

Foundations of Contemporary Revealed Preference Theory\*

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*I imagine that you are right that many individuals looking at this paper will be induced to believe that there is after all very little, and very little of interest, in the modern theory of consumer's behavior. However, if this is indeed the truth, we should not try to keep it a secret. By all means let us make clear how little and how much the existing theories of economics contain.*

(Paul Samuelson to Hendrik Houthakker July 31, 1952<sup>1</sup>)

## 0. Introduction

Economists predict, explain, and generally understand consumer behavior in terms of a particular version of rational choice theory: utility maximization subject to a budget constraint. In the standard case of risk-free consumer choice, the traditional theoretical framework is ordinal utility theory (hereafter OUT), the utility theory associated with the work of John Hicks and R. D. G. Allen (1934), Vilfredo Pareto (1927), Eugene Slutsky (1919), and others early in the twentieth century and remains the core theory of individual choice in modern economics: in research, in textbooks, and in the way that practicing economists think about consumer behavior.<sup>2</sup>

Another approach to consumer choice is revealed preference theory (hereafter RPT). RPT is based on the idea that consistency of choice – if a consumer chooses bundle A when B is affordable (they have "revealed" that A is preferred to B), they will never purchase B when A is affordable – is all that is required for the analysis of consumer choice. The relationship between OUT and RPT has never been entirely clear. On one hand, the revealed preference literature began with Paul Samuelson's 1938 paper – a paper that was designed to eliminate the "last vestiges of the utility analysis" (Samuelson, 1938, p. 62) from consumer choice theory – and yet by the 1950s most economists considered RPT to be just an alternative characterization of, not a replacement for, traditional OUT (Samuelson 1948, 1950). As Hendrik Houthakker, one of the key contributors to the revealed preference literature, once put it, "the stone the builder rejected in 1938 seemed to have become the cornerstone in 1950" (Houthakker, 1983, p. 63). Later contributions by Sidney Afriat (1967) and others allowed RPT to be applied to finite choice data and set

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<sup>1</sup> [Box 38] Paul Samuelson Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University.

<sup>2</sup> Throughout this paper the term rational choice will be used for the general theoretical framework and particular choice theories – such as OUT or expected utility theory – will be viewed as particular instantiations of that general theoretical framework.

the stage for the most recent version of revealed preference theory – contemporary revealed preference theory (hereafter CRPT) – which has raised even more questions about the relationship between RPT and OUT.

The purpose of this paper is to examine some of the philosophical and methodological issues associated with RPT. The paper will make three separate, but interconnected, arguments. First, it will be argued that RPT is not simply a theory, but rather a broad research program within choice theory. This is important because failure to adequately recognize this diversity has hampered the evaluation of RPT. The second part of the paper discusses a few of the previous philosophical criticisms of RPT. This discussion is not intended to be exhaustive; it will discuss arguments of two well-known critics – Daniel Hausman and Amartya Sen – and focus on explaining why some of their criticisms seem to be less effective against CRPT than against earlier versions of revealed preference. Finally, three additional criticisms of CRPT will be offered that differentiate it from traditional versions of RPT.

## 1. Revealed Preference Theory: A Family History

### 1a. Traditional Revealed Preference Theory

RPT is not simply a theory. It is a broad research program in the theory of consumer choice.<sup>3</sup> The revealed preference research program can be thought of as an extended theoretical family – a family containing various family members with different conceptual insights, theoretical structures, and paradigmatic applications – but all bearing a relatively strong family resemblance: a resemblance that differentiates them from other frameworks for predicting and explaining individual behavior (from both inside and outside of economics). While the revealed preference family tree has grown and branched widely during the seventy plus years since Samuelson's original paper, the main focus here will be on the two main trunks: traditional revealed preference theory (hereafter TRPT) a relatively abstract theoretical literature with origins in papers by Samuelson and Houthakker in the 1950s, and contemporary revealed preference theory (CRPT), a more empirical branch that initially sprouted from Afriat's 1967 paper, but during the last few years has expanded into a more general methodological program. Each of these will be discussed in turn, but the principle focus of the paper is CRPT.

The origin of the revealed preference family is Samuelson's 1938 paper "A Note on the Pure Theory of Consumer's Behaviour," a paper with a

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<sup>3</sup> There exists a more abstract set-theoretic version of RPT (Arrow 1959), but the discussion here will focus exclusively on RPT as a theory of consumer choice.

clearly stated programmatic purpose. One of the goals of the ordinal revolution during the early 1930s was to move away from the hedonistic and cardinal notions of utility that the first generation of neoclassical economists had inherited from utilitarianism, and to move in the direction of the type of observable empirical evidence – objective, not introspective, evidence – consistent with the positivist philosophical ideas of the day. This has been called the "escape from psychology" (Giocoli, 2003) and Samuelson's goal was to put even more distance between consumer choice theory and its hedonistic roots. The early ordinalists had eliminated hedonism and cardinal utility, but still employed terms like "preference" and "utility"; Samuelson's goal was to eliminate such theoretical terms altogether: "despite the fact that the notion of utility has been repudiated or ignored by modern theory, it is clear that much of even the most modern analysis shows vestigial traces of the utility concept ... I propose, therefore, that we start anew in direct attack upon the problem, dropping off the last vestiges of the utility analysis" (1938, p. 61).

Samuelson's approach, which later came to be called the weak axiom of revealed preference (WARP), was to specify consistency restrictions on the prices and the quantities of the goods the consumer purchased at those prices. If the consumer chose bundle  $x^0$  at prices  $p^0$  even though bundle  $x^1$  was affordable, then consistent behavior would imply that if bundle  $x^1$  were chosen at prices  $p^1$  it was because  $x^0$  was not affordable at  $p^1$ . In other words:

$$p_i^0 x_i^1 > p_i^1 x_i^0 \quad (\text{WARP})$$

Notice that WARP provides a test for the consistency of the consumer's choices based solely on (potentially observable) prices and quantities, and it is not necessary to introduce utility or preference into the analysis at all: and in the 1938 paper, Samuelson didn't. The term "revealed preference" was not used in the original paper. The point was to eliminate the scientifically problematic concepts of preference and utility, not to reveal them.

An important point to make about the 1938 paper – a point useful for discriminating among various members of the revealed preference family – is that the primitives for Samuelson's new theory were individual demand functions in general form. Unlike the ordinalists he did not assume that agents possessed well-behaved utility functions, but neither did he start with data about purchased prices and quantities. He simply assumed the consumer's demand  $x_i$  for each of the  $n$  goods,  $i = 1, 2, \dots, n$ ,

was given by a continuous function of the prices  $p_1, p_2, \dots, p_n$  and the consumer's available money income  $M$ :

$$x_i \quad \text{for all } i = 1, 2, \dots, n.$$

In the original paper these  $n$  "known" (i.e. assumed to be potentially observable) demand functions were also assumed to satisfy three additional conditions – zero degree homogeneity in prices and income ( $f_i(\lambda p, \lambda M)$  for all  $\lambda > 0$  and all  $i$ ), the consumer's budget constraint ( $\sum p_i x_i = M$ ), and a non-vanishing Jacobian matrix (an invertibility condition) – although he later demonstrated that some of these assumptions were implied by WARP. The point is that Samuelson's paper introduced revealed preference theory as an abstract "demand function based" approach to consumer choice theory, to replace the "utility function or preference based" approach of earlier neoclassical economists (both those in the cardinal-hedonist tradition and the ordinal utility theorists).

Although Samuelson started with demand functions, rather than utility functions, his theory was concerned with the same set of restrictions on these functions that concerned OUT. These restrictions were: i) Negative Slutsky Substitution Term ( $S_{ii} < 0$  for all  $i$ ), ii) Negative Semi-Definiteness of the Slutsky Substitution Matrix ( $x^T[S]x \leq 0$  for all  $x \neq 0$ ), and iii)

Slutsky Symmetry ( $S_{ij} = S_{ji}$  for all  $i$  and  $j$ ), where  $S_{ij}$  and  $S$

is the  $n \times n$  matrix with representative element  $S_{ij}$ .<sup>4</sup>

It is clear that Samuelson's goal in 1938 was neither the prediction of novel facts nor the development of a new tool for empirical research in consumer choice; it was foundational – to provide more adequate epistemological foundations for the existing restrictions on individual demand functions by eliminating any reference to utility or preference and the associated unobservable mental states.

Samuelson's original 1938 paper only derived two of these three conditions – WARP was not sufficient for iii) – but the absence of Slutsky symmetry was not a problem for Samuelson's approach. Condition iii) is an integrability condition that guarantees the existence of an underlying utility function,<sup>5</sup> and since Samuelson's goal was to eliminate utility from the theory of consumer choice, the absence of such integrability (and the

<sup>4</sup> See the consumer choice chapter of any advanced microeconomics textbook, for example Mas-Colell, Whinston, and Green (1995).

<sup>5</sup> See Hands (2006) and (2011) for detailed discussion of the integrability literature of this period.

associated utility function) was quite consistent with his approach. As Samuelson himself said about integrability: "I cannot see that it is really an important problem, particularly if we are willing to dispense with the utility concept, and its vestigial remnants" (Samuelson, 1938, p. 68).

The next step in the development of the revealed preference research program was Hendrik Houthakker's strong axiom of revealed preference (SARP) in 1950. Houthakker basically strengthened the WARP condition by extending it from pair-wise choices to chains of choices, and in so doing was able to add the third restriction, Slutsky symmetry, to the list of RPT's implications. Of course with the addition of the strong axiom, the circle connecting RPT and OUT was closed. Since revealed preference theory now implied exactly the same restrictions on demand functions as OUT, the two theories were equivalent. One could start with revealed preference restrictions on demand functions, or one could start by assuming that the consumer was maximizing a well-behaved ordinal utility function subject to a budget constraint, and one would end up with exactly the same set of restrictions on demand functions: "The 'revealed preference' and 'utility function' ... approaches to the theory of consumer's behaviour are therefore formally the same" (Houthakker, 1950, p. 173).

The strong axiom ushered in what might be called the "high theory" period of revealed preference. The next few decades produced an extensive technical literature that extended the WARP and SARP approach to consumer choice theory in various ways and connected it up with existing literature on mathematical general equilibrium theory: see for example Richter (1966), Kihlstrom, Mas-Colell, and Sonnenschein (1976), and a number of the papers in Chipman, Hurwicz, Richter, and Sonnenschein (1971). This high theory literature was the dominant version of revealed preference theory for much of the second half of the twentieth century. There were of course other members of the revealed preference family – the aggregate excess demand function version (Arrow, Block, and Hurwicz 1959), the abstract choice function approach (note 3), a stochastic version, and others – but at least until quite recently, the majority of economics papers with "revealed preference" in the title as well as the revealed preference section of microeconomics textbooks concerned this particular version of RPT. Questions have been raised about whether Samuelson changed his mind between the original 1938 paper and his later papers such as Samuelson (1948, 1950),<sup>6</sup> but those issues need not concern us here. For the purposes here we can think of all of this individual demand function-based literature – Samuelson, Houthakker, and high theory – as TRPT: initially the main trunk of the

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<sup>6</sup> By Wong (2006) and others.

revealed preference family tree, but later becoming just one of its two main branches.

### 1b. Contemporary Revealed Preference Theory

Although the motivations for TRPT are relatively straightforward, it does seem a bit strange to call it "revealed preference," since no "revealing" of preferences appears to be involved. It was a series of interrelated theoretical results that used consistency restrictions on abstract individual demand functions (WARP, SARP, and variations) to derive other restrictions on those demand functions (Slutsky symmetry, etc.), and not a set of results that offered any obvious way of "revealing" the consumer's preferences or utility function. Of course, if a set of demand functions satisfies SARP, then, because of the Slutsky symmetry condition (integrability), the existence of a rationalizing utility function is guaranteed: there always exists a utility function that if maximized subject to a budget constraint would generate the same behavior (i.e. those same demand functions).<sup>7</sup> SARP guarantees that the relevant behavior (demand) can be rationalized in terms of budget-constrained utility maximization – that the restrictions on the consumer's demand functions are the same as they would be if they were maximizing an ordinal utility function – but TRPT does not provide any direct bridge to finding such a utility function, or for that matter, any obvious reason why the approach should be called revealed preference.

The next development in RPT was a significant change that led the research program in a more applied and constructive direction. The key research was Afriat's paper 1967 "The Construction of Utility Functions From Expenditure Data," although Samuelson had, to some extent, pointed the way in a paper published in 1948. What Samuelson demonstrated in 1948 was that in the case of only two goods, WARP could be used to derive the consumer's indifference curves, and since indifference curves contain all the information necessary for an ordinal utility function, the result opened a pathway between revealed preference and the construction of a utility function. Unfortunately Samuelson's results were restricted to two dimensions and were not presented in a very useful form.

Afriat's paper changed this situation. He started with finite choice data – prices ( $p_i$ 's) and the quantities of the goods purchased ( $x_i$ 's) – and proved that under a version of the revealed preference hypothesis that later came to be called the "generalized axiom of revealed preference" (GARP),

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<sup>7</sup> It is always "a" utility function, not "the" utility function, since any monotonic transformation of a utility function generates the same demand functions (i.e. if  $U(x)$  is a utility function that would generate a particular set of demand functions, then  $V(x) = F[U(x)]$  with  $F' > 0$  would generate the same demands).

a particular system of inequalities would always have a solution. Since an appropriate rationalizing utility function can be constructed from the solution to this set of inequalities, Afriat's result provided a constructive link between revealed preference and the associated utility function. As Afriat explained, the results of TRPT had been obtained under the assumption of "complete data": "a demand system and, therefore, quantities for every price situation" (Afriat, 1967, p. 68). But of course economists seldom have complete data in this sense. The applied economist generally has only finite choice data rather than functions defined over all possible prices and income, and finite data was precisely Afriat's starting point. As Hal Varian explains Afriat's contribution:

Most of the theoretical work ... starts with a demand function: a complete description of what would be chosen at any possible budget. Afriat (1967) offered quite a different approach to revealed preference theory. He started with a finite set of observed prices and choices and asked how to actually construct a utility function that would be consistent with these choices ... Afriat's approach, by contrast, was truly constructive, offering an explicit algorithm to calculate a utility function consistent with the finite amount of data, whereas the other arguments were just existence proofs. This makes Afriat's approach more suitable as a basis for empirical analysis" (Varian, 2006, p. 101)

Afriat's theorem thus facilitated the development of GARP-based revealed preference theory that, unlike the results of TRPT, started with finite choice data and provided a way to actually construct a utility function (that SARP guarantees exists). Afriat's initial result was extended and refined in later work – e.g. Diewert (1973); Foster, Scarf, and Todd (2004) – and this has in turn led to the development of a "non-parametric" alternative to econometrics-based demand analysis, along with a number of practical techniques for computing goodness-of-fit measures for data that are "almost" GARP-consistent (Varian 1982, 1985; Bronaus 1987; Gross 1995; Blundell 2005; and others).<sup>8</sup> Since it will be necessary to refer to this literature repeatedly in what follows, it is useful to give it a name; I will use the term "empirical revealed preference theory" (hereafter ERPT) for this empirical, generally GARP-based, literature.

Although various versions of RPT have been introduced, there is still one member of the family yet to discuss, and it is the main focus of this paper. It is the most recent version of RPT: "Contemporary Revealed

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<sup>8</sup> This literature is discussed in detail in Varian (2006); Moscati and Tubaro (2011) survey some of the GARP-based empirical literature, while Moscati (2007) provides the most complete historical discussion of empirical work on revealed preference theory prior to 1970.



Preference Theory" (CPRT). In many ways CRPT is a direct extension of ERPT, but it expands the scope of this work in a variety of ways. The ERPT literature, particularly in the form of non-parametric empirical demand analysis, has made an important contribution to applied microeconomics, but it remains fairly humble in its methodological aspirations. CRPT is quite different in this respect. It starts with the same tools and empirical impulse as ERPT, but adds some quite general claims about how economic science is, can, and must be, done.

Since the existing literature does not distinguish between CRPT and any other version of revealed preference, it is important to be clear about the research that I consider to be representative of this approach. The work that is most quoted in the following discussion is Bernheim & Rangel (2008), Binmore (2009a, 2009b) and Gul & Pesendorfer (2008); Douglas Bernheim & Antonio Rangel are quoted primarily because of the clarity of their statements, while Kenneth Binmore and Faruk Gul & Wolfgang Pesendorfer are used because of their explicit methodological focus. It is important to note that while I distinguish between CRPT and other forms of revealed preference, the authors I cite would not consider their approach to be a separate member of the family. For them, their research is simply revealed preference – the only revealed preference – and many consider CRPT to be exhaustive of choice theory (and perhaps even all of microeconomics). For example, Bernheim and Rangel (2008) call CRPT "standard economics" (p. 163) and "positive economic analysis" (p. 159), Gul and Pesendorfer say it is "very similar to what can be found in a standard graduate textbook" (2008, p. 7), and Binmore claims it is the "official doctrine of neoclassical economics, enshrined in all respectable textbooks" (2009, p. 20).

So what is CRPT theory and how is it different from the other versions of revealed preference discussed above? I will discuss three main features of CRPT: (i) like ERPT, it is a finite domain RPT that uses consistent patterns identified within initial set of choice data to predict choice behavior for out of sample data; (ii) it defines preference solely in terms of choice (i.e. behavior) and denies that the theory offers, or that any scientific theory of consumer choice can or should offer, a causal explanation of consumer behavior (the causal utility fallacy), and (iii) it is methodologically imperialistic – it advocates turning ERPT into a general methodological template for all of choice-theoretic economics. Since (i) also characterizes ERPT, while (ii) and (iii) are unique to CRPT, we can define CRPT as ERPT plus the additional philosophical and methodological claims (ii & iii).

With respect to (i), the goal of CRPT is to extend the information contained in a finite set of choice data to other choice sets. As Binmore says, it "assumes that we already know what people choose in some

situations, and uses this data to deduce what they will choose in other situations" (Binmore, 2009a, pp. 8-9). The process works in the following way. The initial set of data is checked for consistency with a version of the revealed preference axiom, and the consistent data is used to estimate an associated utility function. This function is then used to project what a consistent consumer would choose when faced with different parameter values and this is then used for various types of economic analysis. Bernheim and Rangel explain this procedure very clearly (note  $X^D$  is the initial observed choice set and  $X$  is wider set to which it is to be extended):

Usually choice data are not available for all elements of  $X$ , but rather for elements of some restricted set  $X^D$ . The objective of positive economic analysis is to extend the choice correspondence  $C$  from observations on  $X^D$  to the entire set  $X$ . This task is usually accomplished by defining a parameterized set of utility functions (preferences) defined over  $X$ , estimating the utility parameters with choice data for the opportunity sets in  $X^D$ , and using these estimated utility functions to infer choice for opportunity sets in  $X/X^D$  (by maximizing that function for each  $x$ ). (Bernheim and Rangel, 2008, p. 159)

CRPT is thus a framework for making scientific inferences; given a consistent pattern identified within the initial choice data, it makes an inference about the new choices that would be associated with different prices and income (i.e. different decision parameters). It is important to note that this is, according to defenders of CRPT, simply how choice theory in economics is, and must be, done: "A choice theory paper in economics must identify the revealed preference implications of the model presented and describe how revealed preference methods can be used to identify its parameters." (Gul & Pesendorfer, 2008, p. 36, emphasis added).<sup>9</sup>

The second main feature (ii) of CRPT is that the word "prefers" simply means "chooses," and "chooses" simply means "does" (i.e. to behave in a certain way). Preferences are not what causes the consumer to choose particular goods, rather it is the fact that certain goods were chosen (obtained) that makes those goods preferred. Again, Bernheim & Rangel explain this clearly ( $R$  is the revealed preference relation):

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<sup>9</sup> One interesting philosophical question not explored here is the justification of the extension of the patterns revealed in the initial (observed) choice set  $X^D$  onto the wider set  $X$ . It seems that some sort of robustness analysis would be required and it is not clear exactly where that would come from within CRPT (See Woodward 200 and 2006 for a general discussion of robustness and Kuorikoski, Lehtinen, and Marchionni 2010 for a discussion of economics).

Though we often speak as if choices are derived from preferences, the opposite is actually the case. Standard economics makes no assumptions about how choices are actually made; preferences are merely constructs that summarize choices. Accordingly, meaningful assumptions pertain to choices, not to preferences ... Though the terminology suggests a model of decision making in which preferences drive choices, it is important to remember that the standard framework does not embrace that suggestion; instead, R is simply a summary of what the individual chooses in a wide range of situations. (2008, p. 158).

Unlike OUT where the primitives of the theory were indifference curves or some other observables related to utility, or TRRPT where the primitives of the theory are individual demand functions, for CRPT the "primitives of the theory are the choices" (Binmore, 2009, p. 20).

Closely associated with the identity of preference, choice, and behavior is what Binmore calls the "causal utility fallacy." Since the purpose of CRPT is simply to project patterns from an observed set of choices onto previously unexamined parameters, and the preferences used in such projection are simply calibrated redescriptions of previous patterns, then CRPT is in no way a causal or explanatory theory. CRPT-based consumer choice theory "abandons any attempt to explain why people behave as they do" (Binmore, 2009b, p. 542); it offers no explanation of why a consumer chooses one good rather than another and no claim is made that the preferences discussed are causally responsible for the behavior in question. To believe that revealed preference can provide a causal explanation of economic behavior is to fall victim to the causal utility fallacy.

In revealed-preference theory, it isn't true that Pandora chooses b rather than a because the utility of a exceeds the utility of a. This is the Causal Utility Fallacy. It isn't even true that Pandora chooses b rather than a because she prefers b to a. On the contrary, it is because Pandora chooses b rather than a that we say that Pandora prefers b to a, and assign b a larger utility. (Binmore, 2009a, p. 19)

The final (iii) aspect of CRPT was noted earlier; according to defenders, CRPT is not just a useful approach to certain types of applied demand analysis, it is the method, the proper method for scientific economics (at

least choice theory).<sup>10</sup> Sometimes the argument is that CRPT is all that scientific economists actually do; sometimes it is what they must do (in Gul and Pesendorfer because of the available data); sometimes it is what they should do (in Binmore's case to avoid the causal utility fallacy); and sometimes it is all three. In any case it is an imperialistic methodological impulse that is not present in TRPT or in purely applied ERPT.

The next section will discuss some existing methodological and philosophical criticisms of RPT, but before moving on to that topic it is useful to summarize some of the differences between TRPT and CRPT. I will present two sets of differences (a and b). The set a) involves differences between the more theoretical TRPT and the more applied ERPT. Since CRPT employs the same empirical techniques as ERPT, the differences in a) are also differences between TRPT and CRPT. The set b) involves differences between TRPT and CRPT that are associated with CRPT's more philosophical agenda (and as such do not apply to most of the ERPT literature).

a) One significant difference between TRPT and ERPT concerns the domains of the two approaches; Pollak's distinction between "standard domain" and "restricted domain" RPT (Pollak, 1990, p. 146) helps clarify this. TRPT research is "standard domain" because it starts from the standard domain of OUT-based theory – budget sets with an infinite number of price-quantity combinations – while ERPT is "restricted domain" because it starts from an "finite collection of price-quantity pairs  $\{(p^a, x^a), (p^b, x^b), \dots (p^T, x^T)\}$ " (Pollak, 1990, 148).<sup>11</sup> In one respect this is a difference that exists between more abstract/formalized versions of any scientific theory and its empirical applications, but the domain distinction is more than this. It is also important to the different meaning and role of "observation" in the two interpretations. TRPT insists on grounding choice theory in that which is "observational," but like Samuelson in his original paper, observational means "empirically determinable under ideal conditions" (Samuelson, 1938, p. 62) and that is very different from the "actually observed" choice data that forms the empirical basis for ERPT and thus CRPT. A rock on a planet revolving around a star that is barely visible with our strongest telescope, is observable under ideal conditions, but that is very different than saying that what we know about the rock is based on actual observation of it. Like OUT, TRPT was a product of the "potentially observable is the most we can possibly hope for in economics" thinking that dominated

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<sup>10</sup> Defenders of CRPT often say all of "standard economics," but they cannot possibly mean it. I am assuming they mean "choice theory" and not literally everything that goes on in economic science.

<sup>11</sup> This implies that TRPT satisfies OUT's context independence condition while ERPT, and thus CRPT, does not. See Hausman (2012, pp. 16-7) for a discussion of the meaning and importance of the context independence assumption in OUT,

methodological discussion in economics during the 19<sup>th</sup> and early 20<sup>th</sup> century, while ERPT is a product of the computer age with cheap computing and massive amounts of empirical data. This is a serious difference in itself, but it also conditions the way utility is approached in the two different views. TRPT is concerned with the (mere) existence of a rationalizing utility function and its argumentative style is generally proof by contradiction, while ERPT and CRPT are constructive: a utility function exists because we can actually find one. Again, this may be primarily a result of a change in our available instruments and data, but it also makes a significant difference in the character of the knowledge the two versions provide.

Finally, I would like to note a difference that doesn't seem to have much to do with the relevant domain or technological change since the 1950s; it involves the methodological individualism of the two different versions of RPT. The foundational papers on TRPT – Samuelson (1948, 1950) and Houthakker (1950) – were exclusively concerned with individual behavior and individual demand, and this continued to be the case for the vast majority of the high theory literature. Samuelson, in particular, was quite explicit in his rejection of the application of WARP to aggregate excess demand functions (as in Wald 1951),<sup>12</sup> and while other economists did sometimes impose revealed preference restrictions on aggregate excess demand functions (for example Arrow, Block, and Hurwicz 1959) it was almost always the case that the revealed preference restriction was defended, not on the basis of its direct applicability to aggregate demand functions, but rather because it was implied by other restrictions that were thought to be empirically acceptable (such as the gross substitute property). On the other hand, ERPT and CRPT start from price quantity data; it could be data from an individual consumer, but it could also be from a group of consumers, or the entire market, or a single monopsonistic buyer. Choice data is choice data and if it is GARP consistent, it can be used for rationalization and empirical application. Some (Ross 2011 for example) consider this flexibility to be a great advantage over the earlier TRPT, but advantage or not, it is an important difference between the two versions of RPT.

b) My list of differences between TRPT and CRPT that are not relevant to ERPT is much shorter because these differences are the focus of the next two sections. One obvious difference though, is that CRPT tries to offer a broader, and normative, methodological message. This is not the case for

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<sup>12</sup> As Samuelson and his co-authors put it: "Why is this assumption peculiar? Because the demand functions ... are market demand functions, not individual demand functions. 'Rationality' cannot be required of market demand functions because a changes in prices normally change the distribution of income. With a changed income distribution, different 'preferences' will be revealed. In other words, Wald really assumed that there is essentially only one rational consumer" (Dorfman, Samuelson, and Solow, 1958, p. 368). Also see Samuelson (1955, pp. 499-500).

TRPT and it is not the case for the applied ERPT literature. Three other significant differences will be discussed below: TRPT can be consistent with our folk psychological intuitions that preferences cause choices while CRPT explicitly denies this (the causal utility fallacy); CRPT claims to support traditional normative welfare economics but fails to do so; and CRPT also claims to support a normative theory of rationality, including providing tools for the correction of mistakes in rationality, but fails in this regard as well.

## 2. Some Existing Methodological Criticisms of RPT

This section will discuss a few influential criticisms of revealed preference theory – those by the philosopher Daniel Hausman (1992, 2000, 2008,) and the Nobel prize winning economist Amartya Sen (1973, 1980, 1993, 1997). The goal is to situate these criticisms within the context of the different versions of revealed preference presented above. It will be argued that while the criticisms of Hausman and Sen may have been effective against earlier versions of RPT, they are less effective against CRPT. This supports three of the arguments in this paper – that there are different versions of RPT and that methodological analysis needs to be version-sensitive in order to be effective; that CRPT is unique, and in particular, significantly different from TRPT; and finally it opens the door to some additional criticisms of CRPT. The remarks in this section should not be considered a critique of Hausman's and Sen's work on RPT; my remarks in this section should be viewed as setting the stage for a methodological critique of RPT that is in spirit of Hausman and Sen, but more directly targeted at CRPT.

Over the years Hausman has offered a wide range of criticisms of RPT, although here I will focus on just two: that preferences cannot be revealed by choice alone, and that subjective (unlike revealed) preferences cause and rationalize action in a way that is consistent with our traditional folk psychological conceptions of individual choice and human agency. I will discuss each of these in turn.

Suppose you notice someone choosing a can of Coke over a can of Pepsi. If both were available to the individual and the price is the same, you might conclude (by employing a folk version of revealed preference) that the individual preferred Coke to Pepsi, and this information might be used to make predictions about how the individual would respond in future beverage-choice situations. But is merely choosing Coke over Pepsi enough to conclude that the individual prefers Coke to Pepsi, even in this extremely simple case? Don't we also need to know the individual's beliefs – the beliefs relevant to their perception of the choice situation? For example, don't we need to know that the agent believed that Coke was in fact in the Coke can and Pepsi in the Pepsi can

(perhaps it was some deceptive marketing experiment)? Don't we also need to know that the person does not believe the Pepsi is warm, or flat, or poisoned? Perhaps the person does have a preference for Coke over Pepsi, but it is not a robust preference; for instance they prefer Coke in cans but not in bottles. And on and on. The point is that merely choosing A over B does not alone reveal a preference for A over B; the agent's beliefs about other relevant information also matter. Three factors are involved: preferences, beliefs, and choices. Knowing two out of the three can often tell us the third, but having only one – what the person chose – is not alone sufficient to determine either one of the other factors. Hausman has repeatedly raised this criticism of revealed preference theory, as have other philosophers (e.g. Rosenberg 1992). As Hausman explains:

Beliefs mediate the relationship between choices and the preferences with which economists are concerned. Economists can infer preferences from choices or choices from preferences only given premises concerning the agent's beliefs. Different preferences can lead to the same action, depending on what the agent believes. Neither beliefs nor preferences can be identified from choice data without assumptions about the other. (Hausman, 2011, p. 30)<sup>13</sup>

This point seems to be correct if we start from the position that preferences are mental states that cause (along with our beliefs) our actions, such as choosing one good over another. This is of course the standard folk psychological conception of choice behavior: the agent has a well-defined goal/desire and various beliefs about the actions that might achieve that goal and then acts rationally (in an instrumentally rational) way to achieve the goal or fulfill the desire. This is how we explain most human behavior in everyday life and it is also how rational choice theory and utility theory (in all its forms: hedonistic, cardinal, ordinal, expected utility, ...) have traditionally explained individual behavior. The agent has preferences, or a utility function that represents those preferences, and the action – choices – are explained as a result of utility maximization subject to the relevant constraints. In some sense, microeconomics is – or at least has traditionally been – just mathematical folk psychology (Rosenberg 1992). Given this folk psychological interpretation of choice theory – an interpretation by the way that fits the vast majority of the theorizing about individual choice that economists have done since the neoclassical revolution in the 1870s – then Hausman's point seems to be well-taken. Revealed preference

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<sup>13</sup> Hausman (2008, 2011) makes a distinction between "actual" choice and "hypothetical" choice within RPT. This is an important distinction for some debates, but it is not essential to the argument here.

techniques – WARP, SARP, GARP, or whatever – do not allow economists to obtain preferences from choices alone.

Turning to the various versions of RPT discussed above, this would seem to be a difficulty for most of the members of the revealed preference family that are actually concerned with "revealing" an agent's preferences (Samuelson 1948, most of the ERPT literature, etc.). But notice I say most; CRPT seems to dodge this criticism. The entire argument rests on the folk psychological presumption that the behavior we are trying to explain is caused by the mental states – beliefs and desires – of the agent in question, and CRPT makes no such claim. CRPT is not attempting to discover the various mental states that (taken together) cause the behavior in question, and as such is not revealing the (causal) preferences that are familiar from common sense, folk psychology, or the majority of rational choice theorizing. The preferences involved in most of consumer choice theory are supposed to represent the preferences that are behind – and cause – the choices the consumer makes. This is not the case for the preferences involved in CRPT; they simply save (GARP consistent) patterns in the original data and facilitate the projection of the same structural relations onto other sets of parameters. For most choice theory in economics, preference = preference<sub>FP</sub> (where FP indicates folk psychology) and for these preferences, Hausman's point is well-taken; they will not be revealed by choice alone. But preference<sub>CRPT</sub> ≠ preference<sub>FP</sub> and the criticism ends up missing its mark.

A similar argument can be made for another one of Hausman's criticisms: that even if one could use RPT to obtain preferences, those preferences would not be able to provide the scientific predictions and explanations that economists need and want, because what economists need and want are the (intentional, mental state, subjective) causes of the behavior in question. In Hausman's words:

Economists are interested in choices, which are intentional human actions. But this distinction cannot be drawn in terms of revealed preference theory. (Hausman, 1992, p. 22)

The bottom line is that economists generate predictions of choices and give explanations of choice by deriving choices ... from preferences and beliefs. Subjective preferences combine with beliefs to cause actions. Revealed preferences do not. (Hausman, 2008, p. 138)

... economists cannot function without a subjective notion of preference, which does not and cannot stand in any one-to-one relationship with choices. Once economists are convinced of this conclusion, they will have no reason to



speak of "revealed preference" and excellent reason to avoid this misleading terminology." (ibid., p. 132)

Notice that this runs into the same problem as the previous criticism. If in fact what revealed preference is designed to do is to uncover the subjective preferences that cause consumer behavior then the preferences revealed by RPT will not do the job as Hausman says (even if they could be revealed without information about the relevant beliefs), but that is not what CRPT is attempting to do. Perhaps "economists cannot function without a subjective notion of preference," but that is precisely what economists who employ CRPT claim to be doing in their research.<sup>14</sup>

So it seems that neither one of these two criticisms directly applies to CRPT. Both depend on a particular conception of what a proper scientific theory of economic behavior has been, is, and ought to do, and that conception is not shared by the economists who defend CRPT. In fact, supporters of CRPT self-consciously deny that what Hausman argues economists need to do is either what the best modern economic science does, or ought to do. And furthermore, they (particularly Gul & Pesendorfer) argue that many of the perceived methodological problems of contemporary (micro) economics – especially those emanating from behavioral economics and neuroeconomics – exist only because the critics (from the CRPT point of view wrongly) hold a folk psychology-inspired causal view of preference and utility.

Amartya Sen is well-known as a critic of rational choice theory in economics, but I will make no attempt to discuss his wide-ranging contributions to the literature.<sup>15</sup> The focus here is on what he has said specifically about RPT (that is, his criticisms of RPT that are not, or at least are much less, relevant to other instantiations of rational choice). As with Hausman, I will only discuss two of the many issues Sen has raised and they are also ones where the effectiveness of the argument varies among different versions of RPT.

Sen's most influential foray into the critical analysis of RPT as a theory of consumer choice was his London School of Economics Inaugural Lecture in 1973 (Sen 1973). Although he continued the critical engagement for many years – I will focus primarily on Sen 1980, 1993, and 1997 – the later work often involved clarification, extension, and elaboration of his

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<sup>14</sup> It should be noted that in his most recent book (Hausman 2011), Hausman makes additional arguments about the failures of RPT, such as its inability to support game-theoretic reasoning. These arguments may be contested by defenders of CRPT, but the point is that he has expanded his critique of RPT in recent work.

<sup>15</sup> Dhongde and Pattanaik (2010) provide a detailed survey of the various criticisms of rational choice theory that Sen has raised over the years.

1973 arguments. I will discuss two main points, but they are interrelated.

The first is that Sen argues that RPT can not be defended on purely empirical grounds. If a particular revealed preference hypothesis were tested against all possible (or at least all relevant) parameters and demonstrated to be consistent with the data, then it would be a well-confirmed empirical hypothesis and would be justified on strictly empirical grounds. But that is not how RPT theory works. Empirical applications of RPT start from initial (finite) choice data and observed consistency with respect to only the initial subset of data is not sufficient for consistency over all of the relevant parameters. As Sen explains (he is referring exclusively to WARP but the argument of holds for other revealed preference axioms):

... the set of possible choice situations for any individual is infinite – indeed uncountable. To check whether the Weak Axiom holds for the entire field of all market choices, we have to observe the person's choices under infinitely many price-income configurations. In contrast, the number of actual choices that can be studied is extremely limited. (Sen, 1973, p. 243)

Or as Stanley Wong phrased the criticism: "Even if for a given number of market-choice situations the postulate is not falsified, there is no guarantee that it will not be falsified in some future market-choice situation, ..." (2006, p. 57).

Sen's second critical point in the 1973 paper follows immediately on the heels of this argument about the empirical justification of RPT. If empirical verification is not the justification for revealed preference axioms, then what is? Sen's answer is the traditional idea that agents have stable subjective preferences that cause their choices: "the case for its use lies not in verification but in its intuitive plausibility given the preference-based interpretation of choice" (Sen, 1973, p. 246). As Sen explains, the entire idea that inconsistency is in some sense problematic is not based on the empirical fact of the matter – we observe people violating revealed preference axioms all the time – rather it is based on the idea that what really lies behind our choices are stable preferences. As he explains:

Preferring x to y is inconsistent with preferring y to x, but if it is asserted that choice has nothing to do with preference, then choosing x rather than y in one case and y rather than x in another need not necessarily be at all inconsistent. What makes it look inconsistent is precisely the peep into

the head of the consumer, the avoidance of which is alleged to be the aim of the revealed preference approach ... Faith in the axioms of revealed preference arises, therefore, not from empirical verification, but from the intuitive reasonableness of these axioms interpreted precisely in terms of preference. (Sen, 1973, p. 243)

Or as Till Grüne-Yanoff succinctly put it: "all of the intuition behind consistency is derived from deliberation based on mental states; to deny this background while insisting on its intuition is to want the song without the bird" (2004, p. 387).

Sen gives this argument additional support in Sen (1993) and (1997) by introducing the problem of context- or menu-dependence.<sup>16</sup> He argues that many perfectly rational choices will violate the axioms of revealed preference when the choice situation – the context or menu – changes. He discusses two related types of context or menu changes. One is simply that changes in the choice space can effect the relative preference for two different bundles even if both are available before and after the change in the choice space. This seems to be closest to the discussion in recent behavioral economics: preference reversals, endowment effects, context dependency, and such. The second is a variant of a change in the choice space; it is where the change involves the introduction of a moral or social obligation. The problem in this case is that revealed preference – what people chose – reflects what the individual thinks they ought to do (socially, morally, etc.) and does not reveal their true preferences. In both cases the point is that a useful theory of rational choice cannot be constructed on the basis of consistent choice alone; considerations of a more folk psychological sort involving beliefs and desires will always be necessary. In the 1993 paper he uses some interesting examples to make his case and in 1997 the argument is couched in terms of abstract choice functions.<sup>17</sup>

So what are we to make of these two critical arguments by Sen? The first might be called the "finite choice data" problem and the second the "the real (folk psychological) reason consistency matters" problem. Consider the finite choice data problem first.

Since CRPT is restricted-domain, rather than standard domain RPT, the finite choice data problem clearly applies to CRPT, although it does not

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<sup>16</sup> Such argument have become quite popular in the recent literature in behavioral economics (See Kahneman 2003, Kahneman and Tversky 2000, Knetsch 1992, Thaler 1980, Tversky and Kahneman 1991 and host of others).

<sup>17</sup> Bhattacharyya, Pattanaik, and Xu (2011) provide a new, and more general, formulation for Sen's menu-dependence arguments, but end up supporting his basic position.

apply to TRPT (Sen's target in 1973) since TRPT starts from abstract demand functions defined for all possible prices and quantities. But this criticism may not be as damaging as Sen seems to believe even for finite choice data-based theories such as CPRT. The problem is that Sen has simply (re)identified a set of well-known problems associated with confirmation and induction in science, and while these are very important issues in the philosophy of science, they are general problems and have nothing specifically to do with RPT (any RPT). Much of the revealed preference literature has focused on the various revealed preference axioms as tests of consistency interpreted in falsificationist, rather than verificationist, terms; this of course leaves open the problem of how one is justified in making the inference from the initial choice data set to a broader range of parameters even if the relevant revealed preference hypothesis is not falsified by the initial data, but again this is a problem for empirical science, not just RPT. As Dhongde and Pattanaik explain:

In discussing the use of WARP in the theory of competitive consumers' behavior, Sen (1973) rightly points out that the class of budget sets that a consumer may face is an infinite class, and we cannot possibly observe the consumer's choices from an infinite number of budget sets. But, this only shows that, like all empirical hypotheses that take the form of universal statements about infinite classes, WARP is not verifiable. Nonverifiability can hardly be an objection to the introduction of a hypothesis (see Popper ... for a classic discussion of the role of verifiability and falsifiability in science). (2010, pp. 25-6)

The bottom line is that while Sen's finite choice data critique does in fact apply to restricted-domain theories like ERPT and CRPT, it not very damaging since it is not a problem that is unique to (any version of) RPT.

Sen's second criticism – "the real (folk psychological) reason consistency matters" problem – is also not very effective against CRPT. The issue is similar to the weaknesses of Hausman's critique discussed above. In Hausman's case the argument was that an adequate economic theory of individual consumer choice should be grounded in the our commonsense notions about the causes of human choice. Hausman seems to make this case on both naturalistic and normative grounds: it is what economists have traditionally demanded and it is what is necessary for any philosophically justified theory of individual choice behavior (defenders of CRPT of course deny both of these claims). Sen's argument for the ineliminability of folk psychological notions from the practice of the economics profession is similar to Hausman's naturalistic argument. The problem for Sen's argument, like Hausman's, is that it will not be

effective against defenders of CRPT who deny that such notions play any role in either the practice of modern economists or in the philosophical justification of our scientific knowledge about choice behavior.

### 3. Additional Methodological Concerns About CRPT

In this section I want to take a different approach to the methodological critique of CRPT. Even though I am generally sympathetic to the criticisms that Hausman, Sen, and others have raised against RPT, I would like to offer criticisms of a different sort that may be more effective against CRPT: and more importantly, more persuasive to the growing number of economists who seem to support CRPT-based arguments about what choice theory economics is or must be. The strategy is that since CRPT is built on the idea that rationality is solely about consistency, those defending or sympathetic to CRPT should be receptive to internally-focused arguments about various tensions and conflicts among different parts of the CRPT subprogram.

I will offer three arguments here: indicated by 3a, 3b, and 3b. The first is not my argument; it is Hausman's (2008, 2011) and it challenges the claim that CRPT is consistent with standard welfare economics. The second criticism is related to Hausman's point, but focuses on arguments (particularly by Binmore) that CPRT is consistent with rational choice theory as a normative theory of rationality. Finally, I argue that even if one accepts the predictive/instrumental effectiveness of GARP-based empirical techniques in various areas of applied economic analysis, evidence has not been provided to support CRPT's imperialistic methodological claims.

3a. The presumption in all of the discussion thus far has been that the theory in question – OUT or any of the various versions of RPT – is a positive scientific theory of individual choice behavior. The criticism discussed here changes the focus to normative economics: in particular the relationship between choice theory and welfare economics (the normative branch of economics that provides a theoretical framework for microeconomic policy analysis). Welfare economics has traditionally been tightly intertwined with OUT and OUT is an "individual preference satisfaction" theory of consumer choice; individuals have well-ordered preferences over various states of the world (usually commodity bundles) and make choices on the basis of bringing about states of the world in which those preferences are most satisfied (given the agent's beliefs and the constraints they face). If the agent is fully-informed and self-interested, then choosing the most preferred bundle will make them better off than choosing any other available bundle. Piggybacking on this positive theory of individual behavior, standard welfare economics – in any of its traditional forms: Pareto efficiency, the compensation principle,

or cost-benefit analysis – is an individual preference satisfaction theory of the good (of social welfare). Welfare economics takes the OUT characterization of economic agents as given and adds a moral principle: a criterion for a good or welfare-increasing allocation of resources in a world composed of such agents. The principle comes in a number of different formulations, but the simplest is "it is a (morally) good thing if people are better off." Actions which bring about allocations of commodities that are more preferred – that make people better off on the basis of their own preferences – are the things that ought to be done. In other words, standard welfare economics can be reduced to a combination of consumer choice theory and a moral principle connecting individual preference satisfaction to the social good.<sup>18</sup>

Hausman's criticism is that CRPT undercuts such an individual preference satisfaction view of welfare. His basic argument is that "choices," or the preferences that emerge from ERPT exercises on choice data, are not the preferences – or a reasonable proxy for the preferences – that underwrite the individual preference satisfaction view of welfare. One can question whether individual preference satisfaction is the proper way to think about social welfare, but even if one accepts this definition – or I would say particularly if one accepts this definition – the relevant preferences need to be something more than consistent patterns within observed choices. As Hausman explains in his response to Gul & Pesendorfer:

Gul and Pesendorfer's account also makes it mysterious why the activities they describe should be called "welfare economics." If "better for A" is just synonym for "chosen by A," why use the language of "better," "benefit," "advantage," or "welfare" except to sow confusion? And why should anybody care about whether an institution is efficient or not? The answer presumably is that economists think that there is something good about people getting alternatives that rank higher in their preference rankings – where "good" means, of course, something other than "chosen." Gul and Pesendorfer themselves state, "Individuals sometimes make obviously bad decisions." ... What is the meaning of "bad"? It cannot be "chosen." Similarly, "better" and "improve" cannot be synonyms for "chosen." ... The underlying point is obvious. We all know of choices that others have made that we believe were bad for them and of choices we made in the past that we now believe were bad for us ... (Hausman, 2008, p. 144)

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<sup>18</sup> See Hausman and McPherson (2006) or Hausman (2011) for a detailed discussion of welfare economics and its relationship to OUT.

Unlike the criticisms discussed in the previous section, this one seems to be on the mark against CRPT. If one's only concept of preference is that offered by CRPT, then it undermines the individual preference satisfaction view of welfare. Now of course one could respond that welfare economics is either, i) an inappropriate subject for economists, or ii) that it need not hook up in any way with the reigning preference theory; but if one believes that welfare economics is an important subject, and that what it presupposes about human behavior should be consistent with the discipline's dominant theory of human behavior, then CRPT has a problem. It is important to notice that in this case, unlike Hausman's criticisms discussed earlier, the tension is internal to the CRPT program since Gul & Pesendorfer, Binmore, and others involved in the methodological defense of CRPT are committed to the traditional view of welfare economics and argue that it is supported by CRPT.

3b. This section will continue the discussion of the relationship between CRPT and normative economics, but take it in a somewhat different direction. The previous argument was concerned with the relationship between RPT and welfare economics, but not everything that is normative is morally normative. Norms involve rules and action-guiding principles; they are prescriptive, but not all prescriptions prescribe that which is moral. Much of the philosophy of science is normative, but it does not tell scientists what they ought to do to be moral, it tells them what they ought to do to be good scientists (or discover true laws of nature, or save the phenomena, or produce justified scientific knowledge, or ...). There is a long tradition in the philosophy of decision theory that is concerned with rational choice theory as a normative theory of rationality: what individuals ought to do in order to be rational. As Robert Nozick explains:

An elaborate theory of rational decision has been developed by economists ... This is a powerful, mathematically precise, and tractable theory. Although its adequacy as a description of actual behavior has been widely questioned, it stands as the dominant view of the conditions that a rational decision should satisfy: it is the dominant normative theory.

(Robert Nozick, 1993, p. 41)

Notice that nothing in the normative interpretation precludes rational choice theory from also being an adequate scientific theory of individual behavior and in fact one of the main topics within the philosophical literature has been the relationship between these two interpretations (How does success or failure in one domain bleed over into the other? What are the grounds for accepting a theory in each of the domains and are they related? etc.). The specific area within rational choice theory that has attracted the most attention over the years is expected utility

theory, particularly the Savage axioms and the Allais paradox (Allais 1979, Friedman and Savage 1952, Savage 1954). Since RPT is a version of rational choice, it raises the obvious question of whether the revealed rationality associated with RPT can support such a normative interpretation. The previous subsection addressed the relationship between RPT and welfare economics, this subsection will discuss the relationship between RPT (particularly CRPT) and a normative conception of rationality.

Binmore explicitly makes the case that CRPT can support a normative theory of rationality in this sense.<sup>19</sup> As he explains:

Pandora uses the theory of revealed preference normatively when she revises her attitudes to the world after discovering that her current attitudes would lead her to make choices in some situations that are inconsistent with the choices she would make in other situations. (2009a, p. 22)

And he uses Savage's response to the Allais paradox as an example of such normatively motivated mistake revision:

A famous example arose when Leonard Savage was entertained to dinner by the French economist Maurice Allais. Allais asked Savage how he would choose in some difficult-to-assess situations ... When Savage gave inconsistent answers, Allais triumphantly declared that even Savage didn't believe his own theory. Savage's response was to say that he had made a mistake. Now that he understood that his initial snap response to Allais's questions had proved to generate inconsistencies, he would revise his planned choices until they became consistent." (ibid.)

This certainly seems to be a reasonable way to think about how a normative theory of rationality might relate to actual behavior. Perhaps people have a preference for rationality, or perhaps as Jacob Marschak (1950) once argued rational decision theory is like the rules of logic and arithmetic, but in either case people who realize they have made a mistake (i.e. are not behaving rationally) will revise their behavior and correct the mistake. This argument seems reasonable, but the question is whether it can be sustained by CRPT's conception of rationality.

How is it that we correct mistakes? Let us consider a particular example, a recent mistake of mine. I purchased a particular company's stock

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<sup>19</sup> Gul and Pesendorfer do as well, but their discussion of this notion or normative economics is difficult to sort out from their discussion of welfare economics, so I will focus primarily on Binmore.



thinking that it would be a good intermediate term investment (stock price appreciation because of good business prospects, good and increasing earnings, a pattern of increasing dividend payouts, etc.). I was wrong and revised my behavior – I sold it. Given it was a financial transaction it seems reasonable that some sort of rational choice was involved in my original purchase decision; I was not honoring my ancestors or trying to do the fair thing, I was trying to maximize my risk- and tax-adjusted expected return subject to my beliefs and the constraints I faced. I was acting rationally given the available information and made a mistake. What changed between  $t_0$  when the decision was made, and  $t_1$  when I recognized the mistake and sold it, was not my preferences, it was a change in information, a change in my beliefs based on the fact that what I believed about the company proved to be incorrect. If one thinks of choice in causal terms as a series of mental states that cause behavior then the processing of the new information and change in behavior is straightforward. Such behavior seems entirely consistent with the folk psychological interpretation of rational choice and it also seems consistent with Binmore's argument that what people do is revise their behavior "after discovering that her current attitudes would lead her to make choices in some situations that are inconsistent with the choices she would make in other situations" (2009a, p. 22). But how is this consistent with the CRPT definition of preference as just choice? Doesn't it involve some kind of a causal mistake fallacy (arguing that mental states do not cause behavior, unless the mental states are knowledge of previous mistakes)?

Recall how preference and welfare are defined within CRPT:

As its welfare criterion, standard economics uses the individual's choice behavior, that is, revealed preferences. Alternative  $x$  is deemed to be better than alternative  $y$  if and only if, given the opportunity, the individual would choose  $x$  over  $y$ . Hence, welfare is defined to be synonymous with choice behavior. (Gul and Pesendorfer, 2008, p. 8)

Standard economics focuses on revealed preference because economic data come in this form ... Such data do not enable the economist to distinguish between what the agent intended to choose and what she ended up choosing; what she chose and what she ought to have chosen. (ibid.)

In this case, I had the opportunity to buy the stock – I chose to do so – and therefore on the basis of CRPT the choice was "better" than my other available option (not buying it). Since the economist is not able to distinguish between what I chose (the stock) and what I ought to have chosen (the cash) how is it that such a rational choice theory could

possibly provide guidance for the revision of mistakes? Since my objective remained the same for both the mistake and the correct behavior, the "change" is entirely in my (non-observable) beliefs and expectations and thus the change that precipitated my change in behavior (thinking in old-fashioned causal terms) was completely opaque to the economist describing my behavior with CRPT. I can change my behavior and get normative guidance from rational choice theory if the theory is interpreted in the traditional causal and folk psychological way, but rationality as depicted (or revealed) by CRPT provides no advice about what I ought to do since it defines what I prefer exclusively as what I chose, and none of the beliefs that are going into to my calculations show up in the observable data. If a theory of rationality is going to be useful advising me what I ought to do, how I can avoid mistakes in rationality, it would need to speak to me in the idiom that accommodates the possibility that how I think about something, my mental states, actually have an impact on my behavior. If someone says "You made a mistake and are acting irrationally" by showing me inconsistencies in my intertemporal choice patterns, that information cannot help me correct those mistakes unless those patterns are in some way connected, causally connected, to my mental states. If I deny that mental states were the cause of my purchase, how can my understanding of my mistake – also a mental state – be the cause of my change in behavior (the sale)? And if it is not the cause of my changed behavior how can CRPT be an effective normative theory, one that tells me what I ought to do differently? As I noted earlier, this seems to be a causal mistake fallacy.

This criticism is similar in some respects to Sen's second criticism discussed above, but in other respects it is quite different. Sen is focusing on the folk psychological origins of our intuitions about consistency and why it matters – and I do not disagree – but his argument is unlikely to be effective against CRPT since its defenders put no scientific significance on such folk psychological intuitions. Perhaps RPT's concept of consistency has intuitive appeal because of its folk psychological legacy, but for defenders of CRPT this neither supports nor undercuts the scientific value of CRPT. It is like the difference between logic of discovery and the logic of justification in logical positivist philosophy of science; Sen's point is about the logic of discovery of consistency and (for defenders of CRPT) that is irrelevant to the scientific justification of CRPT. My point is not about folk psychological origins, but rather about the tension between two different explicitly stated aspects of CRPT: the causal utility fallacy and the ability of CRPT to provide normative guidance about rational behavior. In the end it seems CRPT fails to provide normative insight into rational decision making in much the same way that it failed to hook up with (ethically) normative welfare economics. Contrary to the claims of most defenders, CRPT does

not seem to be a theory that can tell anyone what they ought to do in order to be rational, or help them correct mistakes in decision-making.

3c. This subsection concerns the more general methodological claim of CRPT – that it is a proper template for how scientific choice theory should, and must, be done – and the relationship between this methodological position and the various arguments of Hausman, Sen, and others, that revealed preference must involve subjective, folk psychological, causal preferences (preferences<sub>FP</sub>). The first point is that if one is solely interested in empirical prediction then perhaps the absence of this traditional causal story is not necessarily a problem. One could take an instrumentalist position and argue that the value of consumer choice theory should be judged solely on the basis of the accuracy of its empirical predictions, particularly relative to the next best alternative (either within or outside of economics) and thus explanation, causality, and naturalist-inspired consistency with other aspects of modern economics or other sciences, are all quite irrelevant. This might seem like a natural position to many economists since the standard interpretation of Milton Friedman's famous essay on economic methodology (Friedman 1953) is precisely such instrumentalism.<sup>20</sup> It might also be supported by the fact that an extensive literature has grown up around the application of ERPT-based nonparametric approaches to a wide array of different empirical applications within choice theory. These revealed preference-based empirical techniques are competing successfully with other more traditional econometric techniques and therefore must be, to some extent, saving the phenomena that economists consider important and thus be a successful theory on strictly instrumentalist grounds. One challenge to this instrumental success thesis of course comes from the growing literature on behavioral economics, neuroeconomics, and experimental psychology that provides evidence that in a variety of different contexts, both field and experimental, real human agents do not behave in the way that rational choice theory (including RPT) suggests. Of course many economists believe that these empirical anomalies can ultimately be explained in rational choice terms, but numerous, repeated, negative empirical evidence coming from many different researchers, places, and subjects, seems to raise serious doubts about the empirical adequacy of rational choice theory and by implication RPT. But my point is not to take a position on this debate. Time will tell. My point is simply that it is not impossible – either logically or empirically – to make a purely instrumental case for ERPT that does not involve an explanatory, causal, or folk psychological role, for preferences.

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<sup>20</sup> Although this is not the only interpretation of Friedman's essay. See Mäki (2009) for a recent discussion of the literature.

But – and here is my main point – this instrumentalist defense does not provide an effective argument for CRPT's broader methodological aspirations (even if the argument for empirical success could be made convincingly); it is at best a defense of ERPT in a particular range of economic applications. Defenders of CRPT do not defend it merely as an applied tool that can be used successfully in certain economic applications; they defend it, as noted previously, as the "official doctrine of neoclassical economics, enshrined in all respectable textbooks" (Binmore, 2009, p. 201), and that, even under the most positive possible reading of the empirical track record, does not follow from its predictive success in a few areas of application. It may be possible to extend ERPT beyond applied demand analysis, but even if that is the case, it is not a convincing argument that it would be possible, and better, to do all of choice-theoretic economics – in macroeconomics, finance, industrial organization, law and economics, cost-benefit analysis, and all of the other things that modern economists do – by merely projecting patterns gleaned from GARP-consistent choice data onto new sets of parameters. There are two separate, but related, problems. First, assuming that instrumental success is all that matters, success in some subset of choice-theoretic economics does not provide an argument for abandoning the existing approaches in other areas of economics. Perhaps if defenders of CRPT were to expand their work and apply the ERPT tools to – and demonstrate the empirical superiority in – all of these different fields, then they would have an argument for replacing the existing techniques with ERPT, but prior to such evidence there is no instrumentalist argument for ERPT in these other areas. This is a notorious problem with instrumentalism – it doesn't travel well from domain to domain and must prove its robustness in each new area of application. If the theory provided reason to believe that the same underlying causal forces were at work in all of these different fields, then one might be able to make the case for the universal effectiveness of ERPT techniques, but such causal similarity is not something strict instrumentalism can provide (and it is something that CRPT claims is unattainable). If the sole ground for supporting a theory or approach is its empirical success against the competition, then one must actually demonstrate that success in each and every case, and current defenders of CRPT have provided no such evidence (and will not be able to for a long time). Second, all of these other approaches also provide causal explanations of both individual behavior and social interaction, hook up in a systematic way with normative welfare economics, and also provide a normative theory of rational action. CRPT explicitly denies that it provides causal explanations and I have demonstrated that it fails with respect to both of these other goals. So if one wants choice theory to do all of the things that existing theory does, then CRPT cannot do the job, but on the other hand, if one is only interested in instrumental success, then the case for CRPT would need to be made on a domain by domain basis, and at this

point, it has not. Of course none of this detracts from the success of ERPT in certain domains, it only challenges CRPT's imperialistic methodological claims.<sup>21</sup>

#### 4. Conclusion

I have tried to do at least three different things in this paper. One is to examine the two main branches of the RPT family tree and show how they are related. There is a fairly extensive literature on the methodological foundations of RPT and much of it – I would say most of it – is less effective than it could be because of the variation within the revealed preference research program. There are fundamental methodological differences – whether the theory is merely a tool for empirical prediction or also an attempt to explain individual behavior; whether it is (or is intended to be) a causal theory; whether it is or is not supposed to (or should) support other, particularly normative, areas of economic theorizing; what is the domain of the theory (standard domain, extended domain, restricted domain, other); whether it is (or is intended to be) a substitute or a complement to OUT; etc. – and since different versions of RPT take very different (and often conflicting) positions on these various methodologically significant issues, it is simply impossible to formulate a coherent methodological analysis of RPT without taking these differences into consideration. The taxonomy provided here should help alleviate this situation, and that in turn will help us to better understand the various positions within the existing literature as well as to move the methodological discussion forward.

The paper's second goal was to discuss some of the existing methodological criticisms of RPT and to point out the ways in which these critiques were often less effective against CRPT than against other forms of RPT. Finally, the paper provided some methodological criticisms that specifically targeted CRPT. The idea was to emphasize more internally focused criticisms – ones that pointed out various tensions within CRPT – rather than aiming the criticism directly at CRPT's core epistemological commitments.

In summary, defenders of CRPT want to (simultaneously) maintain a particular definition of RPT as well as to make a number of key methodological claims about the characteristics of the RPT so defined. The "theory" is defined as a particular technique for predicting the choice behavior of economic agents based on preference patterns extracted from

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<sup>21</sup> It should be noted that it may be possible to combine elements of CRPT with causal mechanisms other than traditional (mental state) preferences: i.e. a RPT theory based on ERPT that offers causal explanations of economic behavior (i.e. is not merely instrumental), but where folk-psychological preferences are not the relevant causes. This seems to be an aspect of Binmore's approach.

sets of consistent choice data. The key methodological claims are that CRPT: i) is a successful descriptive theory in the field of consumer choice as well as in a wide range of other applications within choice-theoretic economics, ii) is not an explanatory theory of consumer behavior, or the behavior of any of the agents it redescribes, iii) is not a causal theory of consumer behavior, or the behavior of any of the agents it redescribes, iv) is a theory that regularly employs the terms utility and preference, but the meaning of those terms comes entirely from within CRPT and does not in any way reflect (or formalize, or approximate, or identify) the common sense (folk psychological) concepts of utility and preference, or the concepts of utility and preference employed in other approaches to choice theory (such as OUT), v) it is, can, and should be exhaustive of contemporary microeconomics (or at least choice theory), and vi) it is consistent with standard normative economics (both where the relevant norms involve morality – welfare economics – and where the relevant norms are norms of rationality). I believe the above discussion has demonstrated, at the very least, that not all of these claims can be consistently maintained for CRPT as defined.<sup>22</sup> 3a) and 3b) exposed various tensions between vi) and ii), iii), and iv), while 3c) demonstrated that the case for v) has not been made (in particular, that it does not follow from i).

The bottom line is that RPT can be good economics without being a good methodological template for all of choice theory. RPT, particularly ERPT, is a successful part of modern economics – and a part that is currently expanding its range of application – and it can, and should be, defended on those grounds. Please carry on. But why take this one small part of economic theory, successful in certain domains, and make broad methodological claims about what such economics does and does not do,

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<sup>22</sup> At this point it should be noted that this entire paper has proceeded along without recognizing a significant elephant in the room. The elephant in the room is the work of Don Ross (2000, 2005, 2008, 2011 and elsewhere) and it is the most philosophically sophisticated defense of CRPT. Ross offers a realist-based defense of CRPT against some of the criticisms that have been raised in this paper, although he also admits that many of the defenders of CRPT, particularly Gul and Pesendorfer, are often confused (or at least overly rhetorical and isolationist) and do little to clarify the CRPT position (Ross 2011). The reason that Ross' arguments are not discussed here is simply that his position involves many deeply interconnected philosophical and scientific moving parts and any summary compact enough to be included here would not do it justice. It requires a separate investigation. Ross starts from Daniel Dennett's intentional stance, but a realist (not instrumentalist) interpretation of Dennett's position: the literature on "real patterns" (Dennett 1991, Ross 1995). It is committed to a particular version of realism – "structural realism" – but ontic structural realism (Ladyman 1998, Ross and Spurrett 2007), not the epistemic structural realism of John Worrall (1989). And finally, but significantly, it employs resources from the rapidly growing literature on neuroeconomics – the relationship between economics and neuroscience – particularly the work of Paul Glimcher (2003). I have criticized Ross' attempt to reconstruct the work historical figures such as Robbins and Samuelson (Hands 2008, 2009) along the lines of CRPT, but at this point I maintain an open mind about the potential for his general philosophical defense of CRPT. It is though, as I said, a subject that requires a separate investigation.

and how all choice theory should be done in this way? I suspect that it is motivated by – this is explicit in Gul and Pesendorfer (2008) – a perceived threat from the recent critical literature coming from experimental psychology, behavioral economics, neuroeconomics, and such. This work has been seen, by both critics and supporters, as a significant (perhaps existential) threat to the basic rational choice framework that has dominated theorizing about the behavior of economic agents for over a century,<sup>23</sup> and some see CRPT as a defense of the rational choice tradition. The problem is that CRPT involves such a radical revision of what it is that one is trying to save that it would not be recognizable once saved. Rational choice theory in general, and OUT in particular, have attained their current position within economics because economists considered them to be effective at predicting, explaining, and helping us understand the causes of, a wide range of interesting economic phenomena, as well as because they connect up comfortably with the discipline's prevailing normative theories. To substantially narrow the focus of choice theory and strip away its ability to simultaneously fulfill all of these various goals is not to save the existing theory, but to destroy it. In any case, I have tried to provide a framework for thinking about the various methodological issues associated with RPT as well as some reasons to question the research program's most recent developments.

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<sup>23</sup> Daniel McFadden's remark captures this nicely: "The work has both fascinated and dismayed economists: it has been like watching master carpenters construct the scaffold for your hanging" (McFadden, 1999, p. 79).

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