EDWIN B. WILSON, MORE THAN A CATALYTIC INFLUENCE FOR PAUL SAMUELSON'S *FOUNDATIONS OF ECONOMIC ANALYSIS*

BY

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Abstract: This paper is an exploration of the genesis of Paul Samuelson's *Foundations of Economic Analysis* (1947) from the perspective of his commitment to Edwin B. Wilson's mathematics. The paper sheds new light on Samuelson's *Foundations* at two levels. First, Wilson's foundational ideas, embodied in maxims that abound in Samuelson's book such as "*Mathematics is a Language*" or "*operationally meaningful theorems*," unified the chapters of *Foundations* and gave a sense of unity to Samuelson's economics, which was not necessarily and systematically mathematically consistent. Second, Wilson influenced certain theoretical concerns of Samuelson's economics. Particularly, Samuelson adopted Wilson's definition of a stable equilibrium position of a system in terms of discrete inequalities. Following Wilson, Samuelson developed *correspondences* between the continuous and the discrete in order to *translate* the mathematics of the continuous of new-classical economics into formulas of discrete magnitudes. In *Foundations*, the local and the discrete provided the best way of *operationalizing* marginal and differential calculus. The discrete resonated intuitively with data; the continuous did not.

I. INTRODUCTION

On November 27th 1940, Edwin Bidwell Wilson acted as chairman of the Examining Committee at Paul Samuelson's thesis defense along with Joseph Schumpeter and

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Overton Taylor at Harvard University.[2] For Samuelson's defense, Wilson wanted a large part of the staff of the Department to attend the examination because he rated Samuelson's work as *summa cum laude*, but knew that he was biased. In his words:

> "I may be prejudiced. I find in [these] developments [of Samuelson's thesis] of a great many things I suggested in my lectures on mathematical economics in 1936 (I believe). I said at the time that I had not the opportunity to develop this line of thought to the perfection which I should deem essential if I were to publish about it but that I was throwing it out to any interested persons in the class. Samuelson has followed almost all the leads I gave besides a great many things that I never mentioned."[3]

In October 1940 just after leaving Harvard for the Massachusetts Institute of Technology, Samuelson had written to Wilson as follows:

> "I should like […] to express, however inadequately, what I feel to be my debt to your teachings. I think I have benefitted from your suggestions, perhaps more than from anyone else in recent years, and even chance remarks which you have let fall concerning Gibbs's thermodynamical

[2] E. Wilson to E. Chamberlain, 22 Nov. 1940, (PEBW, 34).

[3] *Idem*.

> systems have profoundly altered my views in corresponding fields of economics."[4]

Subsequently, Samuelson expanded his thesis into a manuscript that became *Foundations of Economic Analysis* (1947). Following the publication of his book, Samuelson wrote again to Wilson:

> "Ever since my book came out, I have been meaning to write to you to express its indebtedness to your lectures. In fact, the key to the whole work suddenly came to me in the middle of one of your lectures on Gibbs's thermodynamics where you pointed out that certain finite inequalities were not laws of physics or economics, but immediate consequences of an assumed extremum position. From then on, it became simply a matter of exploration and refinement."[5]

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Wilson was an American polymath who played a central role in the constitution of an American community of mathematical economists around 1930 and in the origins of the Econometric Society. He promoted and established a program of mathematical and statistical economics during the 1930s at the department of economics at Harvard, where

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[4] P. Samuelson to E. Wilson, 9 Oct. 1940 (PEBW, 35).

[5] P. Samuelson to E. Wilson, 20 Jan. 1948 (PASP, 77).

Samuelson conducted his graduate studies between 1935 and 1940 (Carvajalino 2016b). Late in his life, Samuelson acknowledged that he "was perhaps [Wilson's] only disciple." (Samuelson 1998, 1376)

Wilson's "importance to Samuelson and hence to *Foundations* cannot be overstated" (Backhouse 2015, 331). In this paper, certain aspects of this importance are examined.[6] By regarding *Foundations* from the perspective of Samuelson's active commitment to Wilson, as regards mathematics, statistics and science, this paper sheds new light on Samuelson's early mathematical economics.

Samuelson's commitment to Wilson was manifest at various levels. First, Wilson's foundational ideas provided a unifying basis for the different parts of Samuelson's thesis and *Foundations*. The projects on which Samuelson worked during his doctoral years, some of which composed the thesis, were rather disparate; in the thesis and in *Foundations,* however, Samuelson presented the different chapters as a unified comprehensive whole, which he thought could serve as new scientific foundations for economics. Such perceived unity was based on Wilson's ideas, which were embodied in the mottos that abound in Samuelson's thesis and *Foundations*, such as "mathematics is a language," "operationally meaningful theorems," and "useful" knowledge. For Wilson, science implied mathematics, and vice versa. He also believed that *much science* could

[6] Roger Backhouse (Forthcoming) is currently writing an intellectual biography of Samuelson. His historical study is comprehensive and traces a great number of significant influences for Samuelson's intellectual development. Backhouse also emphasizes Wilson's relevance for Samuelson's career and work, opening, at the same time, the door for the present paper, which focuses exclusively on the Wilson-Samuelson connection.

be developed *with little mathematics*. By this, he meant that if the mathematics of a scientific contribution was not fully complete, namely fully consistent, but if the mathematical gaps could be filled with intuition related to the subject matter, such a contribution could be regarded as mathematically and scientifically grounded. *Foundations* embodies these Wilsonian ideas; it can be regarded as offering *much economics with little mathematics*.

Second, Wilson's foundational ideas were also significantly influential in the way Samuelson dealt, in the thesis and in *Foundations*, with the study of the economy as a system in stable equilibrium, treating separately and connectedly, depending on the emphasis of the analysis, the microeconomic and the macroeconomic levels of the system. More particularly, Wilson's thought influenced the way Samuelson framed a certain number of theoretical concerns. Through his ideas about how economists should mathematically define a position of stable equilibrium, Wilson was particularly important to Samuelson's work on consumer theory, cost and production theory as well as dynamics. For Wilson, mathematical economics based only on marginal and differential calculus was empirically empty, as the formulas that were developed within these frameworks were defined by abstract, because continuous, relationships. For Wilson, the discrete was more general than the continuous; the discrete was also more cogent with data. Furthermore, since Wilson believed that calculus had emerged as an abstraction of the study of the discrete, he assumed that without loss of generality correspondences between the discrete and the continuous could be established.

Precisely, the most important of Samuelson's Wilsonian concerns in the thesis, and therefore in *Foundations*, consisted of establishing correspondences between the continuous and the discrete, in order to *translate* the mathematics of the continuous, used in standard contemporary economics procedures of optimization and in the treatment of dynamical systems, into formulas of discrete magnitudes. Extant statistical methods for the treatment of economic data, both Wilson and Samuelson felt, remained unsatisfactory and arbitrary. In Samuelson's thesis and *Foundations*, the local and the discrete—in sum the observable in idealized conditions—provided the best way of *operationalizing* marginal and differential calculus in economics. The discrete resonated intuitively with data; the continuous did not. From this Wilsonian perspective, *Foundations* appears not only to be an exercise in mathematical economics, but also and unexpectedly, an exercise in mathematical statistics, based on observable, although not necessarily observed, data.

In the following pages, the master and the disciple will be first briefly introduced. Secondly, we will show how Wilson framed and limited Samuelson's doctoral thesis, being particularly influential in four interconnected dimensions: the opening page where Samuelson wrote "mathematics is a language"; the introductory chapters, where Samuelson presented his thesis as a unified whole; the individual (microeconomic) level of the system; and the aggregate (macroeconomic) level of the system. Lastly, Wilson's influence on Samuelson's expansions of the thesis leading to *Foundations* will be discussed, showing how he contributed to the development of the most mathematically and statistically oriented parts of such expansions.

II. THE MASTER AND THE DISCIPLE

II.1. Edwin Bidwell Wilson

Wilson was born in 1879, in Hartford, Connecticut. He was trained as mathematician at Harvard University, Yale University and at the École Normale Supérieure in Paris around 1900. Wilson subsequently became one of the "most active" members among the American research community of mathematicians during the first decade of the 1900s (Fenster and Parshall 1994). He, however, gradually marginalized himself from that community, disavowing the influence that David Hilbert's structuralist mathematics was then exerting on his American colleagues and concomitantly committing to the traditional applied American mathematics that Josiah Willard Gibbs, his mentor at Yale, practiced.[7] Wilson's career illustrates this process of marginalization, and corollary process of incursion into other fields. First, in 1907, he became associate professor of mathematical physics at Massachusetts Institute of Technology (MIT). Second, in 1922, he accepted the chairmanship of the department of vital statistics at the newly founded Harvard School of Public Health (HSPH), opening the door to his incursion into social science and economics. In parallel spheres, since 1914, when the *Proceedings of the National Academy of Science* (PNAS) was launched, Wilson served as managing editor of this journal until the end of his life in 1964.[8]

The task that Wilson gave himself consisted of interconnecting mathematics and different subject matters. At Harvard, between 1932 and 1943, Wilson gave a course on Mathematical Statistics and another on Mathematical Economics (since 1935),

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[7] On Wilson's criticism of Hilbert's mathematics, see Wilson 1903. On American mathematics around 1900 see Parshall and Rowe 1994.

[8] See Hunsaker and Mac Lane 1973.

alternately every two years since 1935. Wilson aimed his instruction at protecting students from what he disdainfully regarded as the beauty of certain pure theoretical and/or mathematical contemporary works in economics.[9] He thought that students of economics, by learning his American Gibbsian constrained mathematics, would learn how to behave in a scientific way.[10]

II.2. Paul Samuelson

Samuelson was born in 1915, in Gary, Indiana. In 1932, he entered college at the University of Chicago, where he majored in economics. He performed exceptionally well and was awarded in 1935 a selective pre-doctoral scholarship given by the Social Science Research Council (SSRC). With it, he went to Harvard, with all expenses covered. During the 1935-1936 academic year, Samuelson took, in particular, Wassily Leontief's Price Analysis course, and Wilson's course on Mathematical Statistics. Samuelson was then only twenty-one years old. The following spring, he attended Wilson's course on Mathematical Economics. It was difficult, but Samuelson was mathematically well trained. In college, he had taken a significant number of mathematical courses. Also, during the summers of 1935 and 1936, he had taken extra curriculum courses on differential equations and on the theory of equations, where linear matrix equations were treated (Backhouse 2015).

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[9] For Wilson, Maynard Keynes' and Irving Fisher's theoretical economics as well as Ragnar Frisch's econometrics were not concerned with reality. These authors, he thought, based their work on universalizing or structural principles that existed, only, in their minds.

[10] On Wilson's mathematics, see Carvajalino 2016a.

Eventually, Samuelson impressed Wilson. As he wrote to Lawrence Henderson, chairman of the Harvard Society of Fellows (HSF) and Wilson's close friend, when recommending Samuelson as a Junior Fellow of the Society, Wilson believed:

> "one of the most brilliant young men in political economy whom I have ever met is Samuelson. […] I had him in my course in mathematical statistics and he was the most original and inquisitive of all the students."[11]

In 1937, Samuelson was elected Junior Fellow of the HSF. The membership came with a scholarship, and also with the restriction that he could not work towards obtaining a higher degree. Presumably following this rule, Samuelson did not work to complete a comprehensive and well-constructed thesis. Between 1937 and 1940, instead, he conducted research and wrote an important number of papers, not all published, on consumer theory, cost and production theory, capital and investment theory, business cycles, population dynamics, international trade and welfare economics, as well as comparative statics and dynamics. In order to fulfill the requirements of the department of economics and to graduate, however, in 1940, Samuelson took some of his fellowship projects, put them together, added three introductory chapters and a mathematical appendix, and submitted a thesis, defended in November 1940.

"You did a fine job at your doctor's examination,"[12] Wilson wrote Samuelson after the defense. Concerned about career opportunities for Samuelson, Wilson was then

[11] E. Wilson to L. Henderson, 12 Jan. 1937 (PEBW, 28).

actively supporting Samuelson's thesis to be considered, as soon as possible, for the David A. Wells Prize, which was awarded to Samuelson in 1942.[13]

III. THE COMMITMENT: THE THESIS

Samuelson titled his thesis *Foundations of Analytical Economics: The Observational Significance of Economic Theory* (1941a). The dissertation had nine chapters and a mathematical appendix. The first three chapters were introductory; from the fourth to the seventh chapters, Samuelson analyzed optimizing behavior of the firm first (chapter four) and then, in three chapters, of the consumer. In the last two chapters, Samuelson studied stability conditions of equilibrium of aggregate economic systems, first emphasizing comparative statics and then focusing on dynamics and its more formal aspects. In the mathematical appendix, Samuelson covered maximization, especially quadratic forms.[14]

As it will be discussed in this section, Wilson was key regarding Samuelson's opening page (first point) and the introductory chapters (second point); in these parts of the thesis, by reflecting on Wilson's ideas, Samuelson presented the different and

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[12] E. Wilson to P. Samuelson 14 Jan. 1941 (PASP, 77).

[13] E. Wilson to E. Chamberlain, 20 Nov. 1940 (PEBW, 34). Wilson wanted the conditions of eligibility for the award to be changed in such a way that Samuelson could apply already in 1940. Also see Backhouse 2015, 13

[14] In his dissertation, Samuelson did not include capital and investment theory, international trade and welfare economics.

somehow disparate parts of the thesis as a comprehensive whole. At the same time, the chapters that Samuelson included in his thesis corresponded well to the fellowship projects on which Wilson had had the most significant influence. The last two points of this section will explore such influence on theoretical concerns, which eventually led Samuelson to treat as distinct, but interconnected, the individual and the aggregate levels of the economy, regarded as a system.[15]

III.1. The opening page

The first instantiation of Samuelson's commitment to Wilson in matters of mathematics, statistics and science appeared in the opening page of the thesis, where he wrote: "Mathematics is a Language." Samuelson attributed, rightly or wrongly, this motto to Gibbs, legacy of whom was transmitted to him by Wilson, who precisely defined mathematics as a sort of language.[16] For Wilson, mathematics as a language implied two main ideas, which Samuelson probably wanted to evoke, and which set the spirit of the thesis since its opening page.

First, mathematics as a language implied, for Wilson, defining mathematics as connected with science and meaning.

For Wilson, mathematics consisted of establishing correspondences, as *translations*, between purely mathematical abstract entities, which represented certain mathematical

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[15] In the mid-thirties, system thinking *à la* Henderson was intrinsically connected to Wilson's attitude towards mathematics, statistics and (social) science. See Carvajalino 2016a.

[16] On the attribution to Gibbs of the motto, see Rukeyser 1941, 280.

structures, which he called postulates, and conventional working hypotheses found in subject matters, which he called axioms. In these *translations*, postulates and axioms, Wilson claimed, must simultaneously restrict each other: while postulates imposed logical structure on the subject matter, acting thus as a sort of grammar, axioms constrained freedom and abstraction of postulates and gave them meaning connectedly to the subject matter, acting thus as a sort of semantics. Without their corresponding meaning in science, mathematical structures were as beautiful and as useless as pure theoretical treatises of subject matters, Wilson felt. At the same time, without their corresponding mathematical structures, subject matters could not achieve scientific status. For him, mathematical structures were indispensable in science to mediate between theory and data, as they were necessary to determine meaning. Wilson however insisted that emphasis should be placed on meaning and intuition rather than on pure consistency of close mathematical systems (Wilson 1904). He even believed that contributions in which the mathematics was incomplete, namely not fully consistent, but in which meaning and intuition of the subject matter filled the mathematical gaps, could be regarded as truly scientific and well mathematically founded. In his words:

> “whether the [work] is mathematically complete or not does not interest me; this is unimportant. Science advances not so much by the completeness or elegance of its mathematics as by the significance of its facts.” (Wilson 1928a, 244)

At the same time, for Wilson, mathematics necessarily implied immediate usefulness, which could be achieved only if *correspondences* between postulates and axioms were established. In such *translations*, mathematical operators and operations should be used, he explained. Sometimes, new operators and operations should even be developed, in accordance with the immediate problems at hand. This "operational […] side," Wilson believed, required applying "a series of rules of operation often both dull and unintelligible,"[17] generally found in algebra or advanced calculus, but which could be regarded as simply as the arithmetic operations of division and multiplication. These operations, he thought, "are not in themselves of practical or intellectual interest."[18] Operational thinking, Wilson believed, was hence distinct from postulational (intellectual) and axiomatic (practical) thinking.

Wilson's interest in axioms, as conventional working hypotheses, reflected his belief that they corresponded to the ontological invariances necessary for the use of mathematics in science, as they supposedly represented things that "change so slowly that we may regard them for practical purposes as non-changing or at any rate can assign limits to their change in amount and not [in] time."[19] Also, Wilson thought, scientific knowledge resulted from a plurality of working hypotheses. Scientific knowledge was therefore never to be held as universally true, but merely as partial, probable and approximate. Because the reason for prevalence of a certain working hypothesis over

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[17] E. Wilson, unpublished and undated paper (PEBW, 4878.214, Folder: Miscellaneous Papers, Chapter I. General Introduction, p.1).

[18] *Idem*, (p.2).

[19] E. Wilson to C. Snyder, 2 June 1934 (PEBW, 24).

another was not self-evident (Wilson 1920b), scientific knowledge, for him, was also conventional. In this way, as a result of the possibility to "assign limit to their change in amount and not [in] time," working hypotheses conveyed truth and meaning, relative to the problem at hand, only in a certain proportion at given moments in time, Wilson believed. Statistics, he thought, offered an operational way of determining the most likely working hypothesis, as it could be used to quantify that range that carried truth and meaning while connecting theory and data.

All this implied that in defining mathematics as a language, Wilson believed that the mathematician/scientist needed to be familiar with certain mathematical structures (postulates), to master the conventional working hypotheses (axioms) of the subject matter of interest as well as to know how to play with his skills in (multiple) algebra, advanced calculus and (mathematical) statistics in order to develop *correspondences* between postulates and axioms. When establishing these *translations*, Wilson insisted, the mathematician/scientist should endeavor to produce *much science with little mathematics*, by following the idea that meaning and intuition prevailed over mathematical consistency, even when the mathematics was highly sophisticated.[20]

Second, for Wilson, defining mathematics as a language implied regarding mathematics and its operational (algebraic and statistical) techniques as a vernacular, which all individuals could learn (Wilson 1940); and as Wilson stated:

[20] See also Wilson 1928b.

> "there [seemed] to be no present conclusive evidence that learning a particular technique [was] impossible to any person […] and, therefore, each could presumably learn any technique and use it in much the same sense as he could learn any language and write in it." (Wilson 1940, 664)

For Wilson, these operational techniques were the language that economists should learn if they wanted economics to become truly scientific. This was the language that Samuelson learnt and used in his thesis.

III.2. Introductory chapters

III.2.1. Methodology

Samuelson started the thesis by criticizing how, in economics, "bad methodological preconceptions" (Samuelson 1941a, 2) had left the field without sound scientific foundations. During his career, Wilson had diagnosed all the fields with which he engaged as suffering from lack of scientific foundations. As a result, he claimed, practitioners in these fields tended to commit to wrong methodological approaches, either purely theoretical or purely empirical.[21]

These methodological problems, Samuelson believed, had two disastrous consequences for economics. First, because of them, he held that disagreement among economists about applied and theoretical concerns was the rule rather than the exception. Echoing Wilson, Samuelson suggested that consensus was a necessary

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[21] See Carvajalino 2016a.

condition for any scientific practice. Second, because of wrong methodological approaches, economics lacked unity; its different branches, Samuelson deplored, remained unsatisfactorily connected. In order to develop a unifying approach, it was necessary, Samuelson claimed, to build on the high level of generality provided by mathematics. In his courses, Wilson emphasized the greater level of generality that could be attained in economics if mathematics was properly applied. In 1936, when commenting on Wilson-Gibbs vectorial and matrix analysis for a commemorative volume of Gibbs, by quoting the latter, Wilson wrote:

> "We begin by studying *multiple algebras*: we end, I think, by studying MULTIPLE ALGEBRA" (Gibbs 1886, 32 emphasis in original; quoted in Wilson 1936, 160).

Samuelson suggested that he had begun by studying various branches of economics and that he had ended by studying economics in general. In a Wilson-Gibbs spirit, Samuelson aimed at unifying economics and at establishing a methodological balance between economic theory and data representing "empirical human behavior" (Samuelson 1941a, 2). For this purpose, he wanted to achieve minimal consensus about the basic working hypotheses at the foundations of economics.

III.2.2. Basic working hypotheses

Reflecting Wilson's emphasis on conventional working hypotheses, Samuelson claimed that he rejected universal principles. He wanted to establish scientific statements which, "are not deduced from thin air or a priori propositions of universal truth and vacuous applicability." (Samuelson 1941a, 5)

Samuelson worked on the basis of two general working hypotheses, which he took as conventional, which embodied specific ways of dealing with the economy as a system, and which, he thought, embodied other conventional hypotheses in economics.

First, Samuelson regarded optimizing individuals—consumers and firms—as separated and isolated systems in stable equilibrium. This first general working hypothesis supposed a naturalistic assumption reflecting how individuals adapted themselves to their natural and institutional environment in idealized conditions. It implied a correspondence between conditions of stable equilibrium in a system and an individual's optimizing behavior. With it, Samuelson defined individual's equilibrium with respect to specifically demanded and/or supplied quantities that corresponded to the optimal individual's position. At this individual level, such quantities implied therefore simultaneously concepts of stability of equilibrium and optimality.

A simple summation of all individuals' optimal quantities, Samuelson suggested, yielded the corresponding quantity at the aggregate level, at a given moment in time. However, individual's optimality and stable equilibrium, he stressed, did not necessarily imply optimality and stability at the aggregate level. These two notions, he believed, should be studied separately; optimality at the aggregate level implied welfare considerations that Samuelson did not address in the thesis. He rather focused on stability, believing that a comprehensive analysis on the question required analyzing dynamical considerations of aggregate systems as found in business cycles.

Samuelson argued that the second general working hypothesis of his thesis precisely consisted of assuming that the aggregate system of the economy, namely the interaction through time of aggregate variables, was in dynamical stable equilibrium. This working hypothesis involved, for Samuelson, supposing that there was a correspondence between comparative statics and dynamics, as a way of connecting, while keeping separated, optimizing behavior of individuals, a static problem, and the evolution through time of the aggregate system. With such a correspondence, Samuelson presented comparative statics as a special case of dynamics; this *intuitively* implied that individual's optimizing behavior was a special case, related to discrete moments in time, of the continuous evolution over time of the aggregate system at large. In his dynamics, Samuelson suggested, individuals were necessarily optimizing at every discrete moment in time, not over time. Further, at discrete moments in time, their optimizing behavior gave rise to the aggregates of the system, and hence individual optimizing behavior, he argued, "affords an unified approach" in economics. Comparative statics lay thus, he underlined, at the basis of his treatment of dynamical systems.

Building on his two working hypotheses, Samuelson made normative statements about how economists should study, scientifically, the economy as a system and unify economics. With them, he tied together the different chapters of the thesis and presented the individual and the aggregate levels of the economy as distinct problems that could however be studied as interconnected.

Samuelson supported this idea of interconnection appealing to two main arguments. First, the notion of (general) stable equilibrium at the individual and aggregate levels, he explained, yielded that all variables of a given system were simultaneously determined. This implied, he argued, that the subfields of economics *could* be regarded as being interconnected, as the variables of one problem, of interest for a subfield, could be regarded as the parameters of another problem, studied by a different subfield. Second, Samuelson argued that in his research at the individual and aggregate levels and in various fields of economics he had repeatedly "found out" that certain *discrete* inequalities provided the necessary and sufficient conditions of achieving stable equilibrium positions. Eventually, he presented such discrete inequalities as acting as a formal analogy that unified the thesis, and eventually economics. However, as it will be soon discussed, Samuelson's thesis was not mathematically fully consistent. Notwithstanding this, he stressed that such inequalities implied the existence of operationally meaningful theorems.

In all these aspects about the indispensability and applicability of mathematics and unification of economics, Wilson was central. Let interpret how.

III.2.3. Operationally meaningful theorems

In his course on Mathematical Economics, Wilson defined stable equilibrium position of the consumer with certain discrete inequalities and argued that his definition was original relative to the relevant literature, particularly Pareto's economics, as it was more general because it was made "with finite differences [rather] than [only] with derivatives."[22] Also, in his course on Mathematical Statistics, having in mind economic spectral analysis, Wilson taught the fundamental elements in calculus laying behind lag operators, emphasized analytical statistics and numerical mathematics, without covering standard inference theory, of which he was critical. He believed that extant statistical methods in the emerging econometric movement remained arbitrary, for they lay too strongly on probability, of which he was skeptical.

Samuelson's operationally meaningful theorems in economics embodied Wilson's emphasis on operational (algebraic and statistical) techniques that should be used in the *translation* between postulates and axioms. They represented a Wilsonian way of mathematically structuring economic thinking; of attributing meaning to mathematical structures relatively to conventional working hypotheses in economics and; at the same time, of determining the meaningfulness of these working hypotheses by connecting them with data, if only under ideal conditions. Emblematical of his thesis, Samuelson made *correspond* certain mathematical structures, which represented the "structural characteristics of the equilibrium set" (Samuelson 1941a, 15), with the—seemingly—conventional working hypotheses of individuals' optimizing behavior as well as of stability of intertemporal interrelations between aggregate variables. Whereas the use of marginal and differential calculus to study these problems was already standard in Samuelson's time, standard mathematical and statistical economics, Samuelson believed, remained as operationally meaningless as it did empirically empty. Following Wilson, Samuelson thought that the mathematical structures of this mathematics of the continuous had left economics without empirical foundations. In this vein, he sought to

[22] LMP, 7, Folder Econ-theory: Harvard courses Notes 1938-1939, Wilson p. 10.

connect certain discrete inequalities, which represented his mathematical structures, with his working hypotheses while interconnecting the latter with some sort of data. The problem, he believed, was that there was not yet enough available economic data, as detailed quantitative empirical information. In the thesis, the emphasis was therefore placed on observable, not observed, data. In his words:

> One cannot leave the matter here [at the level of marginal and differential calculus], for in the world of real phenomena all changes are necessarily finite, and instantaneous rates of change remain only limiting abstractions. It is imperative, therefore, that we develop the implications of our analysis for finite changes. Fortunately, despite the impression current among many economists that the calculus can only be applied to infinitesimal movements, this is easily done." (Samuelson 1941a, 54).

Data always comes in a discrete form, Samuelson hinted.

From this Wilsonian perspective, Samuelson's operationally meaningful theorems were not only statements in mathematical economics; they also appear—and this is less evident—as statements in mathematical statistics, as Wilson's foundational statistical ideas were also framing and limiting Samuelson's thought. In Samuelson's thesis, there were not standard statistical tests or econometric regressions. Samuelson seemed even to have adopted Wilson's skepticism for—Pearsonian and Fisherian—statistical estimation procedures. Following Wilson's analytical statistics, Samuelson attempted rather to establish correspondences between formulas of discrete elements and equations of continuous elements, in order to show that *old* abstract economics based on marginal and differential calculus had a corresponding form in the more general discrete world (of comparative statics), intuitively more cogent with data. At the same time, such correspondences between the discrete and the continuous did not imply the use of probability theory, of which Wilson was more than skeptical.

All in all, reflecting on Wilson's ideas about mathematics as a language, these translations between the discrete and the continuous represented what Samuelson meant by operationally meaningful theorems: they postulationally helped mediate between economic theory and data, if only in idealized conditions, where "idealized conditions" should be understood as formulas defined in the discrete.[23]

III.3. The individual level

In 1937, Samuelson published his two first papers. He elaborated on the consumer's (1937a) and the entrepreneur's (1937b) behavior, by assuming that they optimized intertemporally. These papers on mathematical economics appeared in February and in May respectively. Samuelson must have finished the first paper before taking Wilson's course on Mathematical Economics; in the May paper, Samuelson briefly referred to Wilson's *Advanced Calculus* (1911) and to Whittaker's and Robinson's *The Calculus of Observations* (1924), both covered by Wilson in his 1936 course on Mathematical Statistics. In these papers, Wilson's deep influence on the way Samuelson approached

[23] Samuelson's reference to operationally meaningful knowledge was *also* a rhetorical argument of authority as it resonated with Percy Bridgman's (1927) philosophy of knowledge.

mathematical economics was not yet evident. Wilson's presentation of Gibbs's thermodynamical systems that "have profoundly altered [Samuelson's] views in corresponding fields of economics"[24] took place almost at the same time that these two papers were published; it is unlikely that Samuelson had had the time to fully engage with its difficult contents. It can be conjectured that once Samuelson explored more in detail Wilson's course material on mathematical economics and thermodynamics, he started then neglecting the *old* Fisherian working hypothesis of intertemporal optimization, as Wilson presented the consumer maximization problem as being independent of time.

In the thesis, with the first working hypothesis, which consisted of assuming an extremum position, Samuelson presented the consumer and the firm problem analogically; his idealized consumer and firm did not optimize over time, but at all moments in time.[25]

III.3.1. Consumer theory

After having attended Wilson's lectures in Mathematical Economics during the spring of 1937, in a series of papers all published in 1938, Samuelson, who was then

[24] See footnote 4.

[25] For Samuelson, the dynamics of the aggregate system resulted neither from consumer's concerns about savings and future consumption, nor from the firm's concerns about future values of its assets. This interpretation of Wilson's influence on Samuelson's consumer and firm theories, according to which the maximization problem was time independent, could explain why Samuelson did not introduce in the thesis his work on capital and investment theory, based on intertemporal maximization.

twenty-three years old, claimed to have established new foundations for consumer theory by developing its empirical implications (1938a; 1938c; 1938d). When Samuelson sent to Wilson the last of the three cited papers for suggestions, the latter responded explaining that he had refereed positively the work for publication in *Econometrica*. Wilson believed,

> "There is no evidence in the style in which the paper is written that you have taken anything other than an intellectual attitude toward any of the questions. If however, there are any particular points where you yourself have any doubt or think other people might have some which you want to take up with me I shall be glad to discuss the matter with you."[26]

In the thesis, Samuelson elaborated on the *Evolution of the Utility Concept* (1941a, 111–34), which eventually culminated, he hinted, at his operationally meaningful theorems, deducible, he argued however, from the standard analysis.

Samuelson regarded utility theory as a convenient convention, which did not yet reflect "the factual behavior of consumers" (1941a, 114). Its relevance, "for better or worse," was due to the fact that it "has occupied an important position in economic thought for the last half century. This alone makes it highly desirable that its meaning be clearly understood" (1941a, 113–14). The notion of utility in economics represented therefore one of those invariants in science that Wilson regarded as necessary for the

[26] E. Wilson to P. Samuelson, 10 March 1938 (PEBW, 31).

applicability of mathematics; determining its operational meaningfulness required then properly connecting it with some sort of data.

Utility theory, Samuelson explained, had evolved as economists tended to reject "utilitarianism, ethical and welfare connotations of […] Bentham['], Sidgwick[' and] Edgeworth[']" early work. "Concomitantly, there has been a shift in emphasis away from the physiological and psychological hedonistic, introspective aspects of utility." In this vein, Samuelson claimed, "many writers", particularly Vilfredo Pareto, William Johnson, John Hicks and Roy Allen, "have ceased to believe in the existence of any introspective magnitude or quantity of a cardinal, numerical kind. With this skepticism has come the recognition that a cardinal measure of utility is unnecessary. That only preference scale, where comparisons of more or less are possible, is required for the analysis of consumer's behavior" (Samuelson 1941a, 111–12). However, Samuelson remarked, some authors, such as Oscar Lange (1934), Irving Fisher (1927), Ragnar Frisch (1932b) and Henry Schultz (1938), among the most significant, still took the cardinal measure of utility as a valid working hypothesis.[27] In the spirit of Wilson, Samuelson suggested that the methodological attitude of this second group of authors was irresponsible: they did not verify applicability, namely the meaningfulness in respect to data, of certain arbitrary "special and extra assumptions," (Samuelson 1941a, 147) which were needed to connect utility theory with consumer's price and quantity behavior.

In his course on Mathematical Economics, Wilson presented consumer theory analogically to thermodynamics by explaining that certain discrete inequalities, which

[27] He probably also had in mind Harold Hotelling (1932; 1935).

he called the Gibbs conditions, characterized the static and stable equilibrium position of thermodynamics *and* economics systems (Carvajalino 2016b). Such analysis did not imply the use of calculus, Wilson argued, but corresponded, in the discrete, to the conditions of stability of equilibrium of standard economic problems of optimization under constraint, in a static world. Wilson's consumer analysis was indeed time independent: "With time introduced, everyone recognizes that preferences change."[28]

In this Wilsonian manner, in the thesis, Samuelson framed his *Meaningful Theorems* (1941a, 134–44) on consumer analysis in a time-independent and static idealized world. He rephrased something that he had called in his doctoral papers the postulate of "consistency in idealized individual's behavior," with which he had connected utility analysis with observable data, by establishing certain correspondences between observable expenditure, the preference-field and the demand function.

In the thesis, Samuelson explained his approach to consumer theory by assuming that his idealized individual could be confronted with two different sets of prices and income: (p_i^1, I^1) and (p_i^2, I^2); in each situation, his consumer would choose two different sets of goods: x_i^1 and x_i^2, respectively. These two situations were not thought of as happening at different moments in time, but simultaneously. Samuelson focused on expenditure for the first situation, $\sum_{i=1}^{n} p_i^1\, x_i^1$. Then, he considered the level of expenditure in the case in which the second set of goods would be evaluated at the prices of the first, $\sum_{i=1}^{n} p_i^1\, x_i^2$. From this little thought experiment (no real data involved)

[28] JTP, 7, Folder "Ec 104b E.B. Wilson", p. 204-6.

implying only discrete magnitudes (prices, income and demanded quantities of goods), Samuelson deduced his operationally meaningful theorem for consumer theory:

$$\sum_{i=1}^{n} p_i^1 \, x_i^2 \leq \sum_{i=1}^{n} p_i^1 \, x_i^1 \; implies \; F[g(x_i^2)\,] \;\leq\; F[g(x_i^1)\,]$$

where $g(x)$ corresponds to an ordinal index of utility, unique except for $F[\cdot]$, a linear transformation. His theorem was general as it was not only valid for compensated changes of prices. It contained the main ideas of his consistency postulate: "If this cost [$\sum_{i=1}^{n} p_i^1 \, x_i^2$] is equal to or less than the amount of money that the first batch actually cost [$\sum_{i=1}^{n} p_i^1 \, x_i^1$], we have conclusive evidence that the second batch is not higher on the individual's preference scale than the first batch; for if it were, the individual could not have been in equilibrium in the first place, since he would not be minimizing total expenditure for the attained level of satisfaction. In other words, if he could have bought the second batch, and he bought the first, we rule out the possibility that he prefers the second to the first" (1941a, 137). Consequently, "the individual always behaves consistently in the sense that he should never 'prefer' a first batch of goods to a second *at the same time* that he 'prefers' the second to the first" (Samuelson 1938c, 353 italics added).

With his approach, which consisted of playing with his skills in logical and arithmetical operations and his knowledge of the economic theory of index numbers, Samuelson was able to infer certain relations in the preference-field from observable expenditure.[29] On this basis, Samuelson was then able to deduce a specific correspondence between such relation and demanded quantity behavior, expressed by the demand function. To accomplish this, and building on his theorem, Samuelson deduced the following relationships:

$$\sum_{i=1}^{n} p_i \, \Delta x_i \ \leq 0 \ implies \sum_{i=1}^{n} (p_i + \ \Delta p_i) \, \Delta x_i \ < 0$$

Following Wilson's lead, Samuelson showed that a discrete inequality relationship, the second one, corresponded to the necessary and sufficient conditions of stability of an extremum position, as found in standard procedures of consumer constrained optimization defined at the margin. The second inequality, Samuelson argued, "contained almost all the meaningful empirical implications of the whole pure theory of consumer's choice" (Samuelson 1941a, 138–39); it corresponded to the well-established negative-slope and stability-concavity restrictions in maximization procedures upon (Marshallian) demand functions.[30] In this way, he connected his consistency postulate,

[29] On Haberler's influence on Samuelson's consumer theory through his 1937 lectures on index numbers as related to international trade, see Backhouse Forthcoming.

[30] From the second inequality, Samuelson derived a negative relationship between prices and demanded quantities: $\sum_{i=1}^{n} \Delta p_i \ \Delta x_i \ < 0$; the negative substitution effects: $\frac{\partial x_i}{\partial p_j} + \ x_j \frac{\partial x_i}{\partial I} < 0$; as well as the negative semi-definiteness of a Hessian matrix: $\sum_{i=1}^{n} \sum_{j=1}^{n} \left(\frac{\partial x_i}{\partial p_j} + \ x_j \frac{\partial x_i}{\partial I} \right) dp_i dp_j \ \leq 0$.

grounded on observable data—not observed data—and the notion of equilibrium, with some structural characteristics of optimization under constraint.[31]

In the standard continuous analysis, however, there was an *empirical* restriction, which Samuelson did not succeed in deriving from his discrete formula: the integrability conditions.[32] In his words:

> Integrability conditions "reflect differential properties of our demand functions which are hard to visualize and hard to refute. […] I have tried, but thus far with no success, to deduce implications of our integrability conditions which can be expressed in finite forms; i.e., be conceivably refutable merely by a finite number or point observations." (Samuelson 1941a, 134, footnote 13).

In spite of the difficulties that he encountered, Samuelson remained optimistic about his approach and hoped that "a proof may still be forthcoming by which [his approach] may be slightly generalized to include the question of integrability" (1941a, 139, footnote 14).

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[31] Stanley Wong (1978) underlined a major logical flaw in Samuelson's consumer theory, as his consistency postulates does not explain why certain bundles that implied the same cost are not bought.

[32] The integrability problem consists of establishing the conditions of existence of the utility function that generates the consumption choices, which are observable and which can be expressed by a demand function. See Hands 2006.

All in all, in consumer theory, Samuelson felt that he had developed something new, based on the old. Because he believed that he had *translated* abstract formulas defined at the margin into a discrete form, Samuelson felt that he had developed the empirical implications of the abstract utility and Marshallian demand theories. He thought that he had failed to encompass integrability precisely because he had not been able to establish such a continuous-discrete connection. From our Wilsonian perspective, it can be argued that the novelty of Samuelson's consumer theory appears in the emphasis that he gave to the working hypothesis of a stable individual's equilibrium, which, following Wilson, had to be defined in the discrete. From this point, Samuelson connected such a definition of the stable equilibrium with certain mathematical structures of optimization and with some sort of data. In this way, he could present his work as *operationally meaningful*, namely as mathematically, theoretically and empirically well founded, emphasizing more one aspect or the other, depending on the part of the thesis. This amalgamation of these three different elements had in Samuelson's thesis the consequence that the notion of stable equilibrium could simultaneously be regarded as mathematically constructed, theoretically well founded and empirically intuitive. His work, however, was not based on actual empirical data, but only on discrete formulas, which he presented as having an empirical nature.

III.3.2. Production and cost theory

Wilson's influence on Samuelson's production and cost theory was less significant than on consumer theory, as Wilson did not cover the theory of the firm in his courses. However, Wilson's influence can be felt at two moments in the fourth chapter.

First, Wilson's criticism of wrong methodological approaches must not be very far from Samuelson's thinking when the latter argues, in his words, that:

> "Economic Theory as taught in the textbooks has often tended to become segmentalized into loosely integrated components, such as production, value, and distribution. There are, no doubt, pedagogical advantages to such a treatment, and yet something of the essential unity and interdependence of economic forces is lost in so doing". (Samuelson 1941a, 68)

Samuelson studied simultaneously the determination of optimal output and optimal input by the firm, two connected problems that had been kept separated in economics, he noticed. [33] In his unifying (not yet dual) approach, cost and production were part of the same technological relation, as embodied in the production function. Minimization of costs given a level of production, he explained, could be regarded as equivalent to the maximization of the level of production given a level of expenditure. The problem of the firm was therefore analogical, he argued, to the problem of the consumer, in which minimization of expenditure given a level of utility and maximization of utility given a level of expenditures were regarded as equivalent. In this spirit, Samuelson solved first the constrained problem of minimization of cost determining optimal demand for inputs

[33] Leontief, who was then developing his input-output framework, must have been influential in the way Samuelson tackled his cost and production theory.

and then the unconstrained problem of maximization of profits establishing the optimal supply of outputs.[34]

The second instance of Wilson's relevance for Samuelson's production and cost theory emerged as the latter found some difficulties when dealing with cases in which "certain costs [were] regarded as completely fixed," or when a "firm [was constrained] to employ the same total of labor."[35] These problems raise new questions about stability when dealing with systems, equilibrium of which depended on "prescribed values of […] 'conjugate variables',"[36] or parameters. They led Samuelson to study thermodynamics, where, he claimed, analogical problems were found, and which implied optimizing with a greater number of constraints.[37] But as the system had more constraints, Samuelson was concerned about the implication for the stability of equilibrium when the system faced changes of a parameter.

In his course on Mathematical Economics, Wilson had treated, in passing, the Le Chatelier Principle as a principle of stability of equilibrium in the case of infinitesimal changes of a parameter. Following Wilson, Samuelson interpreted this principle as implying, in the case of infinitesimal changes, that the greater the number of constraints the system had, the more stable the equilibrium position was in response to the marginal

[34] Samuelson remained vague regarding his references. He only mentioned the lectures of Jacob Viner, his professor in Chicago, Harold Hotelling's work (1932) and certain misconceptions of marginal analysis by Joan Robinson (1933).

[35] Samuelson, "The Le Chatelier Principle of Equilibrium" (PASP, 137, Folder Unpublished Writings Thermodynamics, p. 7-8).

[36] *Idem*, p. 1.

[37] Samuelson's reference was Paul Epstein's *Textbook on Thermodynamics* (1937).

change of a parameter. The question remained to be established whether the principle could be generalized to the case of discrete finite changes.

During his fellowship years, when he was dealing with these issues, Samuelson even wrote a paper on the Le Chatelier Principle, which he sent to Wilson; in Samuelson's words, his

> "manuscript represents a dangerous excursion […] into a field about which I know very little. It was inspired partly by some remarks of yours in class some time ago, [and] partly by some work I have been doing in the field of economic theory."[38]

In his response, Wilson wrote as follows:

> [G]eneral as the treatment is I think that there is a possibility that it is not so general in some respects as Willard Gibbs would have desired. […]. I remember Gibbs used to talk about non-negative quadratic forms meaning those which never had negative values though they might take zero values for values of the variables which weren't zero. Moreover, in discussing equilibrium and displacements from one position of equilibrium to another position he laid great stress on the fact that one had to remain within the limits of stability. Now if one wishes to postulate the derivatives including

[38] P. Samuelson to E. Wilson, 29 Nov. 1938 (PEBW, 31).

> the second derivatives in an absolutely definite quadratic form one doesn't need to talk about limits of stability because the definiteness of the quadratic form means that one has stability. […].
>
> I wonder whether you can't make it clearer or can't come nearer following the general line of ideas of Willard Gibbs as given in his Equilibrium of Heterogeneous Substances, equation 133. He doesn't use derivatives but introduces a condition which is equivalent to saying that his function has to be on one side or in a tangent plane to it. He doesn't even assume that there is a definite tangent plane but merely that at each point of his surface it is possible do draw some plane such that the surface lies except for that point and some other points entirely to one side of the plane."[39]

Following Wilson's disciplining comments, Samuelson acknowledged that his paper "relates to instantaneous rates of change and does not approach the generality of the Gibbs formulation which makes no continuity or differentiability assumptions but only requires certain arithmetic inequalities ('single concavity conditions') to hold."[40] Assuming that he remained in the limits of stability, Samuelson then came to the conclusion, as he wrote to Wilson again, that as a matter of formal definition the Le Chatelier Principle did not hold in the discrete case of finite changes, when several constraints were taken into account. More precisely, in his words:

[39] E. Wilson to P. Samuelson, 30 Dec. 1938 (PEBW, 31).

[40] P. Samuelson to E. Wilson, 25 Jan. 1939 [1938] (PASP, 77).

“Implicitly assuming that we remain within ‘the limits of stability’, I was able through the Gibbs approach to show that

$$\Delta\alpha\Delta x_1|_{(n\ constraints)} \geqq 0$$

This corresponded to the theorems on ~~partial~~ derivatives:

$$\left.\frac{dx_1}{d\alpha}\right|_{(n\ constraints)} \geq 0$$

Intuitively, I had expected that the generalized theorem on the ~~partial~~ derivatives of the form

$$\left.\frac{dx_1}{d\alpha}\right|_{(no\ c.)} \geqq \left.\frac{dx_1}{d\alpha}\right|_{(1\ c.)} \geqq \cdots \geqq \left.\frac{dx_1}{d\alpha}\right|_{(n-1\ c.)} \geqq 0$$

would have an analogous theorem of the Gibbs type of the form

$$\Delta\alpha\Delta x_1|_{(no\ c.)} \geqq \Delta\alpha\Delta x_1|_{(1\ c.)} \geqq \ldots \geqq \Delta\alpha\Delta x_1|_{(n-1\ c.)} \geqq 0$$

> Unfortunately, I was not able to develop a proof of this, and in trying to do so, became aware that such a theorem is not true, at least on the basis of the very general Gibbs curvature assumptions."[41]

In the thesis, however, "By making use of Professor E. B. Wilson's suggestion that [the Le Chatelier Principle] is essentially a mathematical theorem applicable to economics" (Samuelson 1941a, 98), Samuelson claimed that it held for finite as well as for marginal changes, as long as the system remained at the limits of equilibrium (Samuelson 1941a, 43 footnote 12). It corresponded to the economic intuition according to which, for a firm in equilibrium, there was no possible movement that would improve its profits, no matter the number of constraints it had to face.

Samuelson used Wilson as a rhetorical figure of authority in order to introduce, as a general principle, his Le Chatelier Principle. To some extent, Samuelson was not persuaded that the formal analogy embodied in the existence of certain inequalities was formally consistent relative to all the cases that he analyzed; there were substantial differences in the treatment of discrete and continuous cases. The mathematics of the thesis was not necessarily fully complete. By filling the mathematical gaps with meaning and intuition, Samuelson followed his master's reassuring suggestion and the intuitive economics insight, however, which led him to take the Le Chatelier Principle seriously. He also presented his cost and production theory as being operationally meaningful.

[41] *Idem*, p. 2-3, strikethrough text in original.

In the chapter on the firm, Samuelson used these ideas to deal with the possibility of a discontinuous production function, reflecting on the case of fixed production coefficients (Samuelson 1941a, 96–98). He only explicitly referred to Gibbs, when dealing with boundary problems, in which some inputs might not be used (Samuelson 1941a, 84–89).

III.4. The aggregate level

In the thesis, with the second working hypothesis, which consisted of assuming a dynamical stable equilibrium and the correspondence between comparative statistics and dynamics, Samuelson aimed at establishing consensus in the way the dynamics of the aggregate economic system should be studied and to offer operationally meaningful theorems. He analyzed how equilibrium of the aggregate system was determined through time by studying the "stability conditions relating to the interaction between economic units," namely between aggregate variables (Samuelson 1941a, 193), through time.[42]

Such interactions were often studied by analyzing the dynamics and stability conditions of Marshallian or Walrasian aggregate supply-and-demand systems when confronted with changes of prices.[43] But in a Wilson's spirit, Samuelson thought that "the economist would be truly vulnerable to the gibe that he is only a parrot taught to

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[42] Samuelson attempted to connect Ragnar Frisch's (1931; 1932a) and Jan Tinbergen's quantitative economics (1935) with Maynard Keynes' (1936), Haberler' (1937) and Alvin Hansen' (1938) more theoretical economics.

[43] See for example Hicks 1939.

say ‘supply and demand’” (Samuelson 1941a, 192). For Samuelson, Wilson’s “great virtue was [precisely] his contempt for social scientists who aped the more exact sciences in a parrot-like way” (Samuelson 1998, 1376).

Samuelson’s “mathematical dynamics reflects in large measure the beliefs and prejudices of E. B. Wilson” (Weintraub 1991, 58) on dynamical systems. In particular, Samuelson’s ideas about the correspondence between comparative statics and dynamics seemed to have been directly related to Wilson’s lectures in Mathematical Economics, where he discussed thermodynamical systems.

In the early 1920s, in correspondence with Francis Edgeworth, Wilson had claimed that there were two main working hypotheses in quantum theory regarding the treatment of dynamical systems. In the first working hypothesis, it was assumed that atomic nature was dynamical in essence and studied statistically only to ease the analysis. In the second working hypothesis, “the dynamical is a consequence of the statistical”: it was assumed that atomic nature was essentially discrete and that dynamics resulted from arbitrary manipulations with the theory of probability through which the discrete elements (quanta) were averaged and put into aggregates “to develop dynamics on the statistical basis.” Aggregates did not result from a sampling and taxonomical statistical analysis; they and their dynamics, he thought, were freely constructed. He believed that the two approaches were legitimate, depending on the problem in hand. However, he remained skeptical about using probability to freely construct aggregates and their dynamics.[44]

[44] E. Wilson to F. Edgeworth, 12 March 1923 (PEBW, 4).

In 1936, *A Commentary on the scientific writings of J. Willard Gibbs,* in two volumes, was published. In the first volume, Wilson discussed *Gibbs' lectures on theormodynamics*. In the second volume, Paul Epstein, a mathematical physicist at the California Institute of Technology, commented on *Gibbs' Methods in Statistics*. Epstein's argument resonated with Wilson's comment on the different working hypotheses in physics to deal with dynamics. He explained, indeed, that in old quantum theory, there was equivalence between dynamical systems and integrable systems. He also pointed out that in the new quantum theory, based on wave theory, such was not necessarily the case because quanta could jump from one stationary equilibrium state to another and there was no way of determining the probability of a specific trajectory. Epstein then argued that such "probability could only be inferred indirectly and approximately, by classical analogies known under the name of 'principle of correspondence'" (1936, 530). Based on the principle of correspondence, Epstein suggested, modern physicists connected and clarified the relationship between the old and the new quantum theory.

In the thesis, Samuelson did not (yet) call his correspondence between comparative statics and dynamics the Correspondence Principle. He thought however that with it the relation between old and new economics, as had been the case regarding the relation between classical and modern quantum mechanics according to Epstein, could be clarified. At the same time, following Wilson, Samuelson's emphasis lay on comparative statics rather than on dynamics as he focused on the (discrete) properties characterizing stationary equilibrium, more cogent with data, and not on moving equilibrium: "In order for the analysis to be useful it must provide information concerning the way our equilibrium quantities will change as a result of changes in the parameters taken as independent data" (Samuelson 1941a, 192).

Samuelson's dynamics was also informed by his personal research on business cycles and population dynamics.[45] In all these investigations, Samuelson encountered a similar formal difficulty when facing series and polynomials that did not converge. These difficulties led him to entertain the idea that the treatment of stability and dynamical questions required more mathematical emphasis. In this vein, he defined dynamics as the study of behavior through time of all variables of a system from arbitrary conditions and referred to stability—as perfect stability of the first kind—as the cases in which "from any initial conditions all the variables approach their equilibrium values in the limit as time becomes infinite" (Samuelson 1941a, 198). He used the general and mathematical formulation of functionals to map a great number of variables themselves functions of time.[46]

Within his general and mathematical framework, Samuelson used some examples of business cycles as well as examples of aggregate supply-and-demand dynamical systems

[45] On business cycles, see Samuelson 1939a; 1939a; 1940. Samuelson started working on these questions in 1938, when he attended Alvin Hansen's Harvard seminar on business cycles. Also see Backhouse 2012. On population growth, see Samuelson's unpublished papers on the question and his correspondence with Alfred Lotka (PASP).

[46] Given Wilson's skepticism of Frisch's structuralist econometrics, it must not be a coincidence that Samuelson called functional equations (Samuelson 1941a, 196) the same kind of equations that Frisch called structural equations (Frisch 1936, 1–2). The difference is important, as, from Wilson's perspective, structuralist approaches illustrated a sort of universalizing scientific approach; from the same perspective, functionals embodied only a possible operational way to deal with complex systems.

to illustrate his general ideas about stability. He was able to show the correspondence between John Hicks's difference equation-system, related to the dynamics of a multimarket system, with a differential equation system. He also showed, in the Keynes-Hansen business cycles case, that there were important correspondences between the static and dynamical cases, studied either with difference equations or with differential equations systems. In all these cases, certain inequalities represented the necessary and sufficient conditions for stability. Also, in all these cases, the correspondence between difference equations and differential equations embodied the ideal of possible translations between continuous and discrete mathematical formulas, while the correspondence between static and dynamical systems showed, Samuelson thought, that the study of dynamics shed light on comparative statics problems, and vice versa.

In the last paragraph of the thesis, the mathematical appendix excluded, Samuelson concluded, pointing out that the study of dynamics and stability had led him "into the most difficult problems in higher mathematics" (Samuelson 1941a, 250), some of which he had shown in the thesis, and for some of which he did not yet have finite results for.

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After the defense of the thesis, Wilson advised Samuelson to translate the mathematics into English. In his words,

> “What I am interested in in your thesis is to have the thing go out if possible so that good economic theorists who are not primarily mathematical economists can get fairly easily from it the things they need to keep them from making mistakes in their literary or semi-mathematical discussions. You have pointed out in the thesis several places where you have definite results that should preclude certain mistaken discussions on the part of economic theorists but I don’t believe that in the present form the economic theorists will get the point. I think there are too many formulas which would scare them off and that a good deal of the text could profitably be rewritten and considerably expanded for their benefit. If this were done in such a way that your contribution meant a good deal to a wide range of economic theorists it would not only help them but it would help them to appreciate the value of rigorous mathematical economics of which not a few of them are rather skeptical.”[47]

Wilson liked the thesis; it embodied his program for mathematical economics. Notwithstanding this, Wilson believed that, in its too-mathematical form, the thesis would not play the pedagogical role among economists that he wanted it to play.

Two years after the defense, Samuelson communicated to Wilson that he was revising the thesis and would love to have his suggestions. In response, Wilson wrote:

[47] E. Wilson to P. Samuelson 14 Jan. 1941 (PEBW, 37).

> "The thesis is so good and you are so busy [with war work and instruction] that I wonder whether you ought to put your time in revising it at all unless there is something really rather important in the way of improvements which you think you can make."[48]

Eventually, Samuelson did not follow Wilson's advice and kept working on the highly mathematical problems that he had encountered.

Samuelson and Wilson remained in close contact as Samuelson was working on a manuscript based on his thesis, which he would submit for publication to the Harvard University Press at the beginning of 1945. *Foundations of Economic Analysis*, as he titled the extended version of his thesis, wasn't published until 1947, due to publishing delays.

IV. FOUNDATIONS: THE FINISHING TOUCHES

When Samuelson defended his thesis, he was already appointed Assistant Professor of economics at MIT (Backhouse 2014). There, between 1941 and 1945, he was put in charge of graduate elective economics courses. He lectured on *Economic Analysis* and *Business Cycles* and offered a course titled *Mathematical Approach to Economics* and another, in collaboration with Harold Freeman, titled *Advanced Economic Statistics.* He also taught *Public Finance* to engineering (marine transportation) undergraduate

[48] E. Wilson to P. Samuelson, 10 Apr. 1942 (PASP, 77).

students as of 1943.[49] Concomitant with his instructional responsibilities, Samuelson embarked on war work; between 1941 and 1943, he acted as a consultant to the National Resources Planning Board (NRPB) in Washington.[50] Already in July of 1943, he "was engaged in some part-time, technical war work," probably at MIT.[51] In view of this experience "in testing anti-aircraft," Samuelson was released from his instructional duties from March 1944 to July 1945 to work as a full-time staff member mathematician on ballistics at the MIT Radiation Laboratory.[52]

Despite his war research experiences, Samuelson kept unchanged the core of his thesis for *Foundations*, in particular the three introductory chapters. As he wrote to Wilson, "The principle changes have been a new chapter on Welfare Economics, further discussion of dynamics and an appendix on elementary difference equations."[53] In the framing of some of these expansions, Wilson was still highly influential.[54]

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[49] MIT Annual reports, 1942-1946. See http://libraries.mit.edu/archives/mithistory/presidents-reports.html.

[50] For Samuelson's empirical work at the NRPB see Backhouse 2012; Maas 2014.

[51] P. Samuelson to E. Wilson, 29 July 1943 (PASP, 77).

[52] P. Samuelson to K. Compton, 21 March1944 (PASP, 19). For Samuelson's work at the Radiation Laboratory, see Backhouse and Maas 2016.

[53] P. Samuelson to E. Wilson, 27 Feb. 1945 (PASP, 77).

[54] Other significant influences must have been at play at MIT; in particular, Samuelson often interacted with the mathematician Norbert Wiener. Of relevance for the Wilson-Samuelson connection was the fact that Wilson had promoted Wiener's career, writing various letters of recommendation and supporting him for the Guggenheim Scholarship (E. Wilson to N. Wiener 10.6.1925 [PEBW, 9]), which Wiener obtained in 1926.

On dynamics, Samuelson further developed the difficult problems in higher mathematics that he had encountered; these involved studying stability issues of linear and non-linear systems. This time, Samuelson called his correspondence between dynamics and comparative statics the Correspondence Principle.[55]

Further exploring the mathematical difficulties that he had encountered in the thesis involved connecting his dynamics with (analytical) statistics, which he attempted to do in the second appendix on difference equations and in various mathematical and statistical papers that he wrote between 1940 and 1943.[56] Given all his war duties, Samuelson seemed to have used his lectures as a way of making progress in his research. As he wrote to Harold Hotelling in July 1943, with whom he had been corresponding about his research on mathematical statistics,

> "For the last three years, in lectures, and in my notes I have been developing various numerical methods in connection with inverting linear equations, scalar and matrix iteration, determination of latent roots and vectors."[57]

To deal with these complex problems, Samuelson connected statistics with numerical and computational methods; in these efforts, he was not only building on Wilson's lectures on mathematical statistics, he was actually collaborating with Wilson on

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[55] See Samuelson 1942.

[56] See Samuelson 1941; 1942b; 1942a; 1943c; 1943a.

[57] P. Samuelson to H. Hotelling, 21 July 1943 (PASP, 34)

instruction of mathematical and statistical economics by sending him some of his MIT students and letting them write papers (for final examination) "in a cooperative fashion,"[58] in which Samuelson and Wilson would agree on the subject covered.[59]

In 1942, they seemed to have encouraged their students to make some explorations based on the work of Whittaker and Robinson as well as of Alexander C. Aitken. In the middle of the following year, Samuelson sent two papers that he had written to Wilson in which he fully developed on the work of these applied mathematicians. Despite the fact that the rules of the PNAS, which Wilson still edited, prevented him from sponsoring particular papers, he made "an exception to the general rule and [took] them under [his] own sponsorship."[60] The papers appeared in the December 1943 volume of the PNAS.[61] Samuelson was happy about their publication: he "could make reference to them in connection with other work on the fire,"[62] related probably to his war work on ballistics and/or to his appendix on difference equations.

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[58] E. Wilson to P. Samuelson, 10 May 1942 (*idem*).

[59] In particular, Samuelson's Ph.D. student Lawrence Klein took one of Wilson's courses. Wilson was impressed by Klein, and even suggested Samuelson to sponsor him for election at the Harvard Society of Fellows (E. Wilson to Samuelson, 12 Apr. 1943 [PASP, 77]); Samuelson thought that Klein was "topnotch," but was not yet ready to be left alone for independent research (P. Samuelson to E. Wilson, 29 July 1943 [*idem*]).

[60] E. Wilson to P. Samuelson, 2 Nov. 1943 (*idem*). See also E. Wilson to P. Samuelson, 27 July 1943 (*idem*).

[61] See Samuelson 1943; 1943c.

[62] P. Samuelson to E. Wilson, 5 Nov. 1943 (PASP, 77).

With respect to the new chapter on welfare economics, Wilson's influence on Samuelson remained unclear, as Samuelson argued in his doctoral papers on trade theory and welfare economics that there was no way of determining operationally and meaningfully the existence of a unique utility index enabling welfare comparisons (Samuelson 1938b; 1938e; Samuelson 1939b). In the thesis, Samuelson did not include his work on trade theory and welfare economics, probably because he felt that it did not respond to Wilson's call for operationally meaningful knowledge.

In *Foundations*, at the end of the first part, in which he was exploring the consequences of the assumption of extremum positions, Samuelson added his work on welfare economics, introducing it with an extensive historical account of the subject. Samuelson still argued "that the theorems enunciated under the heading of welfare economics are not meaningful propositions" (Samuelson 1947, 220). Samuelson was probably then no longer writing only for Wilson.

V. CONCLUSION

As suggested by Wilson and Samuelson in the opening quotations of this paper, Samuelson's thesis and *Foundations* reflected his active commitment to Wilson as regards mathematics, statistics and science. This paper sought to reconstruct this commitment. For this purpose, similarities in their work and ideas were traced and then used as the common thread that unified the story of this commitment.

Echoing Wilson, Samuelson's recurrent diagnosis of the contemporary state of economics literature consisted of emphasizing the lack of *operationally meaningful* knowledge due to bad methodological approaches adopted by economists. In a Wilsonian spirit, Samuelson treated *mathematics as a language* and attempted to develop *operationally meaningful theorems*: he used his analytical skills and techniques in mathematics and statistics to establish correspondences between the conventional economic notion of equilibrium, at the individual and aggregate levels, and the mathematical structural characteristics of optimization problems under constraint and of functional analysis. At the same time, he thought that this sort of mathematics of the continuous, already standard in his contemporary mathematical economics, which he used, remained empirically empty. In this vein, he sought to connect his work with some sort of data. But by adopting Wilson's skepticism of classical statistics and probability, Samuelson did not embark on standard statistical work of estimation of parameters or regressions; he rather attempted to *translate* formulas defined in the continuous into formulas of discrete magnitudes, following Wilson's characterization of a stable equilibrium position, which was defined with a discrete time-independent inequality. In this way, Samuelson succeeded in comprehensively presenting the notion of equilibrium as simultaneously being empirical (therefore intuitive), theoretical and mathematical. However, he did not consistently show the formal interconnections between the microeconomic and macroeconomic equilibria.

In *Foundations*, Samuelson worked willingly to create the new based on the old. His modern economics was not a break with extant economics; his modern economics was a way of mediating between the new and the old. In the *old* new-classical economics, mathematics of the continuous, as instantiated in marginal and differential calculus, was commonly used. Useful, operational and meaningful knowledge required however connecting conventional working hypotheses of economics with mathematical structures *and* data. Of particular relevance in *Foundations*, Samuelson attempted, albeit in highly abstract and analytical ways, to connect his mathematical economics with data, by means of establishing correspondences between the continuous cases as found in marginal and differential calculus and the finite cases found in the discrete world of economic phenomena.

From this Wilsonian perspective, Samuelson's *Foundations* appears to be an exploration to find formulas composed by discrete magnitudes, observable in idealized conditions. Under this new light, *Foundations* can be regarded as an attempt to provide an alternative approach to the econometric movement. In such an approach, the statistical treatment of economic data was mainly analytical, indeed taxonomical; it implied avoiding probability theory in the construction of central concepts, of aggregates and of their dynamics.

Notwithstanding the emphasis on a discrete economic world, Samuelson did not offer new foundations for economics based on discrete mathematics; instead, he endeavored, as illustrated by his Le Chatelier Principle and Correspondence Principle, to establish correspondences between the discrete and the continuous, developing the mathematics of the continuous. In this work of "translation", he left some aspects of his mathematics incomplete, not fully consistent, and filled the gaps with economic intuition and meaning. In that sense and despite its sophisticated mathematical character for the time in economics, *Foundations*, in the last analysis, appears to have offered in a Wilsonian spirit *much economics with little mathematics*.

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