3

Re-examining Samuelson's Operationalist Methodology

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> Today logical positivism is in a bear market. Quine, Kuhn, and other one-syllable local sages...are supposed to have killed it off. But experience has not enabled me to change my...methodologies. And the scientific guys who win the prizes still judge matters...the way I do. (Samuelson to Stephen Stigler, 5 June 1995)[1]
>
> I rather shy away from discussions of Methodology with a capital M. To paraphrase Shaw: those who can do science and those who can't prattle about its methodology. (Samuelson 1991: 240)

[1] Box 71, Paul A. Samuelson Papers, Economists' Papers Archive, David M. Rubenstein Rare Book & Manuscript Library, Duke University (all archival references in this chapter refer to the Economists' Papers Archive at Duke).

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1 Introduction

Paul Samuelson's ideas about economic methodology have always been identified with Percy Bridgman's (1927) *operationalist* account of the meaning of scientific concepts. Samuelson was exposed to operationalist ideas as an undergraduate at the University of Chicago, and he began thinking about methodological issues in operationalist terms very early; one of his earliest publications began by questioning whether utility analysis had become "*meaningless* in the operational sense of modern science?" (Samuelson 1938a: 344; italics in original). The operationalist theme runs throughout *Foundations* (1947) and also his earlier thesis: *Foundations of Analytical Economics: The Observational Significance of Economic Theory* (Samuelson 1940). In the opening paragraphs of *Foundations,* Samuelson argues that previous economists have not paid sufficient attention to "the derivation of *operationally meaningful* theorems" (Samuelson 1947: 3–4; italics in original).

Not only did Samuelson endorse operationalism as the proper approach to economic methodology, his *Foundations* provided the technical tools for economic analysis along operationalist lines; he also identified the revealed preference theory he introduced in 1938 as the best example of economic theorizing exemplifying the operationalist methodology. As he put it later in life:

> Since the emphasis of my *Foundations of Economic Analysis* on "operationally meaningful theorems" has been brought up, it gives me the opportunity to use my strength ... The doctrines of revealed preference provide the most literal example of a theory that has been stripped down to its bare implications for empirical realism: Occam's Razor has cut away every zipper, collar, shift, and fig leaf. (Samuelson 1964: 738)

Although Samuelson's operationalism received less attention than Milton Friedman's 1953 essay (Friedman 1953), it is fair to say that the ideas of Friedman and Samuelson were two of the most popular subjects for methodological debate by economists during the second half of the twentieth century,[2] at least until they were crowded out by the ideas of philosophers and historians of science like Karl Popper, Thomas Kuhn, and Imre Lakatos during the 1970s and 1980s. All of these developments are now relatively non-controversial and discussed in detail in various surveys of economic methodology (e.g., Blaug 1992; Boumans and Davis 2015; Caldwell 1994; Hands 2001).

The goal of this paper is to *re-examine* Samuelson's operationalist methodology in light of (i) recent historical work on Samuelson (facilitated by the extensive Samuelson Papers at Duke University) and (ii) the economic theory most emblematic of that methodology (revealed preference theory). In both cases, the paper will provide a thicker and more detailed history of what Samuelson wrote about operationalist methodology in *Foundations* and his cluster of papers on the subject during the 1960s.

2 Samuelson, Bridgman, Operationalism, Positivism, and All That

Bridgman's operationalism was part of the positivist tradition in the philosophy of science, but it represented only one particular interpretation of a fairly narrow aspect of the broader positivist tradition. Most operationalists were influenced by positivist ideas, but only a small fraction of positivists would call themselves operationalists.[3] Although there are many different versions of positivist philosophy of science, one early theme was a sharp demarcation between that which is *meaningful* and that which is *meaningless.* In later accounts, the criterion of meaningfulness was softened to cognitively meaningful or cognitively significant, and eventually

[2]This literature includes Archibald (1963), Cohen (1995), Garb (1965), Gordon (1955a, b), Lerner (1965), Machlup (1964, 1966), Massey (1965), Nagel (1963), Samuelson (1955, 1963, 1964, 1965), Simon (1963), and Wong (1973, 2006).

[3]I say "most" and not "all" because there were some American pragmatists—John Dewey in particular—who were sympathetic to operationalist ideas but interpreted them differently than most operationalists and did not self-identify with the positivist tradition. See Hands (2004) for a discussion of these issues.

just scientific, but the notion that some forms of discourse have the epistemic/cognitive right stuff and that others do not is a defining feature of the positivist conception of science.[4] Logic and mathematics were meaningful (when validly derived). However, they were purely analytic and true in all possible worlds; only *science* provided meaningful synthetic knowledge about the empirical world. Given the positivist commitment to empiricism, science started from statements in some phenomenal observation (protocol) language—the empirical basis—and statements involving scientific theories and laws were meaningful only when they were built up from, or rigidly linked to, this empirical basis. Although it was assumed that the empirical basis was purely observational, it was clear that scientific theories and laws involved *theoretical terms*—electron, force, gene, utility, preferences, etc.—that were not purely observational. Thus, cognitively meaningful science required *correspondence rules* (or rules of interpretation) that connected the various theoretical terms and concepts with the empirical basis. Given such correspondence rules, scientific propositions involving theoretical terms would be linked to observation statements and thus could be potentially testable by empirical evidence. As the philosopher Gerald Massy put it in his comment on Samuelson (1964): correspondence rules "legislate the official rates for converting theoretical paper money into factual coin" (Massey 1965: 1159). Such correspondence rules are part of the necessary background for the *verifiability criterion of meaningfulness*; for a proposition to be scientifically meaningful, it must be "in principle verifiable," that is "observational evidence can be described which, if actually obtained, would conclusively establish the truth of the sentence" (Hempel 1965: 103).

Given this background, we can be more specific about what it means to be operationally meaningful in Bridgman's sense. Like so many physicists and philosophers, Bridgman found Einstein's general theory of relativity in the early 1900s extremely disruptive; both the empirical basis and the theoretical concepts that had constituted our bedrock knowledge about the physical universe suddenly changed, and there was a strong desire to find something invariant—"permanent mental relations" (Bridgman

[4]See any of the traditional texts on so-called received view philosophy of science (e.g., Hempel 1965; Nagel 1961; Suppe 1977) or for a discussion of these issues with an eye toward economics, see Caldwell (1994: Chapters 2–4) or Hands (2001: Chapter 3).

1927: 2)—that would render physical science less easily disrupted. Bridgman found the desired invariants in the physical *operations* associated with various scientific concepts and terms. In particular, according to Bridgman's 1927 version of operationalism (the one relevant to Samuelson's methodological writings), a theoretical term or concept is *operationally meaningful* if and only if it can be characterized by a series of specific operations, and its meaning is *defined by*, and is thus *synonymous with*, that set of operations. Bridgman used "length" as an example:

> We may illustrate by considering the concept of length: what do we mean by the length of an object? ... To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.* (ibid.: 5; italics in original)

So why did Bridgman think such operationalism made physics less susceptible to major disruptions? The stabilizing impact of operationalism is at least twofold. On the one hand, much of what was previously taken as the theoretical foundations of physics would need to be given up since it was not operational in this strict sense (ibid.: 28). Less would need to be sacrificed in future theory change because fewer concepts would be considered absolute or universal. Perhaps this is one way to interpret the motivations behind Samuelson's original revealed preference paper in 1938, a paper which also responded to a disruption: the ordinal revolution in consumer choice theory. Secondly, operationalist physics becomes less brittle because theoretical terms are more relative: defined *relative to a set of operations*. As Bridgman explained: "Relativity in the general sense is the merest truism if the operational definition of concept is accepted, and since our concepts are constructed of operations, all our knowledge must unescapably be relative to the operations selected" (ibid.: 25). Thus, the application of operationalism would mean more, and more relative, theoretical terms, which in turn implied that physics would be more flexible because it would no longer rest on a small number of universal foundational concepts.

Of course, there are many potential problems with operationalism,[5] but the one that seems to have received the most attention is the *uniqueness* of the operation-concept relation; each *operation* will define a different *concept* creating a plethora of different concepts. As Bridgman himself notes: "In *principle* the operations by which length is measured should be *uniquely* specified. If we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations" (ibid.: 10; italics in original).

Having a theoretical term defined for each measurement operation might build in flexibility by distributing the epistemic responsibility over more, each less critical, theoretical concepts, but it also leads to less systematic science and more ambiguity. Another common criticism, closely linked to the uniqueness problem, is what might be called the progress incommensurability problem, a new operation that brings about an *improvement* in scientific measurement which also creates a new theoretical concept (see Gillies 1972: 6–7). Because of these and other problems, operationalism has always been a controversial position, so much so that it was eventually disowned by Bridgman himself: "I have only a historical connection with this thing called 'operationalism.' In short, I feel that I have created a Frankenstein, which has certainly got away from me. I abhor the word *operationalism* or *operationism*, which seems to imply a dogma, or at least a thesis of some kind" (Bridgman 1954: 224; italics in original).

In addition to the various logical and practical problems identified by philosophers of science, operationalism has also been a victim of broader historical changes. The intellectual, specifically epistemic, context that initially motivated Bridgman to endorse operationalist ideas, and initially encouraged scientists to find solace in such ideas, is no longer with us.

All this said, operationalism did have a profound impact on certain areas of the social sciences during the second quarter of the twentieth century.

[5]The literature is extensive, but a wide-ranging sample is Bergmann (1954), Chang (2009), Gillies (1972), Green (1992), Hempel (1954), Nagel (1961), and Suppe (1972, 1977).

One clear example is psychology. The philosopher Frederick Suppe offers particularly sharp comments on this influence:

> On the negative side, this century Positivistic philosophy of science has had a profound impact and influence on several of the less advanced branches of science such as behavioral psychology…with consequences that were arguably disastrous – though a strong case can be made that much of this was due largely to the uncritical and unsophisticated acceptance of outdated philosophical views (e.g. operationalism). (Suppe 1979: 330)[6]

One additional background point to be made about Samuelson's methodological arguments concerns his views on the nature of scientific theories and scientific explanation. Samuelson defended a *descriptivist* view of scientific theories and argued for a closely related position that *science does not provide explanations* (at least explanations that go beyond the re-description of observables). The descriptive view of theories requires the direct "translatability of theoretical statements into statements about observable things," and, as a result, it often carries with it the "conception that the sciences never 'explain' anything, but merely 'describe' in a 'simple' or 'economical' fashion the succession and concomitance of events" (Nagel 1961: 118–119). Samuelson repeated his position on description and explanation in his methodological writings throughout his career: "a description…that works to describe well a wide range of observable reality is all the 'explanation' we can ever get (or need desire) here on earth … *An explanation, as used legitimately in science, is a better kind of description and not something that goes ultimately beyond description*" (Samuelson 1965: 1165; italics in original).

And:

[6]When one discusses operationalism in psychology and to a lesser extent economics, one immediately raises the question of the impact of *behaviorism* in these social sciences, since operationalism is frequently—sometimes correctly and sometime incorrectly—associated with behaviorism. But the impact of behaviorism and its relationship to operationalism in the social sciences is a very complex topic that has generated a massive literature in the history and philosophy of the social sciences. Given this, I will stay on task and defer the broader question of behaviorism for another time. Here, I will focus exclusively on Samuelson and his operationalist methodology sans behaviorism.

> Unpopular these days are the views of...crude logical positivists, who deem good theories to be merely economical descriptions of the complex facts ... When we are able to give a pleasingly satisfactory 'HOW' for the way of the world, that gives the only approach to 'WHY' that we shall ever attain. (Samuelson 1991: 242; upper case in original)

Samuelson did provide some argumentation[7] for these positions, but they were never sustained or particularly rigorous philosophical arguments; they generally took the form of strongly worded statements of his own beliefs about scientific theories and explanations, beliefs about both what described (good) science and how it (good science) ought to be done.[8] While the descriptivist position on scientific theories and the reduction of explanation to description were certainly characteristic of early positivism, it is not at all clear how these views relate to operationalism. While descriptivism and operationalism are both strongly empiricist and positivist in spirit, they are not the same thing; being able to translate theoretical statements into purely observational statements is not equivalent to defining theoretical terms by physical operations. Bridgman's operations, as he himself noted (see above), are process-based and relativizing, and this is much more human-centered than the (presumed to be timeless) protocol language of the positivist empirical basis.[9] As Samuelson's biographer Roger Backhouse explains:

> Bridgman's operational methods were adopted by logical positivists and behavioral social scientists (notably Skinner), but were taken in directions

[7]Wong (1973: 319) discussed six of Samuelson's different arguments and raised reasonable concerns about each.

[8]In many ways, it is disappointing that Samuelson continued to say the same things about economic methodology in published work throughout his life. It is disappointing because there are a few places in his correspondence where he expressed not only understanding of, but sympathy for, some of the ideas in post-positivist philosophy of science. For example: "the real objection to positivism in philosophy, I suppose, is that when you get down to the nitty-gritty, at the very frontier of what you mean about meaning, it cannot deliver the goods. When I read...Quine on Two Dogmas...I am distressed – because the simple-minded distinctions that my youthful reading of Ayer and other such types made me think can be maintained turn out to be fuzzy and even self-contradictory. And most logical positivists of the 1930s, who have not gone senile have recanted on their faith in their simplicities" (Samuelson to Hahn, 14 January, 1972, Box 36).

[9]This human and intentional property of operations was precisely that which attracted pragmatists like John Dewey to operationalism even though they were generally anti-positivist. See Footnote 3.

> different from those in which Bridgman himself wished to go. Bridgman held that there was an irreducibly individual, subjective element in all knowledge, and he was critical of attempts…to equate his operational method with their attempts to derive rules by which the objectivity of knowledge could be ensured: that was an impossible goal. Given this ambiguity, we need to be careful in imputing to Samuelson a particular interpretation of operationalism, for though he was to make the idea central to his work, there is little evidence of how much he read and precisely what he made of it…or of personal interactions with Bridgman that might have colored his reading of his [Bridgman's] work. (Backhouse 2017: 200–201)

The bottom line is that although Samuelson consistently said that he endorsed an economic methodology that was positivist, operationalist, descriptivist, and reduced explanation to description, it was not clear exactly how he interpreted each of these terms or how his meaning related to the relevant philosophical literature. Even more importantly, it was not clear how these various philosophical ideas were supposed to hang together into a coherent methodological position.

This section has given us a fairly detailed background on operationalism. This background is useful for the re-examination in the rest of the paper, but that said, most of this information was available when Samuelson was debating various economists and philosophers about operationalism during the 1960s. It is now time to turn to some information that was not available at that time.

3 Samuelson, Bridgman, and Operationalism: A Thicker History

We now have much more information about Paul Samuelson's professional life and ideas than was the case a few decades ago. This is certainly a result of the extraordinary depth and breadth of the personal papers that he left behind (containing, e.g., nearly every draft and piece of professional correspondence from the 1940s on), but it is also due to the diligence of a number of historians of economic thought who have put these papers to very productive use. This is true in almost all areas of Samuelson's professional life—from empirical work, to service, to economic pedagogy, to the many different areas of economic theory where he made important contributions—but it is also true of his methodological ideas. The purpose of this section is to use these newer resources to get a better understanding of Samuelson's methodological thinking than what is available from just his writings on the subject.

As noted above, Samuelson was exposed to operationalist ideas as a Chicago undergraduate, but his main exposure came during his years at Harvard, particularly during his three years in the Society of Fellows. One of the important figures at Harvard during this time

> was physicist Percy Bridgman (1882-1961), a teacher at Harvard since 1910, whose reputation rested on his experimental work on thermodynamics, for which he later (1946) received the Nobel Prize for physics. He attracted the attention of philosophers and students of other disciplines with *The Logic of Modern Physics* (1927), which put forward the method of 'operational analysis'. Samuelson remembered having been introduced to operationalism by Henry Schultz…in Chicago. However, the development and spread of operational analysis was most closely linked to Harvard, where it proved particularly influential in psychology … Samuelson, who was to make much use of operationalism, had probably attended Percy Bridgman's lectures in the autumn term of 1936. In any case, he could hardly have avoided Bridgman, who had close connections with those involved in the Society of Fellows. (Backhouse 2017: 199)

Another aspect of Samuelson's professional life, particularly in the early years, that becomes much clearer from archive-based research is the profound impact that a number of his teachers and colleagues had on the development of his overall intellectual vision: scholars such as Alvin Hansen, Lawrence Henderson, Frank Knight, Joseph Schumpeter, and others. In the area of economic methodology, and particularly the scientific vision of *Foundations* and Samuelson's other early work, the most important such scholar was Edwin Bidwell Wilson (1879–1964) who had been a student of Josiah Willard Gibbs at Yale and tried to carry on Gibbs's mathematical and scientific tradition. Wilson was a mathematician with wide-ranging interests who taught Samuelson mathematical economics and mathematical statistics at Harvard during 1936–1937 and who became a very important influence on Samuelson's thinking about the relationship between science and mathematics (see Backhouse 2015, 2017; Carvajalino 2016, 2018). As Samuelson explains:

> Perhaps most relevant of all for the genesis of *Foundations*, Wilson was at Harvard. He was the great Willard Gibbs' last (and, essentially, only) protege at Yale. He was a mathematician, a mathematical physicist, a mathematical statistician, a mathematical economist, a polymath who had done first-class work in many fields of the natural and social sciences. I was perhaps his only disciple: in 1935-36, Abram Bergson, Sidney Alexander, Joseph Schumpeter, and I were the only students in his mathematical economics seminar. (Samuelson 1998: 1376)

Wilson had a unique take on the foundations of both science and mathematics. On the one hand, he was drawn toward mathematics directly connected to the practice of science and critical of purely formalist or logical approaches, such as those of David Hilbert or Bertrand Russell. But he was also skeptical about the statistical approaches associated with Karl Pearson and Ronald Fisher. Although much of mathematical economics could be done using traditional calculus-based techniques, Wilson worked, and pushed Samuelson to work, toward analysis in terms of finite differences and discrete mathematics. Unlike much of physical science, the empirical data of economic science is discrete, and developing mathematical techniques that could be used in such analysis was an important and original contribution to economics. It was precisely this project that Wilson encouraged Samuelson to pursue:

> Samuelson learned from Wilson the importance of basing economic theory on…the general case where functions were not necessarily smooth and differentiable. Their correspondence makes it clear that Wilson pushed Samuelson to analyze finite changes and, as Gibbs had done, to base conclusions on inequalities linked to generalized notions of convexity. Directing Samuelson to the types of mathematics on which he was to rely in much of his work…techniques that Samuelson was later to use in *Foundations*, and which marked his book out from previous work on the subject. (Backhouse 2015: 333)

Although *Foundations* ended up not being an entirely new approach to economic analysis based on discrete mathematics, which might have been Wilson's ideal case, it provided tools for translating important existing, calculus-based, comparative statics results into a mathematical framework that was closer to Wilson's discrete and more data-ready approach: "a way of mediating between the new and the old" (Carvajalino 2016: 144). Thus, in *Foundations* "Samuelson attempted, albeit in highly abstract and analytical ways, to connect extant economic theory with data, by means of establishing one-to-one correspondences between the continuous cases as found in marginal and differential calculus and the finite cases found in the discrete world of economic phenomena" (ibid.). While it remains an open question how much success Samuelson had in moving mathematical economics out of differential calculus and into discrete mathematics, either in *Foundations* or in later work,[10] there is no doubt that he was *trying* to move in that direction and that Wilson's influence was one of the reasons why.

Finally, it is important to remember that while Wilson's ideas were very important to the development of the young Samuelson's methodological thinking, they were certainly not the only ideas that mattered; his understanding of operational analysis and methodological issues was mediated by his discussions not only with Wilson, but also "Schumpeter, Henderson, and colleagues in the Society of Fellows, all of whom had strong views on scientific method" (Backhouse 2017: 201).

All of these things help us understand why Samuelson's operationalist methodology, although consistently maintained and assertively stated, was never particularly clear or coherent with respect to the philosophical details. A non-exhaustive list of these factors would include: (i) He was

[10]I would suggest that he was a little less successful than Backhouse and Carvajalino seem to argue. Two of the reasons for this are: (i) the sheer fact that the number of pages in *Foundations* (and Samuelson's later work) where the analysis is conducted in terms of derivatives and differential equations is significantly larger than the amount conducted in terms of discrete mathematics and (ii) also the fact that many of the linear inequalities are linear because they come from Jacobian or Hessian matrices and were thus also calculus-based. Note that this is not a criticism of the argument that Samuelson was *trying* to do mathematical economics in a Wilsonian way—it seems clear he was. It simply means that discrete mathematics was extremely difficult in the pre-computer age. Another factor may have been that Samuelson always saw his work as fitting into, and improving on, the grand flow of economic ideas, and doing that, and having it recognized as such, is much easier when the theory is couched in the same mathematical formalism.

very young when his methodological ideas were initially formed, (ii) he maintained a lifelong epistemic preference for the empiricism of Mach and other early positivists even though the prevailing philosophical trends ran against such ideas, (iii) he was profoundly influenced by Wilson, but Wilson himself had a fairly complex view of science and mathematics which, although it clearly nudged Samuelson in the direction of developing the mathematical tools of *Foundations*, did little to help clarify the relevant philosophical—as opposed to mathematical or structural—foundations of economic theory, (iv) operationalism itself is a position that is simultaneously ambiguous and problematic; Bridgman's own views evolved over time, philosophers were generally critical even when positivist ideas were still dominant (although many psychologists embraced it), and neither Wilson nor Samuelson (for different reasons) interpreted operationalism in exactly the way that Bridgman did in his original 1927 book, and finally (v) there were many other intellectual influences on Samuelson early in his career and all of those—Knight, Schumpeter, Henderson, Hansen, as well as others not mentioned above (Abram Bergson, Wassily Leontief, Paul Sweezy, etc.)—had quite different views on methodological issues than either Bridgman or Wilson.

While these factors can account for the various tensions and lack of clarity in Samuelson's methodological writings, they do not really explain why Samuelson failed to revise, or attempt to improve, his methodological arguments. Given that Samuelson's work helped bring about major developments in economics—the mathematical revolution, the Keynesian Revolution, the new welfare economics, a major change in economic pedagogy (initiated by *Economics* in 1948 Samuelson 1948a), and a host of other contributions—one would have expected his methodological ideas to have developed along with his economics, but at least with respect to what he wrote on methodology, they did not. What he said about methodological issues remained the same positivist-operationalist narrative that characterized what he said in the methodological debates of the 1960s as well as what he wrote about the basis of *Foundations* later in life.

Of course, Samuelson was extraordinarily busy doing economics, but so were other influential economists who found time for serious methodological research and supported methodological research among young economists; Lionel Robbins comes to mind. It is also telling that while most influential economic theorists of the middle of the twentieth century had little or no interest in the history of economic thought, it was actually a field where Samuelson made contributions (e.g., see Medema and Waterman 2015). So, yes, Samuelson had tight constraints on his time, but he also revealed a preference to work on things other than methodology. Another personal characteristic that might explain Samuelson's lack of interest in updating his early statements about operationalism may simply be that he did not admit changing his views on scholarly questions very often. There might be something to this, but Samuelson's views on various economic theories as well as his political views did actually change over time. They changed slowly, rather than abruptly, but they changed. A good example is his slow commitment to Keynesian macroeconomics—first critical, then sympathetic to certain specific aspects, then finally developing his own version of "a Keynesianism that was distinct from the one that came to dominate postwar macroeconomic theory" (Backhouse 2017: 525)—and given this, a general unwillingness to admit changing his views does not seem to be an adequate explanation of his methodological stasis.

So why then? I would suggest that much of the resistance came from the simple fact that Samuelson never had much interest in, or appreciation of, methodology or philosophy of science. He certainly was, and clearly thought of himself as, much more of a science *doer* than as a philosopher reflecting on how science should be done (note his comment about methodological prattle in the epigraph). One can find many Samuelson quotes that suggest a low regard for the philosophy of science, but his comment on a 1963 paper by Ernst Nagel is illustrative: "Methodological discussion, like calisthenics and spinach, is good for us, and Dr. Nagel deserves our thanks for taking the time away from other sciences to help straighten us economists out. It is the Lord's work, and we are grateful" (Samuelson 1963: 231).

We actually find this attitude throughout his life, and it helps explain both the amount of ambiguity he was willing to live with in his methodological writing—certainly far more than he would let pass in his economic theory—as well as his tendency to not update or revise what he had written. One particular example, two years before *Foundations* was published, provides additional evidence for this.

Samuelson was presented with an…opportunity to become involved with scientists (and philosophers) when Leontief conveyed to him an invitation to join the Inter-Scientific Discussion Group. This group was a part of the Unity of Science movement … A common theme among those behind the discussion group was an affinity with Bridgman's operationalism … Given Samuelson's emphasis on operationalism in his thesis and the book he was currently writing, it would be natural to deduce that it was these links that induced him to accept Leontief's invitation (Backhouse 2017: 450).

But Samuelson attended only a handful of meetings over the next two years and showed little interest in the discussion group. Given this, Backhouse concludes:

> Samuelson was busy, but given his capacity for fitting commitments into his schedule, it is hard not to conclude that, despite his emphasis on operationalism in his thesis and in *Foundations*…he had no deep interest in the philosophy of science…although he chose to use the term *operationalism* – rather than alternatives such as *testability*, *refutability*, or *falsificationism* – there is no evidence that he engaged seriously with the related philosophical issues. (ibid.: 450–451; italics in original)

4 Samuelson, Operationalism, and Empirical Revealed Preference Theory

As noted above, Samuelson considered revealed preference theory (hereafter RPT) to be an exemplar of operationalist practice in economics. This section will discuss the development of RPT and attempt to trace whatever operationalist thread runs through it. I will try to make the history of RPT relatively brief since I have discussed it in other contexts (see Hands 2013, 2014, 2017a).

Samuelson (1938b) was critical of ordinal utility theory—particularly Hicks and Allen (1934)—because it did not go far enough to eliminate utility/preference from the theory of consumer choice. He sought to replace utility-preference-based theory with an alternative theoretical framework: "I propose, therefore, that we start anew in direct attack upon the problem, dropping off the last vestiges of the utility analysis" (Samuelson 1938b: 62). The goal was to develop an alternative theory of consumer behavior that would have the same implications as ordinal utility theory—negative substitution effects and the symmetry and negative semidefiniteness of the Slutsky substitution matrix—but without assuming the consumer had a well-behaved ordinal utility function or well-ordered preferences.[11]

The specific condition that Samuelson introduced came to be called the *Weak Axiom of Revealed Preference* (WARP). The intuition behind the axiom is that if an individual chooses x_0 when x_1 is available (x_0 is revealed preferred to x_1), then x_1 would only be chosen if x_0 were not available (e.g., if x_0 were no longer affordable). In particular, 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:

$$\sum p_0 h(p_1) \leq \sum p_0 h(p_0) \rightarrow \sum p_1 h(p_0) > \sum p_1 h(p_1)$$

It is clear this condition would hold if the consumer were maximizing a well-behaved ordinal utility function subject to the standard linear budget constraint, but Samuelson's 1938 results demonstrated that WARP—along with a binding budget constraint—implied all of the standard restrictions on consumer demand functions, at least all except one (Slutsky symmetry), without the consumer maximizing, or even having, a utility function. Samuelson's original paper demonstrated that: (i) WARP was necessary for ordinal utility theory (hereafter OUT) so WARP $\leftarrow$ OUT

[11] Since this chapter is primarily concerned with Samuelson's methodology and not his economic theory, it is useful to note that RPT was fundamentally a *methodological* program. Samuelson did not introduce RPT because of some practical problem with ordinal utility theory, for example, to correct for particular empirical refutations or anomalies or to extend the possible range of application of the theory. He was trying to develop a theory that would have the same empirical implications as ordinal utility theory—the exact same Slutsky conditions—but one that would rest on more epistemically palatable foundations. As Daniel Hausman explains: "The *raison d'être* of revealed-preference theory was philosophical. It was supposed to enable economists to rid economic theory of references to subjective preferences or to make those references respectable" (Hausman 2000: 112).

and (ii) that WARP was almost sufficient for OUT (one implication was missing, that one implication being, not coincidentally, the integrability condition that guaranteed the existence of an underlying utility function). The revealed preference condition that was both necessary and sufficient for ordinal utility theory did not come until 1950 with Houthakker's strong axiom of revealed preference (SARP). Since SARP $\longleftrightarrow$ OUT and the two approaches have exactly the same implications, the two theories of consumer choice are *equivalent*; "the 'revealed preference' and 'utility function'...approaches to the theory of consumer's behaviour are therefore formally the same" (Houthakker 1950: 173). So, by the early 1950s the majority of economic theorists were "treating the revealed preference and utility approaches as complements rather than substitutes" (Pollak 1990: 144).

There are contemporary versions of RPT that are direct descendants of WARP and SARP, but the literature that involves Samuelson's original and direct contribution to RPT effectively ends with the papers Samuelson (1948b, 1950). He continued to talk about RPT in various contexts—including the methodological debates of the 1960s, various retrospective papers, and even his Nobel Lecture (Samuelson 1972)—but made no more substantive contributions to the theory of revealed preference and he also never published any empirical studies which applied revealed preference analysis to practical or policy questions.

How does WARP stand up to Samuelson's positivist and operationalist methodological standards? Can the theoretical concept of WARP be translated into statements in the purely observational language of prices and quantities purchased, as positivist strictures require? Does revealed preference analysis produce operationally meaningful theorems? Is the theoretical concept "revealed preferred" defined by, and equivalent to, a series of systematic operations? Or, since the practical question is not whether WARP does all these things perfectly, but whether it does them better than OUT, perhaps these questions should all be reframed in contrastive terms using standard consumer choice theory as the reference point.

It is certainly clear that Samuelson thought his RPT was valid operationalist science or at least more scientifically adequate than OUT. He closed the 1938 paper on precisely these points. His postulates

> are less restrictive than the usual setup, and logically equivalent to the reformulation of Hicks and Allen. It is hoped, however, that the orientation given here is more directly based upon those elements which must be taken as *data* by economic science, and is more *meaningful* in its formulation. Even if this will not be granted, the results...are a useful extension of the restrictions in the older analysis, being directly related to the demand functions. (Samuelson 1938b: 70–71; italics added)[12]

There is a draft of the original revealed preference paper in the archives,[13] and although it is longer and differs in various ways, it closes with the same statement about data and meaningful implications.

In a few places, Samuelson even argued that the Wilson-inspired mathematics of finite differences—which is clearly closer to the structure of data than functions defined over real numbers—was a characteristic of both *Foundations* and his papers on RPT. As he explains in his introduction to the 1983 revised version of *Foundations*:

> [The book] began the systematic use of *finite inequalities* in modern economics. To say that raising price from p_1 to p_2 will lower quantity bought from q_1 to q_2 along a demand curve, $q = f(p)$, one need not be able to say that $f'(p)$ is almost everywhere negative on the interval (p_1, p_2). It will do to know that $(p_2 - p_1)(q_2 - q_1) = \Delta p \Delta q < 0$ for every two distinct points on the demand curve. Where Newtonian calculus helps, economists are grateful. But where it doesn't apply, as when price can take on only integral (or rational) values, we are even more grateful for more general methods. When I stumbled on the notion of revealed preference in 1937, I was shoved into the task of trying to free classical mathematical analysis from its calculus corsets. (Samuelson 1983: xvii–xviii; italics in original)

[12] Samuelson says that WARP was "logically equivalent to the reformulation of Hicks and Allen," but that is not the case in general (although it is true for only two goods), although Samuelson did not know this at the time (and nor did anyone else). It was not until Houthakker's paper that it became clear that it was SARP, not WARP, that was equivalent to OUT. It was not until, as Samuelson put it: "Mr. Houthakker's paper arrived in the daily mail" (Samuelson 1950: 370).

[13] Samuelson "New Foundations for the Pure Theory of Consumer's Behavior," Box 152 (no date, but definitely from the 1930s; it says "Paul A. Samuelson Harvard University"). Backhouse says the paper "is undated but is assumed to be 1937" (Backhouse 2017: 652, fn. 40).

Given all this, it certainly seems clear that Samuelson thought his work on RPT was quite consistent with, even exemplary of, the positivist and operationalist methodology he endorsed. But was he right? The short answer clearly seems to be "no." The first and probably most important problem is that Samuelson did not start with data; there are no tables of numbers in *Foundations* or in his revealed preference papers. But not only is there no actual data, there are no variables that could be placeholders for future data, because the relevant independent variables are not finite, they are real numbers. Samuelson 1938 starts with continuous, in fact differentiable, demand functions with prices as vectors of real numbers:

> 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. (Samuelson 1938b: 62–63; italics added)

Samuelson says these functions are "empirically determinable under ideal conditions," but it is not clear how one could "determine" differentiable functions defined over (infinite) real variables from discrete choice data. One could statistically estimate the functions from such data, but the result would require statistical inference and a number of theoretical commitments that go beyond the strict reduction of all theoretical terms to purely observational statements as required by the narrow positivist interpretation of correspondence rules. As Robert Pollak aptly suggested, it would require a "miraculous revelation of consumer demand functions to the economist-observer" (Pollak 1990: 150). The "observables" that serve as epistemic grounding are demand functions given as purely mathematical objects: $\psi_i = h^i(p_1, \ldots p_n, I)$ for all i, and $\sum_i \psi_i p_i = I$ (see Samuelson 1938b: 62).

However, this concern is with a violation of strict positivist notions of what counts as observational. There seems to be an even bigger problem with respect to the operational meaningfulness of the results. WARP implies that the (own) Slutsky terms are negative and that the Slutsky matrix is negative semidefinite. But what "operation" defines these theoretical terms? The Slutsky matrix consists of derivatives of income-compensated demand functions which are purely mathematical transformations of the given abstract demand functions. This all seems far too purely mathematical and far too remote from any empirical measurement operation to be operationally meaningful. Finally, given the formalist origins of all these transformations, in what sense could the laws (the sign restrictions on the Slutsky matrix) be considered purely *descriptive*?

Since SARP gives both necessary and sufficient conditions, it is a stricter version of RPT, but it starts from the same abstract continuous demand functions and inherits all of the same problems about requiring the miraculous revelation of those, assumed to be observable, functions. Also, given the equivalence of OUT and SARP, the explanatory power of the two theories will be the same. Samuelson notes early on in the original 1938 paper that moving from cardinal to ordinal utility "robbed it of its only possible virtue as an *explanation* of human behaviour in other than a circular sense" (ibid.: 61; italics in original), so given its identity with OUT, the strong axiom seems to be without explanatory power as well (see Wong 1973: 317). Of course, explanation was not something that Samuelson was looking for in demand theory anyway, but it is clear that since SARP inherited WARP's domain restrictions, it also inherited its methodological issues, in particular the inability to describe consumer behavior in a way that would live up to strict positivist standards.

This seems to be a negative conclusion about the scientific contribution of Samuelson's RPT, but it need not be. It is important to be clear about exactly what is being argued here and perhaps, more importantly, about what is *not* being argued. The argument is *not* that there was (or is) no possible way to empirically test Samuelson's RPT. Although extensive empirical research in RPT did not appear until the end of the twentieth century—a literature that will be discussed in the conclusion—there was some earlier experimental work that tried to empirically test versions of RPT. Such tests were difficult and the experimental setups often rudimentary, given the equivalence of OUT and SARP (and how close WARP is to SARP) it was often unclear which version of the rational consumer was actually being tested, and the empirical results were often ambiguous, but such a literature *did* exist (see Moscati 2007; Moscati and Tubaro 2011). In any case, the argument here is *not* that RPT was an inadequate empirical theory according to *any/every* sense of empirical; it is only that in its WARP and SARP form, it was not empirical in the very strict sense required by the early positivist standards that Samuelson identified as necessary for operationally meaningful economics. The point is the failure of Samuelson's RPT to live up to the strict methodological standards he explicitly endorsed—and thus *inconsistency* between his methodological prescriptions and his theoretical practice—and not necessarily the failure of RPT to live up to other, more reasonable, empirical standards.

So, what are we to make of this? I put the responsibility on philosophical problems with Samuelson's stated methodology rather than on any scientific problems with his theory of revealed preference. In other words, the inconsistency is simply a result of Samuelson's commitment to early positivist and operationalist methodological standards, and those standards are problematic.[14] This is certainly the conclusion that the vast majority of philosophers of science have reached about operationalism and the strictest forms of positivism, the views that Samuelson seemed to be most attracted to. In other words, they provided neither a good *description* of what has gone on in the best science nor a reasonable *normative standard* for what ought to be done in scientific practice.

Samuelson committed to these philosophical views early in his professional life—for a number of different reasons—as well as the belief that economics should be guided by these standards. This was consistent with Wilson's view, and it was a motivating scientific impulse behind much of Samuelson's early work. He was skeptical to some degree about the usefulness of philosophy of science even when positivist ideas were dominant and grew more so as philosophy moved away from these views. As a result, he had little interest in correcting the ambiguities of the methodological positions he originally defended. To have seriously revised his

[14] My assessment is thus similar to what Hausman calls the "methodological schizophrenia" of economics, whereby "methodological doctrine and practice regularly contradict one another. This schizophrenia is a symptom of the unsound philosophical premises underlying…economic methodology" (Hausman 1992: 152). I would also note that Samuelson's use of abstract mathematical functions presumed to be "empirically determinable under ideal conditions" was typical of the theoretical economics of his day. It may not seem very "empirical" today, but it was standard practice then. See Hands (2017a) for more details.

stated methodological position was simply not something he was interested in doing. Such "prattle" was neither his comparative advantage nor very rewarding (personally or professionally) and besides, dwelling on the fact that the "simple-minded distinctions" of his youthful views had turned out "to be fuzzy or even self-contradictory" was distressing (see Footnote 8). It was better to stick with economics where he had a clear comparative advantage and where his work was so well received. After all, early in his career was a great time to be an economist and he made the most of it:

> The times were ripe for *Foundations.* Nature abhors a vacuum, and *Foundations* helped fill the vacuum. I have written elsewhere about how much there was back in the 1930s waiting to be discovered and aching to be codified. I was like a fisher for trout in a virginal Canadian brook. You had only to cast your line and the fish jumped to meet your hook. (Samuelson 1998: 1377)

For Samuelson, economics was not only exciting and rewarding, but also socially important. Philosophy of science, on the other hand, was *distressing.* In economics, Samuelson was Mr. Science: "For among economists, Samuelson is Mr. Science. He is widely credited with establishing the scientific ideal in economics at the graduate and professional level with his 1947 *Foundations of Economic Analysis*" (Pearce and Hoover 1995: 184).

For Samuelson, real scientists *do* science, but they do not pick at it. In addition, whatever philosophers of science thought about the methodological positions he defended, or for that matter what other economists thought about them, Samuelson firmly believed that "the scientific guys who win the prizes still judge matters...the way I do" (epigraph).

5 Conclusion

Since I have nothing significant to add to the preceding paragraphs in the way of a conclusion, I will use this section to talk about some developments in RPT during the last few decades that seem to move it more in the methodological direction that Samuelson endorsed.

Despite Samuelson's desire for RPT to be "more directly based upon those elements which must be taken as *data* by economic science" (Samuelson 1938b: 71; italics in original), that was not the case for the RPT of the next few decades. Both Samuelson's WARP and Houthakker's SARP were initially presented as restrictions on abstract demand functions and that is how they were used by economists. An extensive literature developed in mathematical economics—a literature elsewhere that I have called "traditional revealed preference theory" (see Hands 2013)—which treated RPT as one of many useful mathematical restrictions—like gross substitutes, homogeneity, Walras' law, etc.—that could be used to generate useful results in demand theory and Walrasian general equilibrium theory. RPT had almost no contact with, or role in, applied empirical economics, which at the time was exclusively econometrics-based. Houthakker had proven a utility function *rationalization* result—for any demand function satisfying SARP, there always exists a utility function that if maximized subject to the standard budget constraint would generate that demand function—but it was not an empirically useful result, and there was no way to find such a utility function.

An extraordinary paper by Sidney Afriat in 1967—along with some user-friendly simplifications by Diewert (1973), Varian (1982), and others—changed this. What came to be called Afriat's theorem brought two important changes to RPT. First, the relevant revealed preference axiom—what came to be called the Generalized Axiom of Revealed Preference (GARP)—could be used for finite data, the kind of price-quantity data available in consumer choice theory, and second, it provided a way of determining the utility function that could have generated it. As Hal Varian explains:

> Most of the theoretical work [in RPT] starts with a demand function: a complete description of what would be chosen at any possible budget. Afriat (1967) offered quite a different approach to revealed preference theory. He started with a finite set of observed prices and choices and asked how to actually construct a utility function that would be consistent with these choices ... This makes Afriat's approach much more suitable as a basis for empirical analysis. Afriat's approach was so novel that most researchers at the time did not recognize its value ... Several years later Diewert (1973)

> offered a somewhat clearer exposition of Afriat's main results. (Varian 2006: 101)

Over time, these results opened up a new field of empirical revealed preference theory which has, during the last few decades, expanded into a number of different areas of applied economic analysis.[15] These empirical techniques start from (discrete) empirical choice data—in the consumer case, prices, and quantities, but applications have expanded beyond consumer choice theory—and if the data satisfies a version of GARP (there are a number of such restrictions), then it can be rationalized and a rationalizing utility function determined. These results can then be used in a number of ways in empirical-based economic analysis. Although a discussion of this empirical RPT, or the associated methodological issues, is beyond the scope of the current chapter, it does raise a number of interesting methodological questions, the most obvious being: Is this newer empirical RPT more consistent with Samuelson's methodological standards than traditional RPT was? It is a complex topic and certainly deserves a serious investigation, but let me just close with my own opinion about the answer. It seems to me that with respect to the connection between the empirical data and theoretical terms, the answer is clearly "yes." The newer GARP-based empirical RPT may not be consistent with the strictest version of the positivist empirical-to-theoretical relation—no science is—but it is certainly much closer than the abstract mathematical demand functions that were the starting point for Samuelson and others working in traditional RPT. This is of course consistent with contemporary philosophy of science where the observation-theoretical distinction is still important, but it is also much less rigid and more dependent on the local characteristics and pragmatic constraints of the relevant scientific community than it was for the positivists who imprinted their epistemic vision on the young Paul Samuelson. That said, having a unique physical operation define every theoretical term still seems to be very impractical, even if one starts with

[15] 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 et al. (2013), Cherchye et al. (2009), and Crawford and De Rock (2014) provide accessible discussions of the empirical revealed preference literature. Various aspects of this literature are discussed in Hands (2013, 2017a, b).

finite price-quantity choice data. But then given what we know about operationalism—its history and philosophical standing—that is probably a good thing.

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