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D. Wade Hands

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Review Essays / Essais critiques

Revealed Preference, Afriat's Theorem, and Falsifiability

Review essay on:

Chambers, Christopher and Federico Echenique, *Revealed Preference Theory*, Cambridge: Cambridge University Press, 2016, 240 pages, ISBN 978-110745811-6

D. Wade Hands*

> I can tell you of an important new result I got recently. I have what I suppose to be a completely general treatment of the revealed preference problem, which will give a fresh setting for the related work of Samuelson-Houthakker-Uzawa. Calculus methods are unavailable. The methods are set-theoretic or algebraical. (Letter from Sidney Afriat to Oskar Morgenstern June 4, 1964)[1]

Revealed preference theory just isn't what it used to be. Well, it still is what it used to be, but it is now much more. The concept—although not the term, which didn't come until later—first appeared in one of Paul Samuelson's earliest published papers (Samuelson, 1938a) and it was an idea, so the story goes, that came to him during an exchange

*Department of Economic, University of Puget Sound, Tacoma, WA, 98416 USA. hands@pugetsound.edu

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[1] Box 86, Oskar Morgenstern Papers in the Economists' Papers Archive, David M. Rubinstein Rare Book and Manuscript Library, Duke University (all archival references in this paper refer to the Economists' Papers Archive at Duke).

with Gottfried Haberler in an international trade seminar during his second year at Harvard (Backhouse, 2017, 181-182; Samuelson, 1950, 369). The intuition is straightforward; if bundles *A* and *B* are both affordable and bundle A is chosen (A is revealed preferred to *B*), then if B is chosen at a different set of prices, it must be that A is not affordable at those prices. It is a simple and intuitive way to think about individual choice, but over time the original seed that Samuelson planted in 1938 has grown into a fairly lush family tree: the revealed preference research program. Growth was rather sluggish for many years, but eventually, 1950 being a key year, the main trunk began expanding at a slow but steady pace. Over time the numerous branches began to fill in and in recent decades there has been rapid growth in several areas of the revealed preference canopy.

The book under discussion here—*Revealed Preference Theory* by Christopher Chambers and Federico Echenique (2016)—was designed to pull all of these various developments together—primarily the recent research, but some from earlier—into a single volume which summarizes all of the main contributions to the revealed preference research program. The book is written in a formal style with all of the main results presented as theorems with detailed proofs. The book is not historical, but each chapter does end with a section of references that briefly discuss where, when, and by whom, each of the theorems and concepts were introduced. In terms of the way the material is presented, the book is similar to Arrow and Hahn (1971): the canonical statement of mid-twentieth century Walrasian general equilibrium theory. Both books provide a comprehensive coverage of the topic, a theorem-proof presentation of the main results in careful mathematical detail, and have a small amount of reference material to help tie the story together. However, what makes the Chambers and Echenique book quite different from Arrow and Hahn or any other such text in economic theory, is their explicit commitment to, and articulation of, a methodological position grounded in a particular philosophy of science. Chambers and Echenique endorse a strict Popperian falsificationist philosophy of science.

This review article has a number of goals. One is of course to provide a basic review, to explain to the reader what is in the book and how the material is presented. But there are also many other goals. Probably the most important is to make the history and philosophy of economics community more aware of recent developments in revealed preference theory (hereafter RPT). The topic that has received the most attention within philosophy of economics in recent years has been choice theory—rational choice theory, decision theory, behavioral economics, etc.—and this is also increasingly the case among those writing the history of modern economics. But while there is some discussion of revealed preference within this historical and philosophical literature, there are important parts that Chambers and

Echenique examine that have received little or no attention, and so an important goal of this paper is to increase the visibility of these ideas among members of the history and philosophy of economics (hereafter HPE) community. Secondly, RPT is not just RPT; it has, and is having, an impact on areas of economics outside of traditional demand theory. There are many such areas, but the two I will emphasize are: i) its contribution to the broad empirical turn that has recently taken place in economics, and ii) its relationship to Walrasian general equilibrium theory. Finally, I want to comment on the authors' Popperian falsificationism. Just as I would like to help bring recent RPT to HPE, I would also like to help bring recent HPE to RPT, by redirecting the methodological and epistemological impulses of economists doing RPT more in the direction of recent philosophy of economics and away from falsificationism which has long been considered problematic within both the philosophy of science and economic methodology.

1. Revealed Preference Past and Present

In this section I want to discuss the history of RPT and trace through its most important developments starting from Samuelson (1938a). The authors discuss some of this literature—and in far more rigor and mathematical detail than I will—and, as their first sentence indicates, they clearly recognize diversity within the revealed preference literature: "'Revealed preference' is a term with several interpretations in economic theory, all closely related but possessing subtle philosophical differences" (xiii).[2] But even with this recognition, they tend to see RPT as a nested set of theorems and its diversity as a single theory having applications in a number of different areas of economics. On the other hand, I see the various branches of the RPT family tree as having more substantive differences; they are all members of the same revealed preference research program, but they are products of different historical contexts, have different goals, and reflect different epistemic values (or to use Popper's term: they have different *scientific problem situations*). I will try to keep this historical discussion relatively brief since I have told much of the story elsewhere (Hands, 2013). This said, I think it is important to examine some of this background information before turning to the details of the authors' discussion.

As is well-known, the young Samuelson did not believe the ordinal utility theory (hereafter OUT) of Hicks and Allen (1934), Pareto ([1909] 2014), Slutsky (1915), and others, went far enough to remove utility from consumer choice theory. The purpose of his paper was thus to "start anew in direct attack upon the problem, dropping off

[2] Page numbers without additional reference information refer to Chambers and Echenique (2016) throughout.

the last vestiges of the utility analysis" Samuelson, 1938a, 62). The starting point for his exercise was an "idealized individual" with "known, i.e., empirically determinable under ideal conditions, the amounts of n economic goods which will be purchased per unit of time by an individual faced with the prices of these goods and with a given total expenditure" (Samuelson, 1938a, 62). He wrote these demand functions in the general form $\psi_i = b^i(p_1, p_2, \dots p_n, I)$ for i = 1, 2, … , n and assumed that the individual satisfied the standard linear budget constraint with parametric (i.e. competitive) prices (p_i) and income (I).[3]

Samuelson's goal was to find restrictions on these demand functions with the same empirical implications (empirical content) as OUT but without assuming that the individual was maximizing a utility function or well-ordered preferences. The consistency condition he introduced was based on the intuition noted in the previous section. If the bundle $\psi = (\psi_1, \dots , \psi_n)$ is chosen at price vector $p = (p_1, \dots , p_n)$ and bundle $\psi' = (\psi'_1, \dots , \psi'_n)$ is chosen at price vector $p' = (p'_1, \dots , p'_n)$, then it must be the case that:

$$p\,\psi' \leq p\,\psi \;\; implies \; p'\,\psi' < p'\,\psi, \qquad \text{(WARP)}$$

a condition that came to be called Weak Axiom of Revealed Preference (WARP).[4] WARP went a long way toward, but did not completely achieve, Samuelson's goal of finding a consistency condition on demand functions that would imply all of the empirical implications of OUT. WARP implied all except one—Slutsky symmetry—but given Samuelson's problem situation, this was not an issue (see Hands, 2013, 1083 for discussion).

The revealed preference condition that does have the same implications as OUT—including Slutsky symmetry—came with Hendrik Houthakker's Strong Axiom of Revealed Preference (SARP) a little over a decade later (Houthakker 1950). The strong axiom completed the circle of consumer choice theory from OUT to RPT and back. One could start with constrained utility maximization and get consumer demand functions that satisfy the standard conditions, or one could start with a demand function that satisfies SARP and there will exist a budget-constrained utility-maximizing consumer who could have generated it (the demand function is *as-if* it were produced by a utility maximizing consumer). As the authors note, Houthakker's paper provided "the first characterization of the empirical content of ration-

[3] As Chambers and Echenique explain (35) once preferences are introduced, strict satisfaction of the budget constraint implies that preferences are monotonic (non-satiation).

[4] Samuelson (1938a) actually employed three restrictions (his postulates I, II, and III) but quickly realized that WARP was sufficient (see Samuelson, 1938b).

al consumption" (54).[5] But SARP also provided something that had been lacking with WARP, and it was something that helped open the door to the later empirical literature: *rationalization*. Slutsky symmetry implies the *rationalizability* of the demand function, that is, if a demand function satisfies SARP there always exists a utility function that if maximized subject to a budget constraint would produce the demand function in question. Unfortunately, neither Houthakker's paper nor the other revealed preference literature that immediately followed, provided a way of constructing such a utility function. One problem is, as Chambers and Echenique emphasize, that rationalization—the existence of a utility function that could have generated the demand—is an *existential* (there exists) claim (xvii and 29) which makes it extremely problematic for empirical testing or application.

One important thing to notice about Samuelson's original characterization of WARP—and a feature that continued with Houthakker's SARP and almost all of the RPT literature during the next few decades—is that the analysis does *not* start with anything that a contemporary economist would call empirical data. It started with mathematical objects—continuous real-valued demand functions in general form—and not observed price-quantity combinations. Price-quantity data is finite; Samuelson's $\psi_i = b^i(p_1, p_2, \ldots p_n, I)$ are defined over real numbers, and thus infinite. Of course, this is in no way a criticism of Samuelson, Houthakker, or any of the many other contributors to the first generation of RPT: the literature that, following Hands (2013), I will call *traditional revealed preference theory* (hereafter TRPT). This was the acceptable way to do scientific economics at the time. For Samuelson and his generation of economic theorists—economists working in a world without the vast amounts of empirical data we have at our disposal today—the theoretical terms of scientific economics needed to be connected to the domain of the empirically observable, but the connecting tether could be quite thin (Hands 2017). All an economic theory needed to be scientific was that the relevant variables be *potentially observable*: "empirically determinable under ideal conditions" (Samuelson, 1938a, 62). This is a big difference between TRPT and most of the more recent revealed preference research discussed below, which begins with *actually observed* price-quantity data.

Chambers and Echenique recognize this difference, but they frame it in terms of whether all of the relevant data is actually *observed*, or whether it is only *partially observed*:

[5] Or as Samuelson put it in a letter to Amartya Sen, April 4, 1973: "Thanks to Houthakker, we can then show that the generalized revealed-preference formulation completely exhausts the … implications of conventional choice theory. So it is equally unimportant to ask whether Newton's *Principia* is better or worse in English than in Latin" (Samuelson Papers, Box 68).

> In broad terms, revealed preference theory can be divided into two main subfields. The first assumes that all of the relevant and potentially observable economic data are in fact observed … We study a few such questions in this book, but not many. On the other hand, the second subfield assumes that economic data are only partially observable. (xv)

If all of the p_is and the I in Samuelson's individual demand functions could actually be observed, *then* starting with demand functions would in fact be equivalent to assuming "that all of the relevant and potentially observable economic data are in fact observed." But there are infinitely many such "observations" so all of them cannot actually be observed. As Robert Pollak aptly noted, this would require a miracle: "the miraculous revelation of consumer demand functions to the economist observer" (Pollak, 1990, 150), and economists lacking such miraculous powers must be content with finite data. The point is simply that Samuelson's original model and the TRPT literature that followed it, did not start with *data* in the contemporary sense.

In the remainder of this section I will move on from the TRPT literature and extend the historical sketch to more recent, more directly empirical, revealed preference literature. I will discuss how Chambers and Echenique present this topic in the next section; the purpose here is to just introduce the main story line and emphasize how this more empirically-oriented RPT literature differs from TRPT. In addition to Hands (2013), the discussion here draws on a number of sources, including Diewert (1973), Pollak (1990), and Varian (2006).

The key result that facilitated the transition to the more applied RPT of recent decades is a paper by Sidney Afriat in 1967. The two aspects of the existing TRPT literature that Afriat found problematic were i) the presumption of what he called "a complete system of data" (implicitly the assumption that all potentially observable economic data were actually observed), and ii) that even if the data were found consistent with RPT, the theory provided no means for constructing the (or a) relevant utility function. As Afriat explained:

> The revealed preference principle of Samuelson … elaborated by Houthakker … easily gives a condition for the rejection of the hypothesis of existence. But the principle has been absent by which the hypothesis can be accepted or rejected on the basis of observed choice of the consumer, supposed to be finite in number; and, in the case of acceptance, a general method is needed for the actual construction of a utility function which will realize the hypothesis for the data. (Afriat, 1967, 68)

Unlike the previous literature, Afriat started with finite choice data—a finite number of price-quantity combinations—and proved that if the data satisfies a particular revealed preference-based condition, *cyclical consistency* (later identified with the "Generalized Axiom of Revealed Preference" GARP), then the choice data is *utility consistent* (*rationalizable* in the later literature). An additional benefit of Afriat's approach—extended by Diewert (1973), Diewert and Parkan (1985),

Varian (1982, 1983) and others—is that his cyclical consistency condition is linked to a set of inequalities, Afriat inequalities, that can be solved to derive a utility function that rationalizes the dataset. Not only does Afriat's approach—now generally called the GARP approach to RPT—give RPT a direct route to empirical application that did not exist with TRPT, it also generates a rationalizing utility function which can be employed in a variety of different types of economic analysis, including welfare analysis. As Varian explains:

> Afriat's approach … 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 much more suitable as a basis of empirical analysis. Afriat's approach was so novel that most researchers at the time did not recognize its value. (Varian, 2006, 101)

Afriat's theorem and the follow-up literature provided a major impetus to empirical work employing RPT. Houthakker had proven that if demand functions satisfied SARP there would always exist a rationalizing utility function, but provided no practical way to find it, while GARP techniques provided a way to construct such a utility function. This, added to the fact that GARP restrictions concerned finite choice data, set off a burst of GARP-based empirical research that continues to expand at the current time.[6] Although this GARP-based literature is part of the RPT family, it is essentially an empirical technique—a *nonparametric* approach to empirical demand analysis that competes with more traditional (parametric) econometric approaches—rather than a consumer choice theory *per se*. In any case, it has completely changed the possibilities available for RPT-based empirical work. In 1961 Houthakker could accurately say that RPT had "not yet opened up as many new avenues of research as had at the time been hoped" (Houthakker, 1961, 713) and thirty-five years later a paper in the *Journal of Economic Literature* could conclude that in "spite of Paul Samuelson's (1938) high hopes, the revealed preference approach had proved empirically useless" (Lewin, 1996, 1316). But how this situation has changed during the last two decades. To separate this recent GARP-based research from OUT and TRPT, I will, again following earlier work, refer to this literature as "Empirical Revealed Preference Theory" (ERPT).

[6] Since Chambers and Echenique provide a detailed discussion of, and list of references to, this empirical literature in Chapter 5 ("Practical Issues in Revealed Preference Analysis") I will simply direct the reader to their references.

2. *Revealed Preference Theory* by Chambers and Echenique

This section will provide what might pass for a short book review. I think it is useful to briefly run through the chapters of the book and note how it is organized, the author's main arguments, and also to introduce some issues that I want to discuss in the last two sections. But before beginning that brief review, and relevant to the previous discussion of infinite and finite data, I think it is useful to spend a little time clarifying the different *domains* associated with various versions of RPT. I will rely on the terminology in Pollak (1990).

Pollak distinguishes three different versions of RPT: standard domain, restricted domain, and extended domain. The *standard domain* theory is the RPT Samuelson, Houthakker, and the rest of the TRPT literature; it is also the domain of OUT. In standard domain theory, the relevant choices are restricted to budget sets; it "places restrictions on choices from all budget sets, but not on choices from any other type of set" (Pollak, 1990, 146). The most common specification is the infinite budget set of standard consumer choice theory: the subset of R^n_+ constrained by the linear budget constraint with parametric prices and income (or endowment). *Restricted domain* RPT, also starts with budget sets, but with finite budget sets. "The restricted domain version self-consciously confines itself to a fixed, finite set of price-quantity observations and interprets the revealed preference axioms as restrictions on observed behavior" (Pollak, 1990, 149). This is the domain of ERPT. Finally, *extended domain* RPT is the most general characterization of RPT; it is the choice function-based version of RPT introduced in Arrow (1959). The choice function characterization can be applied to a much wider range of choice mechanisms (governments, groups, social planners, and others) as well as accommodating a much broader range of feasible sets than budget sets. With this bit of terminology in place we can move to the contents of the book.

After a survey of some mathematical preliminaries in chapter one, Chambers and Echenique begin with the extended domain RPT in chapter 2. They present the material in a fairly abstract way—the idea of a choice function as a general mapping and its domain as "a set of objects that can possibly be chosen" (15). Although the framework is potentially quite general, they encourage the reader to start thinking in terms of the consumer choice problem. For example, the subsets that "are the potential sets of elements from which an economic agent might choose" are called *budget sets* and the theory "is that of the maximization of some binary relation on X," for example "a preference relation" (ibid.). They characterize a binary relation that *strongly rationalizes* a choice function early in chapter 2, and go on to provide, and prove, a number of important revealed preference results within this framework. The second half of the chapter presents

results on a number of special cases including some topics not traditionally associated with RPT such as Simon's satisficing and the experimental elicitation of choice.

Chapter 3 discusses RPT in the traditional consumer choice context. The chapter covers all of the main results of SARP, GARP, Afriat's Theorem, and many additional results in technical detail. The authors do blend the TRPT and ERPT cases by characterizing the domain in a way which subsumes both types of budget sets:

> We suppose here that we have observations on the purchasing decisions of a single consumer. The consumer makes a sequence of independent choices at different price vectors. The data consists of the consumer's choices, and we seek to understand the implications of rational consumption behavior for such data. (34)

This framework is preserved not only throughout chapter 3 on rational demand, but all of the other cases where the relevant model is a version of the traditional consumer choice problem. Afriat's theorem is presented as the centerpiece of the discussion and they state Afriat's theorem both in GARP-weak rationalization-concave form (40) and also in SARP-strong rationalization-strict concavity form (48). They also offer some intuitions about how the Afriat inequalities follow fairly directly from the first order conditions and the concavity of the utility function. Given the importance of this result it is useful to write down the version of Afriat's theorem most closely associated with standard consumer choice theory:

> Let X be a convex consumption space, and $D = (x^k, p^k)_{k=1}^K$ be a consumption dataset. Then the following are equivalent:
>
> I) D has a locally nonsatiated strong rationalization.
>
> II) D satisfies GARP.
>
> III) There are strictly positive real numbers U^k and λ^k, for each k, such that for all k and l,
>
> $$U^k \leq U^l + \lambda^l p^l \cdot (x^k - x^l);$$
>
> and further if $x^k \neq x^l$,
>
> $$U^k < U^l + \lambda^l p^l \cdot (x^k - x^l).$$
>
> IV) D has a continuous, strictly concave, and strictly monotonic strong rationalization u: $X \rightarrow R$. (48)

However unintuitive the inequalities in III) may seem, they are the heart of Afriat's theorem and the growth of RPT-based research during the last few decades. III) says that if any of the other three conditions are satisfied—GARP being the easiest to work with—then there exists a solution to the inequalities in III). Solving these inequalities provides "a way of constructing a rationalizing utility function" (45).

Chapter 4 extends the discussion and provides rationalization and Afriat-type results for a number of special cases including discrete goods ("cars and houses"), supermodularity (complements) and submodularity (substitutes), homothetic preferences, separability, and others.

Chapter 5 is the chapter most dedicated to ERPT. Here they discuss some of the techniques that have been developed to address various practical concerns in empirical RPT.

> The tests in Chapters 3 and 4 are meant to be applicable to actual datasets, and many researchers have investigated these applications using experiments, consumption surveys, and other sources of data. Naturally, there are complications that arise when one tries to carry out the tests we have described. (71)

A number of practical complications are covered, but I will note just two. One is the problem of the "severity of GARP violations." Revealed preference tests are "all or nothing" (71) in the sense that the data is either consistent with GARP, SARP, etc., or it isn't. But this is not how empirical research in economics generally works; there are many ways that data and measurement can be problematic, so one needs some measure of "the *degree* to which a test is violated" (72). They discuss several such measures and the costs and benefits of each. They treat the question of the power of a revealed preference test in a similar way. The main problem is that budget constraints need to intersect for revealed preference tests to have power. They explain some techniques that have been developed to work around this problem. The chapter also includes a discussion of some of the key contributions to the ERPT literature as well as a number of other practical aspects of empirical RPT.[7]

The next six chapters—chapter 6 on production theory, chapter 7 on stochastic choice, chapter 8 on choice under uncertainty, chapter 9 on general equilibrium theory, chapter 10 on game theory, and chapter 11 on social choice and political science—all survey the different ways that RPT has been applied to, or at least come into contact with, these various research areas within contemporary economics. All of the chapters cover the main results and do so in the same careful theorem-proof style as the earlier chapters. Each will undoubted be of value to scholars working in these particular fields, but with one exception, I will make no attempt to summarize them here. The chapter in this group that I will discuss, because I think is important to HPE, is chapter 9 on general equilibrium theory. The penultimate chapter 12, on systems of polynomial inequalities, is a relatively technical chapter concerned with various computational issues associated with Afriat's inequalities. The final chapter is called "Revealed Preference

[7] Much of this literature has been discussed in greater historical and methodological detail in Moscati (2007a, 2007b) and Moscati and Tubaro (2011).

and Model Theory" and it is the part of the book that is most methodological and where the authors directly connect RPT and Popperian falsificationism. Given its methodological relevance, this too will be discussed in a separate section.

3. A Digression on GET and RPT

Walrasian general equilibrium theory became the core theory of modern economics during the middle of the twentieth century, and even though many economists have turned away from it—both its highly abstract mathematical theorizing and emphasis on perfectly competitive markets—it remains the theoretical framework through which many, perhaps still the majority, of economists conceptualize economic problems and policies; and it still plays a key role in the education of economists. Despite its influence, Walrasian general equilibrium theory has always been a methodological conundrum. How is it possible that a model that is so abstract, so devoid of direct empirical application, and so filled with false assumptions, can play such an important role in economic *Science*? Given that there is no straightforward answer to this question, the majority of methodologically inclined economists and philosophers of science concerned with economics have generally been quite skeptical about the cognitive status of Walrasian theory. For example, Mark Blaug was one of the most influential economic methodologists of the second half of the twentieth century and his dramatic remarks that Debreu's 1959 *Theory of Value* was "the most arid and pointless book in the entire literature of economics" (Blaug, 2002, 27) and that Arrow and Debreu's 1954 existence paper was "a cancerous growth in the very center of microeconomics" (Blaug, 1997, 3) resonated with many within the methodological community.

So what does this discussion of the methodological conundrum of Walrasian general equilibrium have to do with RPT or with the book under consideration here? Actually quite a lot, I think, and some of it is clear from the discussion in chapter 9 and some of it needs a bit more unearthing. This section will examine the relationship between RPT and Walrasian general equilibrium theory (hereafter GET) in a way that I hope will help tie the two literatures together.

On the face of it, there doesn't seem to be an obvious connection between RPT and GET. In its traditional version, RPT was primarily an alternative characterization of consumer choice theory with the same implications as OUT, while in its ERPT version, it is a set of techniques for empirical analysis of certain types of economic data that competes with other empirical techniques. Neither seem to hook up in any natural way with abstract Walrasian theory. Although there are many connections, I will only discuss two here: an earlier one linking GET and pre-Afriat RPT, and a later one involving an applica-

tion of Afriat's theorem. I will only consider the case of pure exchange (although many of the results also hold for production economies) and I will simplify in a number of fairly standard ways (single-valued continuous demand functions, strictly positive prices, etc.).

Consider a pure exchange competitive economy with n goods ($i = 1, 2, \dots, n$) and H consumers ($h = 1, 2, \dots, H$). Each consumer h is described by well-ordered preferences (and the associated ordinal utility function) and an initial endowment vector $\omega^h = (\omega_1{}^h, \omega_2{}^h, \dots, \omega_n{}^h) \geq 0$. Each consumer h has income $M^h = \sum_i p_i \omega_i{}^h$ and the total endowment of each good i is given by $\omega_i = \sum_h \omega_i{}^h$. Each consumer chooses the most preferred quantity of each good subject to the budget constraint and the demand for good i by individual h is given by $d_i{}^h(p, M^h)$ where $p = (p_1, p_2, \dots, p_n) > 0$ is the price vector. The market demand for good i is given by $D_i(p) = \sum_h d_i{}^h(p, M^h)$ and the excess demand for each good i by $Z_i(p) = D_i(p) - \omega_i$. These excess demand functions are homogeneous of degree zero and satisfy Walras' Law: $\sum_i p_i Z_i(p) = 0$ for all p. The equilibrium price vector is p^* where $Z_i(p^*) = 0$ for all i.

Since Walrasian equilibrium is concerned with *market excess demand* and only indirectly with individual consumer choice, it is not immediately obvious how RPT has anything to contribute to GET. Consumer demand functions in such models satisfy WARP, SARP, etc., but these revealed preference conditions do not provide any additional restrictions on individual demand functions beyond what is provided by OUT. In addition, since almost all of these restrictions are lost when they are aggregated into market-level excess demand functions, the usefulness of revealed preference conditions seems unclear. To understand how RPT became involved in GET, it is useful to recall that out of the five main topics of the pure GET research program during its heyday 1950-1970—i) existence ii) uniqueness, iii) tâtonnement stability (local and global), iv) various types of qualitative comparative statics results, and v) the (Pareto) welfare implications of the equilibrium allocation—only i) and v), were successfully addressed without additional restrictions on excess demand functions. The goal of the other three parts of the literature was to find additional restrictions that would be (at least) sufficient for uniqueness, stability, and determinate comparative statics, that were mathematically tractable and also had acceptable economic interpretations. Since, via Samuelson's "correspondence principle" (Samuelson, 1947) comparative statics was inexorably linked to stability, the desired results actually reduced to just uniqueness and stability; it was the search for reasonable restrictions for these two results than pulled RPT into the discussion.

Abraham Wald has used a WARP assumption on excess demand functions to prove existence and uniqueness as early as 1936 (Wald, 1951), so it seemed to be natural place to look for a suitable condition, but there were also other reasons to consider revealed preference restrictions. Since WARP was almost equivalent to utility maximization (and in $n = 2$ is equivalent), and since utility maximization does impose quite a bit of structure on individual demand functions—including the negative semi-definiteness of the Slutsky matrix which is very useful in uniqueness and stability analysis—it was reasonable to think that WARP would provide the additional structure to prove stability and uniqueness (and it did). But there was a problem, and it was that WARP on market demand essentially meant that the market demand function could be *rationalized*—that is was *as-if* it were generated by a single utility-maximizing consumer—and that meant the economy was a one consumer economy driven by the behavior of a single representative agent.[8] Since the main motivating purpose for Walrasian theory was to show how competitive equilibrium prices could coordinate the actions of a large number of agents with different tastes and endowments, imposing WARP directly on market excess demand functions was never really an acceptable solution (see Hands, 2016a). So the direction that uniqueness and stability literature took was to use WARP as a mathematical condition, but to find empirically acceptable assumptions on excess demand functions that implied the WARP condition. The literature of the period systematically displays this strategy, but I will just note two influential examples. Arrow and Hurwicz (1958) and Arrow, Block, and Hurwicz (1959) used assumptions such as gross substitutes which implied a version of WARP to prove stability,[9] and later, in chapter 9 of Arrow and Hahn (1971), all of the known uniqueness results for Walrasian models were derived using this same version of WARP.

These uniqueness and stability results were never satisfying, but in any case this entire literature came to a halt with the Sonnenschein-Mantel-Debreu (SMD) results on aggregate excess demand functions.[10] The bottom line for this important work was that the assump-

[8] Actually full rationalization generally required SARP, but WARP was very close, and in some specific models it was sufficient.

[9] The version was $p^*Z(p) > 0$ for all $p \neq p^*$. Rewriting the (WARP) condition from section 1 above in terms of p, p*, Z(p), and Z(p*), we have $pZ(p^*) \leq pZ(p) \rightarrow p^*Z(p^*) < p^*Z(p)$. But from Walras' Law $pZ(p) = p^*Z(p^*) = 0$, and because $p > 0$ and $Z(p^*) = 0$ we have $pZ(p^*) \leq 0$ for all p, which leaves $p^*Z(p) > 0$.

[10] The authors provide a sketch of the proof of the SMD result (132-134) as well as proving a number of related results. They also discuss the special case of homothetic preferences in Mantel (1976), important because it concerns the representative consumer. They note the three key references for the SMD literature—Debreu (1974), Mantel (1974), and Sonnenschein (1972)—but to these I would add the useful survey Shafer and Sonnenschein (1982) and the historical discussions (Rizvi, 1998 and 2006) and Ingrao and Israel (1990).

tion of well-behaved utility-maximizing agents imposed essentially no additional restrictions on aggregate excess demand functions. Regarding uniqueness and stability, it meant that it was extremely easy to construct examples of non-unique and unstable equilibria that were consistent with the core assumptions of Walrasian theory. As Chambers and Echenique explain:

> The answers to these questions carry a largely negative message about general equilibrium theory. The Sonnenschein-Mantel-Debreu Theorem … shows that, roughly speaking, any continuous function that satisfies Walras' law can be the aggregate excess demand function of a very well-behaved economy. The result implies that any compact set of strictly positive prices can be the set of Walrasian equilibrium prices of a well-behaved economy. (129)

Revealed preference assumptions, or more empirically attractive restrictions that imply such assumptions, could still be used to prove uniqueness and stability, but after the SMD results, such theorems seemed to be *ad hoc* and unmotivated. But a second round of RPT having an impact on GET was only two decades away: the post-Afriat RPT-GET literature the authors discuss in Chapter 9.[11]

The key result for the later contact between RPT and GET was contained in a paper by Donald Brown and Rosa Matzkin (Brown and Matzkin, 1996). The paper was not explicitly an exercise in RPT, but was related to RPT because it involved the application of Afriat's theorem to a pure exchange Walrasian model. Using the theorem they derived certain testable implications in such models and this additional structure provided a way to get around some of the negative implications of SMD.[12] They treated prices and endowments as the observable dataset and focused their analysis on the equilibrium manifold: the set of (p, ω) combinations where $Z(p, \omega) = 0$.[13] Using a version of Afriat's theorem and results from model theory—the Tarski-Seidenberg theorem on quantifier elimination—they demonstrated "that utility maximization subject to a budget constraint does impose testable restrictions on the equilibrium manifold" (Brown and Matzkin, 1996, 250). They even provided some discussion of how the

[11] There have of course been other approaches to circumvent the difficulties associated with the SMD results. The non-RPT-based approach that has received the most attention is Werner Hildenbrand's work on market demand (Hildenbrand, 1994). See Kirman (2006) for a useful discussion of the relationship between Hildenbrand's approach and SMD, and the exchange between Hildenbrand and Houthakker (Hildenbrand, 1995; Houthakker, 1995a and 1995b) for the relationship between his approach and RPT.

[12] Given the amount of work on testable GET that has been produced in the last two decades, it is surprising how little HPE literature it has produced. Rizvi (2006) is a clear exception, a paper that discusses the historical filiation of the relevant ideas, but also provides several criticisms.

[13] The equilibrium manifold has been used in GET to discuss more traditional questions such as stability by Balasko (2009) and others.

model could be empirically implemented and added an appendix on how it could be extended to production economies. These results are summarized nicely by Susan Snyder:

> The well-known theorems of Sonnenschein, Mantel and Debreu can be seem to imply that the competitive equilibrium model is "irrefutable"—without strong assumption on the form of utility functions, there is little we can say about the equilibrium prices and allocations that result … Work by Brown and Matzkin (1996) on the empirical implications of general equilibrium does have refutable implications on the equilibrium manifold; and they explicitly incorporated the limitations of practical data sets into the restrictions by focusing on finite sets of observations of an economy. (Snyder, 2004, 165)

Against the backdrop of the disappointing SMD results, this approach to improving the empirical content of GET models was welcomed and produced a fairly extensive follow-up literature. Although these results are not a direct application of RPT, they share the same conceptual framework of rationalization. As Chambers and Echenique note: "The observation that the theory of Walrasian equilibrium is testable is subtle, and in sharp contrast with the message of the SMD Theorem, but it is not different in nature than the idea that there are individual observations that violate WARP" (137).[14]

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[14] My philosophical concerns will be discussed in the next section, but at this point I would like to make one remark about something the authors say on the very last page of the book because it is about the relationship between philosophy of science and GET (and thus relevant to this section). They say:

> Finally, there is an approach in philosophy of science called *structuralism*, which also comes close to the approach we have taken here. These works also adopt a model-theoretic perspective to investigating theories. Sneed (1971) is a classic reference as applied to physics. Stegmüller, Balzer, and Spohn (1982) present a collection of applications of these ideas to economics. (197)

I have two comments on this remark. The first is that while Sneed-Stegmüller reconstructions are similar in terms of technical tools – both are, as the authors say, model-theoretic – they are not similar in terms of philosophical commitments, and one always needs be careful not confuse what is in the box with the way the box is wrapped. Sneed-Stegmüller structuralism is self-consciously anti-falsificationist and committed to a fundamentally different notion of scientific theory (at least different than Popper). For these structuralists, a scientific theory simply defines a predicate; for example Walrasian theory (W) would define the predicate "is a W," and as such, the "theory" is neither true nor false. The possibility of truth or falsity enters when a sentence is formed, say "x is a W." (see Hands, 1985b; 2001, 341-348 and references therein). This view of scientific theories is certainly not Popper's; with respect to the philosophy of economics literature it is similar (at least minus the set theory) to Hausman (1992). My second comment is that even given these differences, the authors may be right; the two views may "come close." Like the Sneed-Stegmüller structuralists, the authors i) emphasize set-theory and axiomatization, ii) focus of structural relations, and iii) are concerned with the elimination of theoretical terms, so they do seem to have

There are many methodological and practical concerns that can be raised against this characterization of "testable" GET—the question of the observability of endowments for example (see Rizvi, 2006)—but since I am going to raise some broader methodological issues in the next section, I will not discuss these specific criticisms here. But before moving on to methodology it is useful to reflect on the ground that has been covered thus far. Samuelson's original 1938 paper on consumer choice theory generated an extensive theoretical literature on individual demand theory and was also used in GET as a restriction on market excess demand. Afriat (1967) opened the door for the development of the ERPT literature which now competes with other empirical tools in many areas of economics. A version of Afriat's result was also used by Brown and Matzkin (1996) to generate a type of testable restrictions on abstract Walrasian models. Add to this all of the different applications of RPT that are discussed in various chapters of *Revealed Preference Theory* and we begin to get a sense of the richness of revealed preference research program and the various areas of economics it has touched.

4. Methodological Issues

In this section I want to raise some red flags about Chambers and Echenique's Popperian falsificationism, but also point out some of the ideas within contemporary philosophy of economics that might provide a more suitable philosophical prism through which to view RPT. But even though I will be relatively critical in this section, I will start with a positive methodological point. The issue is the *meaning* of the terms like utility and preference.

Chambers and Echenique make it clear that they consider preferences to be *theoretical*, rather than observational, but they *do not* take the position that there is only one correct definition of preferences and/or utility. The definition that often appears in the revealed preference literature is that *preferences are simply choices*. For example:

> Though we often speak as if choices are derived from preferences, the opposite is actually the case. … preferences are merely constructs that summarize choices … 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. (Bernheim and Rangel, 2008, 158).[15]

quite a bit in common. The real tension comes from the authors' falsificationism, which, as I argue in the next section, is problematic anyway.

[15] It is useful to note that these authors may not continue to hold such views: "My own views on this topic have evolved considerably over the past fifteen years. As I no longer find myself in complete agreement with all the positions I have taken previously, this paper is *not* simply a reiteration of my prior efforts (such as Bernheim and Rangel, 2009 …)" (Bernheim, 2016, 13).

This definition denies that preferences cause or determine (even with other factors like constraints and beliefs) choices. This is the causal utility fallacy

> it isn't true that Pandora chooses b rather than a *because* the utility of b 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* (Binmore, 2009, 19).

The problem is that preferences and utility have meant, and now mean, many things in economics. There are different views among different economists and within different subfields. The traditional view is that preferences are subjective mental states that, along with beliefs and constraints, determine choices (e.g. Hausman, 2012). But there are many others views: mental states reflecting subjective feelings of pleasure and pain (many early neoclassical economists), preferences based on externalism about the context of intentional attitude ascriptions (Ross, 2014), views that distinguish between "experienced" utility and "decision" utility (Kahneman, Wakker and Sarin, 1997), and many others. In addition, there are views based on less dramatic distinctions: that preferences in economics are only "considered" preferences, "all-things-considered" preferences, "latent" preferences, "purified" preferences, "laundered" preferences, etc. The problem is that these discussions are quite controversial and often RPT is criticized not because of substantive disagreement about theory or empirical application, but rather because the particular critic identifies RPT exclusively with one conception of preference/utility. It seems that in a book that was written to survey the field and provide a formalism that unifies the existing results, restricting the discussion to one particular characterization of preference would detract from the book's usefulness and potential audience. Thus, I think *not* making a philosophical commitment on this matter was a very good idea.

On the other hand, I have a very different assessment of the book's falsificationist position. Maybe, as in the case of the meaning of preference, it would have been better to *avoid any particular stance* on what science *must necessarily be*—after all it is a book about RPT, not a general treatise on scientific knowledge—but even if some philosophical commitment seemed necessary, falsificationism, particularly the logical falsificationism discussed in the book, is not a good fit.

The authors discuss Popper's falsificationist philosophy of science in various places,[16] but even though they are fairly assertive about

[16] I would note that the paper Chambers, Echenique, and Shmaya (2014) is also particularly useful. The paper presents the same basic argument, but since it is an entire paper dedicated to the methodological issues they were able to provide more detail than in chapter 13. I would also note that there is an expanding litera-

falsificationism being the right framework for (at least RPT) economics, they only cite Popper's *The Logic of Scientific Discovery* ([1934] 1959) and provide no information about how they came to adopt such strong, and now rather unusual, position. One of the problems is that *The Logic of Scientific Discovery* (hereafter *LSD*) was originally published in 1934—the 1959 text was the first English translation—and it certainly does not represent Popper's mature views on philosophy of science. Works like Popper (1965) are generally considered his mature philosophy of science, with those like Popper (1972; 1994) representing his later views. In his mature philosophy of science he moved away from logical falsificationism (discussed below) and toward a more judgment- or decision-based falsificationism called conventional or practical falsificationism, and still later he moved even farther away toward critical rationalism (which is a Socrates-inspired general conception of rationality which subsumes falsificationism as a special case of rationality when certain specific conditions are met). But it is not necessary to go into Popper's later views since the falsificationism that seems to attract Chambers and Echenique is logical falsificationism and Popper rejected it as a guide for scientific practice *even in LSD*.

For Popper, science is not inductive, building up scientific theories from empirical evidence, but rather deductive, deducing certain observable implications from the theory and rejecting it when the evidence conflicts with the theory. At any point in time, our best science consists of all theories that are falsifiable, have been subjected to severe empirical testing, but have survived and not been falsified. A scientific theory that makes a *universal* claim (say all Xs are Ys) is falsifiable, while an *existential* claim (say there exists an X that is a Y) is not falsifiable. From a falsificationist point of view a necessary condition for a theory to be scientific is that it has falsifiable *empirical content*—empirical basic statements that conflict with the theory—and thus existential theories are not scientific. As the authors explain:

> Popper offers the example of the theory that claims 'all swans are white.' This theory is universal, in the sense that it states a property of all swans, or 'universally quantifies over swans.' It is easy to see that, in principle, such a theory can be falsified by finding a single swan that is not white. Contrast with Popper's example of an *existential* theory: that 'there exists a black swan.' The existential theory cannot be falsified. Falsifying the theory would involve collecting all possible swans and verifying that each is not black. We could only do this if we could somehow be sure to have exhaustively checked all the swans in the universe. (186)

Chambers and Echenique endorse this falsificationist view of what demarcates scientific theories from non-scientific theories, and explain how these issues connect up with RPT and Afriat's theorem.

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ture on formal approaches to falsificationism based on economics in general and RPT in particular. See Gradwohl and Shmaya (2015) for instance.

> This approach studies the revealed preference implications of a model by studying what the model rules out. Given data, we want to check whether or not these data falsify the model in question. A canonical example is the equivalence of the generalized axiom of revealed preference (GARP) and rationalizability. GARP is a statement that rules out 'revealed preference cycles,' any demonstration of which falsifies the model.' (xv)

If a choice theory is scientific it must be potentially falsifiable and GARP generates empirical predictions that could be tested; "there is a preference cycle" is an empirical statement that is in conflict with the theory. The problem is that "there exists a preference cycle" is an existential claim. So what is the solution to finding empirical content that would potentially falsify the rational choice theory and make it scientific? Afriat's theorem provides the solution.

> consider Afriat's Theorem: this beautiful theorem establishes that GARP, satisfaction of the Afriat inequalities, and consistency with the maximization of a utility function are all equivalent. Thus, Afriat's Theorem not only provides a direct method of falsifying the model (GARP), but a simple method of verifying when the model is not refuted (the Afriat inequalities) … In practice, however, the more difficult problem is often in exhibiting classes of data which refute the given model, eliminating all unobservable and theoretical concepts. (xvi)

So the main philosophical issues the authors emphasize are: i) configuration of the theory so that it is in universal, not existential form, ii) the contribution of Afriat's theorem to i), and iii) elimination of theoretical concepts from the observation statements that constitute the empirical basis. Issue iii) concerns axiomatization of the theory in a way that its structures/relations are purely in terms of observables, which, for the authors involves "Universal Negation of Conjunction of Atomic Formulas" (UNAF) (187, 191). RPT satisfies all of these conditions but it is not satisfied by rational choice theories which take preferences as givens.

Chambers and Echenique are undoubtedly correct in all of their technical argument about empirical content, universality, existential claims, UNAF, and all that, but I would like to question what kind of a methodological position they are asserting. What does such falsifiability *do* for RPT, for economics, or for the growth of economic knowledge? After all, philosophy of science and economic methodology should provide epistemic and methodological insights into scientific practices that will increase the growth of knowledge. In the interest of the principle of charity, I will consider only Popper's philosophy of science and not compare Popperian ideas with other, particularly later, philosophical positions. And, in addition, since Popper's philosophy of science clearly evolved over time—and generally softened by moving away from his early concern with purely logical

relations between sentences—I will only discuss what Popper said in *LSD*.[17]

The answer to the "what does such falsifiability *do*?" question, is *demarcation*—demarcating science from non-science—and *only* demarcation. Popper's famous *demarcation criterion* requires that a theory is *scientific* only if it is empirically falsifiable, that is "its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience*" (Popper, 1968, 40-41). The authors' logical analysis demonstrates that RPT is *Science* by this Popperian standard, but this is not methodological advice that economists can do any real work with. Demarcation alone doesn't tell you anything about scientific knowledge—how to get it, how much you have, how to get it at a faster rate—it is only a precondition for scientific knowledge (a necessary condition). How does this help empirical practitioners of RPT decide how many and of what types the deviations from the theory's predictions actually matter? How does it help practitioners decide the reliability of RPT inferences for public policy? There was once a time, particularly during the Cold War, when economics was a battle ground of various schools of thought with each claiming that what they were doing was "Science" while what other schools of thought were doing was not. But these days seem to be long gone. There is more diversity within the mainstream today (see for instance Colander et al. 2005; Davis, 2006, 2008), but the disagreements are no longer about demarcation—science vs non-science—they are about *how these different scientific approaches can be improved*: economic science that predicts better, explains better, provides better understanding, can be used reliably in policy and other interventions, and so on. Demarcation is not useful for these problems, and more importantly, Popper *recognized this*, even in *LSD*.

Popper clearly recognized what philosophers of science call the Duhem-Quine problem: that scientific theories are never tested in isolation but always involve a cluster of auxiliary theories and initial conditions: a "test system." If test system TS implies the empirical prediction A, and ~A is the case, then this only says that TS, the test system, is falsified (sometimes called the problem of "where to aim the arrow of Modus Tollens") it does not tell you which of its many parts is responsible and should be changed or removed. Practical falsifiability, actually putting falsificationism to work in science, is not a logical exercise; it involves *decisions* and they are generally social decisions that must be settled within the relevant scientific communi-

[17] My quotes will come from Popper (1968), but the text is the same as Popper (1959). There are added footnotes and appendices, but they are clearly marked with *.

ty. Popper, even in *LSD*, called this the conventional, or practical, aspect of falsification. As he explained:

> From a logical point of view, the testing of a theory depends upon basic statements whose acceptance or rejection, in its turn, depends upon our *decisions*. Thus it is *decisions* which settle the fate of theories. To this extent my answer to the question, "how do we select a theory" resembles that given by the conventionalist; and like him I say that this choice is in part determined by considerations of utility. (Popper, 1968, 108)

> I am quite ready to admit that there is a need for a purely logical analysis of theories, for an analysis which takes no account of how they change and develop. *But this kind of analysis does not elucidate those aspects of the empirical sciences which I, for one, so highly prize* … In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies … will disappear with the advance of our understanding … *If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are*. (ibid., 50, emphasis added)

These are epistemic decisions that need to be made by members of the relevant scientific community, but they are also decisions that can benefit from methodological advice. Chambers and Echenique discuss some of the practical problems and solutions of ERPT in chapter 5, and over time some of these techniques have taken the lead and some have fallen behind, in the practice of RPT-based empirical work; it seems reasonable that there were good methodological reasons for these decisions. It is such methodological decisions—even from within Popperian philosophy of science—that matter to the advancement of science, to RPT, or any other economic theory, and despite the authors' impressive display of logical analysis, their philosophical results offer no help with such decisions.[18]

It is somewhat ironic that precisely these criticisms of logical falsificationism and demarcation were raised repeatedly by those working in economic methodology during the 1980s and 1990s, and a fairly stable consensus was reached that this particular aspect of Popperian

[18] Although my main point is the authors' singular concern with *logical* falsifiability, the point has been made that even successful all-or-nothing tests of GARP—and thus rational choice theory (RCT)—only provide a weak as-if explanation of consumer choice. As Moscati and Tubaro (2011, 119) explain: "the as-if defense alone provides only 'weak' epistemological support for RCT it shows only that RCT offers *a possible* explanation of demand behavior without ruling out other possible explanations. A 'strong' as-if defense of RCT would require showing not only that RCT provides a possible explanation of consumer demand but also that it offers the *best* available explanation of it." This is an important issue about falsificationism and RPT, although it is an issue that goes beyond the current paper and is best left for another occasion.

philosophy had almost nothing to offer economists.[19] Criticisms were raised in both surveys of economic methodology and papers specifically concerned with Popper and economics (e.g. Caldwell, 1982, 231-243; Hands, 2001, 276-286; Hausman, 1988; 1992, 172-191; Latsis, 1983, and many others). Even the economists who were defenders of falsificationism (e.g. Blaug, 1992; Hutchison, 1981) focused on Popper's ideas about the growth of scientific knowledge and practical falsificationism. The economist who is probably the longest running defender of Popperian economic methodology, Lawrence Boland (1982, 2003), did (and does) *not* endorse falsificationism, because for him, as well as a number of others within the Popperian community, Popper's actual view of science is his critical rationalism, not falsificationism. Recent books in philosophy of economics that are self-consciously informed about, and philosophically sensitive to, recent developments in economic theory and practice (e.g. Reiss, 2013 and Ross, 2014) mention Popper only a few times in passing and do not even discuss falsificationism in any serious way. Finally, but in some sense the most important, there is what Popper himself actually said about economics and other social sciences that employ the rationally principle and what he called "Situational Analysis" (e.g. Popper, 1961; 1994, 154-184). Much of the literature on this topic (e.g. Caldwell, 1991; Hands, 1985a; Koertge, 1975) emphasizes that there are a number of tensions between the situational analysis that Popper identifies with economics, and falsificationist philosophy of natural science.[20] And this is just a small sample of the relevant literature.

While this seems to be a fairly extended criticism, it is important to remember that the book is really about RPT and not philosophy of science. And it is an excellent book on RPT that covers a vast about of literature, presents it with great care and mathematical sophistication, discusses the rapidly growing empirical revealed preference literature, and connects RPT to other areas of economic theory. Given the relative weighting of RPT, as compared to methodological discussion, my criticisms of the methodology may be outsized, but it is the historical and philosophical aspects of RPT that will be of the most interest to readers of this journal.

So if I am critical of how the authors use a part of Popperian philosophy of science in their effort to explain and understand RPT, what philosophical framework would I recommend? Just as I did not want to see the authors insist on one meaning for the word "prefer-

[19] Note I say 'this particular aspect" of Popperian philosophy. This is in no way a criticism of Popperian philosophy in general; there is certainly much more to the philosophical contributions of Karl Popper than logical falsificationism.

[20] Economists working in the field of economic methodology often find it surprising that economists who are not generally interested in philosophy or methodology, but are attracted, for one reason or another, to the work of Karl Popper, seldom demonstrate any interest in what Popper *actually said about economics*.

ence," I do not think this is the place to make arguments for one specific approach within contemporary philosophy of economics. But what I will say is that in recent decades the philosophy of economics has expanded in a number of areas of inquiry that might be useful for better understanding, and/or assessing, potential moves within RPT. One development within the philosophy of science is a turn toward modeling. Questions about explanation, testing, epistemic import, and so forth, are now just as likely to be about scientific models as scientific theories. Within this modeling literature one of the key questions is idealization and abstraction; how exactly do highly idealized models containing many false assumptions—often false assumptions that are ineliminable to key results of the model—explain or make other types of epistemic contributions? As part of this literature, a taxonomy of modeling assumptions is developing. What are the various types of idealized assumptions and what role do each play? In what ways can these various assumptions be changed and not damage (or perhaps even increase) the explanatory power, or the measure of some other cognitive virtues of the model? Within the domain of scientific explanation, the traditional Deductive-Nomological model has surrendered to multiple notions of scientific explanation each with its own value in the context of certain scientific endeavors. One type of explanation that has been applied to various economic theories are "how-possibly" explanations which seem to fit certain types of economic model building.[21]

As just one example of this literature, the topic of the role of idealized auxiliary assumptions gets a substantial amount of attention in recent philosophical discussions about theoretical models in economics. These are the auxiliary assumptions that were mentioned as part of the test system in the above discussion of the Duhem-Quine problem. All models have them, they can take on a variety of different forms, and in highly idealized models they are often false and known to be false by the modeler. In some cases they are merely tractability assumptions—say continuity or a boundary condition of some type—that may be perfectly innocent in a wide range of models, but in other cases—for example concavity or convexity in some economic models—that are ineliminable since comparative statics results often depend on such assumptions. How these auxiliary assumptions work in various models and how particular auxiliary assumptions impact the results of the model are serious issues that are increasingly being dis-

[21] This literature is too extensive to cite, but I will just draw attention to some collections of relevant papers: the 2009 *Erkenntnis* symposium on credibility and idealization (see Grüne-Yanoff, 2009 for an introduction) and the 2013 *Journal of Economic Methodology* symposium on idealization and explanation (see Reiss, 2012 for the original paper). I would also note some general discussions of philosophical issues in scientific modeling: Morgan and Knuuttila (2012) and Weisberg (2007 and 2013).

cussed within the philosophy of economics. One of the ways that auxiliary assumptions can often be indirectly tested for their impact on the implications of a particular idealized model is *derivational robustness*. There are many specific forms of derivational robustness, but the general idea is that if the results of the model persist over a wide range of different auxiliary assumptions then they must be the result of core causal factors and auxiliary assumptions are less of a concern.[22]

What is interesting is that the authors seem to come very close to discussing such issues regarding auxiliary assumptions in their comment on "Relative Theories" (192 and also see Chambers, Echenique, and Shmaya, 2014). They say:

> Often a researcher wants to take certain assumptions as given and find the empirical content of his theory *relative to* such given assumptions. For example, decision theorists often regard axioms such as continuity or completeness as 'technical'; they want to build on such axioms by studying other, more substantive, axioms. (Chambers, Echenique, and Shmaya 2014, 2311)

This certainly sounds like auxiliary assumptions that may be merely tractability assumptions and if so the concern would seem to be how these results affect the epistemic value of the model. But the authors never try to analyze such practical problems. What they show is how a theory T can be characterized as relative to another theory T′ and "that the empirical content of T relative to T′ admits an UNCAF axiomatization relative to T′′." (ibid.) As long as the focus is simply the existence of empirical content, such methodological commentary neither provides guidance for practicing economists nor gets at the more serious philosophical issues about auxiliary assumptions: more serious issues that are currently being discussed within the philosophy of economics literature.[23]

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[22] Some of the literature that discusses auxiliary assumptions, tractability assumptions, derivational robustness, and related topics in idealized economic models includes: Aydinonat and Ylikoski (2014), Hands (2016b), Hindriks (2006), Kuorikoski, Lehtinen, and Marchionni (2010 and 2012). Lehtinen (2017), Odenbaugh and Alexandrova (2011), and Verreault-Julien (2017).

[23] One final thought on philosophy of science: the authors' longer philosophical paper makes the remark that "philosophy of science since Popper has focused on the sociology of actual research" (Chambers, Echenique, and Shmaya, 2014, 2315). Hopefully the above discussion makes it clear this statement is quite inaccurate. In addition to philosophy of science there is a vast literature in science studies and the sociology of science, but the two fields are quite different; philosophy of science focuses on justification and sociological approaches focus on explanation. These fields often function as complements as well as substitutes, and there is more complementarity now than in the early 1930s when Popper was writing *LSD*, but they are separate. Philosophy of science is more naturalistic; more informed about the actual practice of science; and more sensitive to the fact that the scientific community is actually a community; than it was in Popper's youth, but

Conclusion

This paper has discussed *Revealed Preference Theory* by Christopher Chambers and Federico Echenique in detail. A brief history of RPT was provided which overlapped with the authors' presentation, but also provided additional historical detail on various aspects of RPT. The contents of the book were surveyed with an emphasis on the particular topics that would be most relevant to the last two sections of the paper. The relationship between RPT and GET was also examined. The authors discuss the RPT-GET connection, starting with Brown and Matzkin (1996), but I also discussed connections in the earlier literature and emphasized the key role of the SMD results for both the earlier and later literature. Finally, section 4 offered some criticisms of the author's methodological position.

The book is an extremely important contribution to the literature, and unlike most other texts which summarize the research in areas of mathematical economics, there is a serious effort to make the reader aware that contemporary RPT is a tool for applied economists that competes with the ever-expanding set of empirical tools now available. Connecting up with the contemporary empirical literature, but also touching on both traditional demand theory and GET, and presenting everything in a very sophisticated and unified formalism, makes the book an extremely important contribution to the literature. It shows that RPT can run with the best of the expanding pack of empirical approaches within contemporary economics; and do so while at the same time staying grounded in the core traditions of modern economics: rational demand and Walrasian equilibrium.

My praise for the theoretical content of this book, its comprehensive coverage including the applied literature, and the careful sophistication of the mathematics, is offset to some degree by its falsificationist methodological commitments. Of course, since the book is about ninety-five percent RPT, and only about five percent philosophy of economics, that still leaves me with a very positive assessment. And, I would add, despite my criticism of its logical falsificationism, I think it is a book that is packed with things that are interesting, important, and not as well-known as they should be, to scholars in the history and philosophy of economics. The empirical turn that has taken place within RPT theory in recent decades is extremely important. It is important because it is consistent with the empirical turn more generally within contemporary economics, but also, because unlike so much of contemporary empirical research—which is, by the traditional standards of economic theory, relatively atheoretical (Backhouse and Cherrier, 2016) – ERPT connects up in a deep and systematic way with how economists have thought about individual

it is still about scientific knowledge: what it is, how we get it, and how we can get more of it.

choice, demand theory, and even general equilibrium for at least a century. Now of course that traditional, dare I say neoclassical, vision, may be on its last legs, but before the economics profession commits to its retirement, it seems only fair that they have a chance to see what that familiar framework can do when it is working with fully upgraded empirical tools and data. And to me, that is exactly what ERPT offers. My plea is only that the epistemic and methodological commitments of RPT be upgraded to the same degree as its economic theory and empirical practice. In closing I would like to invert one of Blaug's most well-known expressions: that economists should practice (in theory and empirical work) what they preach (in methodology). My message to Chambers and Echenique is the reverse of this; it is not to practice what you preach, but rather to *preach what you practice*.

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