

**Economics, Psychology, and the History of
Consumer Choice Theory**

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0. Introduction

The relationship between psychology and consumer choice theory has been long and tumultuous: a veritable seesaw of estrangement and warm reconciliation. Starting with the British neoclassicism of Jevons and Edgeworth, and moving to contemporary economic theory, it appears that psychology was quite in, then clearly out, and now (possibly) coming back in. If one moves farther back in time and takes the view that the classical tradition was grounded in a natural – and thus non-social, non-mental, and non-psychological – order (Schabas 2005), then the scorecard becomes out, in, out, and (possibly) back in. A tumultuous relationship indeed.

The purpose of this paper is to provide a critical examination of this turbulent relationship. The argument is presented in three parts. The first section provides a summary of the standard story about the relationship between psychology and consumer choice theory from the early neoclassicals to the present time. The second section challenges this standard interpretation by focusing on the contributions of a few economists who played a key role in the development of the relevant theory. The third section offers an alternative reading of the relationship between psychology and consumer choice theory. The main point will be that it was never a question of whether economists wanted, or could use, “psychology,” but rather whether the kind of psychology that economists wanted and could use, was the kind that was being offered by disciplinary psychology.

Before embarking it is useful to be clear about how the terms “consumer choice theory” and “psychology” will be used in what follows. Obviously different definitions or conceptualizations of these two subjects will produce entirely different histories of their relationship. The term “psychology” will refer to the science of mental phenomena and mental causality: the autonomous science that predicts and explains human behavior/action in terms of the individual’s mental states or consciousness. In general this mental-state definition coincides with disciplinary psychology – the psychology contained in academic journals and textbooks – but there is one important exception. The exception is behaviorism – the perspective that dominated disciplinary psychology for much of the twentieth century – since behaviorism defined its subject matter as simply “behavior” and explicitly denied the scientific adequacy of any explanation based on mental states or other psychic concepts. As Frank Knight once put it, behaviorism is “less a psychology than a dogmatic repudiation of everything properly to be called by that name” (Knight, 1931, p. 64). The reason for using this mental-state definition, and thus

excluding behaviorism, is that it most closely corresponds to the way the term “psychology” has been used within the consumer choice literature. Early in the 20th century when economists were insisting that “psychology” be removed from economic theory, they did not mean behaviorism (in many cases, adopting behaviorism is exactly what economists meant by getting rid of psychology).

In most cases the term “consumer choice theory” will mean the contents of the consumer choice, or demand theory, chapter in contemporary textbooks; the consumer is assumed to have complete and transitive preferences (and thus could be represented by an ordinal utility function) and chooses the most preferred bundle from the affordable set defined by the standard linear budget constraint. The term “demand theory” will be used for the implications that consumer choice theory imposes on individual and market demand functions, but since the focus is psychology, most of the emphasis will be on the upstream part (individual choice) rather than the downstream part (properties of demand functions generated by such choice behavior).

Since the textbook version of consumer choice theory did not stabilize as “the” theory of consumer choice until the late 1940s, the same term will be used to cover the wide range of utility maximization-inspired theories of consumer decision-making that preceded this stabilization: from early neoclassicals such as William Stanley Jevons (1871) and on into the so-called ordinal revolution of Vilfredo Pareto ([1927] 1971), John R. Hicks and R. D. G. Allen (1934), Eugene Slutsky (1915) and others. This is certainly not to suggest that all of these various approaches were exactly the same – they most certainly were not – but only that they can be treated as members of the same family of theories for the purposes at hand. The term “rational choice theory” will also make an appearance at various points. For the purposes here, rational choice theory is a general framework for analyzing rational behavior and consumer choice theory is a particular application of it. Rational choice theory simply requires a well-behaved choice function/rule defined over a feasible subset of possible alternatives. In the case of consumer choice theory, the set of possible alternatives is the commodity choice space \mathfrak{R}_+^n , the feasible subset is the budget set $B(p, M) = \{x \in \mathfrak{R}_+^n \mid p^T x = M\}$ and the choice function is the utility function $U(x)$. Although these are not the only possible ways to define any of these terms, they are fairly common interpretations, and it is useful to be clear how the terms will be used below.

1. Psychology: In, Out, and Now (Possibly) Back In

In simplified form, the standard story of consumer choice theory is that psychology was out until the neoclassical revolution of the 1870s, then in for the period of cardinal utility theory 1871-1935, then out with the ordinal and revealed preference revolutions of the late 1930s. Starting in the 1870s the history of consumer choice theory is often presented as a series of three progressive stages: the views of the early neoclassicals, the ordinal revolution in the 1930s, and finally the move to revealed preference/consistency starting with Samuelson (1938) but really claiming the title as a separate stage during the 1950s. If one extends this story forward to the current time, then it appears that yet another change – in this case a change back to the explicit consideration of psychology – may be

underway. For the last two decades psychological ideas – particularly ideas from experimental psychology – have begun to have an effect, both critical and constructive, on discussions about how economists employ rational choice theory.

This section will provide a brief discussion of these different stages as well as the recent (possible) turn back in the direction of psychology. In other work (Hands 2006a, 2006c) I have emphasized three discrete stages in the traditional story, but in this case they can be collapsed into just two. Since we are concerned with the relationship between consumer choice theory and psychology, distinctions between economic theories only need to be drawn where they entail substantively different views about the role of psychology. In the case of the ordinal revolution (stage 2) and revealed preference theory (stage 3) the view of psychology essentially remained the same; both took the position that psychology should be out. It is argued that revealed preference did a more effective job at purging psychology than the earlier ordinal revolution, but the eliminativist goal is common to both approaches. Given this, only two stages need to be examined here: the first generation neoclassicals and ordinal utility/revealed preference theory. A third stage will be discussed, but it involves recent developments that suggest a return to – or at least a rejection of the earlier “fear and loathing” (Handgraaf and Raaij 2005) of – psychological issues in consumer choice theory.

The traditional characterization of (particularly British) first generation neoclassical theory is that it was a marginal utility-based choice theory employing a cardinal and hedonistic notion of utility. Cardinal in the sense that differences in the valuation of various bundles of goods took on numerical values – bundle A might give the consumer 20 utiles while a different bundle B might give him/her 25 utiles – and hedonistic in the sense that levels of utility were associated with the amount of pleasurable (or painful) psychic feeling the consumer received from the bundle in question. Consumer choice and the associated exchange theory were thus based (literally) on a the calculus of pleasure and pain. This theory of behavior was obviously linked to a utilitarian view of ethics and policy, but since the concern here is individual choice, rather than social policy, it is not necessary to discuss social utility in any detail. This said, it is useful to note that early neoclassical theory is also identified with the ability to make interpersonal utility comparisons. If the goal is to maximize the sum (or average) of human pleasurable feelings, then it would certainly be useful to be able to add up the feeling/utility levels of different agents. Although early neoclassicism is consistently identified with interpersonal comparability, almost none of the early contributors considered such comparability necessary for the theory of individual consumer choice (social policy perhaps, but not consumer choice).

Early neoclassicism, at least on this standard reading, was clearly psychological. It was a particular kind of psychology – psychological hedonism based on introspection – but it was clearly psychology and it was in. During the last decade of the 19th century and the first few decades of the 20th, this psychological hedonism became the main point attack for critics of marginal utility theory. As Lionel Robbins put it (in his colorful style): “The borderlands of Economics are the happy hunting ground of minds averse to the effort of exact thought, and, in these ambiguous regions, in recent years, endless time has been

devoted to attacks on the alleged psychological assumptions of Economic Science” (1952, p. 83). For a variety of reasons, including the rise of positivist ideas about science, psychological hedonism quickly lost its intellectual credibility, and given the relationship between early neoclassicism and hedonistic psychology, it was necessary for marginalist economics to reform its theoretical foundations or be pulled down with it.

At first the modern theory of value seemed almost a branch of psychology, ... For since the solution of all problems related to the measurableness of psychic phenomena is quite uncertain, a wide field remains subject to controversy. Even aside from the differences of opinion among the followers of the hedonistic school, the very bases of the edifice constructed by this school have been shaken by violent attacks. ... From this it follows that, if we wish to place economic science upon a solid basis, we must make it completely independent of psychological assumptions ... (Slutsky, 1915, p. 27)

As this Slutsky quote suggests, the most common criticism of hedonistic psychology was that it was based entirely on the internal subjective feelings of the individuals in question and such psychic phenomena were not objectively observable. Although the introspective method of “inner observation” was generally considered to be an acceptable method of “observation” during the 19th century, by the early 20th century this was no longer the case. This change in what was considered to be an acceptable empirical “observation” undermined introspection’s epistemic warrant, and thus by implication marginalist economic theory. The economics profession’s desire to separate from its earlier psychological roots initiated the “escape from psychology” which ultimately precipitated out in the ordinalist (and revealed preference) revolutions. This has been called the “empiricist motive” (Giocoli, 2003, p. 43) for the escape from psychology, and in a sense that is entirely correct – feelings and the associated mental states were not empirically observable and thus a properly scientific economics would need to find alternative, more adequate, foundations – but in a sense it was less an “empiricist” move, than a change in the notion of what counted as “empirical.” It was not that the relevant intellectual community suddenly became “empiricist” – remember John Stuart Mill was both a staunch empiricist and also a defender of introspection in economics – but rather that introspective, inner observations, no longer counted as scientific “observations.” In either case though – an increased desire for empirical foundations, or a change in what counted as a proper empirical foundation – the result was the same. The psychology that had provided the foundation for the neoclassical revolution (at least the British wing) had become a problem and was soon to be out.

The solution was the ordinal revolution. According to ordinal utility theory, consumers were still assumed to maximize a utility function but the function only expressed an ordinal – better or worse – not a cardinal, valuation of various commodity bundles. Consumers were assumed to have preferences – various bundles of goods could be ranked as better, worse, or indifferent than/to others – and those preferences could be represented by indifference curves, but a particular indifference curve was not associated with any specific level of cardinal utility. If a utility function generated a particular set of

indifference curves, then any monotonic transformation of that function would generate the same indifference curve and thus the same optimal consumer purchases. The distinction between cardinal and ordinal orderings was often called measurability in the literature; ordinal utility, and the demand theory based on it, did not require the measurability of utility (as, it was implied, the previous cardinal utility theory had).

Of all Pareto's contributions there is probably none that exceeds in importance his demonstration of the immeasurability of utility. To most earlier writers, to Marshall, to Walras, to Edgeworth, utility had been a quantity theoretically measurable ... Pareto definitely abandoned this, and replaced the concept of utility by the concept of a scale of preferences. (Hicks and Allen, 1934, p. 52)

Since cardinal utility was associated with hedonistic psychology of pleasures and pains, the move to ordinal utility theory was also viewed as a rejection of such hedonism. Again, Robbins minced no words on the matter:

Recognition of the ordinal nature of the valuations implied in price is fundamental. It is difficult to overstress its importance. With one slash of Occam's razor, it extrudes for ever from economic analysis the last vestiges of psychological hedonism. (Robbins, 1952, p. 56, n. 2)

Thus, according to the traditional interpretation, the ordinal revolution simultaneously freed consumer choice theory from of the yoke of psychological hedonism and placed it on more objective scientific foundations. It also facilitated the rejection of interpersonal utility comparisons – a feature of the new theory that also gave it additional support. The bottom line is that psychology was identified with hedonistic psychology and it was out.

An even stronger case was made in Samuelson's attempt to reconstruct consumer choice theory along completely behaviorist lines: what came to be called the theory of "revealed preference." Samuelson started from the position that the ordinal revolution had not gone far enough: "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" (Samuelson, 1938, p. 61). For Samuelson in 1938, even though ordinal utility theory was a step in the right direction, it was still a utility theory, and needed to be replaced by a theory of consumer behavior grounded completely on operational and observational concepts. Samuelson's original revealed preference theory was an attempt to forge a purely behaviorist theory of consumer choice and as such represented "the culmination of the neoclassical economists' 45-year-long escape from psychology" (Giocoli, 2003, p. 99).

Since ordinal utility theory and revealed preference theory continue to be "the" theory of consumer choice in contemporary economics, one might get the idea that the relationship between psychology and neoclassical choice theory was a tale of simple rejection: psychology was in during the late 19th century, but was then completely rejected early in the 20th. But the story does not end there. Although textbook consumer choice theory is

essentially as it has been for over fifty years, there seems to be a growing movement within contemporary economic theory to pay greater attention to psychology and perhaps even modify choice theory to better account for the results of recent psychological research. Daniel Kahneman, an experimental psychologist, receiving the Nobel Prize in Economics in 2002, as well as the economics profession's willingness to listen to the criticisms of, and entertain the various theoretical alternatives offered by, Kahneman and his associates (Kahneman and Tversky 2000) are clear indications of this trend, but the recent psychological turn goes well beyond the influence of any one particular group of psychologists. Extensive surveys of the literature on "psychology and economics" now appear regularly in mainstream economic journals (Rabin 1998, 2002), the field of "behavioral economics" has taken off during the last decade to establish itself as an important new framework for understanding economic behavior in a wide range of specific contexts (Camerer, Loewenstein and Rabin 2004, Gilad, Kaish, and Loeb 1984, Rabin 2004), experimental economics has firmly established itself as an important new field and maintains close linkages to experimental psychology (Guala 2005, Hertwig, Ortmann, et al. 2001), and finally, the field of neuroeconomics has appeared on the scene and quickly captured the attention of the economics profession (Camerer, Loewenstein, and Prelec 2005, Glimcher 2003). All in all it seems like the mental life of agents – and thus psychology – is back into economic theory. As the editors of a special issue on economics and psychology recently explained.

With regard to fear and loathing . . . it is good to see that those days are now behind us. A common language is emerging and more collaboration and cross-fertilization are budding. In fact, for some people it is hard to determine whether they are actually a psychologist or an economist; their affiliation more and more become the only cue. Even though, it may be a bit premature to substitute "fear and loathing" with "trust and love," the current mutual interest and respect seems a good foundation for a fruitful future. (Handgraaf and van Raaij, 2005, p.391)

In evaluating the possible impact of these changes it is useful to remember that there were at least two major rounds of similar criticisms raised against neoclassical choice theory during the 20th century – the criticisms of institutionalist economists like Thorstein Veblen and Wesley Clair Mitchell early in the century (Coats 1976) and the first generation of "behavioral" economics, or "old" behavioral economics (Sent 2004)¹, associated with the work of Herbert Simon, James March, and others (March 1978; Simon 1955, 1956) – and although the mainstream responded more positively to the latter than the former (Simon's Nobel Prize being a case in point), there was not a substantial change in the core consumer choice theory in either case. The current revival of interest in psychology may have similar consequences (i.e. none), but this said, there are differences between the current situation and those of earlier periods. Let me just mention three.

¹ Sent (2004, pp. 740-42) divides "old" behavioral economics into four separate schools of thought. These various research programs are also discussed in Earl (1990) and Gilad, Kaish and Loeb (1984).

First, there seems to be a general willingness among economists to accept certain systematic stylized facts (or anomalies) from the literature on experimental psychology – the endowment effect, framing, loss aversion, anchoring, preference reversals, mental accounting, etc. – as facts; that is, to accept them as things that need to be explained. Of course for many economists such anomalies do not require any substantive change in economic theory, but even those who are primarily concerned with explaining away such anomalies by creative application of rational choice theory, still consider the relevant psychological facts to be facts worthy of attention. This seems to be different from the profession’s response to the “psychological facts” raised in earlier rounds of criticism. Second, the presence of experimental economics as a well-established field of economic research – with PhDs, articles in respected economic journals, and academic job openings for specialists – accommodates a rapprochement with mainstream economists that did not exist in earlier periods. It is not a coincidence that Kahneman’s 2002 Nobel Prize was shared with Vernon Smith. It is important to note that the availability and acceptability of experimental evidence is a substantive change in economics; the core writings in economic methodology from Mill (1874), to John Neville Keynes (1917), to Robbins (1952), to Milton Friedman (1953), all started from the “fact” that controlled experiments were not generally available in economics. Now they are. The third difference is that we now have instruments that can monitor mental activity in a way that was not previously available. Edgeworth longed for a “hedonimeter” to measure human psychic responses (1881, p. 101), but the MRIs and other neural imaging devices employed in neuroeconomics now promises to actually deliver such observations. Again, this does not necessarily imply the rejection of traditional rational choice theory, but it does mean that mental states have been given a new (epistemic) lease on life, and that suggests that psychology, the science of mental states, may again be important to economic theorizing.

This ends the introductory discussion of the role of psychology in consumer choice theory. Starting from the neoclassical revolution psychology was in until the ordinal and revealed preference revolutions – where it was clearly out – but seems to have come back in during the last few decades as a result of developments in experimental psychology, behavioral economics, experimental economics, neuroeconomics, and related research. This historical sketch was based on the profession’s traditional (progressive) interpretation of these events – that scientific progress required theory to be grounded firmly on empirical observation and that required an escape from psychology – but it also (and less understandably) seems to be the standard story among many of those who are critical of mainstream theory (Bruni and Sugden 2007. Davis 2003, and Giocoli 2003 for instance). It is now time to muddy the water around this traditional narrative.

2. Looking Closer: The Plot Thickens

Reinterpreting various aspects of the standard story about the history of demand and consumer choice theory from Jevons to Samuelson has been a popular research topic during the last few years. Some of the many contributors to this literature include Backhouse 2003, 2006; Bruni and Guala 2001; Chipman 2004; Chipman and Lenfant 2002; Creedy 1992; Davis 2003; Fernandez-Grela 2006; Giocoli 2003; Hammond 2006;

Hands 2004, 2006a, 2006b; Hands and Mirowski 1998; Hausman 2000; Lenfant 2006; Lewin 1996; Maas 2005; Mandler 1999; Marchionatti and Gambino 1997; Martinoia 2003; McDonough 2003; McLure 2005; Mirowski 1989, 2002; Mirowski and Hands 1998; Morgan 2006; Moscati 2005, 2007; Peart 1996; Ross 2005; Teira Serrano 2006; Weber 1999a, 1999b, 2001, and Weintraub 2002. In a few cases these authors are challenging the standard interpretation of broad swatches of the history, but the majority focus on the work of a single individual or the development of one specific aspect of the theory. The bottom line is that the history of consumer choice theory and the related literature is being critically re-examined and as a result, many aspects of the conventional wisdom are starting to look a little worse for wear.

The goal of this section is to undermine this standard narrative about the relationship between psychology and economics by focusing on a few examples of the many ways in which psychology was not as “in” as it was supposed to be when it was in, nor as “out” as it was supposed to be when it was out. I will only be able to provide four examples in the space available here – one case where psychology was not as “in” as normally assumed (Jevons) and three cases where psychology was not as “out” as the standard interpretation would suggest (Slutsky, Robbins and Samuelson) – but additional examples can be found in some of the literature cited above.

The recent literature on Jevons raises serious questions about whether psychology – the science that explains human behavior on the basis of consciousness and mental phenomena – was really “in” Jevons’s Theory of Political Economy (1871) in the way that has traditionally been suggested. For example Maas (2005) argues convincingly that in Jevons’s hands “the calculus of pleasure and pain” took on a decidedly mechanical hue. Jevons’s guiding insight was that of the mechanical balance; this, combined with his psychophysiological view of the mind which recognized no “categorical distinction between the phenomena of mind and matter” (Maas, 2005, p. 270), produced a characterization of exchange behavior that was thoroughly mechanical (rather than motivational or volitional). Unlike Jeremy Bentham and others who employed individual utility to explain behavior, Jevons did not consider such “calculations” of pleasures and pain to be a prerequisite to a decision or choice, but rather as mechanical forces that automatically brought about the relevant behavior. As Maas explains:

To provide for a mechanism, it is not sufficient to consider feelings of pleasure and pain, like Bentham, ... Rather, pleasure and pain have to be considered not just as numbers, but as forces that move the will automatically, just like forces that move the balance ... Jevons’s mathematical rendering of pleasures and pains formed the “laws of human enjoyment” in terms of man’s physiology – as natural forces instead of motives. (Maas, 2005, pp. 273-74)

Although Maas is certainly not the only person who has emphasized the mechanical character of Jevons's economic agent,² he insists that it more than just a metaphor – “For Jevons, the ‘laws of human wants’ in which ‘the basis of economy’ was to be sought, became grounded in mankind’s ‘chemical and physiological conditions’” (ibid., p. 275) – and thus makes a strong case that Jevons employed utility, pleasure and pain, and other such terminology in a decidedly mechanistic and thus non-psychological way.³

Why is this mechanical, non-volitional, view of behavior non-psychological? It is non-psychological because it is physiological. A precondition for an autonomous science of psychology is that there is something sufficiently unique about human mental life and mental causality to warrant a separate science that studies it. If there is no difference between the laws of mind and the laws of matter – if the same (knowable) mechanical processes govern both – then psychological processes reduce to physical processes and psychology as a separate science of mental causality can be eliminated. Of course contemporary philosophy of mind provides a number of ways to rationalize a separate science of mental life even if it is ultimately all just chemical action in the brain (supervenience, functionalism, and the intentional stance for example),⁴ but in Jevons's day the physiological approach was a way to eliminate (introspective, associationist) psychology from exchange/value theory.

The autonomous psychology of late 19th century Britain had transformed the earlier “science of soul” into a “science of mind” (Hatfield 1995), but the new science still carried moral responsibilities (and thus the necessity for free will and volition) and still employed introspection as the unique tool for obtaining knowledge about mental phenomena (roughly using “inner observation” to replace the revelation of the earlier generation). Both of these were in direct conflict with the psychophysiology that inspired Jevons. As Lorraine Daston explains:

“Prior to 1850 the Cartesian dualism of mind and matter had validated the separation of psychology from the natural sciences in Britain. By maintaining a two-tier ontological system, the explanatory reduction of mental phenomena to a mechanistic substratum was excluded as a possibility. The peculiar nature of mind dictated the proper methods for its investigation, namely introspection. While mind's incommensurability with matter did not preclude its scientific study in the sense of a systematic explanation of mental phenomena, it did prohibit the inclusion

² This is, for example, how Philip Mirowski interpreted Jevons in *More Heat* (1989) and in Mirowski (1994). Other major works on Jevons (Peart 1996, Schabas 1990) also discuss his mechanical, and balance-based, conception of exchange, but do not put as much emphasis on the physiological element.

³ It is also useful to note that Maas clearly recognizes that Jevons was not always black and white on this issue – thus explaining why most readers have failed to notice the purely physiological character of his argument. For example Maas (p. 273) notes the “tension” between the (more volitional) way Jevons (and his interpreters) explained his theory of exchange and the purely mechanical characterization that comes out of his mathematics, diagrams, and the way that *The Theory of Political Economy* relates to the rest of his life's work.

⁴ See Davis (2003) and Ross (2005). I discuss these issues briefly in *Hands* (2001, pp. 165-71 and pp. 334-341).

of both the sciences of mind and matter within a unitary explanatory or methodological system. (Daston, 1978, pp. 194-95)

In order to understand the reluctance of British psychologists to pursue the deterministic conclusions of psycho-physiology with respect to human volition, and their concern with the moral potential of an applied psychology, it is necessary to take into account the importance which educated Victorians assigned to morality, and especially to the moral role of volition. (ibid., p. 197)

All of this seems to lead us inevitably to the conclusion that Jevons's mechanical and psychophysiology-based theory of exchange value was not, after all, anything like an effort to bring psychology into economics; rather it was a concerted move to drive psychology out. Economic theory would provide mechanical explanations of human behavior that were grounded on the best, psychophysiological, science of the day and expressed in the same mathematical formalism as physics; it would thus be able to free itself of the introspective soul-searching that characterized the separate mental-state science of psychology. When one combines this with the fact that Jevons is generally considered to be the most hedonistic and psychologically-grounded of the neoclassical revolution's big three (Jaffe 1976) – William Stanley Jevons, Carl Menger, and Leon Walras – one is left with serious questions about the degree to which the early neoclassicals brought psychology into economic theory.

Close examination reveals that similar doubts can be raised about the exit of psychology during the ordinal and revealed preference revolutions. I will focus on just three individuals – Slutsky, Robbins, and Samuelson – but similar claims could be, and have been, raised about other economists and theoretical approaches as well. I will begin with Eugene Slutsky (Evgenii Evgenievich Slutskii) whose 1915 paper is generally considered to be one of the two most important theoretical contributions of the ordinal revolution (Hicks and Allen 1934 is the other)

Slutsky has traditionally been characterized as the paradigmatic ordinalist – the first person to articulate a constrained optimization-based theory of consumer choice that was completely devoid of cardinal utility and hedonistic psychology. As the Slutsky quote in the previous section makes clear, his famous paper began with the claim that “if we wish to place economic science upon a solid basis, we must make it completely independent of psychological assumptions” (Slutsky, 1915, p. 27), and as every contemporary microeconomics textbook will attest, the paper's mathematical argument remains at the heart of contemporary consumer choice theory. The problem is that when one looks closer at Slutsky's original paper, and/or the arguments in two of his later, and recently translated, papers on economics (Slutsky 1926, 1927), the picture of Slutsky as an ordinalist, anti-hedonist, and psychological eliminativist starts to blur.

First, as a number of recent commentators have pointed out (Giocoli 2003, Weber 1999a), even if one focuses exclusively on the 1915 paper, “Slutsky's position with respect to the relation between economics and psychology was not as straightforward as

the above quotation seems to suggest” (Giocoli, 2003, p. 75). Even the next few sentences of Slutsky’s introduction make it clear that “it does not seem opportune to disregard all connections existing between the visible and measurable facts of human conduct and the psychic phenomena by which they seem to be regulated” (Slutsky, 1915, p. 27), and by the end of the paper he reasserts the need to “consider it necessary to complete the formal concept of utility in such a manner as to put the economic aspect of the problem of utility in close relation with the psychological one” (ibid., p. 53). In addition to such general statements about the importance of psychology, Slutsky’s technical discussion in sections 10 and 11 of his paper betrays the ordinalism that he was supposed to champion. These sections conduct comparative statics analysis on a consumer with an additively separable utility function. Now while additive separability was a common assumption in early consumer choice theory – employed by Jevons, Pareto and Walras among others – it is in fact a cardinal restriction since the signs of second partial derivatives of a utility function – even that $U_{ij} = \partial^2 U / \partial x_i \partial x_j = 0$ – is not independent of a monotonic transformation. Thus a key portion of Slutsky’s technical analysis was based on a cardinal assumption that “would no doubt have seemed like a step backward to the days of W. S. Jevons, Alfred Marshall, and H. H. Gossen rather than a step forward” (Weber, 1999a, p. 413). Nicola Giocoli thus summarizes the results of Slutsky’s classic paper in the following way:

In conclusion, Slutsky’s contribution to the escape from psychology seems closer to that of Pareto than to that of, say, Hicks or Samuelson ... He too believed in the traditional view of economics as a system of forces and wished to preserve a specific role for the individual and his/her psyche in the analysis. However, unlike Pareto, he was ready to accept introspection as a valid source of information, hoping to confirm its findings through a largely unspecified experimental method. He even argued that were the experiments to provide such a confirmation, then economists would be allowed to proceed further in the investigation of the psychological aspects of utility. (Giocoli, 2003, p. 78)

Even in 2003, when these remarks were published, it was fair to say that “Nothing of Slutsky’s view has survived in the literature beyond his exceptional analytical contribution” (ibid., p. 78), but that is no longer the case. As a result of efforts by John Chipman, two of Slutsky’s previously unpublished papers on economics have recently been translated (Chipman 2004, Slutsky 1926 and 1927) and these papers, along with the limited discussion of the philosophical foundations of Slutsky’s theory that exists in the secondary literature (Smolinski 1984), further undermine the argument that Slutsky’s main goal was to improve the empirical foundations of economics by purging psychic phenomena. Slutsky did have critical things to say about hedonism – Böhm-Bawerk’s hedonism in particular – but the alternative framework he advocated was nothing like the empiricist, more directly observable, foundations that have traditionally been associated with the ordinal revolution. Although the details of Slutsky’s methodological position are not entirely clear from these two papers – perhaps because of the constraints his social political context imposed on his writing (see Barnett 2004) and/or the fact his argument was couched in the “opaque terminology of the [Husserlian] phenomenological

philosophy of science” (Smolinski, 1984, p. 63) – the bottom line is that Slutsky endorsed a praxeological approach to the foundations of economics that seems much closer to the priorism of Ludwig von Mises than the proffered empiricism of Hicks, Allen, or Samuelson. As Smolinski explains;

Slutsky believed that economic laws cannot be derived by induction from the study of concrete phenomena, from empirical statements about concrete events. They can only be deduced from an axiomatized system postulates of praxeology, the parent science of economics, the pure theory of all purposive activity, of which pure economics covers only one type. (Smolinski, 1984, p. 68)

Thus in the late 1920s, as in his 1915 paper, Slutsky’s views of the foundations of consumer choice theory seem to be very different than the ordinalist-empiricist caricature that is offered in the standard story.

Lionel Robbins is the second major contributor to the ordinal “escape from psychology” to be discussed in this section. Although, Robbins is best known for the rejection of interpersonal utility comparisons – and the tax and income distribution policies that followed from them – he is also known as a harsh critic of hedonism and a defender of the ordinalist revolution. Since his arguments about the cognitive meaninglessness of moral judgments drew on the positivist ideas of the day, his interpretation of choice theory seemed to neatly combine the rejection of both psychology and interpersonal utility comparisons with positivism, the quest for more scientific foundations of economics, and anti-hedonism (Davis 1990, 2003; Walsh 1996).

Though the cardinal utility theorists had thought human psychology sufficiently intelligible to justify making such comparisons, and then using them to make normative recommendations, Robbins’ skeptical argument received an assist from changing views about the relationship between psychology and moral judgments in the first decade of the century. Emotivist thinking in ethics (a companion movement to positivism in science and philosophy) treated moral judgments as expressions of attitude or emotion ... that moral judgments, as expressions of attitude or emotion, were essentially irrational. In this way, they succeeded in discrediting both moral judgment and human psychology. Robbins’ arguments regarding the irrationality of interpersonal utility comparisons were made in this climate, and had the effect in economics of ruling out interpersonal utility comparisons ... (Davis, 2003, pp. 28-29)

Although Robbins and his supporters successfully convinced the profession that rejecting interpersonal utility comparisons was a corollary to the broader rejection of hedonism and cardinal utility in scientific economics, Robbins’ own characterization of consumer choice theory – both in his influential An Essay on the Nature and Significance of Economic Science (1952) first published in 1932 and in his later discussions of the topic – was decidedly non-eliminativist about mental-state psychology.

As is well-known, Robbins shifted the definition of economics from that which involved a domain of inquiry (decisions involving wealth) to a particular type of decision (that which involved choice under scarcity). For Robbins, scarcity is the “scarcity of given means for the attainment of given ends” (Robbins, 1952, p. 46, emphasis in original) and the “end” in consumer choice theory is the satisfaction of the agent’s preferences:

It does not require much knowledge of modern economic analysis to realise that the foundation of the theory of value is the assumption that the different things that the individual wants to do have a different importance to him, and can be arranged therefore in a certain order. This notion can be expressed in various ways and with varying degrees of precision, from the simple want systems of Menger and the early Austrians to ... the indifference systems of Pareto and Messrs. Hicks and Allen. But in the last analysis it reduces to this, that we can judge whether different possible experiences are of equivalent or greater or less importance to us. (ibid., p. 75)

How do we know that economic agents have such ordered preferences? Robbins’s answer was that we only have to look at everyday inner experience (i.e. introspection):

The main postulates of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so ... We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious. (ibid., pp. 78-79)

Since preferences, the ends of consumer choice, are forward-looking they can not be – as with behaviorism, which Robbins calls a “queer cult” (ibid., p. 87) – purely objective or discovered by observation of behavior; they are thus fundamentally psychological.

But even if we restrict the object of Economics to the explanation of such observable things as prices, we shall find that in fact it is impossible to explain them unless we invoke elements of a subjective or psychological nature ... It is obvious that what people expect to happen in the future is not susceptible of observation by purely behaviourist methods. (ibid., p. 88)

The bottom line is that because economics is fundamentally about choice under scarcity it must be based on a forward-looking notion of purposive behavior, and that notion is fundamentally psychological and cannot be reduced to objective observation of the positivist-behaviorist sort: “It is really not possible to understand the concepts of choice, of the relationship of means and ends, the central concepts of our science, in terms of observation of external data” (ibid., pp. 89-90). The proper value theory for Robbins is “the subjective or psychological theory of value; and, as we have seen, it is clear that the

foundations of this theory is a psychical fact, the valuations of the individual” (ibid., pp. 86-7, emphasis added).

Not only does Robbins require subjective psychic phenomena for choice theory, he also makes such concepts central to his argument against interpersonal utility comparisons. Why are such comparisons not available? Because they are not accessible to introspection.

Introspection does not enable A to measure what is going on in B’s mind, nor B to measure what is going on in A’s. There is no way of comparing the satisfactions of different people. (ibid., p. 140)

Interpersonal comparisons are not available in economics, but introspectively obtained knowledge about the qualitative character of our psychic satisfactions certainly is, and the presence of the latter is the reason for the absence of the former. Robbins clearly needs psychology and introspection to be in, not out, of economics.

The last theorist to have his escape from psychology deconstructed in this section – Paul Samuelson – actually requires the least documentation. The reason is that unlike the above discussion of Jevons, Slutsky and Robbins, the argument that Samuelson ultimately reneged on his 1938 promise to remove the last “vestigial traces of the utility concept” (Samuelson, 1938, p. 61) from consumer choice theory is now fairly well-established in the historical and methodological literature. Given that variations have been presented in Cohen (1995), Giocoli (2003), Hausman (2000), Hands (2001, 2004), Houthakker (1983), Lewin (1996), Wong (2006) and other research, I will only sketch the argument here and leave the reader to these works for a more detailed discussion.

Samuelson’s original presentation of the consumption theory that came to be called revealed preference theory was in fact a distinct break from previous theories of consumer choice – cardinal and ordinal – and was sufficiently behaviorist to be correctly called an escape (or at least attempted escape) from psychology. Samuelson 1938 actually tried to do what the standard story credits the revealed preference revolution with doing – constructing consumer choice theory on more observable foundations that did require reference to the psychic mental states of the relevant agents; “dropping off the last vestiges of utility analysis” (Samuelson, 1938, p. 62). The behavioral, operational, and non-preference-based features of Samuelson’s 1938 theory are driven home by the fact that the term “revealed preference” was not introduced in the original paper (the term became common after Samuelson used it in 1948). If well-ordered preferences exist they can be represented by an ordinal utility function and we are right back with the vestiges of utility theory; the point of Samuelson’s original approach was not to “reveal” preferences, but rather to provide a strictly operational theory of consumer behavior without preferences or utility at all.

This objective changes in Samuelson (1948) and (1950). By the 1948 paper – now called “revealed preference” – the goal is to construct indifference curves from the consumer’s revealed choices, and by the 1950 paper the goal is “to complete the programme begun a

dozen years ago of arriving at the full empirical implications for demand behavior of the most general ordinal utility analysis” (Samuelson, 1950, p. 369, emphasis in original). The latter paper, in particular, completes the link to the earlier (now evidently no longer problematic) ordinal utility theory. Samuelson’s original consistency condition – now called the weak axiom of revealed preference – imposed all of the same restrictions on demand functions as the ordinal utility theory of Hicks, Allen, and Slutsky: except one. The missing condition was the symmetry of the Slutsky substitution matrix. This is important because these Slutsky symmetry conditions are the so-called integrability conditions that guarantee the existence of an underlying utility function. Of course in 1938 the fact that integrability was “missing” from Samuelson’s new behavioral theory was not a problem since the goal was to do away with all reference to utility. As he said: “I cannot see that it is really an important problem, particularly if we are willing to dispense with the utility concept, and its vestigial remnants” (Samuelson, 1938, p. 68). But by the 1950 paper – and in light of Hendrik Houthakker’s (1950) strengthening of Samuelson’s condition to the strong axiom of revealed preference which did imply all of the standard conditions on demand functions including integrability – the focus had changed to a characterization that would underwrite both preference and utility. As Houthakker put it “the stone the builder rejected in 1938 seemed to have become the cornerstone in 1950” (Houthakker, 1983, p. 63). What had started out as a behaviorist program that rejected the psychological foundations of choice theory, ended up being means for scientifically bolstering precisely those foundations. As Stanley Wong summed up the situation:

The upshot of our interpretation of the purpose of the theory is that the revolutionary significance of the Samuelson Theory is lost. The development of the theory does not represent a break with the tradition in economic theory in which consumer behaviour is explained in terms of preferences (and material circumstances). Consequently, the attendant philosophical and psychological controversies of utility theory, which Samuelson hoped to evade with his observational theory, are not exorcised from the corpus of economic theory ... (Wong, 2006, pp. 73-74)

Thus we find that with Samuelson, like the others we have discussed, the standard story seems well off the mark. There is no distinct break when psychology was in, then went out. It was often out for those who are credited with bringing it in, and often in for those credited with casting it out. Recognizing that sharp breaks did not exist in the work of those who are credited with inserting such breaks, and noting that the representative economist was less concerned with foundations and more naturally whetted to the old ways of thinking than such pivotal thinkers, leads us naturally to question the veracity of the standard story and to wonder exactly how it came to be the conventional wisdom.

The next section will briefly offer an alternative narrative, one that attempts to explain how the various moves in the history of consumer choice theory 1870-1945 might be understood in relationship to psychology. The alternative interpretation is admittedly just a first step toward a more elaborate history. The standard story has evolved over a long period of time in the writings of vast number of different economists and has the

discipline's stamp of approval; the alternative offered here is my own tentative attempt to reconstruct the history in a more adequate way, one which gives us a better understanding of the various interests and causal forces at work in the relationship between consumer choice theory and psychology.

3. An Alternative Tale

Psychology in the 17th and 18th centuries was “soul science” (Hatfield 1995). From a contemporary point of view this suggests that such psychological research should not be called “science” at all, but such a conclusion fails to distinguish ontological issues from epistemology and scientific method. One does not need to question the existence of uniquely human immaterial entities (ontology of souls) in order to apply the method of science to uncover interesting and useful characteristics of such entities. As Gary Hatfield explains: “if one believes that immaterial entities exist and that some of them inhabit human bodies, it makes good sense to seek to determine the powers and capacities of such substances empirically, by studying the manifestation of the mind in the behavior of others and in one's experience of mental phenomena” (Hatfield, 1995, p. 188).

By the 19th century the soul was replaced by mental phenomena and psychic states – and later consciousness – consistent with Cartesian dualism, but the general commitment to explaining the psychic “it” as scientifically as possible remained the same. The key tool for 19th century psychology was of course introspection – the inner observations which provided the empirical foundations for the science of mind. But the potential moral nexus carried forward from soul to psychic mental states as well, and the desire to maintain volition and free will in a way that was consistent with a scientific approach that uncovered causal mental relationships became an important theme in (particularly Victorian) psychology. As Lorraine Daston explains: “Far from being a peripheral aspect of late-nineteenth-century British psychology, ... this perceived tension between the moral necessity of free will and a law-governed mental science played a central role in the selection of the topics, approaches, and explanations, which dominated psychological discussion (Daston, 1978, p. 192). John Stuart Mill (1874, 1884) provides an excellent example of using the “method” of introspection as the basis for a compromise moral science of political economy that seems to accommodate both scientific law and volition: “individual experiences of free will matched with the proper method to make scientific inferences about the phenomena of the mind: by means of introspection” (Maas, 2005, p. 159).

Early in the 20th century psychology adopted behaviorism and thus turned sharply away from both psychic phenomena and free will. By focusing exclusively on empirical regularities in observed behavior, behaviorism sacrificed volition for science, but did so in a way (unlike psychophysiology) that still left room for a separate science of psychology. These changes coincided with the deflation, and eventually the elimination, of introspection as a legitimate source of knowledge about human consciousness or behavior (Lyons 1986). It is difficult to imagine a sharper break than that provided by John Watson's behaviorism; not only did he offer an alternative research program based

on objectively observable phenomena, he was crystal clear that psychology needed to escape from the “stifling soul cloud” (Watson, 1924, p. 4) of introspection.

Behaviorism claims that “consciousness” is neither a definable nor a useful concept; that it is merely another word for the “soul” of more ancient times. The old psychology is thus dominated by a kind of subtle religious philosophy. (ibid., p. 3)

Later, during the 1960s, disciplinary psychology went through what is often called the “cognitive revolution” and since that time work in cognitive science and related areas has moved forward within a broad psychological research program that is self-consciously scientific and yet reasonably comfortable referring to mental states and positing consciousness (Baars 1986). As Christopher Green explains:

... cognitivism was an answer to the problem: how can we introduce (at least part of) the mental back into scientific psychology while not falling prey to the criticisms that brought down the mentalism of old and led us to behaviourism? In other words, how can we have our mental cake and eat it too? (Green, 1996, p. 37)

Although the cognitive revolution clearly opened the door to the scientific legitimacy of a broad class of psychic phenomena that had been excommunicated by behaviorism, this ecumenicalism has not necessarily been extended to subjects like free will and volition that were so important to the (morally sensitive) psychologists of the 19th century. Scientific investigation of free will and related subjects are no longer seemed to be in the set of things that disciplinary psychology considers worthy of the profession’s attention.⁵

The bottom line on this little history of scientific psychology is that consciousness, psychic phenomena, and introspection as a source of evidence for such phenomena, were clearly in until they were challenged the end of the 19th and evicted by behaviorism during the first part of the 20th century, at which point they were out. Now consciousness and mental states are back in, and as such they are again appropriate topics for scientific inquiry, but the original emphasis on volition, free will, and the associated moral dimension of the discussion has disappeared from disciplinary psychology. Things have changed over time, but at any specific point in time the majority of mainstream psychologists were willing to make a commitment about whether it was more important to retain and defend a notion of human volition or whether it was more important to support disciplinary practice that was in compliance with prevailing views about proper scientific method (as long as the choice retained space for a separate psychological science). Psychology actually took a stand on such matters and as a result discipline had a

⁵ A nice example is provided Susan Blackmore’s Conversations on Consciousness (2006). The book contains interviews with twenty-one influential scholars in psychology, cognitive science, philosophy of mind and related fields, and in each case she asked “Do you think you have free will?” The results were rather surprising. Although almost everyone had an opinion on the matter, there was nothing like a consensus view, and even more surprising, the views that people expressed often had nothing to do with the results of their own research on consciousness (Blackmore, 2006, pp. 8-9).

very different practice and object of inquiry during the middle third of the 20th century – i. e. behaviorism – than it had at the end of 19th.

Economics was not so willing to commit. The central thesis of my alternative interpretation of the history of consumer choice theory is that economists were never wholly willing to commit to one or the other and always wanted both: volition (and its associated normative implications) and causal science (and the predictive power, explanatory understanding, and the epistemic distinction it brings). If we call the problem of retaining a view of human agency based on free will and individual choice the volitional problem, and the problem of providing a theory of consumer choice that seemed to be consistent with dominant views about scientific knowledge the scientific problem, then the profession was never willing to accept a solution to one of these problems that did not also offer a solution to the other. The profession was willing, as one would expect from economists, to make trade-offs between the quality of the answers provided to these two problems on the margin – depending on context and circumstances – but a corner solution that solved only the volitional problem or only the scientific problem was never acceptable.

In the middle of the 19th century when Mill's compromise seemed to provide a solution to both problems all was well – as Mill put it, there were simply a few “unsettled questions” in political economy – but by the end of the century when the volitional gain provided by the introspective solution was being swamped by the growing criticism of the profession's scientific credentials, Mill's compromise was no longer adequate. Although the profession could have accepted a thoroughly mechanical (Jevons's psychophysiology) or behaviorist view (Samuelson's original revealed preference theory) and thus solved the scientific problem, to do so would have abandoned finding a solution to the volitional problem: either by reducing the economic agent to a fully determined physical system or to simply an observed constant conjunction of stimulus and response.

Economists' attitude about behaviorism is particularly important since it was a very powerful force shaping many of the human sciences during the 20th century. Despite the lip-service paid to behaviorism over the years, economists have never been willing to accept behaviorist's totally plastic view of human nature or the corresponding methodological advice that everything worth knowing about human behavior can be learned from pigeons and rats (McDonough 2003). In this sense the ordinal revolution was a perfect solution to the epistemic credibility problem of the late 19th century. It allowed the profession to jettison its problematic reliance on hedonism and cardinalism while at the same time retaining the idea that consumer behavior was based on the agent's free choice of the bundle of goods that most satisfied his/her preferences. Robbins's criticism of behaviorism is particularly telling in this regard, since he clearly saw the task of maintaining a balance between the solution to the volitional problem and the scientific problem as fundamental to the health of economics. Strict behaviorism would have meant that there was no choice in consumer choice theory, and that was never acceptable, to Robbins, or to most economists.

As it turned out, revealed preference theory actually ended up bolstering the scientific credentials of the (ordinal) utility theory that it had originally been designed to replace. After the development of Houthakker's strong axiom, it was just a few short (though mathematically very sophisticated) moves to prove the general equivalence of the revealed preference and ordinal utility approaches to consumer choice theory (Kihlstrom, Mas-Colell and Sonnenschein 1976, Uzawa 1960). Since revealed preference theory was viewed as relying on nothing more than the observed consistency of choice – note it is “choice” and not just behavior – its equivalence with the “utility hypothesis” thus firmed up the empirical foundations of the earlier theory.⁶ The observability of the “revealed” part solved the scientific problem, while the motivational aspect of the “preference” part solved the volitional problem. If economics had gone the way of disciplinary psychology in the 1930s – rushing whole-heartedly into the arms of behaviorism – the volitional problem would need to be surrendered and revealed preference would never have been the position to help offer a simultaneous solution to both problems. Mental state psychology could not be allowed to be totally in during the 19th century because of the scientific problem, but it could not be allowed to be totally out early in the 20th – even though it was out of disciplinary psychology – because of the volitional problem. This failure to commit (or ambiguity, or balance, depending on your perspective), not found in psychology, actually ends up being a source of strength for consumer choice theory and economics more generally.⁷

If one accepts my alternative story, or even just finds it worthy of serious consideration, one is still left with the question: Why? Yes, if the profession was never willing to accept an answer to the volitional problem that did not offer something in the way of a solution to the scientific problem – and vice versa – then it would explain the actual positions of various economic theorists, but that still leaves us with the question of why the profession was so enamored with these two commitments. Why is it exactly that economists were never willing to give up totally on either one of the two problems?

The answer to why the profession needed to adapt to the changing characterization of proper scientific practice seems rather obvious. To do otherwise would have been the death of economic science. Neoclassical economics ascended to its position as mainstream economics in a large part because it won the battle of convincing the profession that it was in fact legitimate economic science (and that other research programs were not). What counted as empirical evidence changed somewhere between 1870 and 1930, and thus psychology (as the introspective non-science of psychic

⁶ This equivalency is of course the source of many of the methodological criticisms that revealed preference theory has faced over the years. On one hand, if revealed preference really is just observed consistency of action then it implies that consumer choice theory can not be a causal explanatory theory (Davis 2003, Giocoli 2003, Hausman 2000, Mandler 1999). On the other hand, if one posits preferences that are being revealed by the observed behavior and doing the explaining, then one has all of the observational problems associated with utility theory; revealed preference cannot have more empirical content than ordinal utility theory if they are equivalent (Wong 2006).

⁷ There is a sense in which much of criticism of institutionalists like Veblen and Mitchell during the 1930s was based on the argument that the profession should change economic theory in a way that would provide a better answer to the scientific question and let the volitional question be damned, while the criticism of many Austrians during the same period can be interpreted as taking just the opposite position.

phenomena) needed to be out. Although the profession always had other interests – including maintaining the notion of the volitional agent – such issues could never be allowed to trump the discipline’s ability to effectively defend its position as science.

Although it comes as no surprise that economists wanted to maintain a respectable scientific profile – and to accept or reject psychology as they saw fit on these grounds – this only answers half the question of why economists were never willing to accept a solution to the scientific problem that did not also provide an answer to the volitional problem. We also need an answer to the question of why the economics profession was never willing to completely abandon the volitional agent, and the answer to that question seems to be less obvious than why it was necessary to be scientific. So why did economists never adopt a version of consumer choice theory – such as strict behaviorism or neurophysiology – that would characterize consumer behavior as anything other than a free and volitional choice?

The simplest answer seems to be that economists wanted to hold on to free will and volitional choice in human action for the same reason the Victorian psychologists and Mill wanted to defend it: because of the normative implications of having it otherwise. Consumer choice is at the heart of the way that not only economists, but members of the general public, think about how market economies work. Market economies are based on consumer sovereignty – consumers freely choose the goods that they most prefer (given the constraints they face) – and such free choice is an essential, and moral, difference between market economies and other ways of organizing economic activity. Theories that provide a mechanical or non-volitional characterization of consumer behavior – either psychophysiology or strict behaviorism – do not support this notion of free choice and thus simply will not do.⁸ Solving the scientific problem may be very important – and the part of the story that is emphasized in the official history – but any such “solution” is only viable if it preserves intact the fundamentally volitional aspect of agent behavior.

Although the normative nexus undoubtedly plays an important role in the profession’s perception of the volitional problem as a problem (or constraint on theorizing), it does not need to be only reason for such a concern. Another reason why economists have been unwilling to surrender the free-willed agent in consumer choice theory is that it was simply the way the problem was originally (and continues to be) defined. If the main problem of consumer choice theory was to explain how consumer choice took place and analyze the implications of such choices for market behavior, then it is hardly surprising that such a theory is what has been consistently defended. If the goal is to explain consumer choice it seems unlikely that one would be willing to accept a proffered “explanation” where the consumer didn’t really have any choices to make. Now it may be a good idea – from the point of view of the scientific problem – to endorse something like revealed preference theory, but not in its original strictly behaviorist form (where there is no choice just behavior), only later after it evolved into yet another, epistemologically better-looking, defense of the rational, yet choosing, agent.

⁸ Milton and Rose Friedman titled their popular book Free to Choose (1990) not “motor responses to brain chemistry” or “conditioned response in the marketplace.”

Although these two explanations – the normative nexus and the it-was-always-about-choice story – undoubtedly do not exhaust the reasons that economists have traditionally been unwilling to accept a theory of consumer behavior devoid of volition, they do seem to go a long way in that direction. Taken together with the earlier argument for why the scientific problem was so important, we seem to have a reasonable explanation for why economists have adopted the traditional story about the history of consumer choice theory (and its relationship to psychology) as well as why the specific theories offered by various contributors took the shape that they did. The scientific problem explains why a particular kind of psychology needed to be officially out, but the volitional problem explains why the discipline was never comfortable doing what psychology did, completely jettisoning the free-willed agent. It also explains why the theory of consumer choice articulated early in the 20th century and refined in various ways by revealed preference, insisted that it was a complete escape from psychology even though it was unwilling to go all the way in the behaviorist direction. If one defines psychology as it has been defined throughout this paper – as mental state-based approach to predicting and explaining human behavior – then when economists were most insistent that psychology was out of economics, such mental state psychology was actually more in in economics than it was in the discipline of “psychology.”

4. Some Concluding Thoughts

My argument was presented in three parts. Section one recited the traditional story about psychology being in, then out, and (perhaps) back in, during the history of consumer choice theory from the neoclassical revolution to the 21st century. Section two tried to sew the seeds of doubt about the traditional narrative by discussing a few specific economists (Jevons, Slutsky, Robbins, and Samuelson) who were supposed to be exemplars of particular points of view – either psychology being solidly in, or solidly out – and yet, on closer examination, seemed to advocate quite different, and in some cases exactly the opposite, positions. In the last section I tried to offer an alternative explanation of the relationship between consumer choice theory and psychology that explains both why economists have taken the positions they have regarding psychology, and why this, seemingly more consistent story, is not the profession’s standard narrative. In the last few paragraphs I will try to address the issue of where all this leaves us now; in other words, what this alternative way of thinking about the history of the relationship between psychology and consumer choice theory might suggest about the possibilities for reconciliation.

So if we fast forward to the recent literature on behavioral economics, experimental economics, neuroeconomics and such – a literature that suggests, at least on the surface, that psychology is, or soon will be, back into economic theory – what do we have? Is this recent research pointing as clearly in the direction of rapprochement as some (including the discussion in section one above) would suggest? Or does it imply something else? Although it is too early to know how things will eventually work out, it is useful in closing to note some of the evidence which suggests that the reconciliation may not be as close, or as warm, as initial appearances suggest. Let me just mention three of the many

things that may indicate that the “trust and love” will not fully develop between economists and psychologists⁹ in the near future.

The first issue is that, despite some convergence, there still exist fairly large differences between the theoretical protocols, and thus the willingness to accept experimental evidence generated in compliance with those protocols, used by experimental economists and those common to experimental psychology (Guala 2005, Hertwig, Ortmann et al. 2001, Sugden et al. 2005). Economists often argue that experimental economics is not directly relevant to economic theory because, among other things, the discipline is only interested in market (not individual) behavior, there is no good reason to believe that behavior in the lab is the same as behavior in “the wild,” and even though experimental economists use cash payoffs they are not sufficient to mirror real economic choices. When one adds these skepticisms about home-grown experiments to the important differences between economics-based and psychology-based experimental science, one has some reason to doubt that economists will fully accept the relevance of experimental psychology.

Second, even though the days of lockstep behaviorism are long gone and psychologists now speak with confidence about the mind, the field is still far more committed to behaviorism than economic theory. The key feature involves “conditioning” – without conditioning or similar observable antecedent to the agent’s “response,” behaviorism is deprived of the constant conjunctions that are fundamental to the whole approach.¹⁰ Economists have traditionally been very uncomfortable with such conditioning. The preferences of the agent are the primitives; how those preferences came to be what they are – how the agent might be conditioned by forces in the environment to “respond” with certain behaviors – is, in the best case, simply not a legitimate subject for economic analysis, and in the worse case, simply not anything that can be rationally explained. In economics preferences are the ultimate wellspring of behavior, in psychology (even today) they are more likely to be simply the conduit for action, and that is a significant difference that may continue to divide the two fields (Di Clemente and Hantula 2003).

A third possible centrifugal force is the rapidly expanding field of neuroeconomics. As discussed in section one above, there is a sense in which the neural imaging techniques and psychophysiological tools of neuroeconomics seem to provide direct empirical access to an agent’s preferences and thus – as with Edgeworth’s hedonimeter – add fuel to the fire of reconciliation between economics and psychology, but this is not the only effect that neuroeconomics might have. Many of the most prominent researchers in neuroeconomics view the field as providing the foundations for a new synthesis in the behavioral sciences. For some, the new synthesis will require a substantive change in standard economic theory: “we believe that in the long run a more ‘radical’ departure from current theory will become necessary, in the sense that the basic building blocks will not just consist of preferences, constrained optimization and (market or game-

⁹ For the rest of the paper “psychology” will refer to the post-cognitive revolution mainstream that exists today within disciplinary psychology.

¹⁰ It is not a coincidence that John Watson spent the end of his career in the advertising industry (Buckley 1982).

theoretic) equilibrium” (Camerer, Loewenstein, and Prelec, 2005, p. 55). But for others rational choice theory will not only remain essentially intact, but will play a key role in a new synthesis of “economics, biology, and neuroscience” (Glimcher, 2003, p. 311):

In sum, neuroeconomics seeks to unify the prescriptive and descriptive approaches by relating evolutionary efficiencies to underlying mechanisms. Neoclassical economics and utility theory on which it is based provide the ultimate set of tools for describing these efficient solutions; and evolutionary theory defines the field within which mechanism is optimized by neoclassical constraints; and neurobiology provides the tools for elucidating those mechanisms. (Glimcher, Dorris, and Bayer, 2005, p. 253)

Notice that whether economic theory will need to be modified or remain essentially the same, there is no real discussion of the active role of psychology (any version of it) in any of this. The prime movers in neuroeconomics are developments within neuroscience and neurophysiology, and the underlying framework is evolutionary biology – not the autonomous science of psychology. Economics and rational choice theory play a key role, but the new synthesis seems to be essentially eliminativist about psychology. As Skinner warned psychologists about neuroscience late in his career: “psychology may find it dangerous to turn to neurology for help. Once you tell the world that another science will explain what your key terms really mean, you must forgive the world if it decides that the other science is doing the important work” (Skinner, 1987, p. 784).

Thus there does seem to be reason to be skeptical about the recent overtures of reconciliation between psychology and economics.¹¹ Things are changing fast and there certainly are forces pulling in that direction, but there are others clearly pulling in the opposite direction as well (and the latter may currently be stronger). So, for the moment at least, it seems that we need to add “psychology may not be coming back in as often suggested” to our list of conclusions from section two. Hopefully though, we now have a better understanding of what really was and was not in (or out) in previous periods and why that was the case.

¹¹ Alternative interpretations of the contemporary reconciliation are offered by Bruni and Sugden (2007), Earl (2005) and Sent (2004).

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