all.

[17] The discussion in the literature of Pareto's "second postulate" is somewhat confusing because, at least in his *Manuale* (1906, p. 191), Pareto suggests the use of this postulate but rejects it because (he thinks) it implies measurability. However, others seemed to read Pareto as if he really did adopt the postulate.

The essence of Lange's argument for the equivalence of Pareto's second postulate and measurability, is the following: If we assume that differences between levels of utility can be compared, then we can say not only whether x is better than y, but also *how much better* x is than y. Is the preference for x over y as strong as the preference for w over z? In an ordinalist approach, we know only whether or not x is better than y. "How much better" has no meaning here; this is a cardinalist concept. Thus, Pareto's second postulate is essentially a cardinalist assumption.

[18] The sign of marginal utility is well defined, but the absolute magnitude is not.

[19] Their paper was certainly perceived in this way by contemporaries. See Bernadelli (1935, p. 71).

theoretically determined by observable behavior, but what difference does this fact make, asks Samuelson, unless we can actually derive these functions from this behavior, rather than relying on introspection? As a good logical positivist, Samuelson argued that the indifference concept must have an empirical referent; otherwise, it is meaningless. It is worth quoting Samuelson at length.

> The discrediting of utility as a psychological concept robbed it of its only possible virtue as an explanation of human behaviour in other than a circular sense, revealing its emptiness as even a construction. As a result, the most modern theory confines itself to an analysis of indifference elements, budgetary equilibrium being defined by equivalence of price ratios to respective indifference slopes.
>
> Consistently applied, however, the modern criticism turns back on itself and cuts deeply. For just as we do not claim to know by introspection the behaviour of utility, many will argue we cannot know the behaviour of ratios of marginal utilities or of indifference directions.
>
> Why should one believe in the *increasing rate of marginal substitution*, except in so far as it leads to the type of demand functions in the market which seem plausible? . . .
>
> Hence, despite the fact that the notion of utility has been repudiated or ignored by modern theory, it is clear that much of even the most modern analysis shows vestigial traces of the utility concept. Thus, to any person not acquainted with the history of the subject, the exposition of the theory of consumer's behaviour in the formulation of Hicks and Allen would seem indirect. The introduction and meaning of the marginal rate of substitution as an entity independent of any psychological, introspective implications would be, to say the least, ambiguous, and would seem an artificial convention in the explanation of price behaviour. (pp. 61–62)

Samuelson proposes "that we start anew in direct attack upon the problem, dropping off the last vestiges of the utility analysis" (p. 62).

To accomplish this goal, he introduces revealed preference theory.[20] This approach allows one to derive indifference curves solely from the observation of purchases in the market. Thus, the theory provides the essential link between individual demand functions and preferences. This link is essential because, logical positivists argued, only individual demand functions (or other behavioral patterns) have an objective empirical meaning, and therefore for preferences to have such a meaning, they must be derivable from demand functions.

In his paper, Paul Samuelson expresses the hope "that the orientation given here is more directly based upon those elements which must be taken as *data* by economic science" (p. 71). Therefore, when it was first proposed, *revealed preference theory was intended as an empirical tool.* Samuelson hoped that, by observing enough market choices of an individual, one could test his conformity with preference theory, and if he satisfied the revealed preference axioms, one could then predict his future behavior. Finally, economics would be completely free of psychology; it would depend only on observable behavior.

V. *The Decline of Behaviorism*

If the Samuelsonian dream had come true, today's microeconomics would be a science as solidly empirical as physics, and we would not have to even trouble ourselves about the importance or unim-

[20] Paul Samuelson (1948) develops this approach further, making use of Little (1949), which was, it appears, actually published earlier. Finally, Houthakker (1950) introduced the "strong axiom of revealed preference," which made revealed preference theory and ordinal utility theory *logically* equivalent for demand theory. This logical equivalence in no way implies methodological equivalence. The motivation for revealed preference remained empirical. Ironically, revealed preference attained its apogee of mathematical elegance just as the popularity of behaviorism was waning.

portance of the realism of our assumptions. (They would simply be realistic!) Of course, this dream was to be elusive. The behaviorist edifice would slowly crumble, for both empirical and philosophical reasons.

Section V.A continues our focus on our antibehaviorist thesis. It discusses the dissident voices against behaviorism within mainstream economics. Critics of behaviorism defended verstehen and pointed out its indispensability to economics. Section V.B lends support to these criticisms, and describes the stifling effect of behaviorism, on economic science in general, and on any potential attempt by mainstream economics to address the institutionalist critique, in particular. Here, we shift back to our interdisciplinary pathology thesis, and we see the close connection between the two theses. Finally, in Section V.C we encounter the empirical stumbling blocks which placed the final nail in the coffin of behaviorist economics. This section illustrates the extreme difficulty of attempting to derive preferences from objective data alone, and it supports the view that preference theory depends on verstehen for its plausibility.

A. In Defense of Verstehen

Perhaps the most convincing evidence that behaviorism did in fact become the politically correct[21] methodology among mainstream economists, lies in the tenor of the arguments that a few economists made against behaviorism and in defense of verstehen. Such arguments go back to Croce (1900, 1901) who engaged Pareto in debate. Before proceeding, one must understand that, within the rhetoric of the time, cardinal utility was more than a particular theoretical concept; it symbolized verstehen. Those who defended verstehen invariably did so within the context of a defense of cardinal utility. Therefore, one should not dismiss the arguments which follow simply because they defend the admittedly problematic concept of measurability. They have a broader meaning as well. We will consider in detail only the ideas of Harro Bernadelli and Frank Knight, who were particularly outspoken during behaviorism's height of popularity.

Bernadelli was one of the few economists in the 30s who defended the notion of cardinal utility. He therefore defended Pareto's second postulate (see Section IV.B.1). This postulate became a symbol of cardinalism, which was itself a symbol of verstehen. In a 1934 article, he criticizes Hicks and Allen (1934) for giving up Pareto's second postulate and Gossen's law, arguing that "the simplest economic reasoning is interwoven with psychological elements, in such a way that their elimination is against common sense" (Bernadelli 1934, p. 71). True, Bernadelli says, for the most part, economics can do without the second postulate, but to give it up

> is similar to the behaviour of a man who cuts off one of his legs, in order to see how he gets on as a cripple. And it is extraordinary how one can get on without the leg of the second postulate, as the results of Pareto, and more recently of Allen and Hicks, prove. Yet this would seem insufficient reason for making a virtue of such an amputation. (pp. 71–72)

Similarly, Knight (1944) questions whether the arguments against the concept of force in physics could be applied against the use of marginal utility in economics.

> Early in the history of modern physics [,] objection was raised to use of the concept of force, on the double ground that it is never open to direct observation and that it is not objective but animistic or anthropomorphic. It was (and is) pointed out that we observe or measure only the effects of forces and con-

[21] Politically correct, but not necessarily dominant. Economists felt pressured to be behaviorists, even if not everyone succumbed to this pressure.

> tended that it would be simpler and more candid to talk only about effects, i.e., equations of motion . . . The place of motive in economic choice presents a closely parallel problem. (p. 305)

Even in mechanics, the arguments against force run into problems, but

> in the field of conduct, the a fortiori argument for the reality of the force (motive) is irresistibly conclusive. . . . everyone is directly aware of it in his own experience and has the most certain knowledge of its reality in others. . . . Our thinking about conduct must conform to common-sense introspection and intelligible intercommunication, which always runs in terms of "reasons" for action or choice, meaning ends or motives. (p. 307)

Knight (1940) criticizes positivists such as Hutchison, who pretend "that knowledge of people's minds is an inference, from the observation of their bodies, of their physical behavior" (p. 161). Nothing can be further from the truth. "What we immediately, consciously, apprehend is the 'meaning,' and if called upon to reproduce the physical facts we should do so chiefly by 'deduction' from the remembered meaning, not from any direct recall of sense data" (p. 162).

Knight (1944) argues that it is not possible to dispense with motives in explaining human conduct, as one can dispense with forces in physics. Whereas it is possible to derive forces from the behavior of physical bodies, it is not possible to derive motives from the behavior of human beings. Among other reasons, "action rarely leads to exactly the intended result, because it is always affected by error . . ." (p. 310). Therefore, if we wish to understand human conduct, we do much better by using our introspection than by relying on unreliable behavioral data which tells us little about the true underlying causes of what we observe. Knight (1924) agrees that the use of introspection prevents economics from becoming a rigorous science like physics. However, unlike his contemporaries who reacted by renouncing introspection, Knight suggests that economics should not try to emulate physics, for it studies inherently different subject matter for which the (logical positivist) scientific approach is inappropriate.

B. *The Sterility of Behaviorism*

Historical developments bore out the arguments of people like Knight; an economics without verstehen could not adequately address the challenges that it faced. Ironically, rather than providing a foundation for a new, solidly scientific economics, behaviorism placed economics in a straightjacket that repressed any constructive response to the first institutionalist criticism, which emphasized the narrow scope of economic theory and the neglect of institutional and sociological factors in economic analyses.

Eventually, economics would broaden its focus somewhat; we are all familiar with the wide scope of today's microeconomics (cf. Becker 1976, 1992; Akerlof 1984). However, this broadening occurred only after behaviorism had declined and psychological language had lost some of its stigma. Whether economics widened its scope in the correct way remains a complex question worthy of attention.[22] But setting this issue aside, in order to broaden its scope while retaining traditional theoretical tools, economics had to adopt a broader view of preferences which allowed elements other than market goods to enter as arguments, as in Kelvin Lancaster's characteristics model.[23] A sociologically com-

[22] Contemporary critics argue that economics should learn from other disciplines such as psychology and sociology, instead of simply exporting a fixed theoretical apparatus derived largely from introspection rather than from observation (cf. Etzioni and Lawrence 1991; Simon 1986; Tversky and Kahneman 1986).

[23] Lancaster (1966) proposes a model of consumer choice in which market goods are merely inputs in the production of more fundamental goods, called characteristics. Thus, the technology

plex economics is, by its very nature, an economics that does not restrict its attention to market phenomena. For example, if we are to model altruistic behavior (as in Becker 1974), we must allow the utility of one individual to enter into the preferences of another. Such a model could never have passed scientific scrutiny during the heyday of behaviorism because it necessarily views utility in very concrete psychological terms.[24]

In fact, Wassily Leontief (1947) argues that the behaviorist atmosphere of the early twentieth century suppressed an otherwise natural propensity to develop more general utility functions. Intuitively, economists had always understood preferences as being defined over "general categories of needs," rather than over "particular individual commodities."

> Earlier theorists such as Irving Fisher tried to rationalize this approach through the concept of services of consumers' goods as separate variables distinct from the goods themselves . . . Although logically consistent [,] this theory, and rightly so, became a victim of the same wave of criticism that swept away the old-fashioned value theory. *If measurable utility proves to be a fictitious concept incapable of operational verification [,] so also will the abstract category of generalized needs.*
>
> The third and latest phase of this theoretical development began with the surrender of all the old untenable positions and a complete retreat to the concept of a general indifference variety described in terms of individual consumers' goods. (pp. 163–64, italics added)

Here, we find a more forgiving explanation for the failure of economists to formulate a more constructive response to the first institutionalist critique. The methodological pressures of the time, for which the institutionalists were partly responsible, restricted economists to a purely theoretical response, such as Davenport's statement that economists placed no limits on admissible preferences. To propose anything more constructive (such as a particular preference relation with some explanatory value), an economist would have been forced to utilize obviously psychological language, which would have drawn even harsher criticism than did his inaction. Quite understandably, economists chose inaction.

C. *The Empirical Failure of Behaviorism*

When Knight and Bernadelli wrote, they were defending the old methodol-

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of consumption is a (usually linear) mapping from the space of market bundles into the space of characteristics combinations. The standard theory of consumer choice is a special case of this model, in which each good provides only one characteristic, and the mapping above is just the identity map. When we allow for more complex market goods which supply several characteristics, the Lancaster model allows one to consider which of a series of goods satisfies consumer needs most efficiently, since not all potential goods should actually be produced. The model also allows us to think in terms of more fundamental (subjective) consumer needs, rather than only in terms of objective market purchases. We can ask, "Where do market preferences come from?"

Actually, Lancaster was not the first to publish this theory. Gorman's lesser known paper (1956) was published earlier.

[24] It may be argued that we can solve this problem by defining altruism behaviorally, and stating that person *A* prefers situations in which person *B* obtains what *B* prefers. However, in order to flesh out such a theory, one would then have to consider how person *A* analyzes the tradeoffs between his own (individualistic) welfare, and that of person *B*. It would be difficult for a theorist to even think about such a question while forcing all thoughts of introspection out of her mind, for an altruistic situation is precisely one in which *A* introspects into *B* before making his decisions. Furthermore, a theorist could not motivate the assumptions she eventually makes about the tradeoffs between the welfares of *A* and *B*, without engaging in introspection. It certainly is possible to avoid this introspection, and to formulate a model nevertheless, but such a model would be rather unconvincing and lacking in insight. The best way to model altruism is to talk about it as it is, a profoundly psychological phenomenon in which the utility of one person enters into the preferences of another. Any other construction would be artificial.

ogy of J. N. Keynes against the new methodology of logical positivism, which seemed then to be enveloping economics. Yet the reign of logical positivism did not last long. As soon as it was born, doubts began to multiply about the coherence, practicability, and usefulness of the behaviorist program. It was simply not empirically possible to base preference theory on behavior alone.

Originally, when Samuelson (1938) proposed the revealed preference approach, he motivated it by arguing that, if preferences are to be a valid theoretical concept in economics, then they must be derivable from objective data, such as prices and quantities. In a 1961 survey article, Hendrik Houthakker praises the revealed preference approach, and emphasizes its role in reinforcing "the emphasis on observable implications that is gradually transforming consumption theory from a mere philosophizing about utility to an essential component of empirical research." Yet, he admits, "The approach has, perhaps, not yet opened as many new avenues of research as had at one time been hoped" (p. 713). What went wrong? The answer lies, in part, in the severe problems facing those who attempted to apply revealed preferences empirically. In the 30s, 40s, and 50s, many psychologists did attempt to derive utility functions experimentally (cf. Louis Thurstone 1931; Harold Gulliksen 1946; Stephen Rousseas and Albert Hart 1951; Frederik Mosteller and Philip Nogee 1951; Mosteller 1951; Purnell Benson 1955). Such experiments concentrated for the most part on deriving expected utility functions, especially after the publication of John von Neumann and Oscar Morgenstern's 1944 axiomatization of expected utility. However, as Duncan Luce (1959) writes, even in this area, "it must be admitted that the data so far collected are most ambiguous" (pp. 76–77).

In their 1942 article on "The Empirical Derivation of Indifference Functions," Allen Wallis and Friedman share Luce's skepticism about attempts to derive utility functions experimentally. In contrast with Luce's decision-theoretic emphasis, these authors are concerned with the more general role of indifference functions in economics. They praise ordinal preference theory for being "free of irrelevant or erroneous assumptions about human psychology" and for its fruitfulness "in *theoretical* economics" (p. 176, italics added). However, Wallis and Friedman argue that, when we are interested in "the organization and analysis of *empirical* data on consumer expenditures" (p. 176, italics added), indifference functions are useless. The approach fails because one cannot derive such functions from available data.[25]

The authors first consider the "experimental approach" which, as we saw above, was quite popular in the 1940s. They stress Thurstone's famous 1931 article on "The Indifference Function," in which he derived an indifference curve made up of two-dimensional vectors containing quantities of coats and hats among which the subject expressed indifference (in a questionnaire format). Wallis and Friedman question the validity of this experiment.

> It is questionable whether a subject in so artificial an experimental situation could know what choices he would make in an economic situation; not knowing, it is almost inevitable that he would, in entire good faith, systematize his answers in such a way as to produce plausible but spurious results. (p. 179)

[25] Wallis and Friedman do not actually cite Paul Samuelson's 1938 paper, which had not yet gained its current fame. However, they do address ideas much like Samuelson's, and they assume that such ideas are popular. Samuelson was, it appears, not alone in seeking an objective foundation for preference theory. He was simply the first to address this concern theoretically.

Furthermore, making such experiments more representative of day-to-day economic decisions is impracticable.

Having rejected the experimental derivation of indifference curves, Wallis and Friedman go on to evaluate the "statistical approach," which uses "data on consumer purchases for the quantitative determination of the indifference function" (p. 183). (This method appears to be almost identical to that of Paul Samuelson, 1938, but they do not mention his paper.) The statistical approach is as problematic as the experimental one, and for much the same reasons. In order to obtain enough data points to perform a decent analysis of any one individual, one must allow a reasonably long period of time. However, in the course of any time period sufficiently long, preferences will surely change, and thus the indifference curve (and preferences) measured will not be accurate.[26] In spite of Paul Samuelson's (1938) high hopes, the revealed preference approach had proved empirically useless. Like the indifference theory it was meant to replace, it too would become an artificial theoretical construct of little, if any, explanatory value.

In response to these problems, Wallis and Friedman argue that a new approach to the prediction of consumer behavior must be found. Refinement and improvement of indifference theory solve nothing, they argue, because the problems are "an inherent part of the theory and represent not uncharted territory but seas in which no solid ground for empirical work exists" (p. 188). Rather than become entangled in a useless theory, economists should utilize the new science of econometrics to derive statistical relationships between consumer demand and various empirically measurable factors that influence this demand.

Wallis and Friedman were not alone in their disenchantment with behaviorally based indifference theory. Little (1949), one of the principal inventors of revealed preference theory, expresses similar concerns. He claims that "There is absolutely no logical reason why market demand curves [rather than consumer preferences] should not be considered to be the basis of price theory." People have held on to the theory of consumers' behavior out of a desire to emulate the success of methodological individualism in physics.[27]

However, Little argues that such a methodology simply does not work in economics.

> But it is only useful thus to push back the analysis [i.e., employ methodological individualism] if laws can be formulated about the behaviour of the "atoms." The theory of consumer behaviour is, however, a deductive system based on a postulated consistency, and, otherwise, it contains no hypotheses based on induction which would enable us to predict the behaviour of prices. The behaviour of human beings seems, in fact, to be less predictable than the behaviour of aggregates of human beings, and, to the extent to which this is true, the theory of consumer's behaviour must, at least as far as positive economics is concerned, continue to be nothing other than a purely logical exercise, because, in price theory, it is the aggregates, and not the individuals, which are of interest. (p. 99)

[26] For other reasons, Wallis and Friedman also reject the use of "data for the same period but many individuals" (p. 184).

[27] The reference here is to mechanics, the subdiscipline of physics best known to the economists of the period. Classical mechanics analyzes matter by reducing it to a collection of particles, each of which is moved by forces, and each of which exerts forces on other particles. Thus, mechanics practices methodological individualism, for it explains the behavior of large objects by breaking them down into their component parts. Similarly, neoclassical economic theory practices methodological individualism, for it interprets macro behavior such as price movements as the product of the behavior of a mass of individual consumers. The consumers of neoclassical economics play the same role as do the atoms of classical mechanics.

By arguing that preference theory should be supplanted by the analysis of prices and quantities, Wallis and Friedman and Little echoed the earlier arguments of Mill (de Marchi 1973), and later Gustav Cassel (1918) and many institutionalists, that economics did not really need a separate theory of value—only a theory of price determination. The adoption of a behaviorist, logical positivist approach to indifference theory forced economists into accepting such arguments. A nonbehaviorist such as Knight could defend preference theory or utility theory by appealing to our introspection; this theory explained the underlying psychological causes behind the prices we observe. By rejecting introspection, behaviorists divested themselves of this argument, so that they could defend preference theory only on empirical grounds. Not even psychological ordinalism could pass scrutiny. According to the logical positivist philosophy, preferences could be a valid concept only if they could be linked to measurements of behavior, or to demand functions, which reflected behavior. Revealed preference theory attempted to provide this essential link. The link had now been broken; in practice, the derivation of preferences from behavior could not be done. Therefore, preference theory was left without any raison d'être at all. In order to be rigorously scientific, economics was forced to limit itself to an analysis of prices and quantities alone.

VI. *Closing Reflections*

In 1950, utility theory was once again in a state of crisis. After several decades of earnest methodological debate, the profession had found no satisfactory response to its critics. How do we account for this failure, and how did utility theory survive? Most importantly, what can we learn from these events?

A. *A One-Sided Response*

The critics of utility theory, both psychologists and institutionalists, were dissatisfied with economics in two main ways. First, utility theory portrayed human beings inaccurately; it trivialized the role of habit, culture, institutions, social pressure, and the like (Copeland 1931; Downey 1910; James 1890; McDougall 1910). Second, utility theory was unscientific, for it was teleological and it talked of unobservable psychic phenomena as if they were objective and measurable (Copeland 1931; Downey 1910; Hadley 1894; Tugwell 1922; Veblen 1909).

Economists did attempt to address the second critique, and as we have seen, they became almost as extreme in their logical positivist views as their critics. However, at least in the mainstream, economists never really dealt with the first critique. In fact, the second critique diverted attention away from the first. Mainstream economists responded to these critiques by (1) declaring independence from psychology, and (2) adopting behaviorist terminology and, at least ostensibly, behaviorist methodology as well. The result was counterproductive. As we have seen, the adoption of behaviorism undermined any (preference theoretical) response economists might have formulated to the first critique, for behaviorism stigmatized the psychological language that would have been essential to such a response. Moreover, by declaring independence from psychology, economists evaded institutionalist criticisms and redefined their position so as to make it explicitly irrefutable and tautological (cf. Davenport 1913; see end of Section III.B). People chose what they wanted, and what they wanted was defined to be what they chose. The theory said no more, and therefore, it could not be falsified. Of course, such evasion con-

tradicted the logical positivist methodology. Those who took positivism most seriously, such as Hutchison, were aware of this problem and called for a more realistic economic theory which answered institutionalist criticisms.

B. *The Absurdity of Behaviorist Mainstream Economics*

Behaviorist mainstream economics was doomed to fail, for the theoretical practice of "behaviorists" such as Samuelson contradicted their own professed methodological views. As Tugwell and Florence argued, if economists were to become behaviorists, they had to do so whole-heartedly and actually learn from the work of behaviorist psychologists. But even as they reformulated preference theory so as to make its behavioral implications more explicit, these mainstream economists nevertheless ignored the work of behaviorist psychologists. They continued to obtain their assumptions from introspection or a priori deduction, rather than looking to rigorous experimental results as their own behaviorist methodology indicated that they should.

Inevitably, the professed behaviorism of mainstream economists backfired. When some began to take seriously the quest for a rigorously empirical economics, they found that utility theory performed quite badly, on the individual level at least. The indifference curves that had been so praised for their empirical rigor could not in practice be derived at all (see Section V.C). This discovery should, of course, have come as no surprise. Utility theory was from the beginning a product not of behaviorist observation, but of verstehen, introspection and deduction. Remember that Knight, a great defender of utility theory, rejected behaviorism precisely because he understood that one could not derive preferences from behavior alone; some introspection or interpretive psychology was essential.

C. *An Indestructible Theory*

Amazingly, utility theory did survive this crisis. In the end, when faced with a choice between behaviorism and utility theory, economists chose the latter, and strict behaviorism lost popularity in favor of a new methodology that could justify the theory. Friedman resolved the crisis by pushing the declaration of independence from psychology one step further. In his 1953 essay, "The Methodology of Positive Economics," he argues that when we make behavioral assumptions about individuals, these assumptions need not be accurate, and they may even be wildly implausible, as long as aggregate data such as prices and quantities behave as if our assumptions were accurate (Friedman 1953). Thus, Friedman simultaneously rejects both verstehen (which insists that assumptions be plausible) and behaviorism.

Economists such as Stigler (and, later, Becker and others) responded somewhat differently, by returning (I submit) to a more psychological, verstehen-infused approach to preference theory. They used this theory to attack head-on the earlier critiques of the institutionalists, and to attempt to prove that economics really could explain complex social phenomena.[28] Psychological intuition permeates such work. Ordinalism remains a tenet not to be challenged, but there has emerged a new, psychological ordinalism. (As Sen has argued, one cannot make sense of modern preference theory in nonpsychological terms. See Section I.A.) This revival has not gone unnoticed, and as it did decades ago, utility

[28] A striking example is Becker's (1992) recent paper on "Habits, Addictions, and Traditions." Here he discusses all three of these phenomena, all within a neoclassical preference theoretical framework.

theory has again come under attack. Many of the same arguments that typified the early twentieth century again appear in major economics and psychology journals.

D. *On Declarations of Independence*

This fact brings us back to the question with which we began this paper: What exactly is the relationship between economics and psychology? Can economics really declare independence? The analysis above suggests that the answer is a resounding "No." As the experience of behaviorism teaches us, an economics that is devoid of psychology is doomed to sterility. Moreover, any illusion of independence only shrouds critical issues and delays their resolution. The fact that Sen's paradox has survived for so long indicates, all the more convincingly, that the methodological questions surrounding the crisis of hedonism were never really resolved properly. They simply lost currency, and economists, who had learned that they were independent of psychology, simply stopped worrying about the realism of microassumptions (after Friedman 1953).

This misplaced sense of independence spilled over into other areas, so that economists have also failed to learn much from the work of sociologists, despite the fact that there is so much to learn here. True, economics has broadened its horizons considerably, so that we are not as vulnerable to the first institutionalist critique as we once were. But we have largely failed to learn from those better acquainted with our newly discovered subject matter. For a broadening of economics to be truly successful, we must acknowledge our need to learn from other disciplines. Our illusion of independence has blinded us to this fact and has once again made us vulnerable to outside criticism.

Here, we find the most important lesson of our historical excursion. We saw above how early twentieth century economists resisted all outside criticism, but to the extent to which they responded, did so only to psychological criticisms, rather than to institutional or sociological ones. Again today, the issue of psychological assumptions in economics has become prominent. Today, we find some important economists taking the issue seriously, and even formulating theories that allow for imperfect rationality or weaken established hypotheses (cf. Mark Machina 1982). Economics can gain much from this development, but we should also be wary of the pitfalls. However interesting it may be to explore the causes of preference reversals and to reconcile them with economic theory (etc.), if we spend almost all of our interdisciplinary energy on such questions to the virtual exclusion of all other issues, we make a grave mistake.

We see no similar flurry of activity among economists to take seriously critiques coming from sociologists (who play the same role today as institutionalist economists did earlier). We see economists formulating theories that invade the traditional territory of sociology, but little work that takes seriously what sociology can teach us about the economy. Again, our attention to outside criticism is skewed toward psychology, and again, institutionalists have encouraged this trend by focusing unduly on the psychological shortcomings of economics, rather than on its more fundamental and more complex institutional shortcomings (Etzioni 1988). Rather than focusing on psychology to the exclusion of sociology, we might actually do better by retaining rational choice as a basic framework, but enriching it by taking the analysis of actual social phenomena really seriously (Granovetter 1985).

Whether we follow Granovetter's ad-

vice or not, we must realize that a satisfactory solution to today's methodological confusion cannot simply involve psychology. Psychology alone is not enough, because we cannot even begin to formulate a more realistic psychological foundation for economics, if we do not recognize the social forces that influence human decision making. True, we must encourage the nascent trend toward a more psychologically realistic economics. In the process, we must overcome our uneasiness about verstehen. We must recognize how much we do in fact use it, and that we ought, therefore, to use it systematically and openly, rather than haphazardly. But even these measures do not suffice, for they deal with only the psychological dimension. More fundamentally, we must remember that economics is a social science, and that, therefore, institutional and social issues are what really count. As in the past, they are in danger of being forgotten in all the flurry over psychology. Herein lies the challenge.

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