

Individual Psychology, Rational Choice, and Demand

INDIVIDUAL PSYCHOLOGY, RATIONAL CHOICE, AND DEMAND: SOME REMARKS ON THREE RECENT STUDIES

D. WADE HANDS*

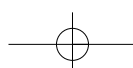
The member's account, and its associated self-evident method, have great instinctive appeal; the social forces that protect and sustain them are powerful. The member who poses awkward questions about "what everybody knows" in the shared culture runs a real risk of being dealt with as a troublemaker or an idiot. Indeed, there are few more reliable ways of being expelled from a culture than continuing seriously to query its taken-for-granted intellectual framework. ...

We need to play the stranger, not to be the stranger. A genuine stranger is simply ignorant. We wish to adopt a calculated and informed suspension of our taken-for-granted perceptions of ... practice and its products. By playing the stranger we hope to move away from self-evidence.
(Shapin and Schaffer, 1985, p. 6)

I. Introduction

The literature on the history of the mathematically formalized areas of economic theory has expanded rapidly during the last few decades. It is

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An early version of this paper was presented at the History of Economics Society Annual Meeting in Toronto, Canada, June 25-28, 2004. I would like to thank John Davis, Nicola Giocoli, Philip Mirowski and an anonymous referee for helpful comments on various versions of the paper. The research was supported by National Science Foundation Grant 0422823.

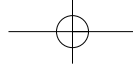


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no longer the case that the only historical narratives available in areas like general equilibrium theory, game theory, and mathematical economics are those contained in the first chapter of standard textbooks. This paper will focus on one particular aspect of three of these recent historical works. I will examine the history of individual demand theory presented in: Sonja Amadae's *Rationalizing Capitalist Democracy* (2003), John Davis's *The Theory of the Individual in Economics* (2003), and Nicola Giocoli's *Modeling Rational Agents* (2003).

Before beginning it is important to note that all three authors discuss much more than 20th century demand theory. Amadae is explaining the Cold War origins of rational choice theory and the key role it played in the (successful) redefinition and reconfiguration of liberal political theory in the post World War II era. Hers is a wide-ranging work in political theory and political philosophy and the discussion of demand theory is restricted primarily to one chapter. The Davis book is a philosophical examination of the concept of individual identity in the history of economic theory that draws on a wide range of resources (from Descartes to Sen). His central thesis is that mainstream economics – despite its rhetoric to the contrary – does not really have a theory of individual identity, and alternatively, that certain versions of heterodox economics do have such a theory. Although Giocoli dedicates a much larger portion of his text to the history of demand theory than either of the other authors, it is still just one stepping stone in his overall narrative: a reconstruction of the history of 20th century economics in a way that emphasizes the shift from modeling economic agents and equilibria in terms of their underlying forces, to modeling them as purely formal relations. Giocoli uses this historical framework to explain the delayed acceptance of game theory in economics (among other things).

It should be clear from just these brief remarks, that all three books tell a story about 20th century economics that involves the way that economists have theorized about demand and consumer choice, but the discussion of such theorizing is just one aspect of a broader historical project. It should also be clear that the authors' central theses are not mutually exclusive; they cover roughly the same period of time and much of the same subject matter, but their emphasis, and where they go with the story, is substantially different. Despite the criticisms contained in the following pages, I am broadly sympathetic to the central thesis of all three books. My critical comments will focus exclusively on what they say about the theory of individual demand (and the associated rational choice theory) and not about other aspects of their narratives. The bot-

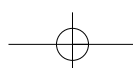
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tom line is that they all tell essentially the same story about demand theory and I think it is both a deeply problematic and a relatively unpersuasive story. In the final section I will explain why I think it is important to make the criticisms I do and why telling the story of 20th century demand theory in a different way would actually strengthen their main points.

II. Rational Choice, Consumer Choice, and all That

Although I will ultimately discuss these three histories of individual demand theory in detail, I will begin not with history, but rather with a relatively analytical discussion of rational choice, consumer choice, and related issues. I do this as spadework for the discussion that follows: because it seems useful to have a model (actually models) of how it might be possible to think about such issues in order to help delineate the various differences, and problematic issues, among the various historical reconstructions. The analytical framework is not presented as the one right way to think about such issues; rather it is offered as a useful frame, a template, that facilitates the organization and clarification of many of the points I wish to emphasize about the three interpretations. I will begin by discussing a particular concept of rationality.

To some extent the history of Western intellectual thought – in social theory, in science, in philosophy, and elsewhere – is a history of rationality. Different notions of rationality distinguish Locke and Hegel, Hume and Kant, Dewey and Russell, Freud and Skinner, Popper and Habermas, and Arrow and Geertz. Although the term rationality conjures a dizzying array of variations, there are particular versions of rationality about which there seems to be substantive agreement. One such special case is instrumental rationality. According to instrumental rationality, rationality lies exclusively in the relationship between means and ends. Being instrumentally rational simply means using the most appropriate, or effective, means for achieving one's given ends. In particular, instrumental rationality says nothing whatsoever about the content of the relevant ends. The ends could be self-destructive, criminal, or have any number of other prima facie negative characteristics, and yet provide perfectly adequate antecedents for instrumentally rational action; they are entirely open as long as one acts appropriately in their realization. As the philosopher Michael Friedman explains:

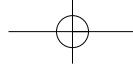


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Instrumental rationality thus refers to our capacity to engage in effective means-ends deliberation or reasoning aimed at maximizing our chances of success in pursuing an already set end or goal. It takes the goal in question as given, and it then attempts to adjust itself to environmental circumstances in bringing this desired state of affairs into existence in the most efficient way possible. [Friedman (2001), p. 54].

Instrumental rationality was raised to philosophical prominence by David Hume in the Treatise of Human Nature (1739). For Hume, all rationality was instrumental; he opposed more universalizing notions of rationality – the idea that “rationality” necessarily required certain acts or beliefs for all humans, in all places, at all times, and independent of the specific context. For Hume rationality was always a contingent affair; given the particular ends, certain means are rational, but rationality alone does not mandate any substantive ends. In other words, all imperatives are hypothetical, not categorical; reason is “the slave of the passions” [Hume, Treatise, Vol. II, Book III, part 3] and the passions are the sole unmoved movers of rational action.

Although instrumental rationality is simply the relationship between given ends and the effective means for achieving those ends, explanations based on instrumental rationality are often tied up with particular kinds of ends. In economics, and in rational choice theory more generally, the ends are restricted by the well-ordered preferences (or a well-behaved choice function) of the relevant agent. In the particular case of consumer choice theory (ordinal utility theory), the ends are given by the preferences of the consumer, but they are not just any preferences; they are assumed to be well-ordered in the sense that they satisfy certain basic assumptions, particularly completeness and transitivity. A “rational choice” explanation thus has two separate parts. First, the agent must have rational preferences (the agent’s preferences must be well-ordered). And second, the agent must act rationally – act in an instrumentally rational way – given those preferences. Even if an agent behaves in an instrumentally rational way, a rational explanation is only possible if the agent’s preferences satisfy some minimal standards of consistency (are not erratic, intransitive, or not well-behaved in some other way). Similarly, even if the agent has complete and transitive preferences/ends, it will not be possible to explain behavior in rational choice terms unless they act systematically on those ends by adopting the most effective means for achieving them (i.e. are instrumentally rational). Being rational (having rational preferences) is one thing, while acting rationally (behaving in an instrumen-



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tally rational way) is something else entirely; if one has/does both then the relevant behavior is rational (and is thus subject to rational choice explanation).

While these two parts – rational ends and efficient means – are the main features of any rational choice explanation, there are at least two additional restrictions that need to be specified: the domain of the agent's preferences and any additional constraints on that domain. The set of outcomes that preferences are defined over is the choice space, and the subset of this choice space defined by the various constraints the agent faces is the feasible set. For example, in the traditional theory of consumer choice, preferences are defined over n-dimensional bundles of non-negative commodities, but not all of these bundles are “feasible” for the consumer. The feasible bundles are the subset of bundles that are affordable, those that lie inside the consumer's budget constraint. This is both a narrowly defined set of outcomes and a narrowly defined restriction on that set, but even in a more general case, the relevant choice set and feasible subsets are a necessary part of any rational choice explanation.

Although there is nothing about rational choice, well-ordered preferences, or instrumental rationality that necessarily requires reference to mathematics, it is nevertheless the case that rational choice explanations are often associated with mathematical optimization. It is relatively easy to see why this is the case. In situations where the relevant agent's ends are highly structured – for instance if they can be represented by a real-valued function – then instrumental rationality, using the most efficient means to achieve those ends, can be reduced to maximizing that function. In other words, if the agent's goals are sufficiently structured to make the optimization tractable, then rational behavior reduces to optimizing behavior.

All of these considerations lead us to the following characterization of Generic Rational Choice Theory (GRCT). It provides a very general model for a rational choice explanation of why a particular agent A exhibited a certain behavior x.

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[GRCT]

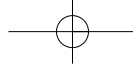
- 1) Rational Preferences: The agent A had rational preferences: well-ordered preferences (generally complete and transitive) defined over a nonempty feasible set of outcomes/behaviors.
 - 2) Feasible Set Constraint: The outcome/behavior x was contained in the specified feasible set.
 - 3) Choice Rule (Instrumental Rationality): The agent behaved in an instrumentally rational way (x was the most preferred element of the feasible set) given 1) and 2).
- Therefore: A did/chose x .

The schema GRCT is quite general.¹ It could be applied to a consumer choosing bundles of consumption goods, a country deciding whether or not to go to war, an elected representative deciding whether or not to support a particular piece of legislation, a corporation deciding to change the price of its product, an investor deciding the asset allocation in her portfolio, or any of a countless number of other possible scenarios. Also notice that GRCT can be applied to decisions made under conditions of certainty (where the agent has perfect certainty about preferences, feasible choices, and the corresponding outcomes) as well as those involving uncertainty (where some of these features are probabilistic in nature). Since our main subject is demand theory, the generic scheme GRCT raises an obvious question: How does this general-generic version of rational choice theory relate to the standard textbook version of consumer choice theory? The answer is that consumer choice theory (CCT) is just a special case (a restricted version of) GRCT.

Consider the following Consumer Choice Theory (CCT) schema for why consumer A chose the bundle x^* at prices p under conditions of certainty.²

1. It is possible to characterize rational choice even more generally where 1) involves a generalized choice function that that assigns a chosen set to every feasible set without any explicit reference to preferences. Given our focus on demand theory, and the literature being discussed, GRCT seems to be sufficiently generic.

2. Although many of the debates about rational choice theory focus on choice under uncertainty, I will restrict my discussion to the simpler case of perfect certainty. This would not be appropriate if we were emphasizing, for example, the literature on the psychological critiques of rational choice theory, but it is appropriate here. The problems that I wish to discuss not only emerge, but are often more clear, under conditions of certainty and it is the case that is most relevant to the history of early 20th century demand theory.



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[CCT]

1) Rational Consumer Preferences: Consumer A had rational preferences with sufficient structure (continuity, monotonicity, and convexity) that they could be represented by a strictly quasiconcave ordinal utility function for all .

2) Feasible Set Constraint: A's feasible set (B) is defined by the consumer's budget constraint $B = \left\{ x \in \mathcal{R}_+^n \mid \sum_{i=1}^n p_i x_i \leq M \right\}$ and $x^* \in B$.

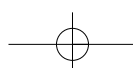
3) Instrumental Rationality: A chooses the that is the most preferred affordable bundle and x^* is this most preferred bundle (x^* is the solution to $\text{Max}_{\{x \in B\}} U(x)$).

Therefore: A chose x^* .

Of course [CCT] is just one possible special case, one possible specialization, of the general framework [GRCT]. All that is necessary to produce different specializations of GRCT is to specify different restrictions on the relevant agent: the preferences, the choice set, the feasible set, and the particular form that instrumental rationality will take.

My goal is ultimately to employ GRCT and CCT in an examination of the history of demand theory contained in the three books cited in the first section, but before turning to these histories it is useful to emphasize some of the more salient features of these two schemes. I will elaborate on these features in a series of notes.

N1: Notice GRCT and specializations such as CCT represent only a small portion of the theoretical activity that goes on within the social sciences that employ rational choice explanations. These schemes only explain the behavior of individual agents, and there is a lot more to social science (even microeconomics) than the behavior of individual agents. In fact, it can be argued that economists have not traditionally been very interested in explaining individual behavior – or at least not interested in explanations that stop at the level of individual behavior – but rather view such exercises as simply a (possibly necessary) step along the path toward an explanation of the social phenomena that emerges from the interaction of such individually rational agents. The way that economists and rational choice theorists from other disciplines have traditionally approached the explanation of social phenomena is to explain it as a property of some equilibrium that emerges from the interaction of a number of instrumentally rational agents interacting within the confines of particular social/institutional constraints/structures. In other words, GRCT



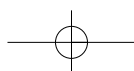


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is a theory of individual action that must be combined with a theory of interaction to explain social phenomena. A classic example of such interaction is the price of a particular commodity that emerges from a perfectly competitive market. The explanation of what is going on behind the scenes on both sides of the market is “based on” specializations of GRCT, and yet the market price that emerges from these interactions is not simply the “rational choice” of either set of agents. According to the standard story, the competitive market price is an unintended consequence of the interaction of instrumentally rational buyers and sellers (optimizing over their own, quite different, well-ordered preferences) who are acting within the context of a particular institutional structure: the perfectly competitive market. Of course if one specifies the institutional structure – the process of interaction – differently then one will get very different results: a prisoner’s dilemma for example. The point is simply that the assumption of rational choice is an assumption about the behavior of the relevant individual agents, and the social outcome will generally depend on their interaction and other factors outside the direct control of these agents. This means that although the institutions and constraints are quite different, the rational choice characterization of individual action can be snapped into a wide array of different types of social (and even biological) explanations, and yet these explanations will all be of the same general form. In this sense the invisible hand, prisoner’s dilemmas, evolutionary stable strategies, the Coase theorem, adverse selection, predator-prey equilibria, the median voter theorem, efficiency wages, Becker’s rotten-kid theorem, most of contemporary contractarian political philosophy, Jevons’s law of one price, Arrow’s impossibility theorem, the efficient market hypothesis, Ricardian equivalence, and Kitcher’s “cognitive division of labor” are all cut from the same methodological cloth. Such explanations do not exhaust social science, but they do cover a large, and expanding, portion of it.³

The expansion of this style of theorizing is an extremely important issue – it is one of Amadae’s main themes and I suspect she is entirely right

3. Obviously not everything that has appeared in 20th century social science is of this form. Outside of economics, political science has been more affected than sociology, and there has been even less impact on anthropology. Within economics it does not fit portions of heterodox economics and is absent from much of Keynesian macroeconomics. In addition, it may also be the case that certain areas along the frontiers of economic research are moving away from such explanations. For example Mirowski [(2002, pp. 551-560] discusses results by Gode and Sunder (1993) involving zero-rationality agents. In such models the institutions of the market do all the theoretical heavy lifting – in this case converging to equilibrium – and the agents are relatively impotent. Perhaps this is the wave of the future in economics, but even if it is, it is not pertinent to this discussion. Even if this is where the profession is moving, it is not where it has been for the last one hundred years, and it is not what was going on in 20th century demand theory.



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about the importance of the Cold War in explaining the success of this methodological campaign – but I will not elaborate further on this aspect of such explanations. The point of N1 is simply to note that GRCT (or CCT, or any other specialization of GRCT) is only one part of this increasingly dominant explanatory genre: the individual agent part. It is almost never the whole story and it is important to separate the individualistic rational choice aspect of such explanations from the additional features that are associated with the structures and mechanisms (that are often not explained in rational choice terms) that interact with the instrumentally rational agents to produce the relevant results.

N2: Note that GRCT need not have anything to do with self-interest. Rational choice explanations restrict the form of preferences (they must be sufficiently well-ordered), and it requires the agent to act in an instrumentally rational way (often maximize), but it places essentially no restrictions on the content of the agent's preferences. The structure of preferences are restricted, but not what the agent prefers; as such, it is entirely possible for the agents in GRCT explanations to prefer the welfare of others, or some state of the world with no direct impact on them, rather than be concerned with their own well-being. In the words of Daniel Hausman and Michael McPherson:

To be self-interested is to have preferences directed toward one's own good, not simply to act on one's own preferences. What distinguishes people who are self-interested from those who are altruistic or malevolent is what they prefer, and utility theory says nothing about what the content of rational preferences ought to be. [Hausman and McPherson (1996), p. 53, emphasis in original].

Of course economists traditionally assume self-interest. The assumption of own-directed preferences is not only an established part of the pedagogy and the rhetoric of economics; it may also be argued that since CCT requires preferences be defined over bundles of commodities (presumably bundles of commodities that the individual agent has access to and can take advantage of) it requires the agent to be self-interested. In any case, self-interest is a standard feature of economist's rational choice models, but that is because they build it into the particular specifications of GRCT they regularly employ.

N3: Note that it is not particularly obvious whether GRCT (or CCT) should be viewed as a descriptive or a normative theory of rational action. On one hand, since GRCT is a core component of (hopefully scientific) explanations in microeconomics and elsewhere in social science, one



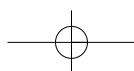
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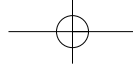
would hope that it correctly describes the behavior of the relevant agents; or if “correctly describes” is a bit too much to ask of such a (or any) scientific theory, then, at the very least, it should provide explanations that are scientifically adequate given the accepted standards for such adequacy. If one uses the term “positive” to label theories that are scientifically adequate, then GRCT (or CCT) should be a positive, not a normative, theory. On the other hand, GRCT can also be interpreted as a normative theory of agent behavior; not ethically normative, but normative in the sense of describing what an agent ought to do in order to be rational. According to GRCT, if one wants to behave rationally then one ought to have rational (well-ordered) preferences and act rationally (in the instrumental sense) given those preferences. To do otherwise would be to behave irrationally, and behaving irrationally is not what a rational person ought to do. In Hausman and McPherson words:

Despite the fact that utility theory makes no substantive claims about what people should prefer, it remains a normative theory concerning how people ought to choose, rather than a positive theory of how people do choose ... It lays down conditions that choices and preferences ought to satisfy. ... To define what rational preference and choice are, is ipso facto to say how one ought rationally to prefer and choose. [Hausman and McPherson (1996), p. 29, emphasis in original]

While many defenders of rational choice theory are perfectly comfortable viewing it as a normative ideal rather than scientific description, this is not the case for most practicing economists. For most economists, rational choice theory – and the economics based on it – is a positive (and adequate) scientific theory of the behavior of economic agents. For the majority of economists, economic agents do in fact have well-ordered preferences, optimize over those preferences, and then act on the basis of that optimization. This may be taken as just a brute fact or it may be supported by various arguments (of the money pump-sort) for the elimination of non-rational agents. Even those who doubt its accuracy for individual agents, may still believe that it is a good approximation for behavior in the aggregate (such as a market). In any case the vast majority of economists do not consider rational choice theory to be a normative theory; they consider it to be a (very successful) positive theory: a powerful scientific instrument for the prediction and explanation of economic behavior.

N4: Of course even if one accepts that GRCT is normative, that does not mean that it is ethical. Economists often equate “normative” with “ethi-



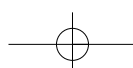


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cally normative,” but this is not way the term normative is used in intellectual life outside of economics departments. Normative simply means that it involves norms or standards; they could be epistemological norms, social norms, aesthetic norms, or any other type of norms. When Karl Popper argued that science requires bold conjectures and severe empirical tests, he was making a normative claim. It was an epistemically normative claim, but it was a normative claim nonetheless. He was saying this is what one ought to do in order to do science.

Like self-interest, ethical normativity in the rational choice context is about what one prefers, the content of one’s preferences, and not simply that they are well-ordered and rationally acted upon. One can rationally commit mass murder if one’s (mass murdering) preferences are well-ordered and one proceeds to achieve those ends in the most expeditious manner. On the other hand if one prefers outcomes that are consistent with particular ethical values, then the rational behavior specified by the rational choice model will also be what one (ethically) ought to do. It can be argued – though controversy still reigns on this matter – that standard welfare economics (Pareto optimality, the first and second fundamental theorem of welfare economics) is an ethically normative theory. One argument is that if preferences are self-interested and one has perfect knowledge about what serves one’s self-interest, then minimal benevolence – that it is ethically good to make people better off – generates the standard normative framework of welfare economics (that “the good” = “individual preference satisfaction”). But it is not necessary to go into such issues; the point here is simply that rational choice theory can be normative without involving ethics.

N5: Note (and this will be extremely important below) that nothing in either GRCT or CCT says anything about feelings, or sensations, or mental states such as happiness or pleasure. Although economists often motivate CCT with statements about the consumer seeking the “satisfaction” they get from consuming various bundles of commodities, there is nothing in either GRCT or CCT that requires, or even suggests, such a mental state interpretation. The theory only requires that people prefer certain outcomes in a systematic or well-ordered way, and says nothing whatsoever about the mental states they would, or believe they would, experience if their most preferred outcome were to actually come to pass. As Hausman and McPherson put it (a bit morbidly): “Ellen’s grandmother’s preference that her granddaughter become a doctor is satisfied if Ellen becomes a doctor, even if Ellen’s grandmother never lives to see that day and cannot feel any satisfaction at the event” [(1996), p. 74, emphasis in original].



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Of course while feelings or satisfaction need not have anything to do with rational choice explanations they certainly can be included, and for many economists they are an indispensable aspect of any such explanation. Here I am not referring to inadvertent slipping into the kind of pleasant-feeling talk that economists (including this one) often do, but rather the self-conscious specification of mental states like happiness or pleasure – psychological hedonism – as both proper to and necessary for the psychological grounding of the entire utility maximizing approach to explaining human behavior. It is safe to say that many of the early neoclassical economists were psychological hedonists in this sense; utility was a mental state that one “sought” and then “felt” when the utility maximizing bundle was consumed.

Modifying CCT to account for such a hedonistic interpretation of utility seems to be straightforward. I will call this specialization of GRCT Utilitarian Consumer Choice Theory [UCCT].

[UCCT]

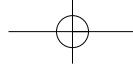
1) Cardinal Utility: Consumer A associates a cardinal level of satisfaction (sensation, feeling) with the consumption of each bundle of goods $x \in \mathcal{R}_+^n$. These levels of satisfaction are given by the utility function $U(x)$ for all $x \in \mathcal{R}_+^n$. This utility function is differentiable, concave, and satisfies the conditions $\partial U / \partial x_i = MU_i > 0$ and $\partial^2 U / \partial x_i^2 < 0$ for all $i = 1, 2, k, n$.

2) same as CCT.

3) Utility Maximization (Instrumental Rationality): Consumer A chooses the that provides the highest level of satisfaction – i.e. that is utility maximizing among affordable bundles – and x^* is that bundle (x^* is the solution to $\text{Max}_{\{x \in B\}} U(x)$).

Therefore: A chose x^* .

I will discuss this in more detail below, but the case has been made that certain early neoclassicals were actually rather conflicted regarding the question of CCT versus UCCT (Giocoli, 2003, pp. 67-73; Mandler 1999, ch. 5). It is argued for example that Pareto believed that humans did in fact get satisfaction from various goods they consumed and that those satisfactions generally exhibited properties like diminishing marginal utility, but while he believed these things to be true of human mental life, he also thought that science should be restricted to observables (and measurables) and thus endorsed CCT, rather than UCCT, as the proper scientific approach to demand theory. If we call those who support CCT

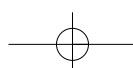


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“ordinalists” and those who support UCCT “cardinalists” (we will see below this is not the only way to define these terms) then it might be reasonable to think of Pareto, and certain other early neoclassicals, as ontological cardinalists and epistemological ordinalists. Such a distinction may help clarify some of the ambiguities that have emerged regarding such matters in the recent historical literature on Pareto [Bruni and Guala (2001) ; Weber (2001)]. In any case, the main point here is that feelings can be made to matter – and they certainly did (and do) matter for certain economists – but they need not play any role in rational choice (even CCT) explanations. Having a preference satisfied is one thing; how you feel if it happens is (can be) something else entirely.

N6: Note that neither GRCT nor CCT require individual human agents. Of course the standard interpretation of CCT is that the relevant agents are individual consumers, but it is not a necessary to apply the schema. First of all, the relevant “individuals” could be some mixture, aggregation, or abstraction of traditional economic agents. Economists often translate the idea of (and assumptions on) individual preferences into (onto) the preferences of some other type of “agent”: countries engaging in international trade, societies represented social welfare functions, households with household utility functions, the “representative agents” of optimization-based macroeconomic models, etc. Even rational choice models as mundane as the theory of the profit maximizing firm involve objective functions that are not necessarily associated with a single individual. But second, why stop at entities that are based on humans? Why not other animals? Why not rats [McDonough (2003)] or monkeys [Glimcher (2003)]? All one needs for rational choice is well-ordered preferences and optimization over those preferences, not the ability to drive an automobile or recite lines from Shakespeare.

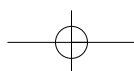
Finally, as long as we are moving away from the traditional individual human agent, why restrict ourselves to living things? Computer programs that offer particular customers special deals on selected products based on information about the past purchases also seem to fit the GRCT framework. Why not roboshoppers as rational agents? Although some may be uncomfortable with the extension of rationality to nonhuman agents, there is a sense in which is a natural for the rational choice framework. Starting with the early neoclassicals, but throughout the history of rational choice theory, the guiding vision has been to reduce “rationality” – or at least any rationality worth serious discussion – to machine-like behavior. Acting consistently on the basis of well-ordered preferences means to act, not spontaneously or whimsically or humanly, but algorithmically.

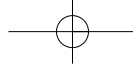




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For the early neoclassicals the model was the physical machine and the scientific metaphor was physics, while much later it changed to the computer and information processing, but despite these shifts: “Machine rationality and machine regularities are the constants in the history of neoclassical economics; it is only the innards of the machine that have changed from time to time (Mirowski, 2002, p. 9). Not only are nonhuman instantiations or GRCT possible, if one wants literal description, software versions of rational agents may actually fit the model better than the actions of their more capricious flesh and blood brothers and sisters. **N7:** Finally, note that a lot gets packed into the phrases “the agent has well-ordered preferences” and “acts in an instrumentally rational way.” First consider the question of well-ordered preferences. The meaning of well-ordered of course depends on the particular theoretical context; in economics it usually means that preferences satisfy completeness and transitivity, but in other contexts various theorists employ other notions of well-orderedness. Although the exact relationship between standard assumptions like transitivity and the general concept of rationality have long been, and continue to be, debated, this is not the only controversial issue surrounding the specification of well-ordered preferences. Most obviously – and in terms of the descriptive accuracy of the theory, most controversially – these preferences need to be stable, that is unchanging, during the course of any variation in any of the other variables involved in the explanation. In the case of CCT, preferences must stay the same through all possible variations of the price vector and money income. Of course when one embeds the particular individual agent in a model where he/she/it is interacting with other rational agents under various institutional and structural constraints – like in general equilibrium theory or game theory – then preference stability becomes very difficult to maintain as a descriptive feature of the agent. As a parade of critics have pointed out over the years, it means that demand theory is effectively instantaneous, and thus impossible to test on the basis of statistically-derived demand curves since the price-quantity data points come from observations taken over time. Not only has this issue plagued any attempt to empirically test demand theory, it is also one of the main points of contention in the much publicized debate between economists and experimental psychologists. The assumption of stable preferences is essentially the assumption that the ordering remains invariant as one moves around in the choice space: that “it” doesn’t depend on what you have, where you start, etc. Experimental psychologists call this reference-independence and find it very problematic.



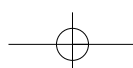


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Economists are thoroughly habituated to the sight of indifference maps, but for someone who has been trained as a psychologist they can be a source of puzzlement. It took me a long time to realize that the representation looked odd because I kept looking for an indication of the individual's current position in the map. There is no such indication, of course, because this parameter is supposed to be irrelevant: preferences for final states of endowment are assumed to be stable over variations of current endowment. This assumption, called reference-independence ... is the interpretation of unchanging tastes with which I am concerned here. [Kahneman (2003), p. 163].

The second issue about preferences concerns existence. Obviously if the theory assumes that agents have well-ordered preferences or utility functions, then it assumes that such things exist. Even without delving too deeply into the philosophical implications, it is clear that such an assumption involves some pretty heavy ontological baggage. In fact since almost everyone who applies CCT, or for that matter any other version of GRCT, claims to be some kind of empiricist, the existence of these utility functions or preferences in agent's heads are a consistently troublesome methodological issue. Those who start with something (ostensibly) observable like indifference curves (Pareto, Edgeworth, ...), or marginal rates of substitution (Hicks and Allen 1934, and later just Allen), or demand functions [Chipman, Hurwicz, Richter, and Sonnenschein (1971)], or consumption bundles satisfying the weak axiom of revealed preference [Samuelson (1938a), (1948)], then one is going to need some kind of integrability assumption in order to guarantee the existence of the underlying, and invariant, preference ordering or utility function. The problem is that most such integrability conditions must be motivated by (and are often deduced from) CCT. Some heavy ontological baggage indeed.

Lastly, and only in order of presentation not order of importance, is the issue of computability. The assumption that the agent acts in an instrumentally rational way assumes that the relevant optimization problem is not only mathematically tractable (has a unique solution, etc.), but also that it is both practically solvable (the agent has sufficient processing capability and speed to actually find the solution) and logically solvable (it is computationally viable). All versions of rational choice theory simply assume these last two issues away. Economists worry about the mathematical structure of the choice problem and the assumptions necessary to guarantee that an optimal solution exists, but then simply assume that since a solution exists it is possible for the agent to find it in reasonable





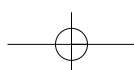
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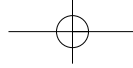
time from any particular initial starting point with the computational resources they have available (remember preferences can't be changing while they are computing). Since the question of computability, and the problems it raises for rational choice theory, are questions of relatively recent vintage and were not a significant part of the literature I am considering, it will not be discussed in what follows.⁴ On the other hand, I will frequently allude to the problems associated with the stability of preferences and integrability.

III. Three Histories – One Story

After this bit of stage-setting, it now time to return to the three texts under consideration. I claim – and here I doubt that any of the authors would disagree – that the discussion of demand theory in of these books takes the position that mainstream consumer choice theory in the Arrow-Debreu era was significantly and substantively different from the consumer choice theory of the early neoclassicals. Not only was there a big difference between the theory of Jevons and Marshall, and the theory of Arrow and Debreu, there was an important “middle-move” – ordinalism – that was significantly different from either the earlier or the later theories. Basically the argument is that early neoclassical choice theory (roughly UCCT) was seeped in sensations, psychological hedonism, and cardinal utility. Then, starting with Pareto, but really coming to fruition during the 1930s with the work of Hicks and Allen (1934), Slutsky (1915), and others, this hedonistic view was decisively overthrown and replaced by an ordinalist interpretation of preferences (essentially CCT). But then, in 1938, a second revolution began with the publication of Samuelson's paper on the weak axiom of revealed preference (WARP); this approach eventually led to the abandonment of preference- and utility-talk altogether and to its replacement by a choice-based theory of demand that redefined rationality, not in terms of maximization, but in terms of the “consistency” of the agent's choices. It was this latter view of consumer choice, that, combined with some heavy mathematical machinery, became the demand theory contained in general equilibrium theory's canonical works such as Debreu (1959) and Arrow and Hahn (1971). This seems to be the essence of the story told by all three authors, and as I said, I doubt if any of authors would substantially disagree with my interpretation of their reading (on this point).

4. Computability and the problems it creates for rational choice theory is a central theme in Mirowski (2002).





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In summary, my claim is that all three authors argue that both ordinalism and WARP constituted significant changes for demand theory. For convenience I will call these theses Significant Change 1 (SC₁) and Significant Change 2 (SC₂).

[SC₁]

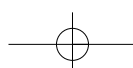
The ordinal revolution was a significant change. Early neoclassical economists thought of consumer choice exclusively in terms of hedonistic psychology and [UCCT]. Rational action involved “seeking” the “satisfaction” received by consuming various bundles of commodities and thus with maximization of a cardinal utility function with the standard neoclassical properties. The ordinal revolution associated with Pareto, Hicks and Allen, Slutsky, and others during the first third of the 20th century, was a radical break with this earlier tradition. The preferences of the agent were taken as the primitives of the theory, and even though these preferences could be represented by an ordinal utility function, the theory of demand was essentially emancipated from its association with mental feelings or hedonistic psychology.

[SC₂]

The “consistency” revolution associated with the weak axiom of revealed preference (WARP) was a significant change. Samuelson’s WARP and the related literature took the ordinalist revolution one (big) step further and entirely replaced concepts such as preference orderings and utility with a set of consistency conditions that (it was argued) could be empirically observed in the actual choice behavior of individual agents. The result was the replacement of the previous preference-based theory of demand with a choice-based theory of demand that rejected both maximization (replacing it with consistency) and introspection (replacing it with empirically observable behavior).

This two-big-breaks – or two significant changes – interpretation of the history of demand theory plays a central role in the story line of all three authors. I will argue in the final section that it need not – that the stories work quite well (perhaps better) without these breaks – but nonetheless it does play a key role in all three narratives.

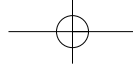
Amadae seems to focus on refuting the claim that the rational choice theory that evolved out of RAND and the Cold War political-economic context, and became so important in political philosophy and political science, was not simply an example of the “imperialism” of neoclassical



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economics. Rational choice theory was a substantive new theory of individual choice that was particularly effective in reconstituting how the Western intelligentsia thought about the individual, democracy, and markets: a way of thinking that is, she argues (and I would agree), still very much with us. Her most detailed discussion of demand theory is contained in chapter seven, where she makes four claims about the difference between marginalism and rational choice theory: “Together these four sections argue that rational choice theory is qualitatively different from marginal economics and presents a new definition of rationality in terms of nonmarket decision making without consideration of scarcity” [Amadae (2003), p. 222]. The marginalists had a “fixation with ‘maximization of utility under a budget constraint’” (ibid., p. 83); this was replaced by Samuelson’s WARP which used only knowledge “gleaned from observing choices among various commodity bundles” (ibid., p. 231); this led in turn (particularly through the work of Kenneth Arrow) to rational choice theory that was “distinct and can only be compared with difficulty” (ibid., p. 231) to ordinal utility theory. The final result is a rational choice theory that “represents a contribution to a modernist epistemology that supports democratic liberalism by upholding the values of free inquiry, universalism, individual autonomy, and government by trade and negotiation, as opposed to autocratic tyranny or irrational mob rule” (ibid., p. 256).

Davis also uses the great divide to support his main thesis about the ontology of individual identity in economics. He has much to say about heterodox economics, but with respect to the mainstream tradition we are examining here, he argues that despite the economics profession’s rhetoric about individualism, contemporary mainstream economists “lack an adequate conception of the individual” [Davis (2003), p. 17]. The early neoclassicals defined individuals in terms of “subjective inwardness,” “satisfaction or happiness,” and assumed they acted in an instrumental rational way in pursuit of these pleasurable subjective mental states (ibid., pp. 26-28). But Pareto and the authors of the ordinalist revolution pushed this subjective psychology farther and farther into the background. Samuelson’s WARP was the final step in the removal of the mind and subjective sensations (ibid., pp. 34-4), leading to the culmination of this de-subjectivization process in the Arrow-Debreu formalism where “preferences finally lost their psychological characterization altogether, since their interpretation now depended on their formal specification rather than on their description as natural phenomena” (ibid., p. 31). Economics went from having an inadequate theory of the individual –



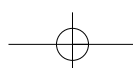
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through these two significant changes – into a formalized rational choice theory which left it “with a void where the individuals had been” (ibid., p. 45). Davis also explains how developments in the philosophy of mind and computer-inspired vision of the agent as an information processor figured into these changes in economic theory, but the two major changes are clearly an essential part of the story. Without these two big changes, microeconomics would still have a theory of individual identity – perhaps a problematic one, but a theory of identity nonetheless – and Davis’s overall story would be quite different.

Giocoli focuses more directly on the subject of demand theory than the other two authors – as he says early on, the notion of rationality is “a good proxy of the overall pattern of neoclassical economics” [Giocoli (2003), p. 3] – but here too, the two significant changes are pitched as key aspects of the overall story. Recall that Giocoli is particularly interested in retelling the history of the formalist revolution in a way that will render certain otherwise inexplicable developments (particularly the delayed acceptance of game theory among economists) more explicable. He employs the distinction between the “system of forces” (SOF) and “systems of relations” (SOR) ways of thinking about social explanation throughout the discussion; SOF is generally thinking in terms of causal forces and determining process, while SOR concerns consistency, mutual co-determination, and formal relationships. His central thesis is that neoclassical economics started out SOF (with subjective psychology), moved (via WARP) to SOR during the period we have been discussing, and that this change explains the delayed, but ultimately whole-hearted, reception of Nash equilibrium (NE) game theory: “It was the transformation of neoclassical economics in the direction of the consistency view of rationality and, more generally, of the SOR image that made possible the rise of non-cooperative game theory and NE to their current outstanding role” (ibid., p. 346). As he explains the process:

We started from the classic notion of a rational agent inherited from the early marginalist writers, who viewed the agent as a relentless maximizer who aimed at pursuing his/her own goals and desires, and ended with the shrinking of rationality to a formal requirement of consistency, where the notion of agency itself was so stripped down of its human peculiarities as to become an all-purpose concept valid for real individuals as well as for groups or machines. (ibid., p. 3)

Again it seems that if these changes had not been so significant, the historical trajectory might have been much different. In particular it would





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mean the change to SOR might not have taken place, and perhaps (if Giocoli's story is correct) his main explanandum would be left without the need of an explanans.

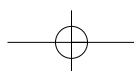
IV. What's Wrong with These Pictures?

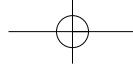
Well, finally we come to the critical portion of my argument. So what exactly is wrong with thinking of ordinalism and WARP-consistency as really significant changes? My answer has many parts, but I will defer my more meta-critical comments until the final section. Here I would like to examine in detail, and in doing so try to undermine, the claims that the ordinalism and WARP were substantive in the way these (and other) authors suggest. My argument will be part historical, part methodological, and part just pointing out various inconsistencies. I will make my remarks on an issue-by-issue, rather author-by-author basis; in some cases the relevant points are emphasized in one of the books much more than the others, while in other cases the comments will apply to all three. Again, as with the above notes, I will simply list my areas of concern (as $A \rightarrow C$). The remarks are not arranged in order of importance, but rather, move in (very rough) historical progression from issues regarding early neoclassicism, to ordinalism, and on to consistency and WARP.

A) Early neoclassicism was not as psychologically hedonist and ordinalism was not as anti-hedonist as suggested. As I mentioned above, neoclassical theory, from the very beginning, reduced rationality to algorithmic processes. Rationality was not only opposed to whim and capriciousness, it was reduced to a mechanical form of rationality; it meant mirroring the logic of mechanical, means-ends, process in the human brain. As Philip Mirowski expressed it.

Perhaps the most efficient way to comprehend the canonical neoclassical model is to first realize that it has nothing to do with the subjective psychology of the mind, as it is understood in the Western dualism of mind and body. In fact, as Fisher said, it has nothing to do with utilitarianism as a political or psychological theory. [Mirowski (1989), p. 235].

This is not the place for a detailed discussion of any one particular early neoclassical theorist, but I would like to note in passing that recent work on Jevons's "logical abacus" [Maas (1999)] makes it clear how narrowly, and mechanically, focused Jevons's vision of human rationality actually was: "mind was for Jevons equivalent to a machine" [Maas, *ibid.*, p. 616].

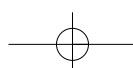


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Where is the subjective consciousness, the feelings, the sweet sensation of pleasure and torment of pain, in such a mechanical brain? My point is that flesh and blood humanity and the associated subjective sensations effectively exited choice theory on day one of neoclassicism. This is not to assert that early neoclassicals never thought in terms of hedonistic psychology – many certainly did – but rather that those who did were essentially resisting the obvious implications of the formalism; once human rationality was equated with maximizing a real-valued function we were already started down the road that carried us completely away from subjective sensations and toward roboshoppers. I see this part of the story less about discrete changes taking place at various nodes in the history of 20th century demand theory, and more about the ripening of a seed planted in the 1870s.

This said, I do not think the only problem concerns the hedonistic characterization of the first generation; the argument about the next (ordinalist) “step” is problematic as well. Although the “ordinalist revolution” is the main topic of the next section (where some relevant definitions will be provided) there are a few important points that can be introduced here. The story in these three books – that the first, subjective hedonist, stage gave way, at some key moment, to a different (and ostensibly agreed upon) ordinalist theory – requires both the existence of a generally accepted hedonist-cardinal view before the change and a generally accepted antihedonist-ordinalist view after the change. If we find that neither of these ideal types were clearly exhibited in the writings of key economists on either side of the great divide, and we also find that breaks were generally messier and less crisp than these authors suggest, it would certainly count as evidence against the stage theory of SC_1 and SC_2 . In fact we find both of these things. Not only is cardinalism more mechanical and not as subjectively-hedonistic as the stage theory suggests, ordinalism is not as consistently anti-hedonist (or for that matter consistently anything) as it is “supposed to be”; in general the literature of the period exhibits much more ambiguity, backsliding, and shape-shifting than would be consistent with any kind of substantive distinct break.

As indicated above in N5, there is reason to believe that many of the economists that were supposedly responsible for the major change were quite conflicted about the psychological foundations of demand theory. For one thing it is possible to believe that people do get satisfaction from the goods they consume, and that these satisfactions matter to the choices they make; and yet not, in one’s more empiricist moments, believe that such a utility function can be “measured” by the observing economic





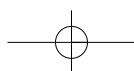
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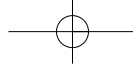
scientist. In other words, one can, in Michael Mandler's apt phrase, support "cardinality, but not cardinal measurability" [Mandler (1999), p. 115]. Mandler makes such a case for Jevons, Marshall, and Pareto, but he is not alone in making such claims about the key figures of the period. The argument that Pareto defended cardinality, but not cardinal measurability, is also supported (though they do not employ these terms) by other recent work on Pareto [in Bruni and Guala (2001) and Weber (2001)].⁵ Another interesting case is Slutsky. Much of the recent research points to certain cardinalist aspects of Slutsky's famous 1915 paper on demand theory [Chipman and Lenfant (2002) and Weber (1999a and 1999b)]. In addition, Smolinski (1984) examines Slutsky's relatively unknown paper on economic methodology (or metaeconomics) published in German in 1926, where he offered a very different view of the foundations of economics (including demand theory): "In this article, Slutsky constructs what amounts to a metalanguage for pure economic theory, with the ultimate aim of stating its basic propositions, definitions and concepts in the form of a self-contained axiomatic system, in the spirit of Hilbert's celebrated Foundations of Geometry [Smolinski (1984), p. 63].⁶ While such an interpretation might be consistent with later formalist versions of demand theory, it certainly is not consistent with the interpretation of ordinalism as a self-conscious move toward a more observation-empirically based theory. In Smolinski's words: "Slutsky believed that economic laws cannot be derived by induction from the study of concrete phenomena, from empirical statements about concrete events" (ibid., p. 68). Thus Slutsky, like so many of the others that were supposedly responsible for the revolution, does not seem to defend any of the positions that are necessary to support the argument for a distinct clear break.

The bottom line is that not only were many key cardinalists not very hedonistic, it also seems that many of the key ordinalists were not, well, very ordinalist. The contention that there was a distinct change, initiated by the work of a few key figures, that captured the moment of transition from early neoclassicism to 1930s ordinalism, starts to look more and more like an artist's conception once one realizes that there never was

5. It is interesting that Giocoli discusses this literature (pp. 67-72), and seems to agree with it, and yet does seem to believe it undermines his argument for SC₁ and SC₂.

6. Giocoli [(2003), pp. 126-127] actually links Von Mises's apriorism with Debreu's Bourbakism, and something like this seems to be what Slutsky was doing in this 1926 paper. Slutsky's paper is now available in English translation (1926).





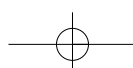
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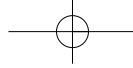
complete agreement about psychological hedonism on one end, and confusion about what exactly ordinalism was on the other. Could it be that the sharp break was imposed on the historical record after the fact by historians of economics armed with (conditioned by their training in) the conception of ordinalism as an alternative that stabilized long after the events in question? After all, if the move to ordinalism was a giant step forward for the scientific foundations of economic theory, as our textbooks have repeatedly told us, then there must have been (an erroneous) view that it constituted a giant step forward over. Oh, psychological hedonism: that's the ticket!

B) Ordinalism was not such a significant change. Let me start by being entirely clear that SC_1 is not just the story told by these three authors, it is the standard story of demand theory – standard in the history of economic thought, standard in textbooks and microeconomic pedagogy, and standard in the culture of the profession.⁷ Given this, it is obviously a topic that will ultimately require a much more elaborate defense than I can give it here. Nonetheless, let me at least try to sketch out some of the major points that I believe can be made against SC_1 in the space available here. Before I start, let me point out that my criticism should really be worded: there is nothing about ordinalism that would require, or even suggest, that the profession should/would consider the move from cardinal to ordinal utility theory a significant change. Obviously significance is in the eye of the relevant community, and in this case the community definitely saw it as a very significant change (see any microeconomics textbook). My point is simply that there was nothing in the logic of the argument, the associated epistemological claims, or the way the various arguments hung together, to indicate that it needed to be, or was destined to be, anything like the big deal it apparently became; it clearly, I argue, could have been otherwise. As I will discuss in more detail in the final section, my point is mainly about the relationship between the mainstream reading of the events/literature and the work of the historian. If there wasn't any obvious reason why it should be considered significant – the case defended here – and yet the community almost unanimously took it to be such, the question is why?

It is probably best to start the discussion with a definition of an ordinal utility function, or ordinal restrictions on a utility function. The definition we use today, and the one I will use throughout, actually took a long

7. Evidence for this assertion will be provided in the final section below.



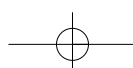


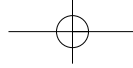
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time to stabilize. A longer history of ordinalism would need to investigate all of the various concepts – mathematical and not-so-mathematical – that percolated throughout the early literature on ordinal utility theory, but the contemporary one will work for our purposes. If $U(x)$ is a real valued differentiable utility function defined on \mathcal{R}^n , then the ordinal properties of the function are those that also hold for all $V(x)$ where $V(x) = F[U(x)]$ with $F' > 0$. In other words the ordinal properties of a function are those that also hold for any monotonic transformation of that function. Notice that properties like monotonicity ($U_i = \partial U / \partial x_i > 0$) are ordinal since $V_i = F'U_i$, as are the properties of indifference curves like the marginal rate of substitution (MSR), etc. Also note that when one maximizes $V(x)$ subject to a standard budget constraint, the resulting demand functions will be exactly the same as those obtained from maximizing $U(x)$ subject to the same constraint. Problems arise when one considers second derivatives of $V(x)$. For example (the most important example) the property of diminishing marginal utility ($U_{ii} = \partial^2 U / \partial x_i^2$) is not an ordinal property since $V_{ii} = F'U_{ii} + U_i^2 F''$. There are also difficulties with the marginal utility definitions (often called the Edgeworth-Pareto definitions) of substitutability and complementarity, the assumption of additive separability, and various other conditions that were standard in early neoclassical demand theory; but the negative implications for the law of diminishing marginal utility was clearly considered to be the most important. As D. H. Robertson so colorfully put it, ordinalism amounted to “slitting the throat of diminishing utility” [Robertson (1952), p. 18].

The emphasis on ordinal, as opposed to cardinal, conditions essentially entered the discussion of demand theory from (both of) two different directions. To use Davis's terms [(2003), p. 28]: it entered from both the internal- and the external-space perspectives of human action. If one stays squarely within the neoclassical tradition that is perfectly comfortable assuming that people have utility functions, and that rational action on the basis of those functions is the best way to explain economic behavior, then the space of action is internal to the agent. From such a perspective ordinalism mattered, but it really didn't matter very much. From this internal (assume people have utility functions) view, ordinalism simply involves the recognition that an x that maximizes $U(x)$ also maximizes $V(x)$. As Hendrik Houthakker explained:

Fisher (1892) and Pareto (1896) realized that if a utility function reaches a maximum at a certain point then any order-preserving transformation of that function also reaches a maximum





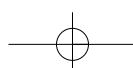
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there; hence such maximization involves only ordinal properties. Neither, however, was able to draw the full conclusions from this realization. [Houthakker (1961), p. 705].

This of course has implications for an explanatory strategy that is grounded in the apparent “fact” of diminishing marginal utility – since the other key feature, maximization, will now be totally disconnected from it – but it still remains primarily a technical, as opposed to more substantive, problem. From this internal perspective the whole argument can still go through (and I would say, did go through for most economists) with just a little mathematical tweaking – like assuming the utility functions satisfy certain concavity, or second order conditions – and dropping the talk about diminishing marginal utility altogether. In any case it doesn’t need to be a significant change.

On the other hand, if one shifts the locus of choice from something that happens inside the head of the agent (internal) to something that is externally observable, then ordinalism also enters, but in an entirely different, and potentially more disturbing, way. If one takes indifference curves as the observable primitives of the analysis, then ordinalism is associated with the problem of measuring (also called determining) the individual’s utility function from the (ostensibly observable) indifference curves. For economists like Pareto, indifference curves were assumed to be observable, and since one can create a utility “index” by simply assigning an arbitrary index number to a particular curve and then scaling up (down) the numbers for higher (lower) indifference curves, the concept of a utility index was also a relatively unproblematic notion. Under these assumptions the utility index was epistemologically unproblematic as well; if the indifference curves were observable, and the index function was constructed from operations on these observable curves, then the utility index would unproblematically inherit the observational (and epistemic) basis of the curves. The problem was of course, given how this index was constructed (from the shapes of the indifference curves), the only (epistemically grounded) properties that the function could have would be ordinal properties. As Davis explains:

The concept of indifference as the underlying basis for understanding preference, moreover, had the virtue of describing the individual choice in terms of a position vis-à-vis possible consumption bundles. It thus shifted the focus from an internal to an external space in which individuals acted, and made observable relationships central to understanding economic behavior. [Davis (2003), p. 28].

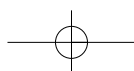




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I think what Davis says in this quote is entirely correct in terms of the way the issue was generally framed by the profession; ordinal utility was characterized as less “psychological” and more “observable” than cardinal utility, and precisely for these reasons. But we also need to remember the context. It was more observable because indifference curves were posited as observable. But if one does not start with this (non-obvious and quite contestable) assumption – if, for example, one does not believe there is any more reason to consider indifference curves (particularly the well-ordered smooth n -dimensional indifference hyper-surfaces that would be required for demand theory in) to be more observable than utility functions or any of the other theoretical machinery involved in demand theory – then the whole “ordinal = observable” claim simply collapses. We can understand exactly how the (this) difference between ordinal and cardinal came to be thought of in this way, but that is very different than saying that ordinalism “made observable relationships central to understanding economics behavior.”

In Herman Wold’s (1953) survey of demand theory he discussed three different approaches to the foundations of demand theory; they were, in historical order, the cardinal (and additive) theory of Jevons and Walras, the “indifference curve” approach of Edgeworth, Fisher, and others, and the “Paretoan” theory that was based on a “behaviouristic interpretation” [(1953), p. 62]. In addition, this “Paretoan” view actually contained three separate approaches to such behavioristic grounding: preference fields (ordinalism) associated with Slutsky, the marginal rates of substitution (Hicks and Allen 1934), and the “demand function” approach that Wold identified with both economists like Gustav Cassel that disavowed any need for rational choice foundations (of any sort) for demand theory and also Samuelson’s WARP. The point here is that the ordinal revolution is supposed to be about observability and observation, and yet even by the 1950s it wasn’t clear whether the relevant “observations” were of indifference curves, ordinal preference orderings, marginal rates of substitution, or demand curves themselves. It is also a bit ironic that ordinalism came to be associated with straightforward and unproblematic observability when two of the authors most responsible for the so-called ordinalist revolution, Hicks and Allen (1934), took their observational givens – marginal rates of substitution – to be something different from the “givens” of almost all of the other economists working in the field. One would think that a minimal condition for being observable, would be that more than two people think it to be so. In addition, both of these authors also ended up rejecting ordinal utility theory (that



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they were supposed to be responsible for) in later work on demand theory; Allen stuck to the core of their original program but insisted that the non-integrable case (not the standard theory) was their important contribution [Allen (1936), (1950)], while by 1956 Hicks based his belief in demand theory on some combination of econometrics and the WARP. If it was such a significant change, why didn't anyone agree about what it was?

Observability was certainly part of the story (of these authors and most others who have attempted to recount it), but it was only part. Ordinalism was also supposed to (finally) get rid of the last vestiges of hedonistic psychology and also (finally) put to rest the perennial bogeyman of interpersonal utility comparisons. I would like to address both of these issues, starting with hedonism.

The standard reading of ordinalism is that it eliminated hedonistic psychology from demand theory.

Pareto ... however, took what is generally regarded as the first important step towards contemporary choice theory when he showed that ordinalist analysis made it possible to avoid making any essential reference to human psychology in explaining individual choice. [Davis (2003), p. 28].

I agree, ordinalism is "generally regarded" as eliminating hedonistic psychology from choice theory, but let's look a little closer at this popular belief. The models UCCT and CCT make it clear that ordinal utility need not have anything to do with subjective feelings like pleasure or happiness. But I would argue that cardinalism need not be associated with those feeling either. If one replaces (in CCT) the general statement about well-ordered preferences with a particular cardinal utility function, one does not have UCCT, one still has CCT, but now with a more restrictive characterization of preferences, and one that still need not say anything about feelings or sensations. Cardinalism is simply the property of being able to measure the strength of the preference difference between any two bundles; it says nothing about the sensations (or even that there are sensations) associated with those differences. Cardinal measurement does not imply psychological hedonism.

But not only does cardinalism not imply hedonism; hedonism also need not imply cardinalism. If one replaces the cardinal utility function and diminishing marginal utility in UCCT with an ordinal utility function that satisfies the second order conditions for the maximization, then one has an ordinal utility theory that is also based on satisfaction and psychological hedonism (I would argue this is the way that most economists

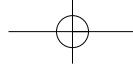
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think about demand theory most of the time). One can know that bundle A gives more satisfaction – has more pleasurable feeling associated with it – than bundle B, without knowing the magnitude of the difference. As Mandler explains:

In reality, the ... psychological doctrines above, though often considered interchangeable, are not. Consider first hedonism and cardinalism. Cardinalism allows assertions such as “a move to is twice as preferred as a move to ,” “a move to is twice as good as a move to ,” or “a move to provides twice as much welfare as a move to .” None of these statements imply that pleasure is the basis of the comparison. Similarly, a hedonist who is not a cardinalist can say “a move from to gives more pleasure than a move from to ,” while remaining agnostic about the exact ratio of the two pleasure differences. [Mandler (1999), p. 84].

So, despite the common association there is no necessary relationship between cardinality and hedonism. Inexorably linking the two is a key aspect of the standard story – the “ordinalist caricature” (ibid., p. 110) – but the linkage is neither necessary nor obvious. This, I believe, leads to the interesting historical question – by the way, a question that does not arise given the way these three authors tell the story – of why it was that such an unnecessary connection was (and still is) considered to be so necessary to so many economists.

Not only can cardinalism be separated from psychological hedonism, it can also be separated from the question of the presence or absence of interpersonal utility comparisons. Davis is certainly right that the generally accepted link between cardinalism and interpersonal utility comparisons is partly historical; many of “the cardinal utility theorists had thought human psychology sufficiently intelligible to justify making such comparisons” [Davis (2003), p. 28]. But just because certain early neo-classicals thought that utility was both cardinal and interpersonally comparable, does not mean that cardinality is sufficient for comparability, or perhaps more importantly, that the inability to make interpersonal comparisons implies the existence of ordinal (or for that matter any well-ordered) preferences. Interpersonal comparison requires both observability and a standard of valuation that is common to those being compared. If people value choices in various ways, but we do not have access to those valuations – either because we cannot see inside their heads or because their consumption patterns only reveal their valuations up to a monotonic transformation (and is thus not unique) – then



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it will be impossible to compare the valuations of two different individuals. Ordinalism, and particularly its champions on these matters such as Lionel Robbins (1932), effectively made the case that even if people had well-ordered preferences and the associated utility functions in their heads that could be discovered by observation (presumably of indifference curves, marginal rates of substitution, etc.), then these valuations would not – because the level of utility is only unique up to a monotonic transformation – give us any usable numbers from making interpersonal comparisons. Thus, it was argued, and in a narrow sense correctly, that ordinalism prevented interpersonal comparisons. But this argument does not say, as Robbins and others suggested, and as ultimately became the conventional wisdom, that i) the absence of interpersonal comparisons implies that agents do have ordinal (or do not have cardinal) preferences, ii) that our inability to make interpersonal comparisons implies that the relevant agents do not base their actions on hedonistically seeking pleasure and avoiding pain, or iii) that any scientific and observational-based choice theory would rule out interpersonal utility comparisons.

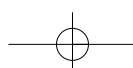
Just to close out this long discussion of ordinalism, the point is that economists commonly consider – and this view is supported by much (though not all) of the discussion in the three texts under consideration – the following set of relationships to be roughly equivalent:

ordinal \leftrightarrow observable \leftrightarrow non-psychologically-hedonist
 \leftrightarrow noninterpersonal \leftrightarrow good science,
 and

cardinal \leftrightarrow nonobservable \leftrightarrow psychologically hedonist
 \leftrightarrow interpersonally comparable \leftrightarrow bad science.

I have tried to make the case – or at least tried to raise historical doubts about – essentially every one of these supposed equivalencies.

C) WARP and the associated “consistency only” revolution was not such a significant change (it could have been, but wasn’t), All three authors argue that Samuelson’s WARP was the missing link, the second big change, as demand theory moved from psychological hedonism to the Arrow-Debreu formalism. Ordinalism was, so the story goes, a halfway house – more observable, more scientific, and less hedonist than the demand theory of the early neoclassicals, but there was still a long way to go. The final link in the chain was Samuelson’s revealed preference theory, originally presented in Samuelson (1938a). WARP thus represented “the culmination of the neoclassical economists’ 45-year-long escape from psychology” [Giocoli (2003), p. 99].





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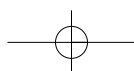
I will discuss WARP in more detail below, but the basic idea was that if an agent chooses bundle x at price vector p when x' was affordable, and $x \neq x'$, then they have “revealed” a preference for x over x' . Thus, if they choose bundle x' when the price vector is p' , it must be that their preferred bundle x was not affordable at p' . The weak axiom of revealed preference (WARP) can thus be written:

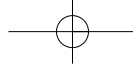
$$p^T x' \leq p^T x \Rightarrow p'^T x > p'^T x' \text{ (WARP)}$$

where x , x' , p , and p' are all n -dimensional column vectors and T indicates transpose. It is easy to see why this is often considered to be a consistency theory of rationality; if agents satisfy this condition, then their behavior exhibits a kind of consistency of preferences. For example, if the antecedent in WARP held for a particular agent but the consequent did not, then it seems that either the person had inconsistent preferences – revealing a preference for x over x' at one price vector, but not a different price vector – or if they actually did have consistent preferences, then their tastes must have changed as the prices changed from p to p' . In either case, inconsistent or changing preferences, the agent appears to violate the assumption of well-ordered (and stable) preferences. Also notice that WARP represents something that seems to be potentially observable; they buy one thing at one price vector and something else at another price vector, and then WARP either does, or does not, hold for those observed choices. Thus, it is claimed WARP simultaneously moves the theory farther away from hedonism and makes it more scientific by focusing exclusively on observable “behavior.”

Samuelson explicitly argued that it is dubious that such complex demand equations are at some level consciously calculated by agents, and held that economists should focus on observed consumer behavior to reveal the agents' preferences. This move to rely on empirical observation, in addition to postulating abstract laws regulating human consumption, gave economics a more credible scientific basis. According to Samuelson's “revealed preference” approach, all that is known about consumer choice is gleaned from observing choices among various commodity bundles. [Amadae (2002), p. 231]

Although all three authors agree that WARP was a significant change, and constituted the final move toward a formal consistency-only, or choice-based, view of consumer behavior, they do evaluate this move in very different ways. Amadae is not explicit on the matter, but since she views WARP as one step on the way toward rational choice theory – and away from marginalism – it appears that she considers it to be a progres-





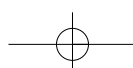
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sive move in the history of choice theory. For Davis, WARP was just one more step in the loss of individual identity and toward the economic agent as a generic information processor – an information processor that could just as well be a computer as a human agent. If that is progress (it is not for Davis), then Samuelson’s move was a significant step forward. Giocoli is much more outspoken on the matter, and more explicitly critical. On one hand, he argues – consistent with Davis and many other commentators [Cohen (1995), Hausman (2000), Lewin (1996), Mandler (1999), Rosenberg (1992), Wong (1978), and others] – that reducing rationality to consistency in this way essentially eliminates its ability to explain human behavior: “rationality-as-consistency cannot explain behavior, but just describe it” [Giocoli (2003), p. 110, emphasis in original]. The rise of the consistency approach has forced neoclassical economics to abandon no less than its major theoretical goal, namely, the explanation of the individual’s behavior. From the characterization of rationality as a mere consistency restriction there emerges a purely formal representation of the decision-maker that fits any kind of agent, be ‘it’ human, a group, an institution, or even a computer. (ibid., p. 42)

In addition to this loss of explanatory power, Giocoli also argues that Samuelson’s WARP (and the related literature) fails on its own terms: that it did not provide the observational replacement for utility that its promoters have consistently argued. Giocoli is also not alone in this argument, but he makes the case quite strongly:

The experimental flavor underlying Samuelson’s approach is in fact purely fictional: no real consumer is really required to make choices that can be used to reveal his/her preferences and the whole setup is just an intellectual construction much like Pareto’s “experiments” on indifference curves. The individual guinea-pig mentioned by Samuelson [(1948), p. 243] exists only in the economist’s mind: it is “pure-make-believe,” the “illusions” of an experiment, ... [ibid., p. 110].

Even though I disagree that WARP represented the substantive change that these arguments suggest – or perhaps, as with my remarks about ordinalism, I should say that it is not obvious that it should have been viewed as such a significant change – I do think the critical points made by Giocoli about WARP failing on its own terms are extremely important. Rather than analyzing his argument, my approach in the rest of this section will be to come to similar conclusions from a slightly different direction. My story will begin with a review of the trajectory of WARP from Samuelson (1938a) to the presentation in modern advanced text-



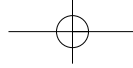
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books [say, Mas-Colell, Whinston, and Green (1995), or McKenzie (2002)].

I have argued elsewhere [Hands (2004)]⁸ that Samuelson's original presentation of the weak axiom in 1938a was actually an attempt to radically change the way that economists thought about consumer behavior: and change it in the behaviorist direction. The stated goal of the original paper was "dropping off the last vestiges of the utility analysis" [(1938s), p. 62]. It is important to note that this original paper was not only strictly behaviorist and eliminativist about utility, but also about "preferences" as well. For example, the term "revealed preference" did not appear in the original paper (Lavoisier didn't talk about "revealed phlogiston") and he judiciously avoided using the term "choice" (which of course has no meaning if one is being strictly behaviorist – winks and blinks are observationally equivalent). In this sense I claim that Samuelson's original WARP was not an instance of GRCT. Despite the frequent rhetoric to the contrary, strict behaviorism is based solely on recording constant conjunctions of observed events preceded by (equally observable) conditioning. Such explanations involve neither well-ordered preferences nor instrumental rationality. I would add that such strict versions of behaviorism have essentially disappeared from the human sciences, but in its original strict form I contend it was not a specialization of GRCT. In this sense, if his approach had actually done what Samuelson originally intended (ironic word under the circumstances) then WARP would have in fact been a very significant change. But that is not at all what happened. By 1948 revealed preference was no longer radical behaviorism, but rather had become just an ostensibly empirical way of revealing preferences; and by 1950 it was re-marketed as attempting to determine "the full empirical implications for demand behavior of the most general ordinal utility analysis" [Samuelson (1950), p. 369]. In Houthakker's apt words: "the stone the builder rejected in 1938 seemed to have become the cornerstone in 1950" [Houthakker (1983), p. 63]. Or as I would put it, it really wasn't the major methodological change it might have been.

Despite these remarks about the weak axiom's transformation from radical behaviorism to reassuring mainstream practice, it is nonetheless the case that many insist it was the key move in rebuilding the theory of consumer behavior on purely consistency or choice-based (rather than preference-based) foundations, and thus, the usual story goes, rendering

8. As have Cohen (1995), Hausman (2000), Lewin (1996), Rosenberg (1992), Wong (1978) and others.

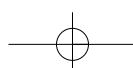
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it more observable and scientific. This is the story Amadae tells; it is Davis's story except that it ends up being a bad thing from the viewpoint of individual identity; and it is also Giocoli's story (except that the resulting theory was devoid of explanatory power). This suggests that even if the weak axiom did not reconstruct the theory of consumer behavior on totally new behavioristic foundations, it was still significant in that it caused a substantive change in the dominant way of thinking about consumer choice and moved it in a more observational (and thus less introspective) direction. I disagree, but in order to understand how people make the case that it was, it is necessary to look at the revealed preference literature that followed Samuelson's original paper and narrative that eventually came to surround those moves.

The argument that revealed preference theory is more observational than ordinal utility theory draws on the simple definition of WARP given above. It is argued that one can observe such choices and then empirically reconstruct the agent's preferences, but in fact there are serious (actually prohibitive) practical problems associated with trying to implement such a "theory" for deriving preferences. Since the standard choice space is X , one would need to test an infinite number of points (actually there are an infinite number of bundles in any subset of the choice space); since the "observations" take place over time, one has no way to differentiate between inconsistency, changes in taste, and changes in other factors. One would need to check an infinite number of bundles without having any (observational) way to know that the agent's preferences were not changing during the course of such "revealing." This implementation failure is a serious methodological problem, but it is also well-documented⁹ and not the issue that I want to emphasize. An equally important, but more historical point, is that discovering preferences was never what revealed preference theory was about.

From Samuelson's original presentation, through the important technical results in Uzawa (1960), Richter (1966), and Kihlstrom, Mas-Colell, and Sonnenschein (1976), to the expositions in modern advanced textbooks, WARP has always been about generating certain standard results of ordinal utility theory – primarily the negative semi-definiteness of the Slutsky substitution matrix – from an alternative set of assumptions involving the WARP condition. The significant contribution of Houthakker's (1950) modified version of WARP – the strong axiom of revealed preference

9. See the references in the previous note.

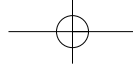


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rence (SARP) – was that it allowed one deduce the symmetry of the Slutsky substitution matrix as well. The problem for those who insist on the empirical or observational nature of WARP – and as I have noted, this is almost everyone who writes about it – is that the “theory” is not the WARP condition alone; the theory involves this condition, but it also involves a large number of other assumptions that as an ensemble imply the desired negative semi-definiteness of the Slutsky matrix.

In Samuelson’s original paper he assumed, along with WARP, that: a) the consumer had demand functions $x_i(p, M)$ for all n goods, b) that these demand functions were single valued, c) they satisfied the standard budget constraint, d) that they were differentiable, e) that each demand function was homogenous of degree zero in all prices and money income, and f) that the demand functions were invertible (and this would need to be global invertibility since the results were supposed to hold for the entire choice space). In the addendum (1938b) he dropped the separate assumptions of single-valuedness and zero degree homogeneity (by demonstrating that they followed from the other assumptions of the model). Now it seems clear that having a continuous demand function for all (and there are of course infinitely many) bundles of goods in \mathcal{R}_+^n , is no more obvious than having a utility function for those same commodities; at least a utility function is only one function of n variables, WARP requires n functions of $n+1$ variables. And more importantly, even if one could make the case that they “have” such a system of demand functions, those functions would be no easier for the observing economic scientist to get at, no more empirical or observable, than individual utility or preferences. Modern textbooks can employ more sophisticated mathematics and get rid of some of the assumptions that Samuelson originally needed (like invertibility) – for example McKenzie [(2002), p. 22] requires only the existence of continuous demand functions and the budget constraint (or homogeneity) – but again, and always, the demand functions are assumed to exist and have nice mathematical properties. There is no obvious reason why such a theory should be considered to be more observable, empirical, or reduce choice to mere consistency.

The issue is a relatively simple application of the so-called Duhem-Quine underdetermination problem from the philosophy of science. CCT and demand theory based on WARP are test systems not single assumptions or theories. Given all the parts one can deduce various implications believed to be observable (like the semi-definiteness of the Slutsky matrix). Putting aside both the problem of trying to render the WARP condition practically observational, and the question of the observability of Slutsky



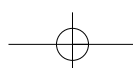
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definiteness or symmetry, one still has a test system, not a single assumption. In isolation, and here I am getting to the main point, WARP may look (with all the previous caveats) more observational than an ordinal utility function, but the only way that one gets (again with all the previous caveats) empirical implications is to combine it with a host of other assumptions – particularly the existence of a system of mathematically well-behaved demand functions – that are not any more observationally palatable than the various parts of the CCT system that revealed preference was supposed to replace. As Giocoli says, the purely observational claim is “fictional,” but not because the relevant theorists have lied about the implications of the theory. Rather it is for more Kuhnian reasons, that they only see the distinction between observational and nonobservational from inside the theory itself. The same theory, by the way, that has filtered the way that the majority of the profession have “seen” such things since early in the 20th century. Thus, as I said, WARP is not a significant change.

V. Some Parting Thoughts

Suppose that one accepts my, or even most of my, critical remarks in the previous section: What are the implications for the history of economic thought in general, or the history of demand theory in particular? First, let me be clear what point I am not trying to make. I am not suggesting that these three works are not important contributions to the history and philosophy of economics, or even that I disagree with their main story lines. The difficulties I have all concern the particular spin(s) they put on the history of demand theory, and not with their central theses. In fact, I believe the central thesis of all three works is consistent with an alternative history of demand theory that has the features I have emphasized. Let me briefly discuss each book.

Amadae focuses primarily on political philosophy, political science, and the relationship between rational choice theory and developments in these fields. She argues not only that Cold War strategic concerns conditioned the development of rational choice theory, but that the rational choice way of thinking played a key role in rebuilding the intellectual infrastructure of liberal political philosophy: “From the closing days of World War II to the mid-1980s, rational choice theory rebuilt the conceptual cornerstones of Western ideas” [Amadae (2003), p. 3]. This central thesis seems to me to be correct and deserves much more recognition

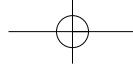


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than it has received. This said, I also do not see any reason why rational choice theory needs to be presented as something that is completely incommensurable with consumer choice theory in order to make the case for the philosophical impact of rational choice theory. I do not see why one could not characterize various specific applications of rational choice theory, and various economic theories (including CCT), as simply particular special cases of GRCT and still say exactly the same things about the social, political, philosophical, and restabilizing role of rational choice theory that Amadae lays out so eloquently. I think that such an interpretation would actually strengthen the overall case.

Davis's main argument about mainstream economics (he also has much to say about heterodox theory) is that despite all the rhetoric of individualism – methodological, political, cognitive, and otherwise – modern mainstream economics really does not have any theory of individual identity. Early neoclassicals had flawed theories of identity, the contemporary mainstream simply has none: “the individual, which economics has long claimed to be successful in explaining, turns out to be something of a black hole in the subject” [Davis (2003), p. 187]. This also seems to be correct and it is also a point that is not nearly as appreciated as it should be, but I also think it is a story that would sit comfortably with the less abrupt-break-oriented narrative I have suggested. First, the claim that the subjective individual was antithetical to (even early) neoclassicism's algorithmic concept of rationality only strengthens the indictment of mainstream economics (i.e., despite numerous claims to the contrary they never had a theory of individual identity suitable for flesh-and-blood humans). Second, there are various places – not everywhere, but places – where Davis's words seem to suggest that the two big moves in the history demand theory were in fact associated with ever-increasing observability (rather than just the rhetoric of increased observability). This seems to set up a tension between being scientific on one hand, and having an adequate theory of individual identification (and reidentification) on the other. It gives the defender of mainstream practice the epistemological high ground: “sure we lost the individual, but we gained science.” On my reading, they lost the individual without necessarily providing any compensating gain. Again, I think my arguments would actually strengthen Davis's overall case (at least the part that is critical of the mainstream).

Finally, recall that one of Giocoli's main goals is to present the history of the formalist revolution in a way that helps explain, among other things, the delayed acceptance of game theory. As discussed above, he employs

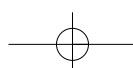


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the distinction between SOF and SOR throughout the book. He argues that the movement from early neoclassicism to Arrow-Debreu represents the victory of SOR over SOF, and that this in turn explains both the profession's original, and its eventual, response to game theory: "In a nutshell, I maintain that the routes of game theory and neoclassical economics could converge only after the transformation that turned the latter's image from the traditional SOF to the contemporary SOR one was completed" [Giocoli (2003), p. 346]. Although Giocoli presents the same SC_1 and SC_2 stage theory of the other two authors, he is explicitly critical about the epistemic progress embodied in these moves. He basically accepts the received view of the history – what each theoretical move "did" – but offers a methodological critique of the theory that eventually emerged from this historical process. My concern is more historical and less methodological. My point is not that mature demand theory fails to live up to some particular methodological standard (though it may), but rather that the received view of the stages the theory passed through in reaching maturity cannot be sustained by the historical record. If there is a methodological critique in my argument, it is internal – that various theoretical moves simply did not "do" what the standard story says they did. There are places where Giocoli makes similar historical points, but his main thrust is the (critical) methodological evaluation of the finished product. These are significant differences, but I do not see why any of his main points, about game theory in particular, would suffer from any of the alternative interpretations I have presented.

So, many of the points that I have emphasized regarding the history of demand theory appear to be consistent with the main story line of all three authors, but this still does say much about my motivations for making the various points I made: about ordinalism, WARP, or whatever. What then is the general point of my – now rather extensive – remarks? Why am I so concerned, and what exactly is it that I am so concerned about?

I think my main concern, or perhaps the main reason I am so concerned, is historiographical. It involves issues I have regarding the general relationship between the narratives provided by those producing dedicated historical works on modern theoretical economics, and the narratives of the mainstream itself: the self-narratives of the relevant theorists, displayed in the opening chapters of textbooks, or residing in the disciplinary conventional wisdom. My presupposition has generally been that as the thickness, and general historical sophistication, of dedicated historical work increases, the result would be the production of detailed histo-





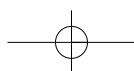
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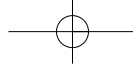
rical studies that increasingly diverge from the mini-histories offered by the economics profession. This seems to me to be a rather obvious (and frankly not very philosophically deep) point about what happens when the pace and seriousness of historical research is accelerated. If we suddenly had a flurry of historical research on the medieval catholic church, one would expect the research to uncover a far different set of events, interests, and causes, than what had previously been available in the official church histories. One would expect historians' history to be different than the winning-party's history: and the deeper the research the bigger the difference. And yet this is not what I find here. It seems to me that all three authors – at least to some degree and on the specific topic of demand theory – tell a story that is essentially the same as the mainstream's own narrative. Even Davis and Giocoli, who are quite critical of where the theory ended up, essentially accept the standard story about how it got there. It is that element, I think, that troubles me.

Now of course it is possible that my concerns are misplaced. They could be misplaced because the endorsing tone I seem to hear in these texts simply isn't there (i.e. the problem is my reading, not their texts). Or perhaps my concerns are misplaced, not because the arguments that concern me are not there, but rather because this is not something that dedicated historians should properly be concerned about. Since it seems doubtful that I will settle such grand queries in the few paragraphs that remain, let me just try to accomplish two things in the last few pages. First, I will try to persuade the reader that despite the impressive historical and philosophical scholarship of these works, the main story regarding what happened in 20th century demand theory is basically the standard textbook story. And second, let me try to say a little more on why I think that is problematic.

Since "this is basically the standard story" is a fairly serious accusation, it is useful to begin with a long quote from an advanced textbook. Consider the following, which appears on the first page of Chapter 1 – Preference and Choice – of Mas-Colell, Whinston, and Green's massive and highly respected *Microeconomic Theory* (1995). Here they are comparing the "more traditional" preference approach (and the associated ordinal utility theory) with the choice-based approach (based on the WARP) they favor.

There are two distinct approaches to modeling individual choice n maker's tastes, as summarized in her preference relation, as the primitive characteristic of the individual. The theory is developed by first imposing rationality axioms on the decision maker's





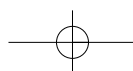
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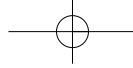
preferences and then analyzing the consequences of these preferences for her choice behavior (i.e., on decisions made). This preference-based approach is the more traditional of the two, and it is the one that we emphasize throughout the book.

The second approach, which we develop in Section 1.C, treats the individual's choice behavior as the primitive feature and proceeds by making assumptions directly concerning this behavior. A central assumption in this approach, the weak axiom of revealed preference, imposes an element of consistency on choice behavior, in a sense paralleling the rationality assumptions of the preference-based approach. This choice-based approach has several attractive features. It leaves room, in principle, for more general forms of individual behavior than is possible with the preference-based approach. It also makes assumptions about objects that are directly observable (choice behavior), rather than about things that are not (preferences). Perhaps most importantly, it makes clear that the theory of individual decision making need not be based on a process of introspection but can be given an entirely behavioral foundation. [MasCollel, Whinston, and Green (1995), p. 5].

Let's take stock here. How does this differ from the interpretation provided in the three texts under discussion? It seems very little. Let me just list the various places where the position expressed in the quote is consistent with the historical reconstructions of Amadae, Davis, and Giocoli. 1) there are two different approaches – preference-based and choice-based – not merely different instantiations of the same GRCT. 2) the choice-based approach concerns the “consistency” of choices (and apparently the preference-based approach is not). 3) The choice-based form allows for “more general forms of individual behavior.” 4) the choice-WARP-based approach makes assumptions (ostensibly exclusively) about “objects that are directly observable.” 5) the choice-WARP-based view (as opposed to the preference-based view) “need not be based on a process of introspection.” and finally 6) that WARP provides an “entirely behavioral foundation” for demand theory.¹⁰

10. Claims 4), 5) and 6) are all related, but are in fact different. 4) is about what WARP-based theories do, and by implication what preference-based theories do not do; 5) is about what WARP-based theories do not do, and by implication what preference-based theories do; and 6) is a statement about philosophy of psychology – in particular – what constitutes sufficient conditions for “an entirely behavioral foundation” for a theory of individual behavior. One could accept (or reject) any one of these claims without necessarily accepting (or rejecting) any of the others.

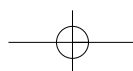


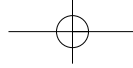


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Now to be fair, it is clear that not everything in this quote receives equal support from all three of these interpretations of demand theory. Most obviously MasCollé, Whinston, and Green claim that the features of the choice-based approach make it more “attractive” than the preference based approach. Certainly Davis and Giocoli make no such claim, and in fact argue to the contrary. For both of them, the move toward a choice-based approach has contributed to the mainstream’s current difficulties; for Davis because they have entirely lost any concept of individual identity and for Giocoli because it was the final step in the movement toward a purely formal (SOR) view of human action. For Amadae it is not entirely clear; the move to a choice-based theory is fundamental to her story, but it is less clear how she would appraise the attractiveness of that move. I would also note that Amadae and Davis at various points seem to be conflicted about 4), 5), and 6). In places they sound like they are only making the case that various economists endorsed WARP because those theorists considered it to be “directly observable,” less “introspective,” or rested on more solid behavioral grounds – a position that seems to me to be unassailable – while in other places they could be read as defending or endorsing these various epistemic claims about WARP. Giocoli is not conflicted in this sense; 4), 5), and 6) are all things that mainstream theorists say (and evidently believe), but upon closer examination are just “fictions.” Although the interpretations of the three authors do differ in some ways from such textbook renditions, there is also a rather substantive overlap. There are two distinct approaches (not just different specializations of GRCT); the consistency approach is substantively different (than CCT); the choice-based view allows for a wider range of potential applications (not just demand theory); and WARP is observable, non-introspective, and behaviorist (or at least more so than its predecessors). It has been a long paper and I will not repeat my arguments here, but I have tried to demonstrate in the previous pages that the historical record does not support any of the claims 1) → 6), and I was particularly strident in my criticism of 1), 2) and 4).

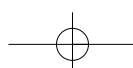
Let me close by just trying to be a little more clear about the motivations behind my concerns. In other words, trying to at least partially answer the sympathetic critic who says “Okay, I agree with most of your remarks, but still don’t understand why you think these issues are so important.” I will make two quick points. First, I think that such narratives put a tight linear structure on the history (in this case of demand theory) that not only isn’t accurate, it actually suppresses a very important feature of the mainstream’s success: its ambiguity. As Philip Mirowski and I have argued

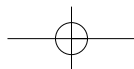


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[Hands and Mirowski (1998), Mirowski and Hands (1998)] one way to understand the strength of mainstream economics is to realize that it is never a single thing; it is a tightly woven skein of different ideas that actually gives it greater strength and resiliency than could be provided by any single strand. It is not that first demand theory was psychologically hedonist, then second it was ordinalist, then finally it became choice-based; it is hedonist, and ordinalist, and choice-based all at the same time. Listen to how economists talk about demand curves – listen to how economists teach about demand curves – they are based on individuals seeking satisfaction from the consumption of goods; it does not require us to actually “know the numbers in their heads” or make interpersonal utility comparisons; and of course it could all be reduced to observing the actual choices of real consumers. Subjective sensations, ordinal preferences, and observational consistency of choice are not distinct features endemic to particular stages in history of demand theory; they are interwoven aspects of the theory that serve it effectively in various contexts (and with various audiences). The period-by-distinct-period story suppresses (and by doing so reinforces) the mainstream’s greatest strength. Tell it like it is: ambiguous, fraught with tensions, and yet enormously successful.

Second, I would like our histories to be sensitive to the nuances of culturally conditioned selective significance. By this I mean the feature of cultural communities – not just economists of course, but that is the one we are talking about here – to elevate and obsess about some issues, while at the same time being in complete denial about other things that from any reasonable outsider’s point of view are quite serious and important. This is essentially a pitch for the history of economic thought with an anthropologist’s, or perhaps a Veblenian, eye. How can economic theorists be so obsessed about the mathematical details – upper semi-continuous correspondences versus upper hemi-continuous correspondences for example – and do all that close-work so very very well, and then a few sentences later, say things about observability that would make a first year philosophy student howl? How did all of the non-equivalencies I listed at the end of section 3.B come to be seen as equivalencies? These things can be explained – they are selected for and survive in the particular culture of academic economics for reasons – but in order for those reasons to be understood they need to be problematized, and they can only be problematized if they are presented as something other than obvious or inevitable. As Shapin and Shaffer say in the opening epigraph, in order to problematize “what everybody knows” requires that we “adopt



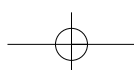


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a calculated and informed suspension of our taken-for-granted perceptions of ... practice and its products." The job (or one of the jobs) of disciplinary history is to facilitate and provide space for such "informed suspension of our taken-for-granted perceptions." I believe these three authors have accomplished quite a lot in this regard, but I also think there is more to do.

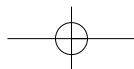
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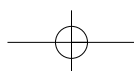
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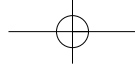
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Abstract

Recent histories of 20th century economics have emphasized the transformation of demand theory that occurred during the period between early neoclassicism and Arrow-Debreu. This paper examines three contributions to this recent literature – Amadae (2003), Davis (2003), and Giocoli (2003) – and offers an alternative interpretation of the theoretical developments that occurred during the so-called ordinalist and revealed preference revolutions. The central thesis is that while this recent research constitutes a significant step forward in our historical understanding, there are some common, and I think problematic, aspects to all three narratives.

Key words

Résumé

Mots-clés
Classification JEL :

