

Introduction to New Edition
Stanley Wong, *The Foundations of Paul Samuelson's Revealed Preference Theory*

“Wong’s *Foundations* after 25 Years”

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I can still remember when I first encountered Stan Wong’s little gem of a book. It was around 1980, and I was engaging in one of my favorite pastimes of browsing the bookstores in Cambridge, Massachusetts. Prior to that point I had certainly been curious about “economic methodology” and the philosophy of economics, but I bore the impression that much of it then consisted of pointless wrangling over Milton Friedman’s incoherent paper on method, or yet another attempt to force some subset of economics into Kuhn’s Procrustean bed of ‘paradigms’, or endless harangues over the insidious disease of ‘positivism’. I had not been aware of this volume, and it was more than a little forbidding in its demeanor, with its modernist green bookjacket and numbered sections and paragraphs resembling nothing so much as Wittgenstein’s *Tractatus*. But the topic was intriguing, and I decided to splurge and buy it. It was one of the best impulse purchases in a long sequence of snap judgments, and it retains its position of eminence on my bookshelf down to the present.

Wong’s book on Samuelson remains a classic in the philosophy of economics for a number of reasons. First and foremost, it pioneered the close scrutiny of modern postwar economic theory from a critically sophisticated point of view, rather than merely bewailing the supercession of some outdated previous school of economics by the American neoclassical orthodoxy, a modality of expression which even still tends to be the genre favored by disgruntled Keynesians, Marshallians, Austrians, Marxists and so forth. Wong took pains to try and understand the objectives of the particular research program in question from within, as it were, before he set out to criticize it, thus avoiding the a-historical Whiggery which has beset the history of economic thought, then and now. Second, he pioneered the notion of a research program as an entity moving through time, which may or may not amend its *raison d’être* in tandem with its models, procedures and mathematical expression. Wong’s summary of permutations of the tricky proposition that one could actually ‘test’ revealed preference theory is still unsurpassed, after a quarter-century of further commentary. His work still stands as a salutary protopaedeutic to the bland nostrums concerning revealed preference that one still finds in textbooks and other purveyors of the myth of neoclassical price theory as a monolithic and consistent edifice (Mas-Colell et al, 1995, pp.10-14, 28-35). Third, and perhaps most significantly, he had the *chutzpah* to confront one of the icons of the postwar neoclassical establishment and insist that the emperor had no clothes; and what’s more, he did it calmly, with

gravitas and style. There are a plentitude of aspects to admire about this volume, if only because it has rarely met its match in the subsequent literature of economic methodology.

The major thesis of this volume is that the program of providing a “revealed preference” alternative to standard neoclassical utility theory turned out to be a failure; but more curiously, Paul Samuelson persisted in portraying the *denouement* as though it were a triumph, and somehow, the postwar profession bought it. It was a failure because the weak axiom of revealed preference was either a tautology if defined at a point in time (no one would buy the same basket twice) or else entirely toothless if time is allowed to pass (violations could be discounted as changes in tastes, other things not held constant, etc); its supposed operationalist credentials were therefore baseless¹; but furthermore, it was a failure because Samuelson simply pretended he didn’t mean what he had written in 1938 when it was pointed out to him that the weak axiom was not sufficient to derive the so-called Law of Demand. Far from denying the relevance of utility, by 1950 Samuelson was suggesting that he had hopped on the utility bandwagon from the very start of his career. Hendrik Houthakker, who first demonstrated that the required Strong Axiom was isomorphic to standard utility theory, noticed the change of heart: “The stone the builder had rejected in 1938 seemed to have become a cornerstone in 1950” (in Brown & Solow, 1983, p.63).

Why did the profession let him get away with it? And, more telling, why have they simply ignored the devastating case that the revealed preference program was and is empty of content provided by Wong? First, I think it is fair to suggest that Wong’s arguments have *never* been seriously addressed or criticized by adherents to the neoclassical orthodoxy. Instead, one simply is tendered all manner of hand-waving concerning what the author in question believes Samuelson ‘should have meant’ when he postulated the existence of revealed preferences. One observes this, for instance, in (Houthakker in Brown & Solow, 1983; Mas-Collel in Feiwel, 1982). Of course, all this third-party speculation could have been settled quite easily by recourse to the principal protagonist himself; but here is where the plot thickens. Paul Samuelson seems to be incapable of publicly admitting any errors or disappointments other than what he deems to be minor slips later clarified by others. Wong had already noticed his disturbing habit of trying to paint any criticism of himself as criticism of the method of the natural sciences *tout court*, surely a case of mistaken identity. One wonders where the supposed semantic clarity and syntactic discipline induced by mathematical expression has gone in this instance. The main piece of evidence about his unwillingness to confront the issue can be found in (Samuelson, 1998). First he throws up a veritable historical fog around the issue by situating the supposed origin of the doctrine in the natural sciences, namely, “a marriage between Haberler-Konus index number theory and Gibbs finite-difference formulations of classical phenomenological thermodynamics of the 1870s” (p.1380). Then he suggests that the doctrine of revealed preference was ‘played down’ in his book on *Foundations of Economic Analysis* because he had already suspected before the fact that his weak axiom was insufficient to guarantee transitivity of comparisons (something nowhere hinted at in his published work). Then Samuelson simply repeats his version of events first retailed in 1950:

As a result of my (uncharacteristic) modesty in playing down revealed preference in *Foundations*, some writers have suspected some failure in the paradigm. On reflection,

¹ Samuelson’s misrepresentation of Operationist philosophy as found in the history of the natural sciences is discussed by (Mirowski, 1998; Hands, 2001).

thanks to Houthakker, all I hoped for (or could rationally have hoped for) was attained by it... Gibbs led me to the promised land before there was a promised land. (1998, p.1381)

The reader can easily judge for themselves, after reading Wong, whether Samuelson led his minions into the promised land, or whether they are still wandering aimlessly in the desert. But what is clear is that his attempt at revisionist history manages to evade every single one of the deep questions at issue. Whatever happened to Samuelson's renunciation of psychology? And whatever happened to the early conviction that revealed preference had something to do with observable behavior? How did the disputatious issue of integrability get shoved under the rug in the 1950s? And even worse, what has happened to all those ambitions to 'ground' the law of demand in neoclassical rational choice theory after the advent of the Sonnenschein/Mantel/Debreu theorems in the 1970s? Rather than striving for generality, wasn't Samuelson concocting the most narrow of special cases, calculated to put neoclassicism in a favorable light? Samuelson himself disparaged controlled experimentation, as Wong points out, but the efflorescence of experimental economics in the interim has revealed just how many auxiliary hypotheses are required to subject neoclassical preference theory to structured tests, and just how badly the theory of revealed preference fails when someone really treats it in a recognizably scientific manner (Sippl, 1997). It makes one wonder just how did neoclassical price theory manage to come to defeat all its rivals in the American context, and what role did Samuelson actually play in the victory —transparently this did not happen on the basis of merit alone. This predisposition on the part of Samuelson and others to fill the journals with virtual history, wishful thinking, red herrings, and most of all, the rhetoric of physics envy, reveals to us the crying need for a vibrant culture of methodological discourse and critique, if not situated within the economics profession, then at least nurtured from without. Compared to Samuelson's own retrospective and the average microeconomics textbook, Wong's little volume is a beacon of clarity in exposition and argument.

That is not to assert Wong's meditations could not be improved upon a quarter-century later. For myself, I might suggest that the version of the philosophy of science associated with Popper and Lakatos really did not actually contribute all that much to the structure of the argument. Popper's method of 'situational analysis' was really just a rehash of neoclassical rational choice theory, as he admitted in his own autobiography *Unended Quest* (p.117), and as such would seem particularly unsuited to serve as a basis for critique of that very same theory. As for Lakatos' method of rational reconstruction of research programs, Wong himself admits that he does not expend effort to document how the joint program of 'revealed preference' played itself out amongst and between the subsequent set of relevant authors, but limits himself consciously to the writings of Samuelson himself. Given that Lakatos was happy to admit that any single author could resort to inconsistency, ambage and worse in pursuit of scientific success, but that the rationality of science would reside in the critical activities of the community, then the present volume would have great difficulty recruiting Lakatos to its critique. But luckily, it seems Wong readily understood that he need not lean too heavily on Popper or Lakatos to underwrite his theses when he composed this volume, and mostly they are safely confined to the footnotes.

The other amendment one might suggest to the narrative herein is a redoubled quest to reconstruct Samuelson's problem situation in the 1930s and 1940s, in order to better understand the deafening silence which greeted his 180 degree turnabout in 1950. Wade Hands and I have begun to do this by proposing that neoclassical economics in America, and in particular price

theory, was nowhere near a unified body of doctrine in this period, but was beset by a number of obstacles bequeathed it by Harold Hotelling and Henry Schultz at the end of the 1930s (Hands & Mirowski, 1999; Mirowski, 2002, chap. 4). Samuelson, a student of Schultz, had been brought to an appreciation of the empirical failures to ‘test’ the supposed restrictions on demand curves dictated by existing utility theory, and thus was prompted to search for a way around what seemed at the time as an insuperable obstacle to a “scientific” neoclassical economics. In the 1930s, it looked as though neoclassicism might lose out to other schools of economics, but by the early 1950s, the neoclassicals had triumphed, and thus were able to treat their vanquished rivals with some contempt. This rise to dominance was not predicated upon a single monolithic doctrine, but rather three loosely related but fundamentally opposed versions of neoclassical price theory, centered at Chicago, the Cowles Commission, and (subsequently) MIT. Each sub-program adhered to a divergent position with regard to utility and the law of demand, thus rendering the program as a whole impervious to criticism. Samuelson’s behavior can be better understood within this interactive complex, than as an isolated set of self-sufficient propositions which could be sharply characterized as ‘true’ or ‘false’.

Whatever the additions and revisions made to Stan Wong’s original account, this book will continue to serve as an inspiration and indispensable guide to anyone who wants to understand the course of American economics in the 20th century.

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