The Road to Rationalization: A History of "Where the Empirical Lives" (Or Has Lived) in Consumer Choice Theory*

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0. Introduction

This paper is about the history of consumer choice theory. The theory originated in the neoclassical/marginalist revolution of the 1870s which, unlike classical economics, focused primarily on demand and individual utility maximization. The concept of utility it employed originated in utilitarianism and was subjective, cardinal, and hedonistic (based on individual feelings of pleasure and pain). The details of various versions of this early theory varied among different contributors – William Stanley Jevons, Leon Walras, and Carl Menger, and others – but the broad theoretical framework provided a foundation for the science of individual and market behavior, as well as the normative science of welfare economics.

By the first quarter of the twentieth century this hedonism-based theory had come to be widely criticized. Primarily in response to these criticisms, the theory was substantially revised during the ordinal revolution of the 1930s and 1940s. The concept of subjective utility maximization was retained, but several features of the earlier theory were changed. Cardinal utility was abandoned in favor of purely ordinal – better or worse – rankings, utility lost its connection to individual hedonistic feelings of pleasure and pain, marginal utility was de-emphasized (since diminishing marginal utility is a cardinal concept), and interpersonal utility comparisons were rejected as the basis for welfare economics. Although the early neoclassicals had recognized that the agent's constraints had an impact on choice behavior, there was no consensus about exactly how to build this into the utility-maximizing framework; with the ordinal revolution the linear budget constraint became a standard feature of consumer choice theory. Helped along by key papers such as Hicks and Allen (1934) and Slutsky (1915), foundational contributions like Pareto (2014[1909]), and influential texts such as Hicks (1939) and Samuelson (1947), by the middle of the twentieth century ordinal utility theory had become *the* economic theory of individual consumer choice. I will call this theoretical framework *ordinal utility theory* (hereafter OUT); and while this way of modeling, teaching, and thinking about, consumer choice is still with us, it came to be challenged by another framework during the latter half of the twentieth century.

The third approach to consumer choice theory was introduced by Paul Samuelson in 1938, and came to be called *revealed preference theory* (hereafter RPT). Although there are several versions of RPT, all share the same core idea of grounding choice theory in a type of *consistency*: basically that if an individual chooses A when B is available (A is revealed preferred to B), then B would only be chosen if A were not available. As we will see below, the question of the exact relationship between RPT and OUT remains contested, so at this point I will

simply note how it is currently depicted in advanced textbooks. Mas-Colell, Whinston, and Green (1995) explain it in the following way:

> There are two distinct approaches to modeling individual choice behavior. The first, … treats the decision maker's tastes, as summarized in her *preference relation*, as the primitive characteristic of the individual. … The second approach, … treats the individual's choice behavior as the primitive feature and proceeds by making assumptions directly concerning this behavior … This choice-based approach has several attractive features. … It … makes assumptions about objects that are directly observable (choice behavior) rather than about things that are not (preferences). (Mas-Colell, Whinston, and Green, 1995, p. 5)

This brief sketch was offered as background to the topic of this paper: the empirical content of consumer choice theory. The connection is that each of these changes was initiated in part in order to make consumer choice theory more empirical or observational. Hedonistic utility involves the consciousness of the agent and is not directly observable, and this fact raised questions about the empirical foundations of early neoclassical theory. Ordinal utility remained in the mind of the agent, but the economists of the ordinal revolution argued that ordinal preferences were more observational and gave OUT much better empirical grounding than the earlier theory. And this trajectory continued with RPT, which was originally intended to move consumer choice theory entirely away from preference and utility. But despite the perception of improvements in empirical foundations driving theory change, those advocating and using different versions of choice theory never achieved any consensus about exactly what the empirical content was for any of these theoretical approaches.

The goal of this paper is to historically document this ambiguity regarding the empirical content of consumer choice theory. In particular, the paper will examine how theoretical terms like utility and preference were thought to connect to the relevant empirical evidence. I call this the problem of "where the empirical lives" in consumer choice theory and I will argue that it has lived in many different places: not only between the main approaches, but also within different versions of each of them. Although all three moves were motivated to some extent by the desire to give the theory more solid empirical foundations, there was consistently less agreement about how theoretical terms like utility and preference were supposed to hook up to the empirical evidence than about the structural features and theoretical axioms that had ostensibly evolved because of the improvement in the empirical foundations.

In order to clarify the paper's historical approach it is useful to contrast it with three other approaches that could have been used to address this question. One

would be to rationally reconstruct the history of consumer choice theory in terms of some particular normative philosophy of science. Such an approach would examine not only what the empirical content was considered to be for the relevant economists, but to *assess the adequacy* of that empirical content, by asking whether it was actually observable, appropriately linked to the theory, successfully tested, etc. The aim would be to pass judgment on the scientific character of the specific theories involved.[1] Such philosophical justification (or critique) is not the goal here. Ideas about scientific knowledge will certainly be involved in the story, but only because such ideas often influence the ideas of economists. A second, and very different, approach would be to embed the various characterizations of the empirical content of consumer choice theory into the broader social, political, and epistemic context in the way that empirical facts have been historicized in works like Poovey (1998) or Shapin and Schaffer (1985): i.e. to write a constructivist history of the neoclassical fact. This is also not the goal here. It would be a much larger project that would need to expand the discussion well beyond the economists and economic theory examined here. The final approach to note is the work on *observation* and (or vs.) *observing* in the history of economics by Harro Maas and Mary Morgan (2012). They emphasize the difference between observing (as a process) and an observation (as an outcome) and use it to help us understand various episodes in the history of economics, particularly the transformation from earlier observing practices to modern statistical observations. I will not employ their exact distinction, but I will employ a related one: the distinction between being *observable* (or potentially observable under ideal circumstances) and actually being *observed*. As I will argue below, this distinction has played an important role in how economists think about empirical evidence in consumer choice theory, particularly RPT.

In an effort to keep the project manageable, I will only discuss the literature from the beginning of the twentieth century. In addition, I will focus exclusively on consumer choice under conditions of certainty and parametric prices. Although adaptations of this core framework to risk, probability and expected utility; intertemporal choice and discounting; strategic choice; expectations; and other topics; have played an important role in the development of modern economics, they add layers of complexity that are not necessary here. Similarly, there will be almost no discussion of welfare economics; such normative issues certainly influenced the history of consumer choice theory, but the focus here will be choice theory as an attempt to describe, predict, or explain the behavior of individual consumers. Finally, even though there has been a significant amount of important work on *economic models* in the history and philosophy of economics in recent years,[2] I will treat the terms "model" and "theory" roughly as

[1] See chapter 6 of Blaug (1980) as an example of such an approach applied to OUT.

[2] Some of this recent work on models is more historical and some is more philosophical, but it all has aspects of both. This literature includes Grüne-Yanoff (2009, 2013), Mäki (2009, 2013), Morgan (2012), Morgan and Knuuttila (2012), Sugden (2009) and many others.

substitutes since that is how they were generally used by the economists discussed here; there was a general tendency to think of a theory as more general/abstract and a model as a more specific/concrete, but that was not always the case, and even when it was, the distinction does not seem to do any heavy-lifting with respect to the issues of concern here.

1. Some Background Remarks

Although this paper is historical it is useful to spend a little time reviewing some arguments about the structure of scientific theories from the philosophy of science literature. Even though it is generally agreed that the positivist-inspired "Received View" (Suppe, 1977) within philosophy of science did not provide an adequate characterization of scientific theorizing (either descriptively or normatively), it is also clear that it often conditioned the way that scientists, including economists, thought about things like the relationship between theory and evidence. Given this, a brief discussion of this background literature seems reasonable. Pat Suppes provides a nice summary:

> A scientific theory consists of two parts. One part is an abstract logical calculus. In addition to the vocabulary of logic, this calculus includes the primitive symbols of the theory, and the logical structure of the theory is fixed by stating the axioms or postulates of the theory in terms of its primitive symbols. For many theories the primitive symbols will be thought of as theoretical terms like "electron" or "particle" that are not possible to relate in any simple way to observable phenomena.
>
> The second part of the theory is a set of rules that assign an empirical content to the logical calculus by providing what are usually called "co-ordinating definitions" or "empirical interpretations" for at least some of the primitive and defined symbols of the calculus. It is always emphasized that the first part alone is not sufficient to define a scientific theory, for without a systematic specification of the intended empirical interpretation of the theory, it is not possible in any sense to evaluate the theory as a part of science, although it can be studied simply as a piece of pure mathematics. (Suppes, 1967, p. 56)

Consider how this characterization scientific theories fits the basic textbook version of OUT. The primitives of the theory include vectors of commodity bundles $x = (x_1, x_2, \ldots x_n)$, the associated price vectors $p = (p_1, p_2, \ldots p_n)$, the consumer's money income (M), and the consumer's utility function U(x) (or preferences that can be represented by a utility function). Under the standard assumptions on the variables (x's), the parameters (M and the p's), the budget

constraint, and the objective function (U), the consumer choice problem – the main axiom of the theory – is a well-defined constrained optimization problem:

$$\text{Max } U(x)$$
$$\text{subject to: } \sum_i p_i x_i = M.$$

The solution to the problem is a set of *n consumer demand functions*:

$$h_i = h_i(p, M) \text{ for all } i = 1, 2, \ldots, n.$$

The four main implications of the theory are that 1) each h_i is homogeneous of degree zero in prices and money income, and 2) three restrictions on the Slutsky matrix: 2a) $S_{ii} < 0$ for all i, 2b) $S_{ij} = S_{ji}$ for all $i \neq j$, and 2c) S is negative semi-definite.[3] Where the elements of the Slutsky matrix are defined by the Slutsky equation:

$$S_{ij} = \partial h_i/\partial p_j + h_j(\partial h_i/\partial M) \text{ for all i and j.}$$

The primitives are symbols (x's, p's, M, and U) and axioms (the linearity of the budget constraint and utility maximization). The implications of the theory (or theorems) are the four restrictions on demand functions given by 1) & 2a-c).

Although this axiomatic structure seems to fit with the first paragraph of the above Suppes quote, what about the second paragraph? What about the "set of rules that assign an empirical content to the logical calculus," that make this a scientific theory rather than "a piece of pure mathematics"? According to the Received View these "empirical interpretations" (or correspondence rules) were necessary for a formal structure to qualify as an empirical scientific theory:

> "However, a deductive system can function as a theory in empirical science only if it has been given an *interpretation* by reference to empirical phenomena. We may think of such interpretation as being effected by the specification of a set of *interpretative sentences*, which connect certain terms of the theoretical vocabulary with observational terms." (Hempel, 1965, p. 184)

The requirement that scientific theories hook up in some way with the empirical realm seems reasonable, but the "consumer choice theory" specified above doesn't look like it satisfies this requirement. In order to be a scientific theory the theoretical terms employed – particularly non-observational terms like utility and preference – would need to be connected in some systematic way with the world of empirical observations and such a connection is not provided by the above "theory."

[3] See any advanced microeconomics textbook.

The necessity of an empirical connection provided by correspondence rules may seem reasonable, but understanding exactly how this works in real science often proves to be extremely difficult. In fact, this problem – the problem of correspondence rules – became one of the serious difficulties with the Received View of scientific theories. The bottom line is that the theoretical terms of any scientific theory must be connected to things the relevant scientific community regard as empirical, but exactly how that is done is quite context-specific. It depends on the relevant community; the particular theory or version of the theory; various intermediate theories (theories of data, experiment, computation, etc.); the available technology, mathematics, data, other constraints; and a host of other factors.

With this bit of background it is now possible to restate the goals of this paper. It is to provide a detailed historical discussion of the variety of different ways that economists have tried to connect the theoretical terms of consumer choice theory – particularly "preference" and "utility" – with "the empirical," "the observational," or "the observed." To clarify exactly what this does and does not mean, it is useful to make a distinction between two different aspects of the theoretical-empirical nexus. The first concerns the *empirical content* of the theory: the question of what parts of the theory are intended (or supposed, or assumed, …) to be empirical according to the economists who proposed and endorsed it. The second is the question of whether these things have actually been observed in the way the theory suggests. The first involves the *existence of an empirical transmission mechanism* – the empirical content – while the second is whether that transmission mechanism actually works; that is whether the evidence actually supports the theory. This paper will focus on the former, existence, issue. In other words, this is not a paper about testing economic theories and the success or failure of these tests. It is a paper about the variety of different ways that economists characterized the empirical content of consumer choice theory.

2. Observational Ambiguity in Consumer Choice Theory I (OUT)

Although it is certainly possible to identify observational ambiguity in the work of early neoclassical economists,[4] I will move directly to one of the key figures in the ordinalist revolution: Vilfredo Pareto. Although substantive debate remains about how Pareto's views on such matters changed over his various economic works – Bruni and Guala 2001, Chipman 1976, Giocoli 2003, Lenfant 2012, Mandler 1999, Marchionatti and Gambino 1997, McLure 2005, Montesano 2006, Moscati 2007b, Weber 1999a, 2001, and others[5] – I will focus on the primary way he dealt with the matter in the *Manual* originally published in 1909 (English

[4] This is particularly true for William Stanley Jevons (1871). See for example, Maas (2005), Schabas (1990), and White (1989, 1994).

[5] His work was "as rich as it is ambiguous" (Moscati, 2007b, p. 136).

language variorum edition 2014). Here Pareto viewed individuals as motivated by subjective utility, but that he did not consider such utility, or the measurability thereof, to be necessary for (or desirable in) a scientific theory of consumer choice: the position Michael Mandler calls "cardinality, but not cardinal measurability" (1999, p. 115). This said, Pareto differed from early neoclassical economists like Jevons in at least two important ways. First, he did not share the British utilitarianism or the desire for hedonistic-based policy tools, but second, and more importantly here, he introduced a theoretical term that he argued was directly observable – *indifference curves* (indifference lines) – the various bundles of goods that were "equivalent for this individual, i.e., for which the choice is *a matter of indifference*" (Pareto, 2014, p. 309). As Pareto explained, indifference curves were previously introduced in Edgeworth (1881), but Edgeworth used them in a different way:

> The concepts of indifference lines and of preference lines were introduced into the science by Professor F. Y. Edgeworth. He took as his starting point the concept of *utility* (ophelimity), which he assumed to be a known quantity, and he deduced from it the definition of these lines. I have inverted the problem. I have shown that by starting from the notion of indifference lines – *a concept that is given directly by experience* – one can succeed in determining economic equilibrium, and work back to certain functions, one of which is ophelimity, if it exists. (Ibid., p. 309, emphasis added)

For Pareto, at least in the *Manual*, is was the indifference curve, not the utility function, that was the primitive of his theory of economic behavior and provided its empirical contact point ("given directly by experience").[6]

> … this whole theory, … *rests on only one fact of experience*, namely, the determination of the quantities of goods that form *combinations to which an individual is indifferent* … The theory of economic science thus acquires the rigor of rational mechanics; it deduces its conclusions from experience, *without bringing in any metaphysical entity*." (ibid., p. 79, emphasis added)

Pareto explains how indifference curves achieve this significant goal in a theoretical appendix. His argument is that since indifference curves (indifference surfaces in higher dimensions) were observable, the differential equation that represented the slope of the line connecting any two points on an indifference surface also inherited that empirical significance (it too "could be obtained

[6] Pareto is not very clear about exactly how indifference curves are "given by experience"; "he used the word *experiment* in a broad and fluctuating sense" (Lenfant, 2012, p. 119).

directly from observation" ibid. p. 393). Under the assumption of *integrability*,[7] this differential equation in turn passed that empirical significance on to the associated utility index (ordinal utility function). In the end utility functions were empirically meaningful because they could be recovered (up to a monotonic transformation) from differential equations derived from observable indifference surfaces (Hands 2006).[8] The differences between the views of Pareto and early neoclassicals like Jevons on this matter are perhaps best explained by differences in their mathematical and epistemic cultures (Maas 2005, Weintraub 2002), but for the purposes here the point is simply that they had very different conception of "where the empirical lives" in the theory of consumer choice.[9]

Two economists who have received much of the credit for initiating the ordinal revolution are John R. Hicks and R. G. D. Allen for their two joint papers on "A Reconsideration of the Theory of Value" in 1934. The relevant observables for Hicks and Allen were the little local tangent slopes at the specific chosen bundles – what Samuelson called "little thumb-tacks" (Samuelson, 1950, pp. 365-66) – or the marginal rate of substitution. This concept can certainly be related to Pareto's indifference curves, but it is different than this earlier interpretation. For Pareto it was the indifference surfaces that fill the entire choice space (not their tangents), while for Hicks and Allen it was the marginal rate of substitution (slope) at each chosen point in the choice space. These are different notions of how the theoretical analysis, and the theoretical terms associated with that analysis

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[7] Integrability will come up a number of times below and it is in many ways a difficult topic. There have been many different conceptions of "the integrability problem" in the history of modern consumer choice theory (Hands 2006). These different conceptions are quite important in the stabilization of OUT during the 1940s and 1950s, but a discussion of these issues is not necessary here. For the purposes here, integrability conditions are conditions sufficient for *the rationalization of demand*, that is, conditions that will guarantee that a particular demand function *could have been generated by a consumer maximizing an ordinal utility function subject to a linear budget constraint* (See Mas-Colell, Whinston, and Green, 1995, pp. 75-80). The four conditions 1) & 2a-c) noted above are the implications *of utility maximization for demand* (i.e. necessary conditions), while integrability conditions are restrictions *for the rationalization of demand* (i.e. sufficient conditions), and as it turns out the main integrability condition is Slutsky symmetry (condition 2b). The integrability literature goes back to Antonelli (1886) and key papers for the contemporary version include Samuelson (1950), Hurwicz and Uzawa (1971) and Hurwicz and Richter (1979). Surveys of the technical results are provided in Chipman (1982) and Hurwicz (1971) and it is discussed in historical context in Hands (2006, 2011). Other aspects of the integrability question will be introduced as needed below.

[8] Pareto (in translation) even used the language of correspondence: "… it is possible to obtain a one-to-one correspondence between the quantities given by experience that determine the indifference lines, … and the pleasures (ophelimities) enjoyed by the individual …" (Pareto, 2014, p. 319).

[9] It is important to point out that many continued to consider indifference curves the empirical content of demand theory long after Pareto. Some of this literature will be noted below, but at this point it is a useful to mention the empirical support given by the psychologist Thurstone (1931) and the criticism of Wallis and Friedman (1942); see Lenfant (2012) for a detailed discussion.

(utility, indifference, etc.) are conceived to hook up to the empirical world and thus constitute different views of the empirical basis of demand theory.

But the story for Hicks and Allen involves even more ambiguity because the two authors disagreed about the foundations of their theory. They both took the little local slopes to be observable, but had very different interpretations of what that meant, or how it was related to, the model's other theoretical commitments (Chipman and Lenfant 2002, Fernandez-Grela 2006, Hands 2006, Samuelson 1950). For Hicks (in manner similar to Pareto) the implicit *localism* of their theory was not an important issue; the little slopes were observable, but they were just marginal rates of substitution for the continuous convex indifference curves that filled the choice space and represented level sets of the underlying utility function. The underlying utility function was ordinal, in that its indifference curves would be the same shape for any monotonic transformation of the utility function, but it was still a *utility* function. Hicks never questioned whether an ordinal utility function existed for the consumers in question; for him non-integrable demand was a "will-o'-the-wisp" (Hicks, 1939, p. 19). Later in *A Revision of Demand Theory* (1956) Hicks presents what he termed the "econometric approach" to demand theory, but the content is not significantly different from his earlier interpretation.

For Allen the interesting and important feature of their joint work was quite different. It was that their theory did not necessarily require the existence of an utility function (even an ordinal one) defined over the entire choice space; it was a theory that accommodated *non-integrable* demand. In earlier work Allen (1932) had proposed a model where the observational basis of choice theory was the *economic actions* of consumers – the consumer's movements in the choice space as exchange altered their holding of the various goods – and it was these actions (not "the motives which give rise to the actions"), that "form the subject-matter of pure economic theory" (Allen, 1932, p. 199). Following the mathematical practice in physics, these movements could be converted into differentials which resulted in the same key equations that Pareto had derived starting from indifference curves. But the underlying story about where the empirical lives in the resulting theoretical formalism is quite different. For Allen, the little local movements in the choice space were the only reasonable basis for a scientific choice theory because consumers can only "make a choice between very small changes (in the limit, infinitesimal changes) from any particular combination" (Allen, 1932, p. 297).[10] The standard integrable case characterized the consumer as having a utility function defined over the entire choice space, and for Allen, that was a very special and generally unrealistic case. Samuelson summarized the differences between Hicks and Allen on these matters in his 1950 paper on integrability of demand:

[10] See Hands (2011) for a more detailed discussion of Allen (1932).

> Allen was unacquainted with Slutsky's[11] work but refers to most of the other important early writers. He entertains the hypothesis of non-integrability ; and if I dare impute any differences to the separate components of the Hicks-Allen composite commodity, I would say that Hicks consistently rules out the non-integrability case, while Allen accepts it as the more general hypothesis. At least Allen … deals at length with non-integrability, while Hicks … goes out of his way to make it clear that he is against non-integrability. (Samuelson, 1950, p. 357)

From Allen's point of view, the important contribution of Hicks and Allen 1934 was that it subsumed *both* the integrable (Pareto's complete indifference curves) case *and also* the non-integrable case. It was a more general approach than Pareto's that allowed for the possibility of consumers making locally rational choices that would not necessarily be consistent with the existence of a traditional utility function. This generality is, for Allen, the most important contribution of their paper and what makes it an advance over the work of others in the inchoate ordinal utility tradition. The bottom line is that the empirical content of Hicks and Allen's work was different from the empirical content of the economists previously discussed, but also had the added ambiguity of being associated with two different interpretations of *why* it constituted the proper empirical basis of the theory.

Since the discussion of Allen's choice theory has introduced the subject of *non-integrable* demand theory, it is useful to briefly note this literature. I will not devote much space to it, since the topic is the history of mainstream consumer choice theory, and non-integrable demand theory was never considered to be mainstream, even though a number of influential economists including Allen, Ragnar Frisch, Nicholas Georgescu-Roegen, Oskar Morgenstern, Pareto, Samuelson, as well as mathematicians such as Griffith Evans (1930), did, at various points, contribute to the literature. Perhaps the easiest way to understand non-integrable demand theory is to think of it as a theory of individual demand that is based in some way on individual rationality, but does not characterize the consumer as having a well-ordered utility function defined over the entire choice space. In some cases non-integrable theories of demand did not involve traditional utility or preferences at all – as we will see below,

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[11] Slutsky (1915) is an extremely important work in the history of OUT – recall that three out of the four standard implications of OUT have Slutsky's name on them – but in the interest of space considerations will not be given a separate discussion here. See Chipman and Lenfant (2002) for a discussion of Slutsky's work, its context, and impact. Allen (1936) provides a clear discussion of what he saw as the differences between Slutsky (1915) and Hicks and Allen (1934) and why the latter was more general. A number of commentators (Giocoli 2003, Hands 2010, Weber 1999a and 1999b) have suggested that Slutsky was not quite the staunch ordinalist he is often taken to be.

Samuelson's earliest version of revealed preference theory was this type of non-integrable demand theory – but this also implies that many of the contemporary theories of individual behavior that travel under the labels of behavioral economics or bounded rationality, would be, if projected back into the conversation about demand theory during the 1930s and 1940s, considered non-integrable theories.[12] But there were also non-integrable theories of demand which did presuppose utility maximization, but only in a "local" way; preferences or utility functions did not order choices throughout the choice space, but only "close" to the current endowment point. Nicholas Georgescu-Roegen's theory of "directed choice" based on consumers having a "psychological threshold" that prevented the valuation of bundles a long way from the current endowment (Georgescu-Roegen 1936, 1950, 1958) is one example of such a theory, but then so is Allen (1932), and there were others.[13] These non-integrable theories represented a fairly rich tradition during the first half of the twentieth century – as Samuelson put it "it was not an 'error' in the early 1930s to have an interest in more general axiom systems than 'integrable' ones"[14] – although it faded away after the 1950s with the stabilization of OUT as *the* theory of consumer choice.[15] Of course these non-integrable theories often involved ideas about where the empirical lives that were quite different from (any of) those in the mainstream literature of concern here.

Returning to the main story line, an entirely different approach to the empirical content of demand theory has focused on testing the implications of the theory by direct econometric estimation of demand functions. The earliest versions of statistical demand analysis go back into the nineteenth century.[16] In an early survey of this literature, Henry L. Moore, who was an important contributor to such research during the first quarter of the twentieth century, called his own work and that of his statistical predecessors "the inductive statistical complement to the pure science" of economics (Moore, 1908, p. 2).[17] But Moore's research, like other early scholars engaged in finding statistical relationships between prices and quantities purchased, did not directly involve utility

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[12] For example, even though Kahneman and Tversky (1979) presented reference dependence and loss aversion within the context of risky choice, the idea is equally applicable to the choice under certainty (demand theory) discussed here. See Kahneman and Varey (1991), Knetsch (1989, 1992) and Thaler (1980). See Hands (2011) for a discussion of the relationship between the non-integrable demand theory of the 1930s and the more recent literature in behavioral economics.

[13] See Lenfant (2015) and Zamagni (1999) on Georgescu-Roegen.

[14] Samuelson letter to John Chipman, June 29, 1999, Box 22, Paul Samuelson Papers at Duke University.

[15] Although several economists continued to work on non-integrable demand theory (e.g. Katzner 1970, 1971) and more recently new mathematical techniques have been applied to the problem (e.g. Reinhard 2004, 2007).

[16] And even earlier if one counts the King-Davenant law from the late seventeenth century. See Creedy (1986), Stigler (1994), or White (1989) for discussion.

[17] Moore (1908) discusses a number of the early contributors this literature. Also see Morgan (1990), Schultz (1938), and Stigler (1939, 1954) on early statistical demand theory.

maximization and thus did not directly address the question of the empirical content of consumer choice theory as it has been discussed here. The most important early figure who was involved in the direct statistical testing of the implications of utility maximization-based consumer choice theory was Henry Schultz (1933, 1935, 1938). Schultz had been Moore's student – perhaps "Moore's only real disciple" (Stigler, 1962, p. 17) – but unlike Moore, Schultz started from consumer choice theory and saw the function of statistical demand theory as testing the empirical implications of OUT (1 & 2a-c introduced above) as well as applying the theory to the analysis of specific market prices and quantities. This approach clearly involves a shift from an upstream to a downstream notion of empirical content;[18] instead of trying to directly connect upstream theoretical terms like utility, preferences, or indifference curves with things considered observable, the theory was taken as axiomatic and its downstream observable implications were tested against the available evidence. Schultz was thus a key figure in the early development of econometrics-based demand analysis later represented by Wold and Juréen (1953), Brown and Deaton (1972), Barten (1977), Deaton and Muellbauer (1980), Blundell (1988) and others, research programs that remain active within contemporary economics.[19]

Schultz's most significant work was *The Theory and Measurement of Demand* (1938). The goal of this research was to build on Moore's research, but enhance as well as extend it by explicitly taking OUT into account. As Schultz explained key problem of statistical demand theory:

> [T]he problem is simply this: The statistical data by themselves give only one observation – a point – on the unknown demand curve or surface for each time interval. We are required, nevertheless, to

[18] In the nineteenth century when scientific laws were often viewed as being built up from empirical evidence by an *inductive process* – one black crow, two black crows, three black crows, …. All crows are black – the empirical was viewed as entering very early (*upstream*) in the process of scientific investigation. Scientific inquiry started with specific observations and the scientific laws came later as a result of inductive generalization. But by the early twentieth century, scientists and philosophers of science increasing came to think of science in a more hypothetical-deductive way where the abstract theory came first and the empirical testing (contact with the empirical) came later, *downstream*, in the investigation. With the hypothetical-deductive view, the hypothesis that all crows are black could originate from anywhere – the context of discovery was considered cognitively neutral – but the hypothesis only became a candidate for a scientific law as a result of the successfully testing (confirmation) of its empirical content. According to this view the empirical content of a scientific theory is all of its empirically observable implications (all black crows).

[19] The later econometric research of course employed much different, and improved, econometric techniques and it should also be noted that while Schulz and later economists both employ OUT, they do not employ it in exactly the same way. The common ground is the use OUT as the theoretical backdrop for empirical demand research, not the exact way it is used. See Morgan (1990) for a detailed discussion of how Schultz's fit into the broader history of statistical demand theory and econometrics more generally.

> deduce the concrete, statistical equation of the entire surface. If the form of the dynamic demand function
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> (1.1) $x_i = F(y_t, \ldots, y_n, R, t)$
>
> and the interrelations of the variables entering into it were known, the difficulty … would not be so serious; but they are not. Is there a way out?
>
> The answer is obvious. It is impossible to derive a demand curve from statistics without making some assumptions regarding the nature of the theoretical function and the interrelations of the variables." (1938, p. 61)

For Schultz the "the nature of the theoretical function and the interrelations of the variables" was provided by consumer choice theory; it guided his overall approach to statistical demand estimation and those estimates were in turn used to test the theory's empirical implications.

Although it is not necessary to go into the details of Schultz's results, some discussion is useful to situate it relative to the empirical content discussed above. Schultz estimated market demand functions for sixteen different agricultural commodities: thirteen U.S. and three Canadian. Most of these estimates were presented in part II and then used to test certain implications of consumer choice theory in part III.[20] In order to test the three Slutsky matrix implications of OUT 2a-c, it was necessary to estimate the demand for each good x_i as a function of all prices $p_1, p_2, \ldots p_n$. Once one has cross-terms on demand functions, it is possible to discuss whether various commodities are completing (complements) and competing (substitutes). Schultz tested for the symmetry (integrability) of the cross-partial derivatives (later called reciprocity conditions) of these interrelated demand functions in two separate ways. The first was to test the integrability condition from Hotelling (1932) which was symmetry on regular demand functions, i.e. $\partial x_i/\partial p_j = \partial x_j/\partial p_i$ for all $i \neq j$. He derived this in a different way than Hotelling – Schultz assumed the constancy of the marginal utility of income with respect to the price of all goods – and while he made several arguments about why this was an interesting version of symmetry to test (pp. 580-81), he also recognized that it was not one of the core empirical implications of OUT. Secondly he also tested for the standard Slutsky symmetry condition: (condition 2b: $S_{ij} = S_{ji}$ for all $i \neq j$) using the Slutsky equation in much the way that it would

[20] One difference between Schultz's approach and those previously discussed is that Schultz is working with *market*, not individual data. Since OUT is a theory of individual choice, this of course raises all sorts of issues about aggregation, representative agents, and such. These are important issues, but not of direct relevance here. Even though Schultz is using market data, his goal is to test OUT *as* a theory of individual rational choice and that makes his efforts just as much about the empirical content of consumer choice theory as efforts by others using more explicitly individualistic approaches.

be done in later literature. He considered these symmetry conditions to be key to testing the theory of consumer choice: "When these conditions are satisfied, we may be reasonably certain that the market behavior of the consumers in question is *consistent* or *rational*" (p. 599, emphasis added).[21] This of course means that for Schultz these symmetry conditions constituted the main empirical content of consumer choice theory and checking to see if the conditions held on empirically estimated demand functions was a test of the theory (or, alternatively, a test of the rationality of the consumers in question).

As it worked out the empirical results of Schultz's test of rationality were not generally successful. Several of the terms had the wrong sign and "the Hotelling condition and the corresponding Slutsky condition are of approximately the same order of magnitude" (p. 744). There are of course many reasons why such tests might be problematic, but Schultz did not give up on his teacher's project of "the inductive statistical complement to the pure science" and continued to endorse the viability of the downstream approach to consumer rationality (Mirowski and Hands 1998, Teira Serrano 2006). In general he argued that the poor results were simply problems with the available data, not the theory: "The greatest obstacle in the way of determining the extent of which actual human behavior is rational is the lack of accurate statistics on the consumption and prices of related goods" (p. 604). Schultz died unexpectedly in 1938 and his work on statistical demand theory ended with his magnum opus, but it clearly provides us with yet another example of the variety of ways that economists have thought about empirical content of consumer choice theory.

Finally before moving on to RPT, it should be noted that a small amount of experimental research on consumer choice theory appeared throughout the period 1930-1970. Ivan Moscati (2007a) provides an excellent discussion of this literature and I have nothing specific to add to his discussion. Notice that *experimental* literature on consumer choice theory means attempts to test the theory with data from *controlled laboratory experiments* rather than market or individual choice data as was the case with Schultz and the econometric tradition. Unlike Schultz's downstream empirical approach, many of the contributors to this early experimental literature – Thurstone (1931), Rousseas and Hart (1951), and MacCrimmon and Toda (1969) – returned to Pareto's emphasis on indifference curves. But unlike Pareto, who was content with the possible *observability* of indifference curves, these authors sought *observations* and

[21] Although these symmetry conditions are not the way that economists would empirically test for "consistency" or "rationality" today, in the intellectual environment of 1930s mathematical economics it is understandable. The integrability problem was in the air for theoretical economists and these symmetry conditions were sufficient for integrability. But integrability implied rationalization – that the demand functions in question could have been generated by utility maximization – and utility maximization (then, now, or in between) is how modern economists think about rationality.

constructed them directly from the responses of laboratory subjects.[22] As Moscati explains, these results were readily available to the economics profession, and yet seemed to have almost no impact on the development of consumer choice theory, or even the experimental economics literature that exploded later. It is useful to draw attention to this literature, even though it returned to Pareto's conception of empirical content, because it did so in an original way. Most importantly it was *experimental*, which constitutes a fundamental change in the idea of appropriate empirical content: experimentally derived observations rather than armchair thought experiments about what could possibly be observed. This is quite different from earlier indifference curve-based empirical content and thus another example of the diversity in the observational basis of consumer choice theory.

3. Observational Ambiguity in Consumer Choice Theory II (RPT)

The third theoretical framework for consumer choice theory is revealed preference theory (RPT). RPT is not a particular theory, but rather a broad research program containing a number of different specific subprograms (Hands 2013, 2014). Since, unlike utility theory, the revealed preference approach does have a single point of origin, it is useful to begin the discussion there.

The revealed preference program began with Samuelson (1938a). Samuelson was only twenty-three years old when it was published and it was only one of his papers on consumer choice published that year (1938b, 1938c, 1938d). The particular axiom he introduced came to be called the *Weak Axiom of Revealed Preference* (WARP). As noted above, the basic intuition was to ground consumer choice theory on a kind of consistency condition rather than utility or preference. Specifically, if p_0 and p_1 are n-dimensional price vectors, and h(p) is the consumer's demand function, so $h(p_0)$ is the quantity purchased at p_0 and $h(p_1)$ is the quantity purchased at p_1, then the consistency condition was given by:

$$\sum p_0h(p_1) \leq \sum p_0h(p_0) \rightarrow \sum p_1h(p_0) > \sum p_1h(p_1). \qquad \text{(WARP)}$$

If $h(p_1)$ was not chosen when it was affordable, then the consumer has revealed a preference for $h(p_0)$, so if $h(p_1)$ was chosen at a different price vector, it must be that $h(p_0)$ was not affordable.

Samuelson's motivation for developing this new approach was that Hicks and Allen had not gone far enough in eliminating utility from consumer choice theory, that "much of even the most modern analysis shows vestigial traces of the utility concept" (1938a, p. 61). His goal was to replace the entire utility-preference-based theory of demand with a new theory: "I propose, therefore, that

[22] There was also a small experimental literature that focused on testing the transitivity of preferences.

we start anew in direct attack upon the problem, dropping off the last vestiges of the utility analysis" (ibid., p. 62).[23] The term revealed preference was not used in the 1938 paper – the goal was to *eliminate* preferences from consumer choice theory, not *reveal* them – and in fact the word "preference" occurs only once in the paper and it is in the first paragraph when he is talking about OUT.

The main technical result in Samuelson's original paper was to demonstrate that WARP (along with the other assumptions discussed below) implied three out of the four standard implications of OUT.[24] The missing condition was *Slutsky symmetry* – condition 2b) that $S_{ij} = S_{ji}$ for all $i \neq j$ – and given Samuelson's motivation for his new approach this was not a problem. Recall[25] that Slutsky symmetry is an *integrability* condition that guarantees the existence of an underlying utility function; the lack of such an implication is hardly a problem for a theory designed to eliminate utility from demand theory. As Samuelson explained: "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" (ibid. p. 68). This of course means that Samuelson's original WARP was a *non-integrable theory of demand*; it was a theory of individual choice that was based on a version of rationality – in this case consistency in the sense of WARP – and yet did not assume that the consumer had well-ordered preferences defined over the entire choice space.[26] As Samuelson later explained, in those early years –before the Houthakker (1950) result discussed below – he was influenced by various

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[23] The terms *behaviorism* or *behaviorist* are often used to describe Samuelson's position, but I am not certain these terms convey very much information (including when previously employed by the current author). There are many versions of behaviorism and while all commit to a very narrow definition of empirical science, they also have many differences. For example, while the early radical behaviorism of John Watson (1925) purged all intentional and folk-psychological theorizing from psychology (consistent with Samuelson's elimination of preference and utility), it also endorsed a narrowly causal explanation of behavior – behavior caused by conditioning – which is no part of Samuelson's approach to choice theory. Since similar things can be said about other versions of behaviorism, it is generally not clear what the term means when applied to Samuelson (or any other economist), so I will avoid using such terms here.

[24] Actually in the original paper he had *assumed* demand functions were homogeneous of degree zero – one of the four *implications* of OUT – but in an addendum published the same year Samuelson (1938d) demonstrated that homogeneity was also implied by WARP and the other assumptions of his model.

[25] From note 7.

[26] The ideas of RPT, as well as a variety of other choice-theoretic frameworks, can all be characterized more abstractly in terms of the choice-function-based formalism introduced in Arrow (1959). Although this meta-framework can be very useful in addressing certain questions about choice theory (Richter 1979 being an excellent example), it is a less useful tool for discussing the class of questions that concern us here. When the character of the *empirical content* of various approaches is the question, a less abstract framework is more effective, and of course this was the framework employed by the relevant economists. See Pollak (1990, p. 148) for a discussion of the problems of using the choice function approach – what he calls "extended domain" RPT – to examine the approach of Samuelson and other contributors to RPT.

attempts to construct a non-integrable theory of demand: a more general theory of constrained rational choice that would subsume OUT as a special case.

> I tended to side with Roy G. D. Allen rather than with Hicks's insistency on integrability. Why not be general and be happy to posit non-integrability and global non-transitivity? In those cases, only the Weak Axiom could be validly posited as a constraint on empirical demand observations. My reading of Griffith Evans (1930), Allen (1932), and Nicholas Georgescu-Roegen (1936) softened me up for such a half-way house compromise. (Samuelson. 1998, p. 1381)

Samuelson (1938a) was a young man's impressive effort to provide such a non-integrable theory of consumer choice and to finally bring the long movement away from utility and preference in the explanation of economic behavior to its final conclusion.[27]

So Samuelson's original paper was an attempt to provide a "new foundation" for consumer choice theory, but what was its presumed *empirical content*? How did Samuelson hook up his new theory with something that he and his readers would consider *empirical*? In addressing this question the first thing to note is that his approach was unlike any of the approaches discussed thus far. Samuelson was not advocating testing the empirical implications of the theory like Schultz and others,[28] and yet, it was also not based on direct observation of (measurable) utility, or indifference curves, or marginal rates of substitution, since utility and preferences were terms nowhere to be found in the theory. The answer is that the empirical content came from *individual demand functions*. The

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[27] The radical nature of Samuelson (1938a) is driven home by comparing the manuscript version of the paper with the published version. A version of the original manuscript is in the Samuelson archives at Duke University (Box 152) and while it has no date, it is clearly either the version submitted to *Economica* or an earlier version of it. Also since Samuelson notes that he "never had a chance to read proofs of the original *Economica* article" (Samuelson letter to I. M. D. Little, June 21, 1948, Box 48) it is easy to identify the differences between Samuelson's manuscript and the published version. The title is particularly telling. The manuscript was titled "New Foundations for the Pure Theory of Consumer's Behavior" which is certainly more radical than "A Note on the Pure Theory of Consumer's Behaviour." Unlike the published version, the draft contained a long historical discussion explaining the gradually diminishing role of utility within consumer choice theory. The published version also excluded many of Samuelson's long matrix manipulations. While thinning out some of the steps in matrix algebra is of course standard editorial practice (then or now), in this case it did somewhat influence the final content; the manipulations were particularly messy because of Samuelson's commitment to the general, non-integrable, case, which resulted in non-symmetric (i.e. messy) quadratic forms and matrices.

[28] And specifically with respect to the Slutsky symmetry conditions he emphasized that "I have little faith in any attempts to verify this statistically" (Samuelson, 1938a, p. 68).

empirical objects of consideration were *potentially observable* individual demand functions that satisfied a budget constraint.[29] As Samuelson explained:

> I assume in the beginning as known, i.e., *empirically determinable under ideal conditions*, the amounts of n economic goods which will be purchased per unit time by an individual faced with the prices of these goods and with a given total expenditure. It is assumed that prices are taken as given parameters not subject to influence by the individual. … For mathematical convenience we assume that all our functions and their derivatives of the desired order are continuous with no singularities in the region under discussion. (ibid., pp. 62-63, emphasis added)[30]

In other words, in Samuelson's original contribution to RPT, the empirical lived in abstract real-valued demand functions $h_i = h_i(p, M)$ for all i = 1, 2, …, n. The *empirical content* of Samuelson's original RPT was thus the set of all n-dimensional real valued functions h(p, M) that were consistent with WARP and the other assumptions of his model.

There has been an explosion of RPT-based empirical work in applied demand theory in recent years. This research invariably begins with *choice data* – actual *observations* of specific prices and the quantities purchased at those prices (either by an individual or an entire market). It is often suggested that being based on such finite observed choice data is the *defining feature* of RPT as opposed to OUT: see for instance the earlier quote from Mas-Colell, Whinston, and Green (1995) where the advantage of RPT is that it "makes assumptions about objects that are directly observable (choice behavior) rather than about things that are not (preferences)." But this characterization does not really fit the framework of Samuelson's original RPT paper (or as we will see below, much of the later work in RPT). The demand functions that formed the empirical basis of Samuelson's model were *potentially observable under ideal conditions*, but certainly not observable in any practical sense; the domain of prices and money income are a subset of the real numbers (and thus infinite). Of course one could start with

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[29] Since the focus here is mainstream theories of consumer choice, no note has been taken of those like Gustav Cassel, Augustin Cournot, and others who *started from demand functions* in the analysis of consumer behavior – in the case of Cassel (1967) because be believed that demand functions provided much firmer empirical ground than any notion of utility – but it is easy to see why some also put Samuelson (1938a) in that camp (Wold 1951), although Samuelson himself often denied such identification (Samuelson 1993).

[30] It is useful to note that this represents a significant reversal in the role of demand functions. Traditionally demand functions have been the thing to be explained (explandandum) by consumer choice theory – as Samuelson himself noted in *Foundations*, the derivation of demand functions "is the whole end and purpose of our analysis of consumer's behavior" (1947, p. 97) – and yet here they are the observational content of the explanation (explanans) of consumer behavior. This issue has been discussed by Amartya Sen (1973) and others, but here it is probably sufficient to simply note the issue in passing.

actual choice data and then, *á la* Schultz, *estimate* the demand function, but such estimation explicitly involves various theoretical presuppositions and the resulting demand functions end up theoretically tainted and not strictly observational (see Pollak, 1990, p. 150 for a discussion of this problem). As we will see below, this is quite different from the recent empirical work based on RPT which starts from a finite number of actual observations and thus the *observed* (not just the potentially observable). But this is getting ahead of the story. The main take-aways from Samuelson (1938a) at this point are that Samuelson: i) proposed a fairly radical non-integrable theory of demand, ii) introduced WARP as a consistency condition that would replace both utility and preference in consumer choice theory, iii) demonstrated that the implications of the new theory included all but one (Slutsky symmetry/integrability) of the implications of OUT, and iv) offered a certain set of mathematical structures – abstract demand functions – as the new theory's empirical content.

The next phase of RPT began with the publication of Hendrik Houthakker's important 1950 paper introducing what came to be called the Strong Axiom of Revealed Preference (SARP).[31] Houthakker strengthened the WARP condition by extending it from binary choices to sequences of choices and in so doing created a stronger version of RPT that implied *all four* of the standard OUT implications: including integrability-symmetry. Given Houthakker's result, it became clear that OUT and RPT were observationally *equivalent*. One could start with a consumer maximizing a well-behaved ordinal utility function subject to a linear budget constraint and get demand functions satisfying 1) and 2a-b), or one could start with demand functions satisfying the budget constraint and SARP and get 1) and 2a-b). With these developments RPT was transformed from *an alternative to* OUT to an alternative *characterization of* OUT (a way of *revealing* preferences). In Houthakker's own words: "Though originally intended 'to develop the theory of consumer's behaviour freed from any vestigial traces of the utility concept,' i.e., as a substitute for the 'utility function' and related formulations, it has since tended to become complementary to the latter" (1950, p. 159).[32]

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[31] Actually Samuelson (1948) represents and intermediate step (as did Little 1949). In that paper Samuelson demonstrated that if consumption choices satisfied WARP then it was possible to construct the consumer's indifference curves. The problem is that the demonstration was restricted to two-dimensions and was more of a geometrical than analytical argument. In terms of empirical content this seems to be a move back toward Pareto.

[32] Historically of course this raises the question of whether Samuelson himself changed his mind about revealed preference theory, and the evidence suggests that he did. In fact, the most reasonable interpretation seems to be that any desire he had to produce a new non-integrable theory of demand was relatively short lived and he quickly started to think of RPT as a complement to rather than a substitute for OUT. For example, Houthakker notes that in *Foundations* – which treated consumer choice theory in the traditional way – Samuelson only used RPT "to express the empirical meaning of utility analysis, to which he apparently no longer objects" (1950, p. 159). This said, Samuelson's own view still remains somewhat uncertain since he never seemed to make any clear direct statements about such a change in print or in correspondence. See Hands (2014).

Although Houthakker's paper was the key technical result, Samuelson's 1950 paper on integrability further clarified the relationship between RPT and OUT. Since SARP implied integrability, and integrability implied that the demand function could be *rationalized* – that is, there exists a utility function that could have generated it – SARP not only implied the four standard implications of OUT, it also implied the existence of a rationalizing utility function.[33] With SARP, the integrability that Samuelson wanted to avoid in 1938 became equivalent to RPT; both started with individual demand functions and provided ways of determining if they could have been generated by a budget-constrained utility-maximizing consumer. As Marcel Richter explains, the task is the same, only the tools differ:

> Economists do not observe preferences. They may, however, observe demand behaviour – the choices made by consumers. Is there a way for economists to tell whether the observed behaviour is generated through the maximization of a preference relation or utility function? …
>
> Revealed preference theory answers this question by characterizing choice behaviour that is generated by preference or utility maximization. Relating choice behaviour and preference maximization is also a goal of integrability theory. What distinguishes the theories from each other, and from the other parts of rationality theory, is the special nature of their tools, … (Richter, 2008, p. 151)

Houthakker's result set off a period of "high theory" in RPT: various, often mathematically quite sophisticated, attempts to draw out additional implications of versions of RPT, weaken the mathematical assumptions of the existing results, and relate various versions of RPT to other areas within economic theory such as general equilibrium theory. The literature is extensive, but key results include Kihlstrom, Mas-Colell, and Sonnenschein (1976), Richter (1966, 1979), and a number of the contributions to Chipman, Hurwicz, Richter, and Sonnenschein (1971). Following Hands (2013), I will call this branch of the revealed preference family tree *Traditional Revealed Preference Theory* (TRPT); it is this literature, inspired by Samuelson (1938a), but grounded in Houthakker (1950) and Samuelson (1950), that became the main branch of RPT during the second half of the twentieth century.

So, returning to the main topic, what is the empirical content of consumer choice theory for TRPT? The answer is that it is basically *Samuelson 1938 with a*

[33] One way to think about the difference between OUT and RPT (post-SARP) is in terms of *engineering* and *reverse engineering*. Traditional demand theory engineers demand functions from utility-maximization, while RPT reverse engineers utility-maximization from demand.

rationalization twist. The empirical content of Samuelson's original paper consisted of all individual demand functions consistent with WARP and this continues with the later TRPT literature. The twist is that consistency with SARP also implies *rationalization* – consistency with utility maximization – and so, post-SARP, the empirical content of RPT became the set of all *rationalizable individual demand functions*. This is not to suggest that during the years of high theory RPT most economists thought of the empirical content of consumer choice theory as rationalizable demand functions. First of all, most practicing economists did (or do) not think explicitly in terms of *any* narrow definition of empirical content; for most economists consumer choice theory was/is empirical science (and not a mere axiomatic system) because it was/is "about" observable consumer behavior. Those who were/are more specific might point to the extensive work on econometrics-based empirical analysis of demand, but there are undoubtedly others who would also point to the observability of indifference curves, or the marginal rate of substitution, or various other ideas about where the empirical might live in consumer choice theory. But for those working within TRPT, the empirical basis of consumer choice theory became rationalizable individual demand functions.

While it is true that the empirical content of RPT became rationalizable demand functions, that fact didn't have much impact on the practice of TRPT. Individual demand functions defined over a subset of R^{n+1} are not things that can be used to empirically test RPT or put it to work in empirical applications. Such empirical content may well be potentially observable under ideal conditions, but it is not actually observed. The theoretical perspective of most economists involved in high theory RPT came through the prism of Walrasian general equilibrium theory and the so-called formalist revolution of the middle of the twentieth century. The research produced by these economists was deductive mathematical economics, not empirical testing or empirical applications. This is not a criticism, it is just a description of theoretical practice within this area of economics (and frankly given the progress that mathematical economics had demonstrated in the previous two decades this focus was quite understandable; deductive Walrasian economics was on a roll). Although there is every reason to believe that these economists were fully committed to doing empirical economic science and not just mathematics, the epistemic culture was such that as long as one felt confident that the relevant theoretical structure was in some way connected to something potentially or ideally observable, the requisite empirical condition was met and the mathematical analysis could proceed along at speed. Empirical content was something that needed to exist, but not something that anyone actually needed do anything with. There were a few efforts to empirically test RPT – including Koo (1963), Koo and Hasenkamp (1972), Koo and Schmidt (1974), and Sippel (1997) – but the results were at best mixed and more importantly, the test involved *finite observations* of consumption choices, not demand functions. As Houthakker would say a decade after his 1950 paper,

while RPT had "not yet opened as many new avenues of [empirical] research as had at one time been hoped, … but future work may yet modify this slight disappointment" (Houthakker, 1961, p. 713). It is to that next, empirical, branch of RPT that we now turn.

The most recent branch to emerge from the revealed preference family tree is very different than the high theory literature, in particular it is much less abstract and more a tool for empirical analysis. Again following Hands (2013), I will call this literature *Empirical Revealed Preference Theory* (ERPT). It was noted above that TRPT was not well-suited for empirical application because the empirical content involved abstract demand functions – i.e. it required the "miraculous revelation of consumer demand functions to the economist-observer" (Pollak, 1990, p. 150) – rather than the finite combinations of prices and quantities that normally constitute the data for applied analysis. But this was not the only problem; it also did not provide a way to derive a utility function that rationalized the data. Applied economics often involves questions about whether a particular change makes consumers better off or worse off. Houthakker (1950) proved that if a demand function satisfied SARP then it could be rationalized – there existed a utility function that could have generated it – but it did not provide a way of actually finding such a function. What was needed for empirical work was a version of RPT defined over finite combinations of price-quantity data and would allow for the estimation of a rationalizing utility function (often called recoverability). This was provided in Sidney Afriat's 1967 paper introducing what came to be called the "Generalized Axiom of Revealed Preference" (GARP). Afriat demonstrated that GARP-consistent choice data guarantees the solution to a particular set of linear inequalities which can in turn be solved for an associated utility function. Although Afriat's original paper has been called "virtually impenetrable" (Pollak, 1990, p. 148), the main result was used in the follow-up literature – Diewert (1973), Varian (1982), and others – as the foundation for ERPT: GARP-based empirical techniques for demand analysis (also called non-parametric empirical demand theory).[34]

Although the details obviously vary from one ERPT-based study to the next, the generic version of the approach is as follows. The analysis starts with a finite set of empirical choice data – prices and quantities purchased – that is consistent with a version of GARP (some goodness-of-fit techniques are often employed since much choice data is only almost-GARP). If the data is sufficiently consistent, a version of Afriat's theorem can be used to construct a utility function that rationalizes the choice data. This utility function can then be used to

[34] See Varian (2006) or Vermeulen (2012) for a general discussion of the GARP-based literature and the importance of Afriat's work in its development. Moscati and Tubaro (2011) discuss some of the early applications of these techniques, while Andreoni, Gillen, and Harbaugh (2013), Cherchye, Crawford, De Rock, and Vermeulen (2009), Cosaert (2015), and Crawford and De Rock (2014) provide accessible discussions of the ERPT literature.

make out of sample predictions and/or be used in various comparative statics exercises.

Given this characterization of ERPT we can see that its empirical content is a modification of the empirical content of TRPT. The empirical content of TRPT is all (potentially observable) individual demand functions that are consistent with (can be rationalized by) SARP, while the empirical content of ERPT is all observed data sets that are consistent with (can be rationalized by) GARP. This characterization makes it particularly easy to see how the framework of rationalization is the same, but the underlying commitment to where the empirical lives is quite different for TRPT than for ERPT; for TRPT it is potentially *observable*, while for ERPT it is *observed*. And with this, it seems that we have finally arrived at the end of the long road to rationalization.

But have we? Maybe there are other ways of characterizing the empirical content of ERPT. This is clearly one way of defining the empirical content of ERPT, but this approach seems to be a very *theory-first* way of thinking about empirical content. If one is thinking primarily in terms of *theory,* then the question is: Which data set lives up to the requirements of the theory? i.e. Is the data rationalizable by some set of preferences (ordinal utility function)? But ERPT is more the product of a contemporary "big data and fast computers" scientific culture where data is more fundamental. It could be argued that the relevant question for ERPT is not whether the data set lives up to the theory, but whether there exists an instantiation of the theory that lives up to the data set. There are many different preferences (many ordinal utility functions) that if maximized, would generate demand functions that are consistent with GARP. So for the *data first* interpretation of ERPT the question is *not* whether the data set is rationalizable by some preferences, but rather whether there are specific preferences that rationalize the data set. This leads to thinking about the empirical content of various choice theories – OUT, WARP, SARP, etc. – as the classes of preferences consistent with these theories that could rationalize the given data. In this case the empirical content is a set of preferences rather than a data set or demand functions. This is the way the empirical content of ERPT is discussed in Chambers, Echenique, and Shmaya (2014):

> We define theories as hypothetical 'extensions' of data sets. For example, if one can observe revealed preference demand data, then a theory consists of a class of preferences whose consistency with the data is to be tested. If there is a preference in the theory that could rationalize the data, then the data would be called *rationalizable* ... (Chambers, Echenique, and Shmaya, 2014, p. 2303)[35]

[35] These authors provide a definition of empirical content – "the class of all structures … that do not rationalize any data set that falsifies" the theory (p. 2308) – but I didn't quote it here since their approach is Popperian (empirical content couched in terms of falsifiability rather than

This is not the place to try to compare these two ways of thinking about empirical content in ERPT. This second approach was only introduced to make the point that not only was consumer choice theory steeped in observational ambiguity when economists were thinking about empirical contact points as measurable utility, or observable indifference curves, or marginal rates of substitution, or in any of the other ways that have been discussed here, but that ambiguity continues with the most recent work in ERPT.

4. Conclusion and Final Thoughts

This has been a fairly long story so I will keep this concluding section short and to the point. This has been a history of consumer choice theory – the core of modern economics if there is one – from an unusual perspective: where the relevant economists indicated the empirical lived. The paper does not attempt to answer either the historical question of why specific economists were attracted to the particular notions they were, or the methodological question of what they should have, in the name of scientific knowledge, taken as the empirical content for various versions of consumer choice theory.

So if these are some of the things the paper does not do, what exactly does it do? The first thing it does is to tell an important story about the history of modern economics that has not been previously told. It provides a new prism through which to view the last one hundred plus years of economic theorizing about consumer choice. While contemporary historians of economic thought have done an excellent job shedding new light on the history of economics by looking at the details of personal experience; social, political, & cultural context; the impact of ideas from other social sciences and the natural sciences; and a host of other perspectives; the story here provides a very important perspective that has been neglected in the previous historical literature. Secondly, it provides the necessary background for additional, more narrowly focused, investigations going forward: both historical and philosophical. Although this paper did not offer any detailed historical explanations of why particular economists characterized empirical content in the way they did, it certainly opens the door for this type of close-focused historical research. It provides a similar backdrop for more close-focused methodological investigations of what particular economists should have taken as empirical content and whether the content they suggested actually provided a justification for the consumer choice theory they endorsed.

In conclusion, if I had to provide a simple take-away for readers of this story what would it be? I guess I would need two: one for historians of economics and

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confirmation) and because it is a very "general characterization of the axiomatic structure of empirical content" (p. 2314), and both things make it difficult to compare their definition directly to the other concepts of empirical content discussed here.

one for economic practitioners. For historians I would note that the paper may prompt a re-thinking of the role that empirical content played in the history of consumer choice theory. It is clear that the desire for increasing and/or improving empirical content was a driving force throughout the history, but it is equally clear that there was never a consensus – either at any point in time or across time – over what the empirical content of the various versions of consumer choice theory actually was. Recognition of such ambiguity seems likely to prompt a closer examination of why particular economists characterized empirical content in the (different) ways that they did.

Finally, for the practitioners, I would simply note that if you are told that, as an economist, you *must* take a particular approach to choice theory because there is one and only one type of empirical evidence available to economists,[36] such remarks should be taken with a fairly large grain of salt. It would be a miracle, given the variety and variability of the evidentiary base of modern consumer choice theory over time, that it would suddenly become permanently fixed, or that anything one particular group of contemporary practitioners are saying on the subject would end up being the last word.

[36] For example: "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. Revealed preference earns such a central role in economics because *this is the form of evidence that is available to economists …*" (Gul and Pesendorfer, 2008, p. 36, emphasis added).

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