UNIVERSITÉ DU QUÉBEC À MONTRÉAL

EDWIN B. WILSON AUX ORIGINES DE L'ÉCONOMIE MATHÉMATIQUE DE PAUL SAMUELSON: ESSAIS SUR L'HISTOIRE ENTREMÊLÉE DE LA SCIENCE ÉCONOMIQUE, DES MATHÉMATIQUES ET DES STATISTIQUES AUX ÉTATS-UNIS, 1900-1940

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DEDICATION

A Decci, una mujer de fuego y de nieve,
y a Blanca Sofía, que en paz descanse.

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LIST OF ABBREVIATIONS, ACRONYMES AND INITIALISMS

AAAS	American Association for the Advancement of Science
AEA	American Economic Association
AMS	American Mathematical Society
ASA	American Statistical Association
HSPH	Harvard School of Public Health
HUA	Harvard University Archives
IMS	Institute of Mathematical Statistics
JTP	James Tobin Papers
LC	Library of Congress
LMP	Lloyd Metzler Papers
MIT	Massachusetts Institute of Technology
NACA	National Advisory Committee for Aeronautic
NAS	National Academy of Science
NASA	National Aeronautics and Space Administration
OVP	Oswald Veblen Papers
PASP	Paul A. Samuelson Papers
PEBW	Papers of Edwin Bidwell Wilson
PNAS	Proceedings of the National Academy of Science
SSRC	Social Science Research Council
TAMS	Transactions of the American Mathematical Society
UQAM	Université du Québec à Montréal

RÉSUMÉ

Cette thèse porte sur l'interconnexion, trop souvent négligée, entre les histoires de la science économique, des mathématiques, des statistiques, des sciences naturelles et sociales aux États-Unis, entre les années 1900 et 1940. La thèse étudie deux personnages scientifiques américains, Paul A. Samuelson (1915-2009) et Edwin B. Wilson (1879-1964), ce dernier étant le professeur d'économie mathématique et statistique avancées de Samuelson, à l'Université de Harvard au milieu des années 1930. Sur la base de recherches d'archives, cette thèse reconstruit, la carrière professionnelle de Wilson et étudie le travail en économie mathématique de Samuelson au début de sa carrière. De cette façon, les histoires de plusieurs disciplines scientifiques entre 1900 et 1940 sont reconstruites et les racines, profondément américaines, des idées au sujet des mathématiques et des sciences de Samuelson sont identifiées.

Wilson était un mathématicien formé autour des années 1900 à l'Université de Harvard, à l'Université de Yale, où il reçut son doctorat, et à l'École Normale Supérieure de Paris. Wilson se développa en tant que mathématicien en s'engageant avec une tradition mathématique américaine établie par Josiah Willard Gibbs, son mentor à Yale, et en rejetant le type de mathématiques proposées par l'allemand David Hilbert, dont l'influence au sein de la communauté de recherche des mathématiciens américains modernes était remarquable. Aux débuts de sa carrière, Wilson appartenait à cette communauté, étant même parmi l'un de ses membres les plus actifs. Cependant, au cours de sa carrière, graduellement et progressivement, Wilson se marginalisa de la communauté américaine des mathématiciens, comme conséquence de son rejet de Hilbert et son engagement avec Gibbs. De façon concomitante, Wilson entra en contact avec d'autres communautés scientifiques et d'autres champs de recherche, notamment les statistiques et l'économie.

Après avoir suivi les cours de Wilson en économie mathématique, pendant lesquels il enseignait la théorie du consommateur da façon analogique à la thermodynamique de Gibbs, et en statistiques mathématiques pendant son doctorat à Harvard, Samuelson s'engagea et adopta l'attitude Gibbsienne envers les mathématiques de Wilson.

Trois chapitres composent cette thèse. Tout d'abord, les efforts de Wilson pour connecter les sciences américaines avec les mathématiques entre les 1900 et 1940 ainsi que sa définition de la rationalité mathématique et scientifique sont discutés. Deuxièmement, l'influence de Wilson sur la montée de l'économie mathématique aux États-Unis entre les années 1920 et 1930 est étudiée. Finalement, l'influence significative de Wilson sur les *Foundations of Economic Analysis* de Samuelson est détaillée.

Mots clés: E. B. Wilson, P. Samuelson, histoire de la science économique moderne, histoire des mathématiques et des sciences, biographie intellectuelle.

ABSTRACT

This is a thesis in the interwoven, yet often presented as separated, histories of economics, mathematics, statistics, social and natural sciences in the United States of America between 1900 and the 1940s.
The focus is placed on two American scientific figures, Paul A. Samuelson (1915-2009) and Edwin B. Wilson (1879-1964), Samuelson's professor of advanced mathematical and statistical economics at Harvard University in the mid-1930s. Based on documents found in Samuelson's and Wilson's archives, Wilson's professional career is reconstructed and Samuelson's early work in mathematical economics is studied. In this way, the histories of various scientific disciplines in America between 1900 and the 1940s are reconstructed, and the deeply American roots of Samuelson's ideas about mathematics and science are identified.
Wilson was a mathematician trained around 1900 at Harvard University where he attended college, Yale University were he received his Ph.D., and at the École Normale Supérieure in Paris where he spent one postdoctoral year. Wilson developed as a mathematician, committing to an American mathematical tradition established by Josiah Willard Gibbs, Wilson's mentor at Yale, and in clear contrast with David Hilbert's kind of mathematics, which played a significant role in the development of the American mathematical research community. At the beginning of his career, Wilson belonged to that community. He was even one of its most active members. However, because of his rejection of Hilbert and his commitment to Gibbs, over his career, Wilson gradually and progressively marginalized himself from the American community of mathematicians. Concomitantly, he made incursions into other scientific fields and communities, in particular statistics and economics.
Having attended Wilson's courses in economics in which Wilson taught consumer theory analogically to Gibbs thermodynamics and mathematical statistics having in mind spectral analysis for the study of business cycles during his doctoral years at Harvard, Samuelson fully engaged with Wilson's Gibbsian kind of mathematics.
Three different essays compose the thesis. Firstly, Wilson's efforts to connect American sciences with mathematics between 1900 and the 1940s and his definition of mathematical and scientific rationality are discussed. Secondly, Wilson's influence on the rise of mathematical economics in America between the 1920s and 1930s is studied. Thirdly and finally, Wilson's catalytic influence for Samuelson's *Foundations* of Economic Analysis is examined.
Key words: E. B. Wilson, P. Samuelson, history of modern economics, history of mathematics and science, intellectual biography.

INTRODUCTION

In the opening page of his doctoral thesis, defended in 1940, and of his *Foundations of Economic Analysis* (1947), an expansion of his thesis, Paul A. Samuelson (1915-2009) wrote: "Mathematics is a language." He also attributed this motto to Josiah Willard Gibbs, and suggested at the same time that he had adopted the Gibbsian mathematical style in his work in mathematical economics.

The present thesis in the history of economics, composed of three distinct but interconnected essays,[1] aims at historically reconstructing the discourse that shaped Samuelson's mathematical thinking and led him to regard mathematics as a language.

With this in mind, in the following pages we will endeavor to bridge the interconnected yet often presented as separate histories of economics, mathematics, statistics, social and natural sciences in the United States of America between 1900 and the 1940s. The focus is placed on Edwin B. Wilson (1879-1964), Samuelson's professor of advanced mathematical economics and mathematical statistics at Harvard University in the mid-1930s, and on the great but yet understudied influence that Wilson had upon Samuelson's attitude towards mathematical *and* statistical economics, which eventually framed and limited Samuelson's mathematical thinking, and which therefore shaped Samuelson's thesis and *Foundations.*

Wilson was an American polymath who was educated around 1900 at Harvard University where he attended college, at Yale University where he obtained his Ph.D. in mathematics and where he met his mentor Josiah Willard Gibbs, and at the École Normale Supérieure in Paris where he met all the luminaries of the French

[1] The three essays were written for different audiences. The format of references of each was adapted accordingly and kept unchanged in this thesis. As the three essays are directly connected to Wilson, but from different angles, the reader will find, here and there, certain repetitions.

mathematical community. Wilson played an important yet unexplored role in the history of American mathematics and its interconnections with various sciences—physics, statistics, economics, sociology—and their development.

Key to understanding Wilson's intellectual legacy is knowing his suspicion regarding what he saw as the excessive abstraction of German structuralist mathematics. Wilson believed that mathematics, following Gibbs' kind of mathematics, should be immediately useful for operational purposes, where the *operational* implied the *practical*. Regarding mathematics and science, Wilson's ideas about the operational and the practical embodied a sequence of two elements. Firstly, they implied establishing and adopting a specific *discipline in the practice* of mathematics, which should consist of restraining pure mathematical abstractions, as mathematical structures, by the prevailing working hypotheses and the data of the subject matter to which mathematics was applied. Only in this regard, for Wilson, was operational mathematics relevant for the practice of science as truly scientific. Secondly, in his view, once practitioners of the subject matter eventually adopted the "right" kind of Wilson's Gibbsian discipline of mathematics, mathematics and science would become useful for *practical* purposes of individual life (how to choose and select among various alternatives) and collective life (enhancing national security, public health, prosperity, etc.) in a sound scientific way.

Samuelson was an American economist who received his undergraduate training at the University of Chicago and his graduate training from Harvard University. He spent his career as professor at the Massachusetts Institute of Technology (MIT), receiving the Nobel Prize in Economics in 1970. Samuelson was an architect of the modern economics discipline (Arrow 1983; Hahn 1983; Brown and Solow 1983). Various generations of economists have been trained with some of his books. It is significant that Samuelson declared himself a disciple of Wilson (Samuelson 1998), who taught him the tools of continuous and numerical mathematics related to Gibbsian thermodynamics.

In the literature of history of economics, it has been argued that Samuelson was influential in establishing the identity of economists as being scientists (Mirowski 1989). Although some accounts have expounded on how economics became a mathematical discipline at large (Düppe and Weintraub 2014; Leonard 2010; Giocoli 2003; Weintraub 2002; Mirowski 2002; Ingrao and Israel 1990), sometimes claiming a formalist revolution (Blaug 2003; Landreth and Colander 2004) and how, in America, economics was transformed during the interwar period (Morgan and Rutherford 1998; Johnson 1977), and further how certain American mathematical economists, uncritically inspired by a physical metaphor, participated in the mathematical transformation of the discipline (Hands and Mirowski 1998; Mirowski and Hands 1998), the figure of Wilson has been relegated to footnotes. Neither Wilson's imprint on the history of economics as intertwined with the history of other disciplines in America, nor his influence on Samuelson's attitude towards mathematics, statistics and science, have yet been the subject of detailed historical analyses.

In a similar way, Wilson has been relatively disregarded by the history of mathematics, science, statistics and social science.

If Wilson played such an important role in the development of American science, and if he was so relevant for the shaping of Samuelson's ideas about mathematical and statistical economics, and therefore for the history of economics, one may naturally wonder: why has he been neglected by the literatures of history of mathematics, science, statistics and economics?

A possible reason for this historical oversight emerges from the fact that, traditionally, the history of specific scientific disciplines has been concerned with internal questions. Although Wilson actively participated in the process of specialization of various scientific fields by offering contributions in mathematics during the 1900s, mathematical physics and relativity during the 1910s, statistics during the 1920s and 1930s, as well as economics and social science during 1930s, he was not necessarily the most original at the creative front. Consequently, his legacy in the subsequent and separated development of each one of these fields was not necessarily central. In this way, it seems normal that Wilson has remained relatively unnoticed by the internally concerned history of each one of the fields to which he contributed.

A possible reason explaining the relative neglect of Wilson and his influence on Samuelson by the history of economics is related to the fact that his main influence on the discipline was at the organizational and educational fronts. Wilson's influence on Samuelson's mathematical *and* statistical economics took shape precisely during Wilson's graduate courses at Harvard. Historians of economics who have studied Samuelson's work and who potentially could have explored Wilson's relevance in economics more in detail, have focused their attention on specific theoretical concerns on which Samuelson's ideas played an important role for subsequent developments of the discipline.[2] From the perspective of these forward-looking historical narratives, it seems also normal that Wilson has been relegated to passing comments, as his relevance for economics, if merely regarded through Samuelson's work, would have required a backward-looking perspective.

Samuelson passed away in 2009; only since 2011 have historians of economics been able to fully consult his personal archives, which contain inestimable evidence of his commitment to Wilson's ideas about mathematics, statistics and science. Of particular relevance for this project, Roger Backhouse is currently writing an

[2] Such as Samuelson's dynamics (Weintraub 1991) or his consumer theory (Hands 2014; 2014; 2013; 2006; Mirowski and Hands 2006; Wong 1978).

intellectual biography of Samuelson (Forthcoming). 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 the door for the present thesis at the same time, which focuses exclusively on Wilson and on the Wilson-Samuelson connection.

Historians of economics who studied Samuelson's work before 2011 did not have access to this archival material. After having visited Samuelson's archives, located at the David M. Rubenstein Rare Books & Manuscripts Library at Duke University, we decided to dive into Wilson's archives, located at Harvard University Library.

On the basis of the material consulted in these archives, it became evident that Wilson's legacy was significant in establishing interconnections between American scientific fields and the development of interdisciplinarity. Illustrative of such influence, since the first publication of the *Proceedings of the National Academy of Science* in 1915, and until he passed away in 1964, Wilson served as its managing editor. Wilson's organizational and educational efforts embodied a specific project to connect mathematics to other fields in order to make them "really" scientific, and, at the same time, to provide mathematics with its operational intelligibility. He worked willing to establish the right kind of practices of present and future American mathematicians and scientists. Wilson's activism clearly aimed at reforming American academia, following ideas that he himself developed about the foundations of mathematics and science, which were unique and original as well as based on his understanding of Gibbs' mathematical style.

The history of science has only relatively recently been concerned with the interconnected histories of different scientific fields; Wilson became relevant for historical accounts only from that perspective. His career and work underlay a story of disciplinary migration from mathematics to other fields, especially to statistics and economics.

The approach that we adopted consists precisely of historically exploring and reconstructing Wilson's career and work in order to study the Wilson-Samuelson connection as well as Samuelson's early work.[3] What did Samuelson mean by "mathematics is language"? What was the influence that Wilson exerted on Samuelson's early (mathematical and statistical) thinking? How did this influence come to be? What did he thought about the foundations of mathematics and science? Were his foundational ideas original? Did he define mathematics as a language? How did he come to be involved with mathematical and statistical economics? And how did he come to teach these fields to graduate students in economics at Harvard University where he met Samuelson? These will be the questions that will guide our thinking in this dissertation.

In this spirit, by following Wilson through his academic professional life, the emphasis on interconnections between the histories of various fields in America simply emerged as evident. In the same vein, by following Wilson until he met Samuelson, and by following their close intellectual relationship, the focus on Samuelson's commitment to Wilson's ideas about mathematics and science also emerged as evident. As Wilson himself committed to mathematics of the Gibbsian kind, by studying the intellectual lives of Wilson (extensively) and Samuelson (early professional years), the present work reconstructs a very American story of the Gibbs-Wilson-Samuelson intellectual genealogy.

Eventually, this research sheds new light on Samuelson's *Foundations*, connecting it with Wilsonian concerns about the foundations of economics, mathematics *and* statistics. It contributes to our understanding of how economics became the modern

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[3] On biography as a historiographical category in the history of economics, and of science, see (Weintraub and Forget 2007). Robert Leonard's *Von Neumann, Morgenstern, and the Creation of Game Theory: From Chess to Social Science, 1900-1960* (2010) as well as Till Düppe's and Roy Weintraub's *Finding Equilibrium: Arrow, Debreu, McKenzie and the Problem of Scientific Credit* (2014) are significant contributions to the history of economics that adopt such biographical framework.

mathematical and statistical scientific discipline that it turned to be in the aftermath of the Second World War.

In the first chapter, Wilson's efforts in connecting sciences in America between 1900 and the 1940s will be discussed; it will be emphasized that even though he professionally engaged with various fields, such as mathematics, physics, statistics and economics, his ideas about mathematical and scientific rationality, which he developed as he embarked on foundational discussions of mathematics and science (which will be explained) constituted the common thread connecting the different and distinct stages of his career. (Mathematical) rationality, he claimed, was the only valid invariant in science, in a strict sense, as *everybody* could learn its techniques and think with it, as everybody could learn a language.

In the second chapter, Wilson's influence on the rise of mathematical economics in America between the 1920s and 1930s will be explored; the focus will be laid on showing how, on the grounds of his foundational ideas and his definition of scientific rationality, Wilson worked at the organizational and educational levels to modernize economics. The chapter will thus discuss the ways in which he was key in the constitution of the first organized community of American mathematical economists; his crucial role in the origins of the Econometric Society, which bears new lights on the constitution of the econometric movement; and his leadership in promoting and establishing the first program in advanced mathematical and statistical economics at Harvard as well as his two courses to economists, Mathematical Economics and Mathematical Statistics.

Finally, Wilson's catalytic influence on Samuelson's *Foundations of Economic Analysis* will be explained. The accent will be laid on exploring the various ways through which Wilson framed and limited Samuelson's mathematical and statistical thinking during his doctoral years at Harvard. The analysis will consist of studying Samuelson's thesis, which constituted the body of *Foundations* itself, as well as

Foundations, while detailing the ways in which these works were done in a Gibbs-Wilsonian spirit. In this way, Wilson's influence on Samuelson's general ideas about mathematics and science, as well as his influence regarding certain Samuelson's theoretical concerns in economics will be explored.

By historically reconstructing the intellectual experiences of Wilson and Samuelson, and drawing from the Gibbs-Wilson-Samuelson connection developed in our thesis, this project connects Samuelson's "Mathematics is a language" to a deep, sophisticated and complex discourse about the foundations of mathematics and science, which was far from a naïve statement about the nature of mathematics and science. This discourse was embodied in Wilson's Gibbsian style of mathematical and scientific thinking, in the spirit of which Samuelson developed his thesis and *Foundations*.

CHAPTER I

EDWIN B. WILSON AND SCIENTIFIC RATIONALITY: CONNECTING AMERICAN SCIENCES, 1900-1945

1.1. Introduction[1]

On January the 18th, 1920, E. B. Wilson was invited to deliver a talk on relativity to the Royce Club, which was an informal interdisciplinary group of scholars from Boston and Cambridge, Massachusetts. He opened his talk as follows:

> "There is [...], to-day, in the physical world a general unrest, […]. This, unrest leads physicists to alternate, according to their temper, between a despair of ever settling the physical bases of the many new facts which experiment is thrusting upon them and a desperate grasping at any theoretical straw that offers even a feeble chance of support in the flood.

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[1] E-mail: carvaja5@gmail.com. I am grateful to Roger Backhouse, Leo Corry, Till Düppe, Robert Leonard, Karen Parshall, Ted Porter and Roy Weintraub for their comments on this chapter. The usual caveat applies. I am also thankful to archivists at the Harvard University Archives (HUA) and at the Library of Congress (LC) in Washington, DC. Papers of Edwin Bidwell Wilson (PEBW) were consulted at HUA, HUG4878.203 (indicated if different) and Oswald Veblen Papers (OVP) at the LC, DC.

> In this generally febrile condition of our science it is particularly unsafe to draw philosophical conclusions."[2]

E. B. Wilson (1879-1964) was an American polymath trained in mathematics around 1900 at Harvard University, Yale University and the École Normale Supérieure in Paris. He taught and conducted research in mathematics at Yale (1903-1907), in mathematical physics at the Massachusetts Institute of Technology (1907-1922), in statistics at the Harvard School of Public Health (1922 and thereafter) and in social science and economics at Harvard (1932 -1945).

Wilson's diagnosis of physics in 1920 was similar to the diagnosis that he had offered for mathematics around 1900, and to the diagnosis that he would later make for statistics, social science and economics: he believed that the scientific foundations of all these fields were missing. Consequently, he felt that his scientific era was one of unrest. Facing this lack of sound foundations, and confronted by the difficult choice regarding the kind of attitude that they should adopt in their creative practices, Wilson believed, mathematicians, physicists, statisticians, social scientists and economists, perplexed, tended to commit to wrong methodological approaches. Noticing such unrest, early in his career as a mathematician, Wilson offered, in a unique and original way in regard to his mathematicians colleagues, his own foundations of mathematics and science. Such efforts led him to define in his terms mathematical and scientific rationality and to explain how it interacted with scientific methodology in natural and social sciences.

In Wilson's definition of mathematical and scientific rationality, intuition and personal judgment, indeed meaning, were as relevant as rigor and logic. Invariably using his ideas about mathematical and scientific rationality as a guideline and the

[2] Edwin Wilson, "Space, Time, and Gravitation," *The Scientific Monthly* 10, no. 3 (1920): 217.

bridge connecting sciences, Wilson played a significant role in the development of interdisciplinarity in American sciences between 1900 and 1945.

This influence emerged as Wilson left and entered different scientific communities and fields. In this process, over time, as he was confronted with different questions and different contexts, his foundational ideas were subject to modifications. Theses changes took place as Wilson rejected certain European intellectual traditions, such as the David Hilbert structuraltist mathematics and Karl Pearson's statistics, and as he engaged (partially) with Henri Poincaré's conventionalism and Charles S. Peirce's pragmatic notion of probable inference, (fully) with the American mathematical and scientific attitude of Josiah Willard Gibbs, his mentor at Yale, as well as with the work of those who, he thought, worked in a Gibbsian spirit, such as the physiologist Lawrence Henderson, Wilson's colleague at Harvard. At the same time, ideas relating to the American nation in the context of the First World War, as well as ideas about society and culture in the context of the American social unrest of the 1930s will appear in his foundational discussions. Such elements did not only provide a historical context; they gradually entered in his epistemology as entities shaping (his) intuition: they provided meaning and determined soundness of scientific statements, often even in more significant ways than formal consistency.

Wilson's epistemology was also intrinsically connected with pedagogy. Through (reforms of) high education, he thought that his mathematical and scientific rationality could help mold, indeed discipline, the behavior of scientists, and eventually of citizens. After situating the context of Wilson's education as a mathematician, this chapter will then discuss Wilson's definition of scientific rationality. Subsequently, his nationalistic pleas for reforms of education of mathematics, his work in mathematical physics, his statistical turn, his work in social science and economics—made hand in hand with Henderson—, and his 1945-1946 *Stevenson Lectures in Citizenship* in Glasgow will be successively discussed.

1.2. A doubly difficult situation

E. Wilson grew up in Middletown, Connecticut. His father, Horace, a Yale alumnus, was the Principal of schools at Middletown, before establishing his own private secondary school. E. Wilson was educated at his father's school. When E. Wilson was sixteen years old, the Wilsons moved to Cambridge, Massachusetts. The same year, E. Wilson entered Harvard College. Horace's two other sons would also study at Harvard, and would become doctors. E. Wilson concentrated on mathematics.[3] In 1899, he moved to New Haven, where he obtained a Ph.D. in 1901 in mathematics at Yale.

At Harvard and at Yale, Wilson was instructed by mathematicians of two contrasting generations: an American traditional generation of applied mathematicians and a young European-educated generation of modern mathematicians.[4] This generational contrast marked a transitional period during which the meaning of mathematical rigor was changing: for the traditional American applied mathematicians the criterion of rigor for valid generalizations involved qualitative deductive reasoning *and* quantitative measurements, which constantly constrained each other, giving rise to the term of *constrained mathematics*, and for modern mathematicians the rigor criterion consisted of unconstrained chains of deductive reasoning based on clearly specified assumptions.[5]

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[3] See Jerome Hunsaker and Saunders Mac Lane, "Edwin Bidwell Wilson, 1879-1964, A Biographical Memoir," *National Academy of Sciences (US)*, 1973, 283–320; Edwin Wilson, Interview with Dr. Edwin Wilson, interview by Bruce Lindsay and James King, 1962, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/5065.html.

[4] The contrast between these two traditions was not bold. See Karen Parshall and David Rowe, *The Emergence of the American Mathematical Research Community, 1876-1900: J.J. Sylvester, Felix Klein, and E.H. Moore* (United States of America: American Mathematical Society, 1994). In Wilson's thinking, however, already around 1905, the idea of a sharp contrast started to emerge.

[5] See Giorgio Israel, "Rigor and Axiomatics in Modern Mathematics," *Fundamental Scientiae* 2, no. 2 (1981): 205–19.

Around 1900, Wilson simultaneously engaged with both kinds of mathematics. At Harvard he wrote his Master's dissertation on geometry; at Yale, in 1901, he obtained his Ph.D., officially, with a paper on geometry.[6] This work was made under the supervision of young mathematicians, Maxime Bôcher (1867-1918) at Harvard and of Percy Smith (1867-1957) at Yale. At the same time, in New Haven, having attended almost all the courses in mathematical physics given by Gibbs (1839-1903), the organizing committee of the Yale Bicentennial commemorations taking place in 1901 asked Wilson to write a book on vectors based on Gibbs' course materials. Gibbs was too busy writing another book and told the committee that Wilson was the person best suited to do the job.[7] Wilson succeeded in having a textbook just in time for the Yale festivities.[8] Through this effort, Wilson became proficient in Gibbs' mathematics and was able to offer a comprehensive and even deeper account of his master's notation of vectors.[9]

This simultaneous engagement with two contrasting attitudes toward mathematics put Wilson in a doubly difficult situation.

His *choice* of engaging with Gibbs's traditional mathematics contrasted with the orientation toward (pure) mathematics of young modern mathematicians. At the same time, Wilson's work on geometry, vector analysis and multiple algebra helped him to be accepted as a peer among leaders in the community of American modern mathematical research. During the first decade of the 20th century, Wilson was indeed

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[6] Edwin Wilson, "The Decomposition of the General Collineation of Space into Three Skew Reflections," *Transactions of the American Mathematical Society* 1, no. 2 (1900): 193–96.

[7] See Josiah Gibbs, *Elementary Principles in Statistical Mechanics* (New York: Dover Publication, Inc., 1902).

[8] Edwin Wilson, *Vector Analysis: A Text-Book for the Use of Students of Mathematics and Physics* (United States of America: Yale University Press, 1901).

[9] Michael Crowe, *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System* (New York: Dover, 1985).

one of the “most active”[10] members of that community, with numerous original publications and reviews. In August 1904, he popularized Gibbs’ vector analysis at the third International Congress of Mathematics in Heidelberg.[11] That same year, and until 1916, he cooperated in the publication of the *Transactions of the American Mathematical Society* (TAMS).

In return, this engagement with mathematics put him in a difficult situation vis a vis the rest of the American scientific community. Indeed, while American mathematicians gained institutional autonomy focusing on pure mathematics, American scientists tended to make their marks in terms of observation, experimentation and laboratory-based-work, where measurement was central, rather than in mathematical terms.[12]

1.3. Defining scientific rationality

1.3.1. A pedagogical question

Wilson had written his *Vector Analysis* with pedagogical purposes in mind, presenting it as a textbook for students of mathematics and physics; after the thesis, while he was instructing himself in mathematical physics, he felt:

[10] Della Dumbaugh Fenster and Karen Parshall, “A Profile of the American Mathematical Research Community: 1891-1906,” in *The History of Modern Mathematics: Images, Ideas, and Communities*, ed. Eberhard Knobloch and David Rowe, vol. 3 (Boston: Academic Press, 1994), 179–227.

[11] During the same session, Hilbert talked about the foundations of arithmetic.

[12] See John Servos, “Mathematics and the Physical Sciences in America, 1880-1930,” *Isis* 77, no. 4 (1986): 611–29.

> "What the type of our instruction in theoretical mechanics shall be, whether we shall lean toward the English, the German or the French, is a question which is not yet settled."[13]

Starting from this pedagogical question, Wilson embarked on foundational discussions developing a unique and original position in regard to his American mathematicians colleagues. His diagnosis consisted of remarking that because foundations of physics were missing, physicists tended to adopt wrong methodological attitudes. On one side, he wrote, English authors tended to use naïve intuition and *a priori* innate concepts with no mathematical refinements. On the other side, authors of the German-rigorist kind tended to build on strict geometrical logic by employing abstract mathematical ideas. The latter, Wilson stressed, dominated the field because the laws stated by Newton were no longer satisfactory.

> "This lack of satisfaction is but one of the many similar manifestations of the present state of mathematical instruction and mathematical science. *We* are no longer content to bear with superficially clear statements which seldom if ever lead into actual error—nor does it suffice to start with inaccurate statements and, as we advance, to modify them so as to bring them into accord with our wider vision and our more stringent requirements."[14]

The English and the German being excluded, Wilson would lean toward the French. Jacques Hadamard, a French mathematician, attended the Yale Bicentennial festivities. Taking seriously his invitation to come to France, Wilson spent one year of leave in Paris between 1902 and 1903. In Paris, Wilson attended lectures at the

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[13] Edwin Wilson, "Some Recent Books on Mechanics," *Bulletin of the American Mathematical Society* 8, no. 8 (1902): 343. See also Edwin Wilson, "Some Recent Books on Mechanics," *Bulletin of the American Mathematical Society* 9, no. 1 (1902): 25–39; "Some Recent Books on Mechanics," *Bulletin of the American Mathematical Society* 8, no. 9 (1902): 403–12.

[14] Wilson, "Some Recent Books on Mechanics," 1902, 342 italics added.

École Normale Supérieure, the Sorbonne and the Collège de France. He audited courses given by Henri Poincaré, Hadamard, Camille Jordan, Émile Picard, Joseph Boussinesq and Henri Lebesgue.[15] In Paris, Wilson found an attitude toward mathematics, consistent, for Wilson, with Gibbs', one in which pedagogy and epistemology were interconnected. In what Wilson called the "modern French School," professors assumed limited knowledge of mathematics so that students could quickly learn it, command it and use it in their own investigations. For Wilson, the French School offered hence the best available solution to the problem of foundations because it was balanced; it reconciled past and present works; rigor and intuition; logic and judgment; the pure and the applied; as well as abstract and empirical emphases. It was also simple and useful; it limited freedom of abstraction with Hadamard's motto according to which reality drove the mathematics. Inversely, it compelled scientists to engage in deductive reasoning.[16]

Upon his return to New Haven in the summer of 1903, Wilson was told about Gibbs's passing away. Wilson would face difficult times at Yale, because leaders of the department wanted, Wilson felt, applied mathematics out of the department[17] and distrusted intuition.[18]

[15] E. Wilson to M. Frechet, 26 July 1916 (PEBW, Box 1, Folder 1915-16 C). On Parisian mathematics around 1900 see David Aubin, Hélène Gispert, and Catherine Goldstein, "The Total War of Paris Mathematicians," in *The War of Guns and Mathematics: Mathematical Practices and Communities in France and Its Western Allies Around World War I*, ed. David Aubin and Catherine Goldstein (USA: AMS, 2014), 125–78.

[16] See Edwin Wilson, "Review: Émile Borel, Leçons Sur Les Fonctions Méromorphes," *BAMS* 9, no. 9 (1903): 506–7; "The Theory of Waves," *BAMS* 10, no. 6 (1904): 305–17; "Review: Emile Picard, Sur Le Développement de l'Analyse et Ses Rapports Avec Diverses Sciences: Conférences Faites En Amérique, and Emile Picard, La Science Moderne et Son État Actuel," *BAMS* 14, no. 9 (1908): 444–48.

[17] E. Wilson to D. L. Webster, 2 Oct. 1926 (PEBW, Box 10, Folder C).

[18] See James Pierpont, "On the Arithmetization of Mathematics," *BAMS*, 8, 5 (1899): 394–406.

1.3.2. The role and the nature of hypothesis

In Paris, Wilson's foundational concerns became concerns about the role and nature of hypothesis in mathematics and science. Following Poincaré's *La Science et l'Hypothèse*, which he regarded as best representing contemporary French views on science, Wilson believed,

> "'If the universe is governed by laws expressible by mathematical formulas there must be something which is invariant.' This is about as much and about as little as a conscientious scientist of to-day can say."[19]

In this vein, Wilson justified the use of Gibbs's vectorial analysis, arguing that **"**vectors [were] to mathematical physics what invariants [were] to geometry."[20] He, however, did not yet specify what he thought the nature of invariants was. He only stated what he believed it was not; and it was not the kind of invariants found in David Hilbert's mathematics, which was then significantly influencing young American mathematicians.

For Wilson, Hilbert "created an epoch in the technique of mathematics."[21] However, echoing Poincaré's appreciation of Hilbert's work,[22] Wilson stated that Hilbert's pure mathematics displayed a "strict regard for absolutely perfect logic and a natural corresponding disregard for that intuition which hitherto [had] played such a preponderant role in geometry."[23] For Wilson, as Hilbert disconnected mathematical

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[19] Edwin Wilson, "Mach's Mechanic's," *BAMS* 10, no. 2 (1903): 85.

[20] Wilson, *Vector Analysis: A Text-Book for the Use of Students of Mathematics and Physics*, xii.

[21] Edwin Wilson, "The Foundations of Mathematics," *BAMS* 11, no. 2 (1904): 77.

[22] Henri Poincaré, "Poincaré's Review of Hilbert's 'foundations of Geometry,'" *BAMS* 10, no. 1 (1903): 1–23.

[23] Edwin Wilson, "The So-Called Foundations of Geometry," *Archiv Der Mathematik Und Physik* 6 (1903): 104.

deduction from intuition and judgment, his pure mathematics lost its usefulness for immediate purposes. Hilbert's work, Wilson felt, was an emblematic expression of a "mania" in vogue for rigor, logic and arithmetization.[24] This mania, Wilson diagnosed, was threatening the work of his contemporary American mathematicians colleagues; it was yielding in America "unrest which is not good for thorough work."[25]

Under this feeling of threat, Wilson provided his own *Foundations of Mathematics* and his own *Foundations of Science* by qualifying in his own terms, the notion of mathematical and scientific hypothesis.

Wilson found enlightening Bertrand Russell's solution to the lack of mathematical foundations, which consisted of basing mathematics on logic, so that "[p]ure mathematics [became] the class of all proposition of the form 'p implies q.'"[26] For Wilson, logical inference offered a solution because "mathematical or other reasoning presupposes a mind capable of rational, that is, non-selfcontradictory ratiocinative processes."[27] Persistence of errors, Wilson underlined, resulted from a lack of analytical attitude and careful definition of terms. Russell, Wilson argued, had delivered an improved definition of logical calculus, segregated into three different branches: the calculus of classes (as nouns), of relations (as verbs) and of propositions. With such a system of logic, presented as rules of language, Wilson believed that it was possible to determine definitions, operations, relations and equations that were at the basis of mathematics and of all fields to which mathematics was applied.

[24] Ibid., 122.

[25] E. Wilson to O. Veblen, 25 May1904 (OVP, Box 17, Folder E. Wilson 1904-26).

[26] Wilson, "The Foundations of Mathematics," 76.

[27] Ibid., 79.

Wilson, however, was not keen to accept that mathematics could be reduced to formalism and symbolism. For him, Russell offered a solution to philosophical questions only; his treatment of physics remained purely idealistic.

For Wilson, thanks to Hilbert and Russell, the technique and the philosophy of mathematics were better defined. Pure mathematics and symbolic logic, however, were not sufficient to found mathematics on solid grounds, he claimed; they needed to be interconnected with each other and, most of all, with science. A practical problem, Wilson deplored, was that pure mathematicians were not paying attention to logician's work and logicians themselves were ignoring pure mathematicians' research. [28] To fill the gap, following the American mathematician Edward Huntington,[29] Wilson distinguished between (applied) mathematics and pure-logical mathematics by breaking the term hypothesis into two distinct but complementary categories: postulates and axioms.

According to Wilson, postulates were marks in the mind disconnected from any kind of experience and represented the only *a priori* truths in mathematics. They were the technical (Hilbert) and philosophical (Russell) pillars structuring pure mathematics and logical calculus respectively. As such, they determined the correct technical and deductive way of reasoning in applications of mathematics. Postulates represented a

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[28] Edwin Wilson, "Symbolic Logic," *BAMS* 14, no. 4 (1908): 175–91.

[29] Edward Huntington, "Sets of Independent Postulates for the Algebra of Logic," *Transactions of the American Mathematical Society* 5, no. 3 (1904): 288–309. Wilson's use of the term postulates is understood here from the perspective of the American postulationists, a group of young American mathematicians, to which Huntington was part. Although Hilbert himself argued that intuition and empirical experience were fundamental in the constitution of (his) axioms—which had little resemblance with the way Wilson approached axioms—, these young American mathematicians cut the Hilbertian axiomatic approach from its empirical emphasis. They changed the emphasis of the study of geometry as the science of space toward the study of systems of axioms, which became an issue of autonomous interest. They were committed to a new way of dealing with mathematical objects as detached from their (physical) meaning. See Michael Scanlan, "Who Were the American Postulate Theorists?," *The Journal of Symbolic Logic* 56, no. 3 [1991]: 981–1002; Herbert Mehrtens, *Moderne Sprache, Mathematik: Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme*, 1 Aufl edition (Frankfurt am Main: Suhrkamp, 1990); Jeremy Gray, *Plato's Ghost: The Modernist Transformation of Mathematics* (Princeton, N.J.: Princeton University Press, 2008).

sort of grammar, with which to structure scientific thinking. As purely technical and deductive reasoning, postulational thinking also implied getting rid of psychological considerations, to be considered only *a posteriori*. For Wilson, postulational thinking was therefore a necessary condition for mathematical thinking; it was, however, not sufficient.

Axioms, Wilson stated, were self-evident truths, namely those approved by our experience, intuition and judgment, about the "actual" world. They existed in connection with specific subject matters that studied specific "facts" of nature. Indeed, they were like working hypotheses prevailing over the history of the specific subject matter of possible material universes or empirically verified assertions concerning actual material universes (laws of motion, axiom of parallels). In other words, axioms offered meaning and semantics to the mathematics.

For Wilson, mathematics was like a language. It consisted of imposing certain mathematical structures, as grammatical rules, to subject matters, which, in return provided the semantics, as meaning. Defining mathematics as a language supposed therefore that mathematics consisted of a permanent back and forth between postulates (grammar) and axioms (semantics), in which axioms (meaning) were preeminent. As Wilson wrote, the "basis of rationality must go deeper than a mere set of marks and postulates. It is foundation of everything and must be more *real* than anything else."[30] Therefore, postulational thinking was intelligible only if it had a meaning in terms of the specific subject matter to which mathematics was applied. Such was the message of Wilson's *Foundations of Science*[31], in which he tried to establish a *correspondence* between his postulates and axioms and Poincaré's ideas about hypotheses, as found in *La Science et l'Hypothèse*.

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[30] Wilson, "The Foundations of Mathematics," 81, footnote.

[31] Edwin Wilson, "The Foundations of Science," *Bulletin of the American Mathematical Society* 12, no. 4 (1906): 187–93.

Stating that science helped us to classify and to deal with experience, Wilson elaborated upon the role, the nature and the value of scientific hypotheses as defined by Poincaré. For their role, valid for postulates and axioms, hypotheses were idealizations held to establish a theory of science, or about science. In this function, hypotheses could be mutually contradictory. However, if contradictions were kept separated, Wilson argued, inconveniences remained solvable by our analytical capacities.[32]

Regarding their nature, Wilson stated that in Poincaré's discussions on geometry and mechanics hypotheses were idealizations held as convenient conventions, which could be held arbitrarily as long as they were in agreement with our intuition and judgment. In this conventionalist vein, Wilson found Poincaré's motto enlightening: theory never rendered a greater service to science than when it broke away. Wilson had no great difficulty in making his postulates correspond with these ideas in respect to pure mathematics and symbolic logic; in both cases, psychological considerations were secondary. Similarly, Wilson regarded these ideas as compatible with his axioms, in the sense that convenience could serve as a pragmatic doctrine in the operationalization of mathematics.

Wilson agreed with Poincaré that a hypothesis, as an axiom, was of value if it taught us how to measure something without necessarily defining it. However, Wilson believed that Poincaré dogmatically used some convenient conventions as arbitrarily endorsed principles[33]—indeed as postulates—; pragmatism, Wilson argued, did not justify forgetting about idealized nature. Scientists should, to a certain extent, care about the thing being measured.

[32] In 1905, Wilson did not hold any more the idea according to which "*we*", mathematicians, did not like any more to change *a posteriori* initial hypotheses in order to better fit theories.

[33] Also see Jeremy Gray, *Henri Poincaré: A Scientific Biography* (Princeton, N.J.: Princeton University Press, 2013).

1.3.3. Epistemology and pedagogy

We have seen that Wilson assumed that psychological considerations were only secondary. To understand what he meant, it must be underlined that Wilson regarded the psychological question as being connected to the interrelation between pedagogy and cognition, not between sense experience and cognition.

In 1905, Wilson began writing a textbook on calculus. Following Poincaré and repudiating Hilbert, he believed that instructing students first and only in formal mathematics was detrimental for their psychological and intellectual development. Constrained mathematics was a better fit for this educational purpose. His *Advanced Calculus* textbook was published in 1911. Presented as a "laboratory of mathematics,"[34] it was addressed to mathematicians, physicists and engineers with the aim of confirming and extending their working knowledge of calculus. This working knowledge, Wilson argued, contributed to the psychological development of students by helping them to affirm their self-confidence: by knowing how to use calculus in "real" problems, students could use their mathematical skills with *vigor* to explore and develop their own fields.

With this kind of mathematics, Wilson wanted to protect American students from what he regarded as being manias in vogue in American mathematics and science.

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All in all, in his foundational discussions, Wilson defined what mathematical and scientific rationality was: it consisted of a permanent back a forth between postulates and axioms, where axioms prevailed over postulates. Wilson believed that with his

[34] On the idea of "laboratory of mathematics" in the American mathematical scene, see Eliakim Moore, "On the Foundations of Mathematics," *BAMS* 9 (1903): 402–24.

constrained mathematics, it was possible to gain consensus regarding the "something which is invariant." But, what was that something? Was it nature? Was it convention? Was it mathematical and scientific rationality as he defined it? Wilson remained vague regarding these questions. Also, Wilson's approach did not elicit any kind of consensus within the community of American mathematicians, who were concerned with closed mathematical systems. For them, mathematics had stopped being a discipline of intuition/judgment and quantities or of correspondences between mathematical notation and nature; it had become a field of relations and deduction,[35] of unconstrained creativity where mathematicians were set free from mechanical analogies and could work on mathematical ones.[36] Ultimately, Wilson remained silent on how consensus was reached. Furthermore, in contrast with Wilson's impressions, it was not evident that a French School of mathematics actually existed. Although Poincaré, Picard, Borel and Hadamard were all luminaries, there was no particular unity in their work; Poincaré's conventionalism, rather than best representing French science as Wilson thought, was indeed a source of disagreement.[37] These elements set the context that would explain Wilson's progressive and gradual marginalization from the community of American mathematicians.

1.4. Gibbs, nationalism and pleas

In 1907, Wilson was appointed associate professor of mathematical physics at the Massachusetts Institute of Technology (MIT). In the transition toward mathematical physics, Wilson suggested that consensus about scientific conventions was reached

[35] Moritz Epple, "The End of the Science of Quantity: Foundations of Analysis, 1860-1910," in *A History of Analysis*, ed. Hans Niels Jahnke, vol. HMath 24 (Providence, RI: American Mathematical Society, London Mathematical Society, 2003), 291–323.

[36] Giorgio Israel, *La Mathématisation Du Réel* (Paris: Seuil, 1998).

[37] Jeremy Gray, *Plato's Ghost: The Modernist Transformation of Mathematics* (Princeton, N.J.: Princeton University Press, 2008).

when masterminds with extraordinary mathematical capacities formulated them;[38] Gibbs was one of them.

At MIT, Wilson became closer to other fields, engineering and economics included. Concurrently, he adopted a disdainful attitude towards most American scientists feeling that their work lacked logical structure. Wilson believed mathematicians were, to a large extent, responsible for this, because they made their instruction at the collegial level too abstract; as a result, most students, and eventually most scientists, abhorred mathematics. At the same time, Wilson claimed, as mathematicians did not train Ph.D. students in applications of mathematics, young mathematicians were unable to understand modern works in science. Hence, there resulted a dislocation between American mathematics and the rest of academia. Consequently, science and technology, as well as pure and applied sciences developed separately.[39] The problem, for Wilson, resulted from the fact that American mathematical leaders

> "who came back from Prussia around 1890 systematically set about running sound mechanics and mathematical physics off the map […]. At the same time physicists with few exceptions were devoting all their attention to experimental rather than theoretical problems."[40]

Such a dislocation, Wilson thought, illustrated a decline in mathematical education in American colleges. To revalorize mathematics, it was necessary to connect it with other disciplines.

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[38] Edwin Wilson, "Bryan's Thermodynamics," *BAMS* 14, no. 3 (1907): 139–40.

[39] Edwin Wilson, "Review: Höhere Analysis Für Ingenieure, by John Perry," *BAMS* 9, no. 9 (1903): 504–6; "Mathematical Physics for Engineers," *BAMS* 17, no. 7 (1911): 350–61.

[40] E. Wilson to D. Webster, 10 Feb. 1926 (PEBW, Box 10, Folder W).

Connecting American sciences was precisely the motto that the National Academy of Science (NAS) adopted around 1910.[41] Of particular relevance for Wilson's career was the establishment of the *Proceedings of the NAS* (PNAS) in 1914 as a vehicle of communication among American sciences, humanities excluded. Wilson became indeed the first managing editor of the PNAS,[42] a position that he held until he passed away. With the advent of First World War (WWI), the NAS encouraged applications of scientific methods that could strengthen national security and prosperity.[43] Wilson himself contributed to the scientific war effort by working on aerodynamics and on ballistics.[44]

In this context of war, nationalism became a fundamental element in Wilson's epistemology. In his foundational discussions, Wilson had emphasized the relevance of intuition and judgment. In a war context, the nation became an emblematic entity framing these elements. Illustratively, he invited Americans to be loyal to Gibbs, whose scientific style truly reflected American values. Wilson did not understand why American mathematical leaders ignored the American turn of mind:

[41] On the NAS, see Hunter Dupree, *Science in the Federal Government: A History of Policies and Activities* (Baltimore: Johns Hopkins UP, 1986); George Hale, *National Academies and the Progress of Research* (Lancaster: New Era Printing Co., 1915); Rexmond Cochrane, *The National Academy of Sciences: The First Hundred Years, 1863-1963* (Washington: National Academy of Sciences, 1978).

[42] Edwin Wilson, *History of the Proceedings of the National Academy of Sciences: 1914-1963* (Washington: National Academy of Sciences, 1966).

[43] See Daniel Kevles, "George Ellery Hale, the First World War, and the Advancement of Science in America," *Isis* 59, no. 4 (1968): 427–37.

[44] See National Advisory Committee for Aeronautic reports 1 (1915); 21 (1917); 26 (1919); 27 (1918); 78 (1919); 79 (1920). In 1920, Wilson summarized his contributions in *Aeronautics: A Class Text* (New York: John Wiley and Sons, 1920.

> "I believe that the great advance in pure mathematics in this country in the last 30 years based upon German influence is essentially foreign to our nature."[45]

It was thus a patriotic gesture not to follow the hitherto leading German kind of mathematics. Before using it, Wilson stated, a process of American naturalization should be followed:[46] mathematics should be rendered simple, intuitive and useful for immediate operational purposes.

In this spirit, Wilson's work at the MIT and the NAS became almost an act of resistance against the increasing influence of the German rigorist attitude toward mathematics in science. Just before WWI ended, Wilson thought a true victory entailed militarily and scientifically taking over Central Powers. As America knew now the benefits of planning science with respect to national objectives, for a final victory, America needed to control the development of science. Such control, Wilson thought, had to remain subtle.[47]

By "subtle control," Wilson meant reforming American collegial mathematical curriculum,[48] which, he claimed, should better reflect national values. American values, he insisted, were precisely embodied by his Gibbsian constrained mathematics. Instruction of mathematics, as it was, obstructed immediate intelligibility and applicability of mathematics, putting in danger national welfare and national security, he argued.[49]

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[45] E. Wilson to J. Whittemore, 23 Apr. 1924 (PEBW, HUG4878.214 Box 7, folder U-V).

[46] Edwin Wilson, *Mathematics and the Engineer*, unpublished paper, (1919) (PEBW, HUG4878.214 Box 3, folder miscellaneous notes).

[47] Edwin Wilson, "Insidious Scientific Control," *Science*, New Series, 48, no. 1246 (1918): 491–93.

[48] Edwin Wilson, "Let Us Have Calculus Early," *BAMS* 20 (1913): 31.

[49] Edwin Wilson, "Some Books on Calculus," *BAMS* 21, no. 9 (1915): 471–76.

1.5. Rapprochement with American science

In his research in mathematical physics at MIT, citing Gibbs, Wilson developed vector analysis and used it in geometrical and algebraic applications.[50] Playing with his skills in analysis, he addressed some theoretical questions of mechanics[51] and statistical mechanics.[52] In collaboration with some of his MIT colleagues, Wilson's most significant efforts were in relativity.[53] With his coauthors, Wilson offered the first contribution to relativity written in vectorial notation reproducing known results in mechanics and electromagnetics and introduced the notation of vectors in differential geometry developing in this way a sophisticated mathematical framework from which generalized relativity could be interpreted and reconstructed.[54]

Eventually, in his practice as a mathematical physicist, Wilson used his definition of mathematical and scientific rationality as the something that was invariant and which enabled the application of mathematics to a subject matter. In his research, Wilson used his mathematics to *translate* into the language of the Gibbs-Wilson vector

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[50] Edwin Wilson, "On the Theory of Double Products and Strains in Hyperspace," *Transactions of the Connecticut Academy of Art and Sciences* 14 (1908): 1–57.

[51] Edwin Wilson, "On the Differential Equations of the Equilibrium of an Inextensible String," *Transactions of the American Mathematical Society* 9, no. 4 (1908): 425–39.

[52] Edwin Wilson, "Note on Statistical Mechanics," *BAMS* 15, no. 3 (1908): 107–15; "Thermodynamic Analogies for a Simple Dynamical System," *Annals of Mathematics* 10, no. 4 (1909): 149–66.

[53] Edwin Wilson and Gilbert Lewis, "The Space-Time Manifold of Relativity. The Non-Euclidean Geometry of Mechanics and Electromagnetics," *Proceedings of the American Academy of Arts and Sciences* 48, no. 11 (1912): 389–507; Edwin Wilson, "Review of A. Einstein and M. Grossmann: 'Entwurf Einer Verallgemeinerten Relativitâtstheorie Und Einer Theorie Der Gravitatio,'" *BAMS* 20, no. 5 (1914); Edwin Wilson and Clarence Moore, "Differential Geometry of Two Dimensional Surfaces in Hyperspace," *Proceedings of the American Academy of Arts and Sciences* 52, no. 6 (1916): 269–368; "A General Theory of Surfaces," *Proceedings of the National Academy of Sciences of the United States of America* 2, no. 5 (1916): 273–78.

[54] For comments on Wilson's and coauthors' work on relativity, see Gerald Holton, "The Formation of the American Physics Community in the 1920s and the Coming of Albert Einstein," *Minerva* 19, no. 4 (1981): 569–81; Scott Walter, "The Non-Euclidean Style of Minkowskian Relativity," in *The Symbolic Universe: Geometry and Physics, 1890-1930*, ed. Jeremy Gray, 1999, 91–127; Karin Reich, "The American Contribution to the Theory of Differential Invariants 1900-1916," in *The Attraction of Gravitation: New Studies in the History of General Relativity*, ed. John Earman, Michel Janssen, and John Norton, vol. 5, 5 vols. (Boston, Basel, Berlin: Birkhäuser, 1993), 225–47.

analysis results at the borderline of physical research; he offered sophisticated mathematical systems presenting them as natural, where natural referred to conventional ways of giving meaning to physical facts; Wilson still focused on intuition and judgment, arguing that axioms prevailed over postulates, or in his words:

> "[l]ogic [was] only the prose of mathematics; imagery [was] its poetry, and there [was] often more inspiration in a vague impressionist poem than in the clearest prose."[55]

The British Blockade, starting in August 1914, cut American scientists off from many of the latest German scientific contributions. Wilson became aware of Einstein's and Hilbert's 1915 works on general relativity only two months after the publication of his *Differential Geometry,* when Eliakim Moore confronted him and sent him a copy of Hilbert's *Die Grundlagen der Physik*, asking for Wilson's opinion.[56] Wilson responded by emphasizing his disagreement with the fact that Hilbert established a causal relationship—where electromagnetic phenomena resulted from gravitation—, based on a purely mathematical correlation. He also wrote:

> "[in] regard to your question as to whether the mathematicians may not be indicating the physics of the future, I should say that they are certainly indicating a possible physics of the future, but that there are infinitely many systems of physics, and that the chances are very small that the particular system indicated, with no basis whatsoever in experiment or

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[55] Edwin Wilson, "The Fourth Dimension as a Text," *Science Conspectus* 2, no. 4 (1912): 104–7.

[56] On Einstein's and Hilbert's general relativity and their cooperation, see Karin Reich, *Die Entwicklung des Tensorkalküls: Vom absoluten Differentialkalkül zur Relativitätstheorie*, 1994 edition (Basel; Boston: Birkhäuser, 1994); Leo Corry, *David Hilbert and the Axiomatization of Physics (1898-1918) : From Grundlagen Der Geometrie to Grundlagen Der Physik* (Dordrecht: Kluwer Academic Publishers, 2004).

> experience, will turn out to be the system realized when experiment shall be sufficient to indicate a system."[57]

This Hilbertian German attitude was becoming increasingly prevalent in physics. Nevertheless, Wilson's work on relativity earned him respectful recognition within the American (Massachusetts) scientific community. This appreciation must have contributed much to Wilson's promotion to full professor in 1912 and to chairman of the department of physics in 1917. Also, in 1920, Lawrence Henderson, secretary of the Royce Club, invited him to deliver a talk on the same subject. Wilson opened his talk with the quote opening this chapter. At that time, Wilson still thought that foundations of mathematics and science were unstable: there was not yet agreement regarding their definition. Wilson then concluded his Royce Club talk as follows:

> "Man's place to-day in physical Nature is far from central. He should be decidedly humble. He knows infinitely little and what knowledge he has is for the most part either a partial understanding of discrete facts or a conventional correlation of different facts based not upon ultimate truth but upon the brief convenience of the leading minds of his time, —to the lesser minds the convenience of the leaders may be a serious inconvenience."[58]

For Wilson, mathematicians and scientists could neither create the structures that rendered the physical world intelligible, nor simply observe the world to deal scientifically with it. They only could establish correspondences. The mechanical point of view of nature, he also suggested, should be replaced by a statistical approach, where no fundamental principles about the universe were required. For Wilson, the conservation of energy and continuity were only temporary convenient

[57] E. Wilson to E. Moore, 19 May1916 (PEBW, Box 1, Folder 1915-16 m).

[58] Wilson, "Space, Time, and Gravitation," 234.

conventions.[59] At the same time, he seemed then to be uncomfortable with the idea that conventions resulted from individual masterminds. In 1923, Wilson resigned from the AMS.

1.6. The statistical turn

Between 1920 and 1922, Wilson sat in the presidential office at MIT acting as secretary of an interim committee directing the Institute. As one of his executive tasks, he took part in the establishment of the Harvard School of Public Health (HSPH), which was founded in 1922. When the school opened its doors, Wilson was appointed chairman of the department of vital statistics.[60] During the 1920s and thereafter, Wilson conducted paramount statistical studies, both empirical and theoretical, in a large variety of fields.[61] His efforts did not go unrecognized. Illustrative of such recognition was his election as president of the American Statistical Association (ASA) in 1929.[62]

As in mathematics and physics, Wilson stated, foundations of statistics were missing; such unrest, he argued, was symptomatic of "the fervid impatience that had

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[59] Edwin Wilson, "Review of The Theory of Relativity by Robert D. Carmichael," *Science* 39, no. 998 (1914): 251–52.

[60] Jean Curran, *Founders of the Harvard School of Public Health: With Biographical Notes, 1909-1946* (New York: Josiah Macy, Jr., Foundation, 1970).

[61] For a summary of Wilson's significant contributions to statistics see Norman L. Johnson and Samuel Kotz, *Leading Personalities in Statistical Sciences: From the Seventeenth Century to the Present* (John Wiley & Sons, 2011), 344–45.

[62] Around 1930, tensions between groups holding contrasting opinions about mathematics in statistics were rising within the ASA. Mathematically oriented statisticians proposed creating a separate organization, the Institute of Mathematical Statistics (IMS). Wilson was part of the movement, but opposed separation. (Patti Hunter, "An Unofficial Community: American Mathematical Statisticians before 1935", Annals of Science 56, no. 1 (1999): 47–68; Stephen Stigler, "The History of Statistics in 1933", Statistical Science 11, no. 3 (1996): 244–52). Instead, he pleaded for reforms of collegial mathematical education (see Edwin Wilson, "Too Little Mathematics--and Too Much," *Science* 67, no. 1725 [1928]: 52–59). During his presidential address, Wilson stated that statistics required knowledge of mathematics and of the subject matter. He noted also that if he had to choose between them, he would always prefer the latter (Edwin Wilson, "Mathematics and Statistics," Journal of the American Statistical Association 25, no. 169[1930] : 1–8).

developed in present times"[63]: statisticians, too, were prone to adopt wrong methodological attitudes. In this compromised condition, he thought, *classical* statistics was not well grounded scientifically for (scientific) practical applications.

In his 1920 Royce Club talk, Wilson had held that knowledge was conventional and partial. As conventional, knowledge resulted from congeries of working hypotheses; as partial, scientific statements conveyed truth only in a certain proportion. The statistical question amounted therefore to quantifying that proportion that carried "truth," so that statistics became useful in the practice of science and in different activities of life. Such quantification supposed a rational way of reasoning, as in mathematics, where, if hypotheses were applicable to the problem at hand, conclusions could be considered as real. Echoing Charles S. Peirce, Wilson stated that the "great accomplishments of science [tended] to give a mechanistic philosophy, to cause us to overlook the rôle of chance which, as statisticians, we must always keep in mind."[64] In this spirit, Wilson sought "an alternative to mechanism, or determinism, [which] might be called by contrast, indeterminism, or statistics,"[65] and offered a tentative definition of statistics.

Wilson believed that Maynard Keynes's *Treatise on Probability* offered the best available foundations of probability theory, which, for Wilson, lay at the basis of statistics. However, he disliked Keynes' too philosophical treatment and the fact that he dismissed Gibbs' and Peirce's American contributions.[66] By building on his own foundational ideas, Peirce's statistics and Russell's logical calculus, Wilson separated

[63] Edwin Wilson, "The Statistical Significance of Experimental Data," *Science* 58, no. 1493 (1923): 93.

[64] Edwin Wilson, *Elements of Statistics*, unpublished and undated textbook, chapter III, p. 9. (PEBW, HUG4878.214, Box 2, Folder Elements of Statistics). At the NAS in 1923, Wilson was asked to write a biography of Peirce. Wilson accepted the task, familiarized himself with Peirce's statistical ideas, without ever completing the biography. On Peirce, see Louis Menand, *The Metaphysical Club: A Story of Ideas in America*, 1st ed. (Farrar, Straus and Giroux, 2002).

[65] E. Wilson to T. H. Morgan, 11 July 1923 (PEBW, Box 5, Folder M).

[66] Edwin Wilson, "Keynes on Probability," *BAMS* 29, no. 7 (1923): 319–22.

analytically three interconnected aspects of the field: sample theory (classes), data analysis (relations) and statistical inference (propositions).

Sample theory consisted of establishing associations of common attributes of individual objects of a given group in order to form an aggregate. Once aggregates were formed, the analysis of the available data consisted of describing it using synthesizing constants, such as the average and the variance. Other times, data analysis implied finding an empirical equation establishing a certain relation, such as causation or correlation, between variables. Although the determination of such formulas often involved *drawing* curves connecting observed values with sophisticated techniques, Wilson thought that the form of such curves was mainly shaped by the statistician's judgment and intuition. Experts, he stated, who had greater experience dealing with statistical techniques, data and the subject matter under study, developed better judgments and intuitions.[67]

Wilson's skepticism towards classical statistics, namely Karl Pearson's and his followers' statistics,[68] was rooted in a personal disdain regarding this analytical emphasis. Pearson, Wilson wrote, "has attempted to keep all the [statisticians] practically in prison intellectually."[69] In his statistics, Wilson argued, formulas tended to gain universal validity while meaning was indiscriminately imposed on data. Eventually, Wilson deplored, everything appeared to be distributed according to the Gauss' Law.[70]

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[67] Wilson, "The Statistical Significance of Experimental Data."

[68] Raymond Pearl (1879-1940) was an American biologist; he had close ties with K. Pearson. Wilson and Pearl engaged in vivid controversy. See Sharon Kingsland, "The Refractory Model: The Logistic Curve and the History of Population Ecology," *Quarterly Review of Biology* 57, no. 1 (1982): 29–52.

[69] E. Wilson to R. A. Fisher, 28 Dec. 1925 (PEBW, Box 8, Folder F).

[70] Edwin Wilson, "First and Second Laws of Error," *Journal of the American Statistical Association* 18, no. 143 (1923): 841–51; Wilson's criticism against Pearson contributed to limiting his influence in America (David Bellhouse, "Karl Pearson's Influence in the United States," *International Statistical Review* 77, no. 1 [2009]: 51–63).

In mathematics, Wilson argued, there was a separation between the process of drawing conclusions, a technical and logical matter, and the acceptance or rejection of hypotheses, a practical matter. This separation implied a relationship of trust between mathematicians, who could take it for granted that the logical consistency of their peers' work was correct. In mathematics, he claimed, it was also customary that those using a formula were responsible for verifying whether or not it was applicable for the case at hand. The pitfall with the idea of responsibility in statistics remained that it was impossible to determine whether statistical premises were true or false because, Wilson remarked, statistical inference was yet to be defined.[71] Such a definition required discriminating core notions of the field. Wilson distinguished probability from chance. For Wilson, probability was a technical word that

> "designates ideal happenings conceived to be taking place under certain specified assumption possibly not realizable in the real world and studied to gain theoretical background, to see what sort of thing may reasonably happen not what does happen; whereas chance is the more general term and must cover the variations unaccounted for in real happenings which seem to our best knowledge to occur in similar cases".[72]

Statistical inference consisted then of mediating between empirical and theoretical emphases by establishing a certain a *correspondence* between "chance" and "probability," namely determining to what extent compiled data was not a lucky strike. To accomplish that, the statistician had to make an argument as to the proportion of truth and meaning carried in the data. Such an argument consisted of comparing observed rates in the data with a range of trust—confidence interval—calculated accordingly to a theoretical probability.[73] Such range of meaningfulness,

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[71] Edwin Wilson, "Statistical Inference," *Science* 63, no. 1629 (1926): 289–96.

[72] Edwin Wilson, *Elements of Statistics*, chapter III, p. 9.

[73] Edwin Wilson, "Empiricism and Rationalism," *Science* 64, no. 1646 (1926): 47–57.

and ultimately such probability, represented an idealized referent with which to calculate the proportion of truth and meaning conveyed in the data.[74] In this direction, statistical inference could only be probable; statistical statements should always be "expressed in a guarded way"[75] to help us determine the significance—likelihood—of the correspondence between theoretical probabilities and data.[76]

All in all, in Wilson's statistics, working hypotheses of a subject matter served as theoretical classificatory, indeed taxonomical, schema for sampling, and probable inference served as an operational technique to determine the best approximate working hypothesis with respect to a problem in hand. As such, statistics helped to arbitrate between reasons by determining degrees of tolerance within which a working hypothesis could be reasonably held. Statistical reasoning, however, did not enable universal applicability. When formulas were taken as structural generalizations, Wilson suggested, statisticians became shamanic priests of given idolatries.[77] For him, statistics, like mathematics and science, could not serve to control (physical or social) nature; it could only help us to adapt to nature to attain given objectives. All this marked Wilson's *statistical turn.*[78]

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[74] Edwin Wilson, "Probable Inference, the Law of Succession, and Statistical Inference," *Journal of the American Statistical Association* 22, no. 158 (1927): 209–12.

[75] Ian Hacking, "The Theory of Probable Inference: Neyman, Peirce and Braithwaite," in *Science, Belief and Behaviour: Essays in Honour of R. B. Braithwaite*, ed. David Mellor (Cambridge: Cambridge University Press, 1980), 149. See also Theodore M. Porter, Trust in Numbers, Reprint edition [Princeton, N.J.: Princeton University Press, 1996], 12)

[76] Jerzy Neyman once declared that Wilson anteceded his and Egon Pearson's notion of confidence interval. See Jerzy Neyman, *Lectures and Conferences on Mathematical Statistics and Probability* (USA: Graduate School, U.S. Dept. of Agriculture, 1952), 222.Ibid.

[77] Wilson, "Statistical Inference."

[78] In the general sense of Lorenz Krüger, Lorraine Daston, and Michael Heidelberger, eds., *The Probabilistic Revolution*, vol. 1: Ideas in History, 2 vols. (Cambridge, Mass: The MIT Press, 1987); Lorenz Krüger, Gerd Gigerenzer, and Mary Morgan, eds., *The Probabilistic Revolution*, vol. 2: Ideas in the Sciences, 2 vols. (Cambridge Mass.: The MIT Press, 1987).

1.7. Reforming American social science and economics

From the HSPH, Wilson engaged also with social science and economics endeavoring to establish lasting connections between these fields, mathematics and statistics; Wilson's program fit well with the inter-disciplinarity agendas of contemporary Harvard presidents, Abbott Lowell and James Conant.[79]

In the middle of the social unrest of the 1929 crisis, as he had done in mathematics, physics and statistics, Wilson diagnosed social science and economics as suffering from lack of sound scientific foundations. Consequently, he argued, social scientists and economists tended to adopt wrong methodological attitudes. In disagreement with the interventionist political platform of the Republican Party of the 1930s and in response to Progressive social science and to New Deal[80] and Institutional[81] economics, he argued that this unrest had led some to "suggest all sorts of *crazy* experiments"[82]; social scientists and economists were rushing and trying "to control the as yet uncontrollable."[83] Eventually, Wilson complained, unrest in these fields had a negative impact on society at large.

As Wilson wrote to Henderson, who, Wilson thought, worked in the truly American scientific style of Gibbs:

[79] On interdisciplinary in human science at Harvard and on Henderson's influence in these developments, see Joel Isaac, *Working Knowledge: Making the Human Sciences from Parsons to Kuhn* (Cambridge Mass.: Harvard University Press, 2012).

[80] See William Barber, *From New Era to New Deal: Herbert Hoover, the Economists, and American Economic Policy, 1921-1933* (Cambridge; New York: Cambridge University Press, 1985); Thomas Leonard, *Liberal Reformers* (Princeton UP, Forthcoming).

[81] See Malcolm Rutherford, *The Institutionalist Movement in American Economics, 1918-1947: Science and Social Control* (Cambridge: Cambridge University Press, 2011).

[82] E. Wilson to H. Burbank, 25 Nov. 1932 (PEBW, Box 18, Folder A-B).

[83] Edwin Wilson, "Are There Periods in American Business Activity?," *Science* 80, no. 2070 (1934): 199.

> "I have a feeling that a good many social scientists have to be protected from themselves and that it is up to people like you and me to do what we can to push social science ahead in a sound way."[84]

Hand in hand with Henderson, Wilson sought to reform education of American social scientists and economists.

Henderson had been developing the idea that society could be regarded as an organic *system* of individuals sharing a similar culture, which could be studied in terms of stable equilibrium.[85] When Henderson began to think about these questions at the beginning of the 1920s, he again invited Wilson to offer a talk before the Royce Club. On March 25, 1923, Wilson discussed Pareto's mathematical economics and paraphrased parts of Pareto's *Manuel d'Économie Politique*. He concluded with the following words:

> "Pareto's method of approach is critical and realistic and hard, as all such approaches must be. For that reason we must not expect many to follow it. […]. What about education in political economy [and social science]? Shall we work along the straight and narrow and difficult path or stroll across expansive country exchanging verbal pleasantries? Says Pareto: Faith alone strongly urges men to action and hence it is not desirable for the good of society that the masses, or even that many, should be occupied scientifically with social matters. … [Those] who wish to make

[84] E. Wilson to L. Henderson, 10 Jan. 1933 (PEBW, Box 21, Folder H).

[85] Inspired by Gibbs's conception of chemical-physical systems in stable equilibrium and impressed by Vilfredo Pareto's 1917 *Traité de Sociology Générale*, Henderson believed that social stability observed in most societies proved that they were in equilibrium. For him, studying society as a system in stable equilibrium was intuitive, not theoretical. See Cynthia Eagle Russett, *The Concept of Equilibrium in American Social Thought* (New Haven, Cann.: Yale University Press, 1966); John Parascandola, "L. J. Henderson and the Mutual Dependency of Variables: From Physical Chemistry to Pareto," in *Science at Harvard University: Historical Perspectives*, ed. Clark Elliott and Margaret Rossiter (United States of America: Associated UP, 1992), 146–90.

> all participate, hazily and without discrimination, in knowledge behave with little wisdom."[86]

In contrast to the dominant American traditions in social science and economics, Wilson rejected social reforms as the main objective of scientific endeavor.[87] Most social scientists and economists, whose work remained literary, Wilson felt, acted based on faith and did not know how to arbitrating between reasons. Through the educational reforms that he supported, Wilson wanted to establish the "right" relationships, first, between high education—of American social elites—and social science and economics and, then, between these fields—as practiced by this educated elites—and American society.

The first transformation implied that for the sake of society only the best "type" of American gentile students, namely those attending universities such as Harvard, should hold with legitimacy the scientific status when studying and dealing with social and economic matters. In order to achieve that scientific status, Wilson claimed, they should be instructed in his Gibbsian mathematics and in Henderson-Pareto style of work.

The second transformation implied, for Wilson, that social science and economics should first help understand the actual functioning of social institutions before trying to control them. For him, controlling and planning the development of science was necessary for controlling and planning *Social Progress.*[88] As nature was unpredictable and uncontrollable, he thought, our mathematical and scientific capacities could help us, as a society, to better adapt to it in order to obtain the

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[86] Royce Club, PEBW, HUG4878.214, Box 1, Folder : Book reviews, letters to the Editor, 9-10.

[87] Social reforms were a major motive for research in American social science and economics around this period of time. See Dorothy Ross, *The Origins of American Social Science* (United States of America: Cambridge University Press, 1992).

[88] Edwin Wilson, "Social Progress," *Proceedings of the National Academy of Sciences of the United States of America* 73, no. 15 (1940): 469–72.

greatest advantages from it and avoid the greatest disadvantages.[89] Also, he thought, the mechanical analogy when dealing with social and economic control had to be taken with skepticism.

Working in these lines, at the national level, Wilson became impressively and effectively active through the 1920s and the 1930s at the organizational and educational fronts in social science and economics. He was particularly responsible for the organization of Section K of the American Association for the Advancement of Science (AAAS) around 1930. Under Wilson's leadership, meetings of Section K became a privileged place where a select group of scientists and mathematicians interested in social science and economics met with social scientists and economists working with mathematical and statistical techniques. Those meetings played a central role in the establishment of the community of American mathematical economists and the Econometric Society. Wilson's activism was also remarkable at the level of instruction at Harvard. Under his leadership, the first program in advanced mathematical and statistical economics was established in the early 1930s. He offered those courses, inspired by Paretian-Hendersonian concerns and in clear a Wilson-Gibbs mathematical spirit.[90]

At the beginning of the 1930s, Wilson also found himself sitting in meetings of the department of sociology. In the first half of 1929, he had taken a sabbatical spent at the University of Berkeley in California.[91] There, he had suggested to people to offer Henderson the Mills Foundation invitation, which Henderson subsequently received and accepted by "suggesting the second half of [1931] and proposing to lecture to undergraduates on the philosophy of science and to give a seminar on Pareto's

[89] Wilson, "Are There Periods in American Business Activity?"

[90] See chapter II.

[91] E. Wilson to L. Henderson, 19 Oct. 1929 (PEBW, Box 38, Folder E-K).

scientific method."[92] At Harvard, Wilson then suggested to Pitirim Sorokin, chairman of the department of sociology, the establishment of a course on Pareto's methodology that would be offered by Henderson, to whom Wilson wrote:

> "I don't entirely share your enthusiasm for […] Pareto's [Sociology] but as a member of the department of Sociology I suggested that you be invited to give the course and I have further suggested to Sorokin that we give you as large a class of competent students as possible."[93]

In 1932, Henderson started teaching, in collaboration with Charles Curtis, a seminar on *Pareto and Scientific Methods.*[94] At the same time, invited by Sorokin,[95] Wilson started lecturing to sociologists on *Quantitative Problems of Population*.

Over the 1930s, Henderson played a preponderant role in the development of Harvard social science promoting the methodology of case studies.[96] Wilson, the mathematician, rather emphasized the notion of generality, which he thought was of immediate practical interest for economics; his Gibbsian vectorial and matrix mathematics could be useful to study the aggregate economy as well as the individual units of the economy and their interactions as being in stable equilibrium. Paul Samuelson, an economist who attended Wilson's courses during the 1930s, often acknowledged that he was Wilson's disciple; his *Foundations of Economic Analysis*[97]

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[92] L. Henderson to E. Wilson, 28 Feb.1930 (PEBW, Box 38, Folder E-K).

[93] E. Wilson to L. Henderson, 25 Jan. 32 (PEBW, Box 19, Folder H).

[94] This seminar would subsequently give rise to the famous Pareto Circle. See Isaac, *Working Knowledge*; Annie Cot, "A 1930s North American Creative Community: The Harvard 'Pareto Circle,'" *History of Political Economy* 43, no. 1 (2011): 131–59; Barbara S. Heyl, "The Harvard 'Pareto Circle,'" *Journal of the History of the Behavioral Sciences* 4, no. 4 (1968): 316–334.

[95] P. Sorokim to E. Wilson, 26 Dec. 1931 (PEBW, Box 17, Folder Sorokin),

[96] Isaac, *Working Knowledge*.

[97] Paul Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass. Harvard University Press, 1947).

was much influenced by Wilson-Gibbs mathematics as well as by Henderson-Pareto style of work that he learnt with Wilson. Samuelson developed the notion of stable equilibrium at the individual and aggregate levels in economics while *translating* it in mathematical terms and presenting it as intuitive.[98]

1.8. The Stevenson Lectures in Citizenship

Wilson retired in 1945, right after the end of War World II. For that occasion, as a reward for his work for American social science and economics, Joseph Willets, director of the Division of Social Sciences of the Rockefeller Foundation, in association with Hector Hetherington, principal of the University of Glasgow, invited him to deliver the *Stevenson Lectures in Citizenship* in Glasgow, during the autumn 1945 and the winter 1946 terms, and to discuss recent American contributions to the study of society. Wilson titled his first lecture *Perplexities of Citizenship.* On that occasion, he conveyed the implicit assumption that had led his foundational discussions since 1900. As he explained, the main problem of individuals, as citizens, consisted of making specific choices by selecting and ordering loyalties and values. Choosing among available alternatives was "one instance of the ever present fact of conflict attending life"[99], where individuals sought to reconcile their allegiances weighing conflicting interests. Implicitly, Wilson's diagnosis of the state of mathematics, physics, statistics, social science and economics had precisely consisted of pointing out that individuals acting in these activities, facing lack of sound foundations, made wrong choices as they adopted wrong methodological approaches. In his foundational discussions, Wilson had systematically proposed his definition of mathematical and scientific rationality as the best available choice for the practice of science: it was inter-mediate and could hence more easily lead to achieve consensus.

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[98] See chapter III.

[99] Edwin Wilson, *The Study of Society* (PEBW, HUG4878.214, Box 4, Folder Stevenson Lecture Chapter I, 1).

He had also systematically pled for reforms of high education in the lines of his Gibbsian mathematics to "help" students make the "right" choice.

Now that he was addressing the "problem attending life," in return, Wilson used his foundational ideas, brought some modifications to them and emphasized again education as a possible solution to the problem of citizenship.

Wilson diagnosed modern citizens as suffering from lack of a rationale to make sound choices; consequently, they and their societies were perplexed, and hence more eager to adopt wrong political and social stances. He stressed that tyranny and demagogy had proven effective in generating emotions, leading individuals and societies to make wrong choices, and inadequate plans (Nazism, Communism). For the sake of democracy, Wilson suggested in Glasgow, citizens and democratic societies should appeal to the authority of—his definition of—science when making choices and plans. There was no a unique way of ordering loyalties and values, he explained. However, he stressed, —his—mathematical and scientific rationality could help individuals and societies arbitrating between reasons; and hence it could help them deal with their perplexities. Science, he stated, could only throw some tiny lights as for the best way of ordering our values. It was however significantly useful to deal with in the operations of society, at the governmental or private levels, "in bringing chosen values towards realization."[100] To be scientific, Wilson specified, plans must be expressed in a guarded way and provide for frequent revisions, as it could not be expected that we could control nature or the long-term development or our social and economic institutions.

In Glasgow, Wilson stressed again the conventional nature of science. "Science is social. Its knowledge is that upon which there is agreement,"[101] he explained. At this

[100] Edwin Wilson, *The Study of Society* (Chapter II, 45).

[101] *Idem*, (10.1).

late stage of his career, Wilson went further and suggested that scientific consensus was a social construct. By appealing to the authority of Harvard president James Conant and of the English art critic John Ruskin, he described science as being embedded in culture. As science was social, he claimed, American social scientists and economists needed to know the culture of science so that they did not become asocial. Wilson's organizational and pedagogical efforts in American social science and economics had precisely aimed at disciplining in the scientific method students of these fields. In 1945, he still believed that reforms in these fields needed to be enhanced; now that he was pleading for spreading the scientific attitude among citizens, Wilson stated, we should also

> "impress [upon the public] with the significance of the state of agreement toward which one [strives]; for it is only by virtue of this agreement that the results of science can be reliably applied."[102]

By this, Wilson meant that popularizing shared conventions in social science and economics among citizens, namely educating and disciplining the public, should also be regarded as being part of scientific planning of social and economic matters. Spreading the scientific attitude could help realize planned social and economic objectives. Indeed, if there was consensus in public opinion about the meaning of social and economic facts, announcements of plans by governmental authorities, when credible, Wilson explained, could produce immediate changes in individual's choices and plans leading to wanted social and economic transformations at the aggregate level. For this educational purpose, Wilson believed, professionals of mass media were as important and influential than scientists.[103]

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[102] Edwin Wilson, *The Study of Society*, (10.1).

[103] Edwin Wilson, *The Study of Society.* (Chapter II).

Ultimately, at the end of his career, having significantly engaged with social science and economics, he eventually started defining science as being social. At the same time, Wilson started regarding social science and economics as being useful for governing people: by spreading the scientific attitude among the public, it could help mold, indeed discipline, the behavior of citizens in practical life.

1.9. Conclusion

Through his career, Wilson jumped from one field to another. His professional trajectory illustrated a process of marginalization from the American research mathematical community and a concomitant process of incursion into other research and organizational scientific communities. In this process, he offered an original and unique definition of scientific rationality, mathematical in its essence. He also connected in effective and important ways his mathematical thinking with various subject matters, following his own ideas about scientific rationality. Over the whole period, Wilson treated mathematical and scientific rationality, as he defined it, as the main form of invariance needed in science. But as he let intuition and personal judgment enter his epistemology, the different historical and professional contexts that framed his daily work over the period became constitutive elements of his foundational thought. In this way, his patriotic, nationalistic, political, social and cultural biases gained preponderance over his ideas about science. They became part of his criteria for evaluating and judging scientific statements; they played the same, or an even more important role than rigor and logic in structuring mathematical and scientific thinking.

For Wilson, the problem of mathematical and scientific foundations was a practical problem in the life of the mathematician and the scientist, each of whom solved it by making a choice as for the attitude they wanted to adopt in their scientific practices. Wilson's definition of rationality stemmed from his definition of mathematics as a set of correspondences between postulates and axioms. His pleas for reforms of

American education of mathematicians and—natural and social—scientists were precisely aimed at molding, indeed disciplining, students to protect them from scientific manias in vogue and help them make the "right" choice regarding their scientific attitude, namely adopting Wilson's definition of scientific rationality.

Wilson also suggested that scientific rationality was useful for citizens in handling the problem of practical, social and political life in society. Through education, the public could be instructed in the standards of scientific rationality; they could be taught how to behave in a scientific, rational way. They could be disciplined. For Wilson, Scientific consensus, for the development of sound science, was as important as social and political consensus in society, for social progress. Such interconnection between science and society was possible because Wilson understood his mathematical and scientific language as a vernacular: everyone could learn it and think with it.

Throughout Wilson's evolution, he made an undeniable and lasting contribution to the enhancement of inter-disciplinarity in America between the 1900s and the 1940s. In the last analysis, Wilson attempted to define not only mathematical and scientific rationality and to modernize science, but the entire foundations of what it meant to be rational and modern in a democratic world.

1.10 References

Aubin, David, Hélène Gispert, and Catherine Goldstein. 2014. "The Total War of Paris Mathematicians." In *The War of Guns and Mathematics: Mathematical Practices and Communities in France and Its Western Allies Around World War I*, edited by David Aubin and Catherine Goldstein, 125–78. USA: AMS.

Barber, William. 1985. *From New Era to New Deal: Herbert Hoover, the Economists, and American Economic Policy, 1921-1933*. Cambridge; New York: Cambridge University Press.

Bellhouse, David. 2009. “Karl Pearson’s Influence in the United States.” *International Statistical Review* 77 (1): 51–63.

Cochrane, Rexmond. 1978. *The National Academy of Sciences: The First Hundred Years, 1863-1963*. Washington: National Academy of Sciences.

Corry, Leo. 2004. *David Hilbert and the Axiomatization of Physics (1898-1918) : From Grundlagen Der Geometrie to Grundlagen Der Physik*. Dordrecht: Kluwer Academic Publishers.

Cot, Annie. 2011. “A 1930s North American Creative Community: The Harvard ‘Pareto Circle.’” *History of Political Economy* 43 (1): 131–59.

Crowe, Michael. 1985. *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System*. New York: Dover.

Curran, Jean. 1970. *Founders of the Harvard School of Public Health: With Biographical Notes, 1909-1946*. New York: Josiah Macy, Jr., Foundation.

Dupree, Hunter. 1986. *Science in the Federal Government: A History of Policies and Activities*. Baltimore: Johns Hopkins UP.

Epple, Moritz. 2003. “The End of the Science of Quantity: Foundations of Analysis, 1860-1910.” In *A History of Analysis*, edited by Hans Niels Jahnke, HMath 24:291–323. Providence, RI: American Mathematical Society, London Mathematical Society.

Fenster, Della Dumbaugh, and Karen Parshall. 1994. “A Profile of the American Mathematical Research Community: 1891-1906.” In *The History of Modern Mathematics: Images, Ideas, and Communities*, edited by Eberhard Knobloch and David Rowe, 3:179–227. Boston: Academic Press.

Gibbs, Josiah. 1902. *Elementary Principles in Statistical Mechanics*. New York: Dover Publication, Inc.

Gray, Jeremy. 2008. *Plato's Ghost: The Modernist Transformation of Mathematics*. Princeton, N.J.: Princeton University Press.

———. 2013. *Henri Poincaré: A Scientific Biography*. Princeton, N.J.: Princeton University Press.

Hacking, Ian. 1980. "The Theory of Probable Inference: Neyman, Peirce and Braithwaite." In *Science, Belief and Behaviour: Essays in Honour of R. B. Braithwaite*, edited by David Mellor. Cambridge: Cambridge University Press.

Hale, George. 1915. *National Academies and the Progress of Research*. Lancaster: New Era Printing Co.

Heyl, Barbara S. 1968. "The Harvard 'Pareto Circle.'" *Journal of the History of the Behavioral Sciences* 4 (4): 316–334.

Holton, Gerald. 1981. "The Formation of the American Physics Community in the 1920s and the Coming of Albert Einstein." *Minerva* 19 (4): 569–81.

Hunsaker, Jerome, and Saunders Mac Lane. 1973. "Edwin Bidwell Wilson, 1879-1964, A Biographical Memoir." *National Academy of Sciences (US)*, 283–320.

Hunter, Patti. 1999. "An Unofficial Community: American Mathematical Statisticians before 1935." *Annals of Science* 56 (1): 47–68.

Huntington, Edward. 1904. "Sets of Independent Postulates for the Algebra of Logic." *Transactions of the American Mathematical Society* 5 (3): 288–309.

Isaac, Joel. 2012. *Working Knowledge: Making the Human Sciences from Parsons to Kuhn*. Cambridge Mass.: Harvard University Press.

Israel, Giorgio. 1981. "Rigor and Axiomatics in Modern Mathematics." *Fundamental Scientiae* 2 (2): 205–19.

———. 1998. *La Mathématisation Du Réel*. Paris: Seuil.

Johnson, Norman L., and Samuel Kotz. 2011. *Leading Personalities in Statistical Sciences: From the Seventeenth Century to the Present*. John Wiley & Sons.

Kevles, Daniel. 1968. "George Ellery Hale, the First World War, and the Advancement of Science in America." *Isis* 59 (4): 427–37.

Kingsland, Sharon. 1982. "The Refractory Model: The Logistic Curve and the History of Population Ecology." *Quarterly Review of Biology* 57 (1): 29–52.

———. 1985. *Modeling Nature: Episodes in the History of Population Ecology*. 1st ed. Chicago: University of Chicago Press.

Krüger, Lorenz, Lorraine Daston, and Michael Heidelberger, eds. 1987. *The Probabilistic Revolution*. Vol. 1: Ideas in History. 2 vols. Cambridge, Mass: The MIT Press.

Krüger, Lorenz, Gerd Gigerenzer, and Mary Morgan, eds. 1987. *The Probabilistic Revolution*. Vol. 2: Ideas in the Sciences. 2 vols. Cambridge Mass.: The MIT Press.

Leonard, Thomas. Forthcoming. *Liberal Reformers*. Princeton UP.

Menand, Louis. 2002. *The Metaphysical Club: A Story of Ideas in America*. 1st ed. Farrar, Straus and Giroux.

Moore, Eliakim. 1903. "On the Foundations of Mathematics." *Bulletin of the American Mathematical Society* 9: 402–24.

Neyman, Jerzy. 1952. *Lectures and Conferences on Mathematical Statistics and Probability*. USA: Graduate School, U.S. Dept. of Agriculture.

Parascandola, John. 1992. "L. J. Henderson and the Mutual Dependency of Variables: From Physical Chemistry to Pareto." In *Science at Harvard University:*

Historical Perspectives, edited by Clark Elliott and Margaret Rossiter, 146–90. United States of America: Associated UP.

Parshall, Karen, and David Rowe. 1994. *The Emergence of the American Mathematical Research Community, 1876-1900: J.J. Sylvester, Felix Klein, and E.H. Moore*. United States of America: American Mathematical Society.

Pierpont, James. 1899. "On the Arithmetization of Mathematics." *Bulletin of the American Mathematical Society*, 8, 5: 394–406.

Poincaré, Henri. 1903. "Poincaré's Review of Hilbert's 'foundations of Geometry.'" *Bulletin of the American Mathematical Society* 10 (1): 1–23.

Porter, Theodore M. 1996. *Trust in Numbers*. Reprint edition. Princeton, N.J.: Princeton University Press.

Ramsden, Edmund. 2002. "Carving up Population Science: Eugenics, Demography and the Controversy over the 'Biological Law' of Population Growth." *Social Studies of Science* 32 (5/6): 857–99.

Reich, Karin. 1993. "The American Contribution to the Theory of Differential Invariants 1900-1916." In *The Attraction of Gravitation: New Studies in the History of General Relativity*, edited by John Earman, Michel Janssen, and John Norton, 5:225–47. Boston, Basel, Berlin: Birkhäuser.

———. 1994. *Die Entwicklung des Tensorkalküls: Vom absoluten Differentialkalkül zur Relativitätstheorie*. 1994 edition. Basel; Boston: Birkhäuser.

Ross, Dorothy. 1992. *The Origins of American Social Science*. United States of America: Cambridge University Press.

Russett, Cynthia Eagle. 1966. *The Concept of Equilibrium in American Social Thought*. New Haven, Cann.: Yale University Press.

Rutherford, Malcolm. 2011. *The Institutionalist Movement in American Economics, 1918-1947: Science and Social Control*. Cambridge: Cambridge University Press.

Samuelson, Paul. 1947. *Foundations of Economic Analysis*. Cambridge, Mass. Harvard University Press.

Servos, John. 1986. "Mathematics and the Physical Sciences in America, 1880-1930." *Isis* 77 (4): 611–29.

Stigler, Stephen. 1996. "The History of Statistics in 1933." *Statistical Science* 11 (3): 244–52.

Walter, Scott. 1999. "The Non-Euclidean Style of Minkowskian Relativity." In *The Symbolic Universe: Geometry and Physics, 1890-1930*, edited by Jeremy Gray, 91–127.

Wilson, Edwin. 1900. "The Decomposition of the General Collineation of Space into Three Skew Reflections." *Transactions of the American Mathematical Society* 1 (2): 193–96.

———. 1901. *Vector Analysis: A Text-Book for the Use of Students of Mathematics and Physics*. United States of America: Yale University Press.

———. 1902a. "Some Recent Books on Mechanics." *Bulletin of the American Mathematical Society* 8 (8): 341–49.

———. 1902b. "Some Recent Books on Mechanics." *Bulletin of the American Mathematical Society* 9 (1): 25–39.

———. 1902c. "Some Recent Books on Mechanics." *Bulletin of the American Mathematical Society* 8 (9): 403–12.

———. 1903a. "Mach's Mechanic's." *Bulletin of the American Mathematical Society* 10 (2): 80–86.

———. 1903b. “Review: Émile Borel, Leçons Sur Les Fonctions Méromorphes.” *Bulletin of the American Mathematical Society* 9 (9): 506–7.

———. 1903c. “Review: Höhere Analysis Für Ingenieure, by John Perry.” *Bulletin of the American Mathematical Society* 9 (9): 504–6.

———. 1903d. “The So-Called Foundations of Geometry.” *Archiv Der Mathematik Und Physik* 6: 104–22.

———. 1904a. “The Foundations of Mathematics.” *Bulletin of the American Mathematical Society* 11 (2): 74–93.

———. 1904b. “The Theory of Waves.” *Bulletin of the American Mathematical Society* 10 (6): 305–17.

———. 1906. “The Foundations of Science.” *Bulletin of the American Mathematical Society* 12 (4): 187–93.

———. 1907. “Bryan’s Thermodynamics.” *Bulletin of the American Mathematical Society* 14 (3): 139–44.

———. 1908a. “Note on Statistical Mechanics.” *Bulletin of the American Mathematical Society* 15 (3): 107–15.

———. 1908b. “On the Differential Equations of the Equilibrium of an Inextensible String.” *Transactions of the American Mathematical Society* 9 (4): 425–39.

———. 1908c. “On the Theory of Double Products and Strains in Hyperspace.” *Transactions of the Connecticut Academy of Art and Sciences* 14: 1–57.

———. 1908d. “Review: Emile Picard, Sur Le Développement de l’Analyse et Ses Rapports Avec Diverses Sciences: Conférences Faites En Amérique, and Emile Picard, La Science Moderne et Son État Actuel.” *Bulletin of the American Mathematical Society* 14 (9): 444–48.

———. 1908e. "Symbolic Logic." *Bulletin of the American Mathematical Society* 14 (4): 175–91.

———. 1909. "Thermodynamic Analogies for a Simple Dynamical System." *Annals of Mathematics* 10 (4): 149–66.

———. 1911. "Mathematical Physics for Engineers." *Bulletin of the American Mathematical Society* 17 (7): 350–61.

———. 1912. "The Fourth Dimension as a Text." *Science Conspectus* 2 (4): 104-07.

———. 1913. "Let Us Have Calculus Early." *Bulletin of the American Mathematical Society* 20: 30–36.

———. 1914a. "Review of A. Einstein and M. Grossmann: 'Entwurf Einer Verallgemeinerten Relativitâtstheorie Und Einer Theorie Der Gravitatio.'" *Bulletin of the American Mathematical Society* 20 (5).

———. 1914b. "Review of The Theory of Relativity by Robert D. Carmichael." *Science* 39 (998): 251–52.

———. 1915. "Some Books on Calculus." *Bulletin of the American Mathematical Society* 21 (9): 471–76.

———. 1918. "Insidious Scientific Control." *Science*, New Series, 48 (1246): 491–93.

———. 1920a. *Aeronautics: A Class Text*. New York: John Wiley and Sons.

———. 1920b. "Space, Time, and Gravitation." *The Scientific Monthly* 10 (3): 217–35.

———. 1923a. "First and Second Laws of Error." *Journal of the American Statistical Association* 18 (143): 841–51.

———. 1923b. "Keynes on Probability." *Bulletin of the American Mathematical Society* 29 (7): 319–22.

———. 1923c. "The Statistical Significance of Experimental Data." *Science* 58 (1493): 93–100.

———. 1926a. "Empiricism and Rationalism." *Science* 64 (1646): 47–57.

———. 1926b. "Statistical Inference." *Science* 63 (1629): 289–96.

———. 1927. "Probable Inference, the Law of Succession, and Statistical Inference." *Journal of the American Statistical Association* 22 (158): 209–12.

———. 1928. "Too Little Mathematics--and Too Much." *Science* 67 (1725): 52–59.

———. 1930. "Mathematics and Statistics." *Journal of the American Statistical Association* 25 (169): 1–8.

———. 1934. "Are There Periods in American Business Activity?" *Science* 80 (2070): 193–99.

———. 1940. "Social Progress." *Proceedings of the National Academy of Sciences of the United States of America* 73 (15): 469–72.

———. 1962. Interview with Dr. Edwin Wilson Interview by Bruce Lindsay and James King. Niels Bohr Library & Archives. American Institute of Physics, College Park, MD USA. http://www.aip.org/history/ohilist/5065.html.

———. 1966. *History of the Proceedings of the National Academy of Sciences: 1914-1963*. Washington: National Academy of Sciences.

Wilson, Edwin, and Gilbert Lewis. 1912. "The Space-Time Manifold of Relativity. The Non-Euclidean Geometry of Mechanics and Electromagnetics." *Proceedings of the American Academy of Arts and Sciences* 48 (11): 389–507.

Wilson, Edwin, and Clarence Moore. 1916a. "A General Theory of Surfaces." *Proceedings of the National Academy of Sciences of the United States of America* 2 (5): 273–78.

———. 1916b. “Differential Geometry of Two Dimensional Surfaces in Hyperspace.” *Proceedings of the American Academy of Arts and Sciences* 52 (6): 269–368.

CHAPTER II

EDWIN B. WILSON AND THE RISE OF MATHEMATICAL ECONOMICS IN THE UNITED STATES OF AMERICA, 1920-1940

2.1. Introduction[1]

In his 1998 *How Foundations Came to Be*, after hinting at his indebtedness to his Harvard professors of the 1930s, Joseph Schumpeter, Wassily Leontief, Gottfried Haberler and Alvin Hansen, Paul Samuelson acknowledged:

> "Perhaps most relevant of all for the genesis of Foundations, Edwin Bidwell Wilson (1879-1964) was at Harvard. Wilson was the great Willard Gibbs's last (and essentially only) protégé at Yale. He was a mathematician, a mathematical physicist, a mathematical statistician, a mathematical economist, a polymath who had done first-class work in many fields of the natural and social sciences. I was perhaps his only disciple […]. Aside from the fact that E.B. knew everything and

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[1] E-mail: carvaja5@gmail.com. I am grateful to Roger Backhouse, Pedro Duarte, Ivan Moscati, Robert Leonard and Roy Weintraub for helpful comments on early drafts of this chapter, as well as to the members and the 2016 fellows of the HOPE Center, where this chapter was presented during a seminar. The usual caveat applies. I am also thankful to archivists of the Harvard University Archives (HUA) and of the David M. Rubenstein Rare Book & Manuscript Library at Duke University (DU). Papers of Edwin Bidwell Wilson were consulted at HUA, HUG4878.203 (indicated if different); Paul A. Samuelson Papers (PASP) and Lloyd Metzler Papers (LMP) were consulted at DU; James Tobin Papers (JTP) can be consulted at Yale University Library. In this chapter, the number of the boxes in which the relevant material was consulted will follow the respective collection.

> everybody, his great virtue was his contempt for social scientists who aped the more exact sciences in a parrot-like way." (Samuelson 1998, 1376)

Although Wilson was central for Samuelson, Wilson's imprint on the history of economics as intertwined with the history of other disciplines in America has not yet been the subject of a detailed historical analysis.[2] A possible reason for this historical oversight: Wilson's main role was not at the creative front, but at the organizational one, through intense academic proselytization and instruction.

Wilson's active role in the promotion of mathematical and statistical methods in economics significantly influenced the rise of mathematical economics in America between the 1920s and the 1940s. In particular, he promoted Vilfredo Pareto's and Irving Fisher's mathematical economics; he also played a central role in the constitution of the community of American mathematical economists. Furthermore, he promoted and established the first program in modern mathematical and statistical economics at Harvard. Through the study of Wilson's influence in the development of mathematical economics, new lights are also shed regarding the origins of the Econometric Society, of which Wilson was a founder member, but of which he rapidly distanced himself.

Behind Wilson's activism in economics lay his belief that, in the foundations of science, matters of pedagogy and epistemology were connected; he believed that American mathematics and science must better reflect American national values, and more significantly serve national security and prosperity. Wilson was confident that subtle planning of science would yield democratic science in America. Central to Wilson's thought was his perennial belief that in science, as in society, progress was

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[2] Yann Giraud (2007), Roger Backhouse (Forthcoming; 2015; 2014; 2013), Bruna Ingrao and Giorgio Israel (1990) as well as Philip Mirowski (1989; 2002) and Roy Weintraub (1991) have all pointed out Wilson's relevance for the mathematical turn of economics, by focusing on his influence on Samuelson's work and career, not on Wilson himself.

too often made by individuals with wrong methodological, political or social positions; he argued for a middle-ground position, which could help scientists deal with unrest created by difficult choices regarding an approach to be adopted. Inbetweenness offered a possible solution for the problem of the creation and protection of scientific knowledge, as it offered a solution to the problem of social order: quantifiable consensus and agreement being Wilson's watchwords.

Wilson's involvement with social science and economics occurred hand in hand with that of his Harvard colleague Lawrence Henderson. The spirit under which they worked was made explicit in a letter to Henderson, where Wilson wrote:

> "I have a feeling that a good many social scientists have to be protected from themselves and that it is up to people like you and me to do what we can to push social sciences ahead in a sound way."[3]

Using archival material, this chapter will first discuss Wilson's incursion into social science and economics by briefly commenting on his reviews of Pareto's and Fisher's work. Then, in the following two sections, it will study Wilson's activism "to push social sciences," with a special emphasis on economics. In particular, the chapter will explain how Wilson became involved with economics, by playing a significant role at the origins of the Econometric Society in America while being highly concerned with the relationship between economics and economic policy, planning and control. Lastly, the chapter will discuss how Wilson, discontent with the evolution of the econometric movement and New Deal policies, turned his efforts towards Harvard and promoted and established the first program in advanced mathematical economics at the university. The chapter will finish by presenting the content of Wilson's Harvard courses on mathematical economics and mathematical statistics.

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[3] E. Wilson to L. Henderson, Jan. 10, 1933 (PEBW, 21). On Wilson-Henderson connection, see chapter I section 1.7. of the present thesis.

2.2. Incursion into social science and economics

Educated as a mathematician at Harvard University, Yale University and at the École Normale Supérieure in Paris around 1900, Wilson was one the "most active" members among the American mathematical research community during the first decade of the 1900s (Fenster and Parshall 1994). Wilson, however, gradually marginalized himself from that community, disavowing the influence that David Hilbert's German structuralist mathematics was then exerting on his American colleagues.[4] 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, Wilson accepted the chairmanship of the department of vital statistics at the newly founded Harvard School of Public Health (HSPH). In parallel spheres, from 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. During the 1920s and thereafter, he also became active and highly influential in numerous national scientific associations and societies.

The task that Wilson gave himself in all the above-mentioned involvements consisted of fostering and establishing lasting connections between mathematics and different subject matters, following Gibbs's kind of mathematics and defining, in an original and unique way, mathematics as intermediate and constrained.[5]

Wilson's interests in social science and economics emerged during his years at Yale (1899-1907) when he came to be associated with the sociologist William Graham

[4] See Wilson 1903c.

[5] On Wilson's Gibbsian definition of mathematics see chapter I.

Sumner and the mathematical economist Irving Fisher.[6] Wilson helped Fisher setting up business and stock market barometers.[7]

These interests persisted and around 1910 Wilson promoted Fisher's and Vilfredo Pareto's mathematical economics; in reviews of their work, addressed to the readership of the *Bulletin of the American Mathematical Society* (AMS) and Science, Wilson presented economics as a field where mathematics could be naturally applied. In its contemporary state, he argued, political economy, as a science, was in its infancy: there was no consensus about theories and conclusions. Fisher's and Pareto's mathematical economics was the path to follow. They based their work on convenient conventional idealizations and they applied postulational reasoning by following the deductive rules of logical calculus. In this way, Wilson argued, these mathematical economists were able to better define their assumptions and to adopt a rational way of thinking. By adopting this attitude, economists would eventually be able to elevate the status of economics to science; to attain agreement regarding the fundamental idealizations and—mathematical—relations of the field; to improve their judgment and their data about economic and social life; and to make better decisions based on modern scientific judgment. However, neither Pareto's nor Fisher's work was yet satisfactory, as it remained mainly theoretical. In this vein, Wilson argued that as some fields of mathematical physics were theoretical exercises because of lack of data, mathematical economics, which still faced similar problems, did not yet properly represent economic affairs but remained at the level of general economic theory. Wilson also questioned the analogy that Pareto and Fisher made between their mathematical economics and physics. For Wilson, comparing economic and physical facts, and using the same formulas without adapting them to the specific problem in hand, was arbitrary. Analogical thinking was useful because it could lead to

[6] E. Wilson to R. Vance, Dec. 19, 1922 (PEBW 3).

[7] E. Wilson to G. U. Yule, May 6, 1924 (PEBW, 7).

unexpected and enlightened perspectives, but must be limited in regard to its applicability (Wilson 1909a; 1912; 1913; 1914). [8]

At the beginning of the 1920s, Wilson felt that he was "further away from those mathematical interests which the persons in control [of the AMS] define to be mathematics." [9] Believing German mathematics was "essentially foreign to [American] nature"[10] and thinking that American mathematical leaders ignored the American turn of mind, traditionally more eager for applications of mathematics than for pure rigor, Wilson resigned from the AMS in 1923. He had just joined the HSPH, where, Wilson often heard President Lowell claiming that the most important function as President of a university such as Harvard consisted of fostering "the development of that kind of work which is particularly fruitful, namely, that which originates in the *'no man's land'* between two accepted fields of study."[11]

During the 1920s, Wilson also became more involved with the functioning of the NAS; he particularly promoted including for membership distinguished social scientists and economists; he was also one of the few members of the NAS who appeared in the roster of the Social Science Research Council (SSRC) when it was incorporated in 1924. [12] Almost simultaneously, he was elected member of the American Statistical Association (ASA) as well as of the American Association for the Advancement of Science (AAAS). In these institutions, Wilson actively promoted cooperation between mathematicians, statisticians, social scientists and economists.

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[8] Wilson's reviews were aimed at showing how other disciplines could gain scientific legitimacy when connected with mathematics and how in return this connection gave to mathematics its practical and operational intelligibility. During the 1910s, Wilson came closer to the community of American political economists. In 1912, he was elected member of the American Economic Association (AEA).

[9] E. Wilson to J. Coolidge, March 30, 1922 (PEBW, 4).

[10] E. Wilson to J. Whittemore, May 23, 1924 (PEBW, 7).

[11] E. Wilson to R. Richardson, 3 Nov. 1928 (PEBW, 13, italics added). On the development of human and social sciences, economics excluded, and their interconnections at Harvard, see Isaac 2012.

[12] See Cochrane 1978.

Through these efforts, he not only enlarged his network; he also embarked on implicit and indirect negotiations with practitioners of different academic fields about the right definition of being scientific, which according to him implied necessarily, but not sufficiently, the use of mathematics. Symmetrically, Wilson also engaged in implicit and indirect negotiations with American mathematicians about the right definition of mathematical practices, which he thought should be redirected towards the establishment of interconnections between mathematics and science.

In such negotiations, Wilson however feared what had pushed him away from the American community of mathematicians:

> "That which has always impressed me most, I think, in moving from one field to another is the tendency of any field to become more technical than the fundamentally established ideas warrant; the tendency in every quantitative field to develop arithmetical, or algebraic, or other mathematical technique far beyond what the data warrant."[13]

2.3. Becoming a mathematical economist

Almost simultaneously to his resignation from the AMS, Wilson, a self-marginalized mathematician, began promoting mathematical economics at the national level in various scientific American associations and academies.

2.3.1. Section K: the American origins of the Econometric Society

In 1928, Wilson, the editor of the PNAS, found himself at the executive committees of the AAAS and the SSRC, in charge of Section K of the AAAS, willing to explore the "no man's land" of scientific territories and willing to modernize social science

[13] E. Wilson to J. Lipka, 17 Dec. 1923 (PEBW, 5).

and economics.[14] Wilson's idea for Section K consisted of gathering in joint meetings people from the AAAS, the AMS, the ASA and the AEA. The difficulty was to coordinate annual meetings of these associations. He particularly aimed at encouraging encounters between a select group of young social scientists and economists working in a mathematical and scientific spirit, whose papers were usually rejected by their own associations, and mathematicians and scientists interested in statistics and economics, whose associations also usually rejected their papers. From the community of economists, both Wesley Mitchell and Fisher reminded Wilson how difficult relationships between the AAAS and national social science associations had been.[15] However, both Mitchell and Fisher[16] were highly interested in Wilson's project. Mitchell was "glad that the AAAS [had] put Section K in [Wilson's] hands."[17] From the community of mathematicians, Ronald G. D. Richardson, Wilson's friend and secretary of the AMS, found Wilson's suggestion interesting.[18] In general terms, Wilson believed that "[i]f both sides do their own job well there should be in the next 25 years a very marked convergence of interest and understanding from both sides toward a common position."[19]

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[14] Wilson's strong commitment to this organizational role became evident in 1927, when he suggested to James Cattell, president of the AAAS, that the association should encourage the scientific development of social science and economics through its Section K (E. Wilson to J. Cattell, Oct. 4, 1927 [PEBW, 11]). Wilson was then put in charge of Section K. The same year, invited by Frederick Mills, Wilson took part to a roundtable during the December meeting of AEA on *The Present Status and Future Prospects of Quantitative Economics* (Mills et al. 1928).

[15] Wilson and Mitchell had met in the round table of the 1927 AEA meeting. They also must have crossed paths in meetings of the SSRC. With Wilson's support, Mitchell was first elected member (W. Mitchell to E. Wilson, May 2, 1928 [PEBW, 13]) and then Fellow (W. Mitchell to E. Wilson, Oct. 2, 1928 [idem]) of the AAAS during 1928. In their correspondence, there is a strong sense of mutual personal and intellectual respect, indeed admiration.

[16] I. Fisher to E. Wilson, June 28 1928 (PEBW, 13).

[17] W. Mitchell to E. Wilson, Apr. 27, 1928 (PEBW, 13).

[18] R. Richardson to E. Wilson, Oct. 19 1928 (PEBW, 13).

[19] E. Wilson to W. Mitchell, May 3, 1928 (PEBW, 13).

Under Wilson's control and with Charles Roos' efficient administrative effort, Section K became a privileged place where there convened a select group of individuals, interested in social science and economics and working with mathematical and statistical techniques.[20] Between 1928 and 1930, in New York, Des Moines and Cleveland respectively, the mathematicians Edward Huntington and Griffith Evans, the mathematical statisticians Alfred Lotka, Paul Rider, Louis Rietz and Harold Hotelling, the mathematical economists Charles Roos and Henry Schultz, Wilson himself as well as the Norwegian economist Ragnar Frisch, among others, presented some of their work and/or acted as chairman during one or two meetings of Section K. The papers presented during these years in Section K dealt with population dynamics, measures of social behavior, business cycles, forecasts of business phenomena, quality controls of production as well as exhaustible resources (See Livingston 1929; 1930; 1931). As noted by Roos to Wilson, "we now seem to have things coming our way in Section K."[21]

In 1930 in Cleveland, Section K had joint meetings with the AMS, the ASA, the AEA and other associations. Wilson was probably responsible for the spirit of convergence that led to the meeting of these associations in the same city, as he presided over the ASA in 1929, and the SSRC between 1929 and 1931.[22] In the same spirit, Wilson conducted the affaires of Section K in such a way as to avoid discord between the

[20] Wilson appointed Roos as secretary of Section K. Roos had conducted doctoral research on mathematical economics under the guidance of Evans, a Harvard Ph.D. mathematician, then at the Rice Institute in Texas.

[21] C. Roos to E. Wilson, June 14, 1931 (PEBW, 16).

[22] Wilson convinced Willford King, president of the ASA in 1930, to go to Cleveland (B. Livingston to E. Wilson, Feb. 12, 1930 [PEBW, 15]; he must have done similarly with other executives in the other associations and societies that met in Cleveland.

different associations. As he wrote to Mitchell, "I don't like secessions, they often turn into civil war."[23]

Of significance regarding Wilson's plan for Section K was the establishment, in America, of the Econometric Society. As noted by Roos to Wilson in a letter dated October 21, 1932, "the Econometric Society was built on the foundation laid by Section K."[24] The Society was a fruit of Frisch's efforts (Bjerkholt 1998). In America, Roos (actively) and Fisher (nominally) supported Frisch's initiative. In an informal evening meeting in Cleveland, the Econometric Society was launched.[25] Wilson, a charter member, was elected fellow of the Society,[26] member of the Council of the Society and, later, member of the Advisory Editorial Committee of Econometrica.[27] Among the elected American members, there were Evans, Fisher, Hotelling, Moore, Mitchell, Roos, Schultz and Wilson.[28] All of them, with the exception of Moore, had been closely related to Section K. Overall, as Wilson noted to Frisch,[29] the number of American members was larger than that of any European country. This was not accidental. To this, Frisch replied as follows:

> "I may tell you quite frankly that I made the American list rather large on purpose, because I wanted to create a safety valve that could function in

[23] E. Wilson to W. Mitchell, Oct. 9, 1928 (PEBW, 13). In the same letter, Wilson emphasized that he did not want scholars to come to Section K as an act of rupture and separation from their respective associations. Section K, he insisted, was not aimed at competing with specific associations but at complementing them. Indeed, as he wrote to Mitchell, he wanted to keep the meetings of the section rather informal and discrete. To this, Mitchell responded that he sided with Wilson, for secession "might also precipitate another futile controversy over methods at large" (Oct. 13, 1928 [PEBW, 13]).

[24] C. Roos to E. Wilson, Oct. 21, 1932 (PEBW, 19).

[25] I. Fisher, R. Frisch, C. Roos to E. Wilson, Nov. 29, 1930 (idem).

[26] Memorandum in re the Econometric Society, Signed by J. Schumpeter and R. Frisch at Bonn, Sep. 28, 1931 (PEBW, 16).

[27] I. Fisher to E. Wilson, Dec. 16, 1931 (PEBW, 16).

[28] Memorandum in re the Econometric Society.

[29] E. Wilson to R. Frisch, Oct. 31, 1932 (PEBW, 18).

> the event of national intrigues coming up between Europeans. Possibly the America group may act as a safety valve."[30]

With his Section K, Wilson significantly helped to create and reinforce in America a sense of community among American mathematical economists. The "American group" that was organized had the particularity that it was supported by the AAAS, a well-recognized American scientific institution. This newly recognized community could therefore be called *scientific* not only because it used mathematical methods but also because the AAAS gave it institutional legitimacy. Furthermore, if Frisch's words were taken seriously, it could be the case that this newly created American community of mathematical economists was reinforced by the unifying and regulatory role that it was supposed to play within the international community of mathematical economists.

The econometric project, however, as he feared with all quantitative fields, rapidly developed "far beyond what the data warrant."[31] Wilson felt that econometricians gave too much emphasis to probability and purely theoretical economics, leaving aside the empirical statistical economics that had been so important in the recent development of American economics.[32] For Wilson, the mathematical statistics of Karl Pearson and Ronald Fisher, playing then significant influence in the econometric movement (see Louçã 2007), were not well grounded empirically. Wilson argued that their approach, based on probability theory, consisted of playing games with pairs of lotteries in which there was no empirical truth; Wilson had

[30] R. Frisch to E. Wilson, Nov. 24, 1932 (idem).

[31] E. Wilson to J. Lipka, 17 Dec. 1923 (PEBW, 5).

[32] Wilson certainly reflected on developments of which Mitchell, particularly at the National Bureau of Economic Research, was an important leader. Even if during the early 1930s, Wilson believed that Mitchell's work lacked mathematical sophistication and rigor, he would later describe the work of the Bureau as being really concerned with factual scientific studies of the economy, aimed at shaping scientific policy (E. Wilson, *The Study of Society from the Standpoint of Recent American Contributions* [PEBW, HUG4878.214, Box 4, Folder Stevenson Lecture Chapter 7]).

suggested that it was not possible to make *scientific* inferences based only on probability theory (Wilson 1926b, 296). Furthermore, Wilson claimed that mathematical statisticians indiscriminately adopted the hypothesis of the normal distribution, regardless of the problem in hand (Wilson 1923a). Wilson felt that econometricians did not understand "that probabilities and statistics [were] different things." His interest in developing the mathematical theory of probability was only due to his "greater interest in science"[33] rather than pure technique.

More particularly, Wilson thought that econometricians lay too much emphasis on probability theory when analyzing dynamical economic systems. This attitude, Wilson must have felt, showed econometricians' incapacity of facing *reality.* Around 1910, Wilson had written some comments on statistical mechanics and argued that despite the fact that statistical mechanics offered the advantage of not requiring improbable hypotheses about the constitution of matter, the use of theory of probability to all kinds of dynamical systems simply as analogy was unintelligible. He showed that the formal analogy between kinetic theory, thermodynamics and hydrodynamics was valid only in restricted cases (Wilson 1908a; 1909b). In the 1920s, he was also skeptical of modern works in quantum mechanics in which physical aggregates and their dynamics were arbitrarily constructed with probability theory; Wilson preferred assuming, simply as a working hypothesis, that nature was dynamical in essence and studied statistically only to ease the analysis; certain correspondences between statics and dynamics could be established on the statistical basis by assuming continuous distributions.[34]

At the institutional and organizational front, Wilson disavowed Frisch's (European) influence in the way certain matters of the Society were being handled. For example, "in regard to the proposal of Alfred Cowles 3rd to subsidize a journal for our

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[33] E. Wilson to C. Roos, Sep. 16, 1936 (PEBW, 27).

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

[Econometric] Society,"[35] Wilson felt that negotiations should be held by Americans, in more American ways. I that was not the case, Wilson suggested, Cowles's money should be invested in another American project. Wilson, as the second American member of the council of the Society with Fisher, thought that Europeans could, in his words:

> "scare Mr. Cowles off. It seems to me that Frisch is too much concerned about a good many things. I wonder if he is essentially a man of good judgment? In many ways I had rather have Col. Rorty's reaction of this Cowles' proposition than Frisch's, or any European's even if the European has had a good many American contacts. I expect to see Cowles in New Orleans. I think he has a little business with the Executive Committee of the AAAS. I think we could well consider whether rather than bother him to go to Europe and interview some foreigners who may not understand him as an American and whom he may not understand because they are foreigners we might perhaps do better to let him give his money to some other organization that won't be so fussy."[36]

With the same nationalistic spirit, and probably reflecting on a clash of personalities between Wilson and Frisch, Wilson also disavowed Frisch's econometrics. Illustratively, as referee of Econometrica, he opposed the publication of a paper dealing with time series written by Frisch.[37] For Wilson, the paper was an example of *too much mathematics* and "did not read the least little bit like the great papers of Willard Gibbs on the Equilibrium of Heterogeneous Substances."[38] Wilson also suggested that mathematical economics needed to follow a process of Americanization in order to succeed in America and claimed that the "best way to

[35] I. Fisher to E. Wilson, Dec. 16, 1931 (PEBW, 16).

[36] E. Wilson to I. Fisher, Dec. 18, 1931 (PEBW, 16).

[37] The paper was titled "Changing Harmonic Studies from the Point of View of Linear Operators and Erratic Shocks" (W. Nelson to E. Wilson, Aug. 23, 1933 [PEBW, 21]).

[38] E. Wilson to W. Nelson, Nov. 13, 1933 (idem).

encourage [Americans] to dig into mathematics [was] to convince them that there [was] some practical use for the mathematics."[39]

Under such circumstances, in which Wilson disliked the too strong Frisch's influence within the Econometric Society in America and in which he also disliked econometrists' unconstrained mathematical attitude, Wilson rapidly distanced himself from the econometric project, without openly embarking on methodological controversies. It was not coincidental that subsequent to the 1930 Cleveland meeting, Wilson chose more empirically oriented economists such as Leonard Ayres (1931), Mitchell (1933) and Carl Snyder (1934) to serve as chairmen of Section K. In contrast, in 1929 and 1930, Wilson had invited Rietz and Evans, who both held a Ph.D. in mathematics, to chair meetings of Section K. In the same process of distancing himself from the econometric project, in 1935, Wilson also asked no longer to be part of the Council of the Econometric Society.[40] Furthermore, he declined four invitations by Alfred Cowles to deliver talks at the Research Conference on Economics and Statistics at Colorado Springs, organized by the Cowles Commission. Wilson appeared in a tentative program for the first Conference in 1935, where he was supposed to talk about the decomposition of times series, which he never did.

As he marginalized himself from the econometric movement, while still indirectly controlling Section K, Wilson retreated towards Cambridge, where he started promoting a program of mathematical economics at Harvard.[41]

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[39] E. Wilson to R. Frisch, Nov. 15, 1933 (PEBW, 20).

[40] E. Wilson to C. Roos, Jan. 21, 1935 (PEBW, 25).

[41] See section 2.4.

2.3.2. Increasing concerns about economic policy

During the 1920s, Wilson adhered to the progressive[42] and institutionalist[43] American idea of regarding social science and economics as social engineering. In a letter to Mitchell on January 1932, Wilson explained that social science could be useful for planning and controlling social and economic affairs. For such purposes, it had to focus on small, manageable, closed systems. Wilson claimed that triumphs in public health had been made in domains where engineering processes had been applied.[44]For Wilson, however, social engineering was more a possibility than a statement about the current state of social science and economics. He thought that the economist could become an expert who would play a central role in the functioning of democracy and in the development of national prosperity with his *scientific* advice to private and public sectors; this expert would be interested in studying how society worked as a system, how social and economic affairs worked in practice and how institutions, especially education (Wilson 1940a), molded and constrained individual behavior.

In contrast to progressives and institutionalists, Wilson disregarded social reform as a motive for scientific endeavor; he criticized their trust in social and economic control and planning. Like most of his Harvard colleagues, Wilson, A Republican, disapproved of New Deal policies and the political platform of the Democratic Party during the 1930s. Wilson's dissatisfaction with New Deal policies, however, was not only based on political grounds; his was a conservative stand based on a concern about, first, the right relation between high education and science and, second, the right relation between science and society. Wilson, the social scientist, regarded society as a natural organic system; Wilson, the mathematical statistician, thought

[42] See Leonard Forthcoming.

[43] See Rutherford 2011.

[44] E. Wilson to W. Mitchell, Jan. 4, 1932 (PEBW, 19).

that science did not aim at controlling nature but at helping us to better adapt to it. Hence, social science and economics were not supposed to directly seek social reforms (Wilson 1940c); their influence over social and economic affairs should only be indirect, subtle.

Wilson had argued in 1919 that subtle planning of science yielded progress in science *and* society. To better understand what he meant, it must be underlined that, for him, planning science and scientific planning of society were two sides of the same coin. He claimed that scientific planning of society required good forecasting of social and economic affairs; however, he argued, "we presume to forecast the as yet unforecastable or attempt to control the as yet uncontrollable." (Wilson 1934b, 199).[45]

This situation led Wilson to write his first original contribution in economics. With a spectral analysis, titled *The Periodogram of American Business Activity*, he argued that there were neither periods nor cycles in the data about American business activity (1934b).[46] Hence, economic policy grounded on forecast of business cycles was useless, even dangerous. As he explained to Mitchell in a personal letter, Wilson believed that it was not yet known whether the managing of the economy would better or worsen the situation.[47] He argued that the analogy between economics and medicine or mechanics led some to talk about economic planning as if economics were at the engineering level.[48] Because it was not yet known if the *remedies* would

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[45] Here Wilson was also criticizing Fisher 1930.

[46] Wilson wrote another paper where he *translated* into English, to the readership of *Science*, the results of his more technical paper (Wilson 1934a).

[47] E. Wilson to W. Mitchell, Jan. 4, 1932 (PEBW, 19).

[48] E. Wilson to I. Fisher, May 25, 1932 (PEBW, 18).

or would not stabilize economic fluctuations, the New Deal policies were, for Wilson, the social problem itself.[49]

If the mechanical analogy was to be taken seriously in economics for economic regulation, as Wilson wrote to Fisher, one first needed to know what the concepts of inertia and friction meant in the economic system and to determine their relative magnitudes so that economic regulation could actually regulate the system.[50] More significantly for Wilson, when studying the social effects of economic depression, it was necessary to adopt a more empirical attitude and to disentangle the effects of the depression on social institutions and the effects of a governmental policy arising out of the depression (Wilson 1938). Above all, Wilson believed that "Sound economic forecasting and sound economic regulation if they shall ever be obtained [were] still […] decidedly in the future."[51] Moreover,

> "There may be this real complication in the social forecasting, viz., that possibly a knowledge of the future if we could gain it from the study of the past would so modify that future that we could not hope to forecast it without taking into account the degree to which such knowledge as we had of it would influence its course." (Wilson 1934b, 194)

Wilson suggested that forecasting in social science and economics could help control our own conduct to take advantage or avoid disadvantage of forecasted events, changing at the same time social and economic events of the future. This implied that social science and economics could *teach* us self-control (Wilson 1934b, 194). Wilson claimed that changes in social science and economics were needed to enhance social progress (Wilson 1940b). In this way, for Wilson, subtle planning

[49] E. Wilson to Mitchell, Jan. 4, 1932 (PEBW, 19).

[50] E. Wilson to I. Fisher, May 13, 1932 (PEBW, 18).

[51] E. Wilson to I. Fisher, July 25, 1934 (PEBW, 23).

required first reforms in the education of social science and economics because it would be then easier to teach future social scientists and economists how to behave. In his words:

> "Now the social scientist has got to learn to have things considered as suggestions. He must not get up and wave his arms around and say that economics today is a totally different thing from economics 40 years ago, that everything is changed because in science things don't change totally. […] Science is as a matter of fact the study of those things which don't change or at any rate change so slowly that we may regard them for practical purposes as non-changing or at any rate can assign limit to their change in amount and not time. This is all very carefully pointed out with its implications for social science by Pareto in his Manual of Political Economy."[52]

The modernization of social science and economics through educational reforms in the sense of the Gibbsian mathematics and Paretian economics, Wilson thought, would eventually help control matters of society and realize planned objectives. Such transformation was possible because

> "there seems to be no present conclusive evidence that learning a particular technique is 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 1940a, 664)

In 1940, Wilson suggested hence that mathematics was a sort vernacular language. Such suggestion embodied his belief that mathematics and science offered an operational and practical way of controlling and planning social and economic matters. However, as illustrations of Wilson's nationalistic and political prejudices, it was clear that, for him, this vernacular should be his Gibbsian American language

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

and the way of controlling and planning should be different from the New Deal policies proposed by the Democratic Party over the 1930s, because he felt, these policies were not based on sound scientific foundations.

Before closing this section, two points of interpretation should be noticed. First, in Wilson's ideas about the foundations of knowledge, the line that separated intuition and personal judgment from prejudice was porous. Second, while he tried to connect science and society, Wilson thought that science, as he defined it, was above society.

2.4. A program for mathematical economics at Harvard

As a discrete way of proposing an alternative to the econometric movement without engaging in methodological quarrels and moved by his concerns about economic policy, Wilson turned his efforts towards Cambridge at the more local level of Harvard, where he assumed effective leadership regarding statistical and mathematical economics.

In 1928, Wilson had declined an offer to teach statistical economics made by Harold Burbank, chairman of the department of economics at Harvard.[53] Then, at the beginning of 1930, Leonard Crum reported to Wilson that Burbank wanted them "to discuss the prospect of further development of the mathematical side of our work in economics."[54] It was then decided that Wilson would start offering a course on statistical economics, titled Foundations of Statistical Theory in the 1931-32 academic year. He wanted to run the course "as a sort of pro-seminar taking the question of the possibility of determining a measure of stability for the economic situation,"[55] working on statistical data and hoping that some "students might really be considering economics more than statistics and getting into a position where they

[53] E. Wilson to H. Burbank, May 29, 1928 (PEBW, 12).

[54] L. Crum to E. Wilson, Jan. 4, 1930 (PEBW, 15).

[55] E. Wilson to H. Burbank, March 23, 1932 (PEBW, 18).

could handle a statistical economic thesis."[56] But most students were auditors. Despite low attendance, "the course met all expectations and needs"[57] of the department. At the end of 1932, invited by Burbank,[58] Wilson started attending departmental meetings.

With the idea of establishing an American school of mathematical economics, in November 1932, Wilson presented to Burbank the possibility of alternatively offering a theoretical (in the sense of mathematical statistics) and an empirical (in the sense of Mitchell) course on statistics. He further proposed the establishment of a new course on mathematical economics (in the sense of Pareto). Each course would be given once every three years. He insisted to Burbank that such courses were necessary, pleading for a more active role "of the university in the changing social order."[59] Schumpeter, an "open-minded" advocate of mathematical economics who was a fellow of the Econometric Society and who came permanently to Harvard in 1932 (McCraw 2010), supported Wilson's efforts. Certainly with Schumpeter's and Crum's endorsement of Wilson's offer, Burbank launched a committee of instruction in advanced mathematical economics composed by Wilson, Schumpeter and Crum at the beginning of 1933. Following Wilson's lead, the committee worked in conjunction with Huntington and William Graustein of the department of mathematics. Eventually, the committee supported the idea of establishing a program in advanced mathematical economics.[60] For Wilson, the aim of such a program consisted of developing, through instruction, the necessary conditions so that young

[56] Idem. Wilson's 1934 paper on periodograms was probably an outcome of his course.

[57] H. Burbank to E. Wilson, Apr. 13, 1932 (idem).

[58] H. Burbank to E. Wilson, Oct. 29, 1932 (idem).

[59] E. Wilson to H. Burbank, Nov. 25, 1932 (idem).

[60] Report, Meeting of the Committee (Wilson, Crum, Schumpeter) on Instruction in Mathematical Economics, Tuesday, April 18 (idem).

economists could learn how to use both mathematics *and* statistics in order to modernize economics. In his own terms:

> "[W]e are training economists not for the next 10 years but for their academic life and that the trend is such that a very considerable number of economists will have to be adequately familiar with both mathematical theory and statistical procedures 20 to 30 years from now."[61]

Following the suggestions of the committee, the department established first an introductory course on mathematical economics. Schumpeter gave it during the 1933-34 academic year. Wilson regarded that course as a temporary "proselyte" course, given by a leading economist, which would help introduce mathematics to the department.[62]

In March 1934, Wilson renewed his offer to Burbank of a more empirical course in statistics and another on advanced mathematical economics.[63] Schumpeter, whose mathematical skills were not sophisticated enough for his introductory course, proposed Wilson to replace him. Wilson declined the offer. He had a more advanced course in mind. Looking for additional support, Wilson argued to Frank Taussig that the situation in mathematical economics was urgent. "Mathematical and statistical economics seem to me both to tend to get away from sound economic theory into mathematical or statistical manipulation. If they do this they can do more harm than good."[64] Taussig agreed.[65]

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[61] E. Wilson to W. L. Crum, May 1, 1933 (PEBW, 20).

[62] E. Wilson to H. Burbank, Apr. 12, 1934 (PEBW, 22).

[63] E. Wilson to H. Burbank, March 23, 1934 (PEBW, 22).

[64] E. Wilson to F. Taussig, March 12, 1934 (PEBW, 20). They regularly corresponded regarding advisory publishing questions of the *Quarterly Journal of Economics,* of which Taussig was the editor.

[65] F. Taussig to E. Wilson, March 17, 1934 (PEBW, 24).

Wilson's activism for the establishment of a program of mathematical economics was rewarded with the acceptance of "a more advanced course in Mathematical Economics—one which [would] fall within the range of [his] interest."[66] The course was opened mainly for graduate students. Wilson's idea of alternating his courses was also accepted, but he should alternate yearly between a course on mathematical statistics and a course on mathematical economics, only. In respect to the latter, as written in the abstract of the course, Wilson wanted it to be a "systematic study of one or more of the classical formulations of economic theory in terms of mathematic symbols with collateral reading from writings of Marshall, Edgeworth, and others, who sometimes used the mathematical methods."[67]

Once Wilson's course was introduced in the list of courses at Harvard, Schumpeter thanked Wilson:

> "I want to say again how intensely grateful I feel to you for giving yourself to the subject and to the cause. You are the first eminent scientist to do so to this extent and if we shall be able to show results at Harvard and establish ourselves as one of the nurseries of economic thought in this field it will be your merit." (J. Schumpeter to E. Wilson, 24 May 1934. In Schumpeter 2000, 269)

Moved by Schumpeter's kind words, Wilson replied, explaining how he understood the configuration of the established program.

> "As I see it, your job is to take people who don't know their mathematics and coach them along encouragingly until they shall be able to plug at specific articles in economics which use some mathematics, whereas my job is to […] encourage him who knows some mathematics to see that he can think in a connected mathematical fashion about his problems. […] I

[66] H. Burbank to E. Wilson, March 30, 1934 (PEBW, 22).

[67] E. Wilson to H. Burbank, May 17, 1932 (PEBW, 22).

> take it that your advanced courses in economic theory would be in many ways pleasanter for you to give if you could have students who could carry a mathematical argument, not merely follow one. On the other hand it would be tragic, it seems to me, if you had to give a lot of your time to teach them to follow a mathematical argument. They ought to have this language at their disposal when they come to you so that they could concentrate on economics as such."[68]

During the 1934-35 academic year, Schumpeter and Wilson offered their respective courses on mathematical economics. Schumpeter's course was well attended by students and by staff of the department (McCraw 2010), whereas Wilson's course, too difficult for average students, had fewer students.

During the 1930s, Wilson became the pillar of mathematical economics at Harvard. With his permission, Schumpeter attended some of his lectures in 1936[69] and in 1937.[70] During the 1935-36 academic year, Wassily Leontief, who had arrived at Harvard also in 1932, replaced Schumpeter and taught the introductory course of mathematical economics. In 1936, Wilson offered his course on mathematical statistics. Back in 1933, he had helped Leontief with "certain mathematical problems which [he encountered in his] research on demand and supply."[71] Until 1943, Wilson offered each one of these courses, alternating them every two years, while Leontief kept teaching the introductory mathematical course.

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[68] E. Wilson to J. Schumpeter, May 29, 1934 (PEBW, 24).

[69] J. Schumpeter to E. Wilson, Apr. 24, 1936 (PEBW, 27).

[70] J. Schumpeter to E. Wilson, May 19, 1937 (PEBW, 28). In the same vein, Mitchell acknowledged that he "never [saw] a piece of [Wilson's] work without envying the skill and the masterly restraint with which [he employed his] mathematical gifts and accomplishments" (W. Mitchell to E. Wilson, Dec. 2, 1932 [PEBW, 19]).

[71] W. Leontief to E. Wilson, Feb. 3, 1933 (PEBW, 21).

2.4.1. Harvard economics courses

During the 1920s, Wilson had established a close relationship with Lawrence Henderson, who was then developing his ideas about stable equilibrium as applied to social science and economics.[72] In this spirit, Henderson invited Wilson to offer a talk on Pareto before the Royce Club, an interdisciplinary discussion group of which Henderson was the secretary. On March 25, 1923, Wilson descriptively discussed Pareto's mathematical economics; he explained that Paretian economics consisted of:

> "1°. Statics, which has to do with any unchanging economic configuration, with economic equilibrium. 2 °. Kinematics, which studies successive equilibria, and which is not yet well developed. 3°. Dynamics, which has to do with economic momentum, [and which] has not been developed at all. […]. We have to study the desires or tastes of people, and the obstacles in the way of their satisfaction, and how the tastes and the obstacles combine into an economic equilibrium. We must proceed with a maximum of reality and measureableness."[73]

In his mathematical and statistical courses for economists, Wilson developed on these Paretian concerns. Over the years, he seemed to have changed the subjects that he covered in each of his lectures. Even though the material covered each year cannot be exactly established, his lectures can be *approximately* reconstructed in various complementary ways. For this purpose, use will be made of archival material found in Wilson's, Lloyd Metzler's[74] and James Tobin's[75] archives. Wilson's published papers in economics, his and others' writings on dynamics and Gibbs thermodynamics will be of help, too.

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[72] On Henderson and equilibrium in social science, see Russett 1966.

[73] Royce Club, PEBW, HUG4878.214, Box 1, Folder: Book reviews, letters to the Editor, p. 5.

[74] Metzler attended Wilson's course in mathematical economics, probably during the spring of 1939.

[75] Tobin attended Wilson's course in mathematical economics during the spring of 1941.

2.4.2. Mathematical Economics

When Wilson was first preparing his 1935 course, he consulted Schumpeter, Burbank, Taussig at Harvard as well as Roos and Mitchell as to the most relevant material and work to be covered. Wilson believed that Marshall's mathematical appendices were "scrappy" and that Griffith Evans, in his *Mathematical Introduction for Economics* (1930), did not "study broad problems or at any rate [did not] give the student a broad point of view as to the applicability of the mathematical method. He seems to treat the whole subject as a series of rather minor problems thus catering to the American students great failure, namely, of being a clever solver of insignificant problems."[76]

Wilson thought he would develop on the works of Cournot, Walras, Pareto, Bowley, Edgeworth, Marshall, Evans and Fisher, and that if he had time, he would also cover the most modern papers by Roos, Frisch and Schultz.[77] In 1935, Wilson introduced[78] the course by covering Arthur Bowley's The *Mathematical Groundwork of Economics* (1924). In subsequent years, he seemed to have only mentioned here and there Bowley's work.

In Wilson's archives, two full folders titled *Notes on Economics* contain what seem to be the *undated* notes that Wilson used when preparing his lectures in mathematical economics. In the first folder, the material relates to Wilson's interpretation and mathematical exposition of extant literature on topics mainly related to consumer theory. In the second folder, the material shows Wilson's presentation of physical systems in stable equilibrium, which he eventually used, as analogy, to re-defining

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[76] E. Wilson to F. Taussig, May 17, 1934, (PEBW, 24). On Evans's mathematical economics, see Weintraub 1998.

[77] E. Wilson to C. Roos, Oct. 6, 1934 (PEBW, 23).

[78] E. Wilson to J. D. Black, July 14, 1936 (PEBW, 26).

the notion of consumer stable equilibrium.[79] Over the years, Wilson complemented his lectures, leaving however a rather disordered track of this evolution in his folders. Based in certain documents found in these folders and in other archives, some conjectures can be made regarding the course.

According to the separation into two distinct folders, it can be the case that Wilson divided the course into two main sections. As for the evolution of the course, it must be the case that in 1935, the first time that the course was given, emphasis was laid on material found in the first folder, as Wilson certainly limited himself to discuss extant mathematical economics in connection to consumer theory; it must also be the case that in 1937, and probably in 1939, Wilson's focus was rather on the mechanical analogy. By then, Wilson had contributed to *A Commentary on the scientific writings of J. Willard Gibbs* with a paper on *Gibbs' lectures on thermodynamics* (Wilson 1936) and had probably had the time to explore more in detail the analogy with consumer theory. Consistent with this conjecture, Schumpeter, who attended some of the 1937 lectures, "was strongly impressed with the immense value to the economists of such lectures [on theoretical mechanics or physics] as [given] in the first part of the course."[80] Also, in the second folder, in one of the various sets of sheets (numerated with roman numbers), Wilson first developed static, kinematic and thermodynamic equilibria (I-XX); then, on this basis, he described first the consumer stable equilibrium analogically to the physical equilibrium, and only then he discussed consumer theory, as found in extant mathematical economics literature.[81] In 1941, and probably in 1943, Wilson developed on questions of independence-substitution-complementary in consumption as well as of Pareto's law of income distribution and briefly talked about thermodynamics in economics. This time, Wilson presented the

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[79] Wilson probably felt that the analogy was not arbitrary, first, because he regarded, with Henderson, the concept of consumer's stable equilibrium as being intuitive, and second, because he did not use probability.

[80] J. Schumpeter to E. Wilson, May 19, 1937 (PEBW, 28)

[81] E. Wilson, *Notes on Economics*, PEBW, HUG4878.214, 1.

material as a sort of critical response to Harold T. Davis's *The Theory of Econometrics* (1941); Wilson suggested then that Davis' statistical inference methods, when dealing with time series, were not yet optimal.[82]

As for the content, in the section of the course where he interpreted the theory of the consumer, as he found it in contemporary literature, Wilson talked about maximization of utility functions with two and multiple variables under budgetary constraint, the marginal utility of money, demand functions in the sense of Walras, Pareto, Marshall and Hicks as well as the connection between utility and demand functions.

At some point in the course, Wilson quoted Henry Schultz's *Theory of Measurement of Demand* (1938, 10–12) and mentioned, in passing, the definition of the operational method as interpreted by Percy Bridgman (1927) and by Schultz himself.[83] Wilson's published papers in economics would have emerged from this part of his lectures.[84] In this way it can be argued that in this section of the course, over the years, Wilson covered the basic elements that would help him offer a *Generalization of Pareto's Demand Theorem* (1935), some comparisons between Pareto's and Marshall's laws of demand (1939; 1943), some mathematical inconsistencies found in extant economics literature such as in Hicks' theory of value (1944a), some discussions on utility functions (1944b) and utility indexes (1946) as well as some comments on substitution (1944a) and complementarity (1945) in consumption. Production seemed to have been out of the syllabus.

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

[83] E. Wilson, *Notes on Economics*, PEBW, HUG4878.214, 1. Wilson clearly identified with such ideas, which, in his own terms, had been central for his foundational discussions. See chapter I.

[84] As Wilson explained to Taussig, editor of the Quarterly Journal of Economics, when submitting his first paper to the Harvard Journal, Gerhard Tintner and Nicholas Georgescu-Roegen, who attended his 1935 course in mathematical economics, had told him that some of the developments that he offered were actually original (E. Wilson to F. Taussig, March 28, 1935 [PEBW, 26]). Similarly, he "dug out some notes of [his] course" to write his 1944 *Hicks on Perfect Substitutes* (E. Wilson to P. Samuelson, Nov. 29, 1943 [PASP, 77]).

In the section of the course where he developed on the mechanical analogy (second folder), Wilson talked about equilibrium of a mechanical system and oscillations leading to it; he described the characteristics of a stable equilibrium and presented the Le Chatelier Principle as interpreted by Jean Baptiste Perrin's *Traité de Chimie Physique* (1903, 188), as a principle of stability of equilibrium in the case of infinitesimal changes of a parameter. Wilson then covered the theory of thermodynamical equilibrium, including the phase systems of Willard Gibbs, suggesting that it could be studied as a static, time-independent problem.[85] He also underlined that stable equilibrium required some inequalities that he precisely defined, in both discrete and continuous cases. Following these lines of thought, he then presented the consumer maximization as a static time-independent problem, and solved it in the discrete case, stable equilibrium of which also required certain discrete inequalities. He then solved it in the continuous case, suggesting that the discrete and continuous cases were equivalent; the former was more general and less abstract, for it did not require derivative calculus.

Wilson developed consumer equilibrium explicitly analogically to the thermodynamic equilibrium asserting that there were some similarities. First, in both complex systems, the analysis resulted from an assumed extremum position; second, both systems "must always be closed"[86] and, third, in both systems some inequalities, which were called by Wilson the Gibbs conditions, characterized the static stable equilibrium position. Such inequalities resulted from an optimization under constraint problem, which was solved at all times, not over time, since "With time introduced, everyone recognizes that preferences change."[87]

[85] Wilson's *Gibbs' lectures on thermodynamics* (1936) probably illustrate the kind of insight about the Gibbs' mathematical style that he gave during his lectures to economists.

[86] LMP, 7, Folder Econ-theory: Harvard courses Notes 1938-1939, Wilson p. 6.

[87] JTP, 7, Folder "Ec 104b E. B. Wilson," p.187.

The novelty of such analysis relatively to extant (Pareto) mathematical economics, Wilson argued, resided in the more general aspect of his study, which was made "with finite differences [rather] than [only] with derivatives."[88] In this vein, after providing a proof of Lagrange multipliers, Wilson explained in one of his lectures:

> "Originally, calculus developed from considerations of finite differences, and formulas were derived by neglecting power terms of finite differences."[89]

Wilson did not cover dynamical systems in connection with business cycles analysis in his course on mathematical economics. He felt that applicability of the different working hypotheses found in the treatment of dynamical systems in physics to deal with the aggregate economic system was not self-evident and required further study.[90] There was not yet a satisfactory postulational foundation for business cycles, nor a sound correspondence between business data and economic theory of cycles, he thought. Before pretending to control the economy, Wilson claimed, economists needed to establish whether or not stabilization policies stabilized or not the system through time.[91]

Such work, Wilson argued, should be done simultaneously in the spirit of Pareto's mathematical economics and Mitchell's institutional statistics. It can be conjectured that with such a Pareto-Mitchell approach, Wilson aimed at developing something intermediate that would counterweight Frisch's structuralist econometrics and Davis's *Theory of Econometrics* (1941) when dealing with business cycles, without embracing directly Mitchell's approach. Such study was necessary, Wilson argued,

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

[89] JTP, 7, Folder "Ec 104b E. B. Wilson," p.177.

[90] E. Wilson to W. Mitchell, Nov. 16, 1936 (PEBW, 27).

[91] E. Wilson to I. Fisher, May 25, 1932 (PEBW, 18).

because sound fiscal, public finance, monetary and price policies were needed. Wilson seems even to have written a draft on the subject. In this draft, as he summarized it to Burbank, he argued that there was an urgent need of a better definition of national income to better understand the effects of spending large fractions of national income through governmental expenses. He suggested also studying rigidities created thereby, the relation between public and private credit, the possibility of a compensatory mechanism for business cycles as well as deficit financing and debt retirement.[92] By explaining to Burbank that Alvin Hansen, who was appointed at Harvard in 1937, agreed with him on these matters, Wilson was certain that this sort of study was

> "a job that is going to make a real reputation for somebody and will ultimately become I am sure the central feature of our school if the school maintains the kind of intellectual level that we want to have it maintain. There is a dreadful lot of statistical work that ought to be done in following up the study of this problem. […]. I found we have to do an awful lot of preliminary statistical work in the public health and epidemiological fields and I don't see why we should not have to in the economic field if we try to handle the real problems of practical importance in an effective way."[93]

On November 1938 Wilson consulted Schumpeter for suggestions for his courses and insisted:

[92] E. Wilson to H. Burbank, May 14, 1937 [PEBW, 28]. Wilson was familiar with Keynes' work on money, which he regarded as brilliant but purely theoretical and inapplicable in reality (E. Wilson to Snyder, Sept. 19, 1934 [PEBW, 24]), but was familiar only with some criticisms of Keynes's *The General Theory* (1936).

[93] E. Wilson to H. Burbank, May 14, 1937 (idem).

> "I can in any way carry part of the load of the teaching of mathematical economics on the more mathematical side I shall be very happy."[94]

Wilson's comments led to a meeting on December between Wilson and Leontief to coordinate the program in mathematical economics. Wilson then reported to Burbank that Leontief:

> "suggests that I take up **dynamical economics** (which he doesn't touch at all except perhaps by implication) on the background of the texts and other writings of C.F. Roos and of Tinbergen who has recently published a 73 page monograph by Herman in Paris entitled an Econometric Approach to ***Business Cycle*** problems and who has further contributions in various journals particularly econometric and the Giornal Degli Economisti."[95]

If Wilson's report were taken seriously, to coordinate the program of mathematical economics, Leontief would have encouraged Wilson to cover dynamical economics to deal with business cycles.[96]

2.4.3. Topics in Statistical Theory

Over the years, in his statistical course, Wilson focused increasingly on mathematical statistics rather than on actual data analysis. Privately, he titled his course *Mathematical Statistics*, as the 1938 notes of his course by one of his (unknown) students attested.[97]

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[94] E. Wilson to J. Schumpeter, Oct. 4, 1938 (PEBW, 31).

[95] E. Wilson to H. Burbank, Dec. 20, 1938, emphasis added (PEBW, 30).

[96] E. Wilson to H. Burbank, Dec. 20, 1938 (PEBW, 30).

[97] E. Wilson, *Notes on Mathematical Statistics,* PEBW, HUG4878.214, 2.

In the course, contemporary works in mathematical statistics that developed on probability were not actually covered; he was rather critical of them, going so far as to "wonder if [the modern concept of probability] has something to do with statistics," rather than with pure mathematics.[98] In the same vein, in the 1930s, despite believing that Ronald Fisher was the leading statistician of his time, he had a "suspicion that he belongs essentially to the group which considers it more important to apply formulae just on ultra refined mathematical considerations than to the group who consider it of the greatest importance to examine carefully how in fact the data do behave and to adapt their statistical methods as simply as possible to the material."[99] The latter group, he suggested, was composed by those of the Scandinavian school, namely Jørgen P. Gram, Thorvald N. Thiele, Carl V. Charlier, and Wilhelm Lexis, whom statistics, Wilson argued, had influenced John Maynard Keynes when writing his *Treatise on Probability*.[100]

In his course of 1938, Wilson's general aim consisted of providing tools in analysis and probability lying behind sample theory to estimate parameters and fitting frequency functions. The emphasis on calculus and lag operators was evident and the references to Wilson's *Advanced Calculus* (1911a) and Edmund Taylor Whitaker and G. Robinson's 1924 *The Calculus of Observations* numerous. In the first chapter, Wilson discussed operators; he introduced them as the fundamental basis of analysis. In the same section, Wilson dealt with difference equations and developed on the Taylor Series, among other formulas of approximation. In the second chapter, Wilson covered gradation, which was Wilson's contemporary term to talk about curve fitting and other smoothing techniques. In the third chapter, Wilson discussed sample

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[98] E. Wilson to F. Mills, Oct. 30, 1935 (PEBW, 25).

[99] E. Wilson to F. Mills, May 28, 1938 (PEBW, 31).

[100] E. Wilson to A. Fisher, Jan. 31, 1924 (PEBW, 6). Wilson believed that Keynes had offered the best postulational foundations of probability, although his *Treatise* remained largely unsatisfactory (Wilson 1923b).

theory, introducing it by stating that a "desirable, but not yet attained, sample theory would be one that was independent of the form of the universe from which the sample was drawn" (p.52). He then presented the theory of moments as well as certain distribution functions. In the fourth chapter, Wilson talked about ways of approximating (asymptotically) probability distributions by discussing the Gram-Charlier series: the series, he explained, "has to converge to have a meaning" (p.76), the faster the better. In the following chapters, after discussing logarithm transforms, Wilson covered the method of maximum likelihood. Wilson closed the course with a brief comment on interpolation, namely on how to construct a curve or a function from a finite number of discrete points or values of a given variable, with an example on population.

Wilson believed that his course was "not of very great advantage to a person who works with actual statistical material;"[101] first because he mainly emphasized analytical statistics, and second, because he did not discuss statistical inference. The emphasis of the course and the absence of inference were probably due to the fact that Wilson felt that a taxonomy of the dynamics of the aggregate economic system as found in business cycles based on postulational thinking was first needed; such an approach was coherent with his emphasis on analytical statistics. In the 1940s, Wilson still believed that "a set of postulates, within which our concepts can have logical meaning, […] for probability theory lying behind statistics"[102] was missing. His analytical statistics appeared therefore to be a discrete alternative to the econometric movement, developed for and presented only in Harvard classrooms.

Wilson's lasting influence in economics was most of all embodied in the work of one of his students who attended both of his Harvard courses: Samuelson, who

[101] E. Wilson to J. Schumpeter, May 30, 1936 (PEBW, 27).

[102] JTP, 7, Folder "Ec 104b E.B. Wilson", p. 158.

acknowledged Wilson's great influence on his thesis and his subsequently famous *Foundations of Economic Analysis.*[103]

2.5. Conclusion

As Samuelson pointed out in the quotation opening this chapter, Wilson knew everything and everybody. He successfully promoted the "no man's land" of inter-disciplinarity in America. Wilson was a community builder. He significantly contributed to the connection of the communities of mathematicians and scientists with the community of economists. At a specific moment in time, he created the necessary institutional support within legitimate scientific communities for the establishment of a community of mathematical economists in the United States. Wilson's punctual support enabled American mathematical economists to hold the scientific label with legitimacy, a legitimacy, which they did not yet have in departments of economics at their universities. Concomitantly, he was central for the American origins of the Econometric Society.

At Harvard, at a more local level, he established the first program in mathematical economics. With his *Mathematical Economics* and *Mathematical Statistics* courses, Wilson wanted to connect economics with data, while developing *much economics with little mathematics*. For Wilson, this idea implied developing modern economics as a compromise, as a balance between past and present contributions in economics and between a certain theoretical emphasis, as offered by Pareto's and Fisher's mathematical economics, and a certain empirical emphasis, as developed by the American institutionalist tradition that focused on statistical economics, of which Mitchell was a worthy representative. Wilson thought that his attitude towards mathematical and statistical economics embodied Gibbs' truly American attitude

[103] On Wilson's influence on Samuelson's thesis (1941a) and *Foundations of Economic Analysis* (1947), See chapter III of the present thesis. See also Backhouse 2015; Forthcoming.

towards mathematics and science; it emphasized the relevance of mathematical reasoning while suggesting that shared intuitions in a subject matter prevailed over mathematical and theoretical structures; it focused on analytical statistics rather than on pure probability theory, as Frisch and his contemporary econometricians, Wilson felt, tended to do. In this spirit, Wilson offered, in his courses at Harvard, a possible alternative to econometrics, as it was being developed around 1940, without arguing that the solution to quantitative economics was merely to be found in Mitchell's work, which lacked mathematical rigor according to Wilson. In his courses, with Henderson, Wilson also presented the notion of equilibrium as intuitive and defined it in discrete terms.

Wilson's lasting influence in economics was most of all embodied at a personal level, as his ideas about mathematics and statistics significantly influenced Samuelson, who eventually wrote *Foundations of Economic Analysis* within a Wilsonian framework.

In the last analysis, if grounded on his attitude, Wilson thought, modern economics could eventually serve to control and plan the economy. Modern economics would eventually yield to modern society, and vice versa.

2.6. References

Backhouse, Roger. Forthcoming. *Maker of Modern Economics: An Intellectual Biography of Paul A. Samuelson*. Vol. 1: Becoming Samuelson. 2 vols.

———. 2013. "Scientific Welfare Analysis: The Origins of Bergson-Samuelson Welfare Economics, 1936-1947." *Unpublished Draft*.

———. 2014. "Paul A. Samuelson's Move to MIT." *History of Political Economy* 46 (suppl 1): 60–77.

———. 2015. “Revisiting Samuelson’s Foundations of Economic Analysis.” *Journal of Economic Literature* 53 (2): 326–50.

Bjerkholt, Olav. 1998. “Ragnar Frisch and the Foundation of the Econometric Society and Econometrica.” In *Econometrics and Economic Theory in the 20th Century: The Ragnar Frisch Centennial Symposium*, edited by Strøm Steinar, 26–57. United States of America: Cambridge University Press.

Bowley, Arthur. 1924. *The Mathematical Groundwork of Economics: An Introductory Treatise*. Oxford: Clarendon Press.

Bridgman, Percy. 1927. *The Logic of Modern Physics*. New York: Macmillan.

Cochrane, Rexmond. 1978. *The National Academy of Sciences: The First Hundred Years, 1863-1963*. Washington: National Academy of Sciences.

Davis, Harold Thayer. 1941. *The Theory of Econometrics*. Bloomington, Ind: The Principia press.

Evans, Griffith Conrad. 1930. *Mathematical Introduction to Economics*. New York: McGraw-Hill Book Company, Inc.

Fenster, Della Dumbaugh, and Karen Parshall. 1994. “A Profile of the American Mathematical Research Community: 1891-1906.” In *The History of Modern Mathematics: Images, Ideas, and Communities*, edited by Eberhard Knobloch and David Rowe, 3:179–227. Boston: Academic Press.

Fisher, Irving. 1930. *The Stock Market Crash - And After*. First Edition edition. New York: MacMillan & Co LTD.

Giraud, Yann. 2007. “L’économie est-elle une discipline visuelle ? (1932-1969).” Paris: Université Paris X Nanterre.

Ingrao, Bruna, and Giorgio Israel. 1990. *The Invisible Hand: Economic Equilibrium in the History of Science*. Cambridge Mass.: The MIT Press.

Isaac, Joel. 2012. *Working Knowledge: Making the Human Sciences from Parsons to Kuhn*. Cambridge Mass.: Harvard University Press.

Keynes, John Maynard. 1936. *The General Theory Of Employment, Interest, And Money*. London: MacMillan & Co.

Leonard, Thomas. Forthcoming. *Liberal Reformers*. Princeton UP.

Livingston, Burton E. 1929. "The American Association for the Advancement of Science: Accounts of the Sessions of Sections and Societies at the Fifth New York Meeting." *Science*, New Series, 69 (1779): 107–31.

———. 1930. "The American Association for the Advancement of Science: Reports of the Des Moines Meeting of the American Association for the Advancement of Science and Associated Societies." *Science*, New Series, 71 (1832): 137–68.

———. 1931. "The American Association for the Advancement of Science: Reports of the Fourth Cleveland Meeting of the American Association for the Advancement of Science and Associated Societies." *Science*, New Series, 73 (1884): 137–68.

Louçã, Francisco. 2007. *The Years of High Econometrics: A Short History of the Generation That Reinvented Economics*. London and New York: Routledge.

McCraw, Thomas K. 2010. *Prophet of Innovation: Joseph Schumpeter and Creative Destruction*. 1 edition. Cambridge, MA: Belknap Press.

Mills, Frederick C., Jacob H. Hollander, Jacob Viner, Edwin Wilson, Wesley C. Mitchell, F. W. Taussig, T. S. Adams, John D. Black, and John Candler Cobb. 1928. "The Present Status and Future Prospects of Quantitative Economics." *The American Economic Review* 18 (1): 28–45.

Mirowski, Philip. 1989. *More Heat than Light: Economics as Social Physics, Physics as Nature's Economics*. Cambridge, England: Cambridge University Press.

———. 2002. *Machine Dreams: Economics Becomes a Cyborg Science*. Cambridge, England: Cambridge University Press.

Perrin, Jean Baptiste. 1903. *Traité de chimie physique*. Paris: Gauthiers-Villars.

Russett, Cynthia Eagle. 1966. *The Concept of Equilibrium in American Social Thought*. New Haven, Cann.: Yale University Press.

Rutherford, Malcolm. 2011. *The Institutionalist Movement in American Economics, 1918-1947: Science and Social Control*. Cambridge: Cambridge University Press.

Samuelson, Paul. 1941. "Foundations of Analytical Economics: The Observational Significance of Economic Theory." Cambridge Mass.: Harvard University.

———. 1947. *Foundations of Economic Analysis*. Cambridge, Mass. Harvard University Press.

———. 1998. "How Foundations Came to Be." *Journal of Economic Literature* 36 (3): 1375–86.

Schultz, Henry. 1938. *The Theory and Measurement of Demand*. Chicago: The University of Chicago Press.

Schumpeter, Joseph Alois. 2000. *Briefe /Letters*. Edited by Ulrich Hedtke and R. Swedberg. Tübingen: Mohr Siebeck.

Weintraub, Roy. 1991. *Stabilizing Dynamics Constructing Economic Knowledge*. Cambridge, England: Cambridge University Press.

———. 1998. "From Rigor to Axiomatics: The Marginalization of Grif.th C. Evans." *History of Political Economy* 30 (Supplement): 227–59.

Wilson, Edwin. 1903. "The So-Called Foundations of Geometry." *Archiv Der Mathematik Und Physik* 6: 104–22.

———. 1908. “Note on Statistical Mechanics.” *Bulletin of the American Mathematical Society* 15 (3): 107–15.

———. 1909a. “Economics.” *Bulletin of the American Mathematical Society* 15 (4): 169–86.

———. 1909b. “Thermodynamic Analogies for a Simple Dynamical System.” *Annals of Mathematics* 10 (4): 149–66.

———. 1911. *Advanced Calculus*. Boston, New York: Ginn and company.

———. 1912. “Mathematical Economics.” *Bulletin of the American Mathematical Society* 18 (9): 462–74.

———. 1913. “Review of The Purchasing Power of Money; Its Determination and Relation to Credit, Interest and Crises by I. Fisher.” *Science* 37 (959): 758–63.

———. 1914. “The Price Level.” *Bulletin of the American Mathematical Society* 20 (7): 377–81.

———. 1923a. “First and Second Laws of Error.” *Journal of the American Statistical Association* 18 (143): 841–51.

———. 1923b. “Keynes on Probability.” *Bulletin of the American Mathematical Society* 29 (7): 319–22.

———. 1926. “Statistical Inference.” *Science* 63 (1629): 289–96.

———. 1934a. “Are There Periods in American Business Activity?” *Science* 80 (2070): 193–99.

———. 1934b. “The Periodogram of American Business Activity.” *The Quarterly Journal of Economics* 48 (3): 375–417.

———. 1935. “Generalization of Pareto’s Demand Theorem.” *The Quarterly Journal of Economics* 49 (4): 715–17.

———. 1936. “The Contributions of Gibbs to Vector Analysis and Multiple Algebra.” In *Commentary on the Scientific Writings of J. Willard Gibbs*, edited by Frederick Donnan and Arthur Haas, 2:127–60. New Haven: Yale University Press.

———. 1938. “How to Study the Social Aspects of the Depression.” *Journal of the American Statistical Association* 33 (203): 505–12.

———. 1939. “Pareto Versus Marshall.” *The Quarterly Journal of Economics* 53 (4): 645–50.

———. 1940a. “Methodology in the Natural and the Social Sciences.” *American Journal of Sociology* 45 (5): 655–68.

———. 1940b. “Social Progress.” *Proceedings of the National Academy of Sciences of the United States of America* 73 (15): 469–72.

———. 1940c. “What Is Social Science?” *Science* 92 (2382): 157–62.

———. 1943. “Pareto on Marshall’s Demand Curve.” *The Quarterly Journal of Economics* 58 (1): 141–45.

———. 1944a. “Hicks on Perfect Substitutes.” *The Quarterly Journal of Economics* 59 (1): 134–40.

———. 1944b. “On Notation for Utility Theory.” *The Quarterly Journal of Economics* 58 (4): 647–50.

———. 1945. “Consumption in Fixed Proportion.” *The Quarterly Journal of Economics* 59 (4): 635–39.

———. 1946. “Notes on Utility Theory and Demand Equations.” *The Quarterly Journal of Economics* 60 (3): 453–60.

CHAPTER III

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

3.2. Introduction[1]

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 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

[1] carvaja5@gmail.com. I am thankful to Roger Backhouse, Nicolas Giocoli and Robert Leonard for their helpful comments on this chapter. The usual caveat applies. I am also grateful to archivists of the Harvard University Archives (HUA), of the David M. Rubenstein Rare Book & Manuscript Library at Duke University (DU). Papers of Edwin Bidwell Wilson (PEBW) were consulted at HUA, HUG4878.203 (indicated if different), Paul A. Samuelson Papers (PASP) and Lloyd Metzler Papers (LMP) were consulted at DU. James Tobin Papers (JTP) can be consulted at Yale University Library. In the following chapter, the number of the boxes in which the relevant material was consulted will follow the respective collection.

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

> 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 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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[3] *Idem*.

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

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

* * *

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 Samuelson conducted his graduate studies between 1935 and 1940.[6] 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 chapter, certain aspects of this importance are examined. By regarding *Foundations* from the perspective of Samuelson's active commitment to Wilson, as regards mathematics, statistics and science, this chapter 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 precisely 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," "operational meaningful theorems," and "useful" knowledge.

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

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[6] See chapter II.

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 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 concern 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 early work, 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, not 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 three dimensions: the introductory chapters; the microeconomic level of the system; and the aggregate 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.

3.3. The master and the disciple

3.2.1. Edwin Bidwell Wilson

Wilson was born in 1879, in Hartford, Connecticut. Educated in the school that his father directed, he was then 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 modern 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 German structuralist mathematics[7] was then exerting on his American colleagues[8] and concomitantly committing to the traditional applied American mathematics that Josiah Willard Gibbs, his mentor at Yale, practiced. 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),

[7] See Wilson 1903d. On Hilbert's structuralist mathematics, see Corry 2004a; Corry 2004b.

[8] On the development of modern mathematics around 1900 in United States of America see (Parshall and Rowe 1994).

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.[9]

The task that Wilson gave himself in all the above-mentioned academic involvements consisted of fostering and establishing lasting connections between mathematics and different subject matters, following his Gibbsian mathematics.[10]

At Harvard, beginning in 1932, Wilson started teaching mathematical statistics to economists; with Joseph Schumpeter's endorsement, he then began lecturing on mathematical economics in 1935. In the latter course, Wilson presented mathematical economics analogically to Gibbs's thermodynamics. From 1935 until 1943, he gave each of the last two courses alternatively every two years. Wilson aimed his instruction in economics at protecting students from what he disdainfully regarded as the beauty of pure theoretical treatises in economics[11] and pure mathematics and pure mathematical statistics.[12] Wilson thought that students of economics, by learning Gibbs's kind of mathematics, would learn how to behave in a scientific way.

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[9] On Wilson' biography, see Hunsaker and Mac Lane 1973.

[10] On Wilson's attitude towards mathematics, see chapter I.

[11] Wilson deeply disdained some of Maynard Keynes', Irving Fisher's and Joseph Schumpeter's work, wherein they addressed, he felt, the question of controlling and planning the economy as if economics had already attained a respectful scientific level. In general terms, Wilson's rejection of these theoretical economists' work reflected his aversion of scientific approaches based on universalizing principles, which he thought, had an existence, only, in the mind of those who developed them. In the same way, he abhorred Hilbert's structuralist approach in mathematics, which, he thought, was only concerned with the elegance of mathematical structures. Rather than developing on structural and universalizing ideas, Wilson preferred to develop conventional working hypotheses convenient for solving specific and concrete (theoretical or empirical) problems at hand. His approach, he felt, was more realistic and useful, as it supposedly remained constantly constrained by phenomena of the real world. In addition to this epistemological point of disagreement, around 1930, Wilson disliked Keynes's and Fisher's interventionist attitude regarding economic policy. See chapter I.

[12] At its beginnings, Wilson was involved with the econometric movement. With his leadership in Section K of the American Association for the Advancement of Science, he significantly contributed to

3.2.2. Paul Samuelson

Samuelson was born in 1915, in Gary, Indiana. His parents were both Jewish Polish immigrants; he grew up in the "nineteenth century Protestant culture of the rural Middle West."[13] During the First World War, the family prospered thanks to their owning a drug store. In the early 1920s, the family moved to Chicago, where Samuelson would later attend Hyde Park High School.

The family's drug store and the arithmetic problems, with which the family was confronted in the preparation of drugs and in economic price-cost projections, probably awoke, at a young age, Samuelson's interests in mathematics and economics. In high school, Samuelson attended extra-curricular mathematical courses,[14] in which he was trained in college mathematics (Shoesmith 1916). Hyde Park High School was located near the University of Chicago, where Samuelson went to college between 1932 and 1935. He majored in economics, while taking a significant number of mathematical college courses. He performed exceptionally well; his excellent results allowed him to obtain a newly created pre-doctoral scholarship given by the Social Science Research Council (SSRC), awarded only to eight of the most promising students in economics in the country in 1935.[15]

With his SSRC scholarship, Samuelson was able to freely choose a graduate program in economics, with all expenses covered. He went to Harvard. During the summer

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the establishment of the 1930s American community of mathematical and statistical economists who subsequently played an active role as econometricians. Rapidly, the project turned toward *High Econometrics* (Louçã 2007) under the lead of Ragnar Frisch's structuralist econometrics, which Wilson regarded as an expression of the European tendency towards pure abstraction and pure theories. Frisch's structuralist approach, for Wilson, did not correspond to Gibbs's truly scientific attitude. Frisch laid too much emphasis on probability theory, which, for Wilson, lacked empirical truth (E. Wilson to F. Mills, 30 Oct. 1935 [PEBW, 25]).

[13] Auto-biographical pieces (PASP, 149, Folder Unpublished Writings, Chapter 1. p.1).

[14] See Barnett and Samuelson 2004.

[15] Auto-biographical pieces (PASP, 149, Folder Unpublished Writings, Chapter 2).

before arriving in New England, he took a course in differential equations. The training that he had received at Chicago, as Samuelson recalled, made his Harvard classwork relatively easy and let him focus on his mathematical interests.[16] During the 1935-1936 academic year, Samuelson took, in particular, Leontief's Price Analysis course, and Wilson's course on Mathematical Statistics.

During the spring of 1936, Samuelson did well in Wilson's course, without obtaining the best mark of the group. Wilson regretted that Samuelson was too concerned with his qualifying examinations and did not concentrate on the course.[17] Samuelson impressed Wilson though. As he wrote to Henderson, when recommending Samuelson as a Junior Fellow of the Harvard Society of Fellows (HSF), 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."[18]

During the summer of 1936, Samuelson took a course on the theory of equations, where linear matrix equations were treated, at the University of Wisconsin (Backhouse 2015). During the following spring, Samuelson attended Wilson's course on mathematical economics. The course was difficult, but Samuelson was mathematically well-trained, and as he later recalled, Leontief's course also prepared him to digest Wilson's more advanced material (Samuelson 2004).

In 1937, Samuelson was elected Junior Fellow of the HSF. The membership came with a comfortable scholarship, and also with the restriction that he could not work

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[16] *Idem*.

[17] E. Wilson to J. Schumpeter, 6 June 1936 (PEBW, 27).

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

towards obtaining a higher degree. Presumably following this rule, during his doctoral years, 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,"[19] Wilson wrote Samuelson soon after the defense. Concerned about career opportunities for Samuelson, Wilson was then actively supporting Samuelson's thesis to be considered, as soon as possible, for the David A. Wells Prize,[20] which was awarded to Samuelson in 1942.[21]

3.3. 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

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

[20] 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 in 1940, despite the fact that his thesis was defended at the end of the year.

[21] See Backhouse 2015, 13.

formal aspects. In the mathematical appendix, Samuelson covered maximization, especially quadratic forms.[22]

As it will be discussed in the first point of this section, Wilson was key regarding Samuelson's introductory chapters; in them, Samuelson extensively used Wilson's Gibbsian ideas about mathematics, statistics and science in order to present the different and somehow disparate parts of the thesis as a comprehensive whole, which could supposedly serve as new foundations of economics. At the same time, the chapters that Samuelson included in his thesis corresponded well to the doctoral 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.[23]

Before discussing these three points, it must be emphasized that 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,[24] legacy of whom was transmitted to him by Wilson, who precisely defined mathematics as a sort of language. 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.

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

[23] Lawrence Henderson, a Harvard physiologist, had developed on the notion of systems in equilibrium for the study of the functioning of society (Russett 1966). Wilson and Henderson had worked hand-in-hand during the 1920s and the 1930s, aiming at introducing their scientific methods into the curriculum of Harvard students of social science and economics. When Samuelson met Wilson in the mid-thirties, system and operational thinking were intrinsically connected to Wilson's attitude towards mathematics, statistics and science. See chapter I.

[24] See Rukeyser 1941, 280.

First, regarding mathematics as a sort of language implied, for Wilson, defining mathematics as intrinsically connected with science.

For Wilson, mathematics consisted of establishing correspondences, as *translations*, between purely mathematical abstract entities, which represented certain mathematical 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 to 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/translation in science, mathematical structures were as beautiful and as useless as pure theoretical treatises of subject matters, Wilson felt. In this vein, he insisted that emphasis should be placed on meaning, provided by shared conventions of subject matters, believing that the "basis of rationality must go deeper than a mere set of marks and postulates" (Wilson 1904a, 81, footnote).

At the same time, for Wilson, mathematical necessarily implied immediate usefulness, which could be achieved only if *translations* 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.[25] This "operational or symbolic side," Wilson believed, required first learning "a series of rules of operation often both dull and unintelligible,"[26] generally found in algebra or

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[25] Illustratively, around 1900 when writing a textbook on *Vector Analysis* (Wilson 1901) based on his notes of Gibbs's courses, Wilson developed new algebraic operators that were needed in vector and matrix analysis (multiple algebra), in order to accomplish operations similar to those found in arithmetic or simple algebra. In particular, he developed *On Products in Additive Fields* (Wilson 1905).

[26] E. Wilson, unpublished and undated paper (PEBW, 4878.214, Folder miscellaneous papers, Chapter I. General Introduction, p.1).

advanced calculus, but which could be regarded as simply as the arithmetic operations of division and multiplication. These operations "have no obvious connection with the meaning of the numbers concerned; they are not in themselves of practical or intellectual interest."[27] Operational thinking, for Wilson, was distinct from postulational thinking. The former represented, in the practice of the mathematical scientist of applied mathematician, the (algebraic) way through which he should establish correspondences between postulates and conventional working hypotheses. This implied, for the mathematical scientist or applied mathematician, being familiar with certain mathematical structures, knowing how to play with his skills in (multiple) algebra or advanced calculus, while mastering the conventional working hypotheses of the subject matter of interest.[28]

Wilson's marked interest in axioms, as working hypotheses, reflected his belief that they represented something necessary for the use of mathematics in science, which could be regarded as invariant 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."[29] 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 another set 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

[27] E. Wilson, unpublished and undated paper (PEBW, 4878.214, Folder miscellaneous papers, Chapter I. General Introduction, p.2).

[28] E. Wilson, unpublished and undated paper (PEBW, 4878.214, Folder miscellaneous papers, Chapter I. General Introduction).

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

a definite way of determining the most likely working hypothesis regarding a specific set of data, as it could be used to quantify that range that carried truth and meaning while connecting working hypotheses and data (Wilson 1926b, 296).[30] In other words, in defining mathematics as a language, (mathematical) statistics or numerical mathematics, for Wilson, played the same operational role than (multiple) algebra or advanced calculus in establishing the *translations* between postulates and working hypotheses.

Second, for Wilson, defining mathematics as a language implied regarding mathematics as a vernacular, which all individuals could learn (Wilson 1940). The core of Wilson's definition of mathematics resided in the process of *translation* between postulates and working hypotheses. To determine such correspondences, the mathematician or the mathematical scientist, needed to know certain operational (algebraic and statistical) techniques. And, as Wilson stated:

> "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 1940a, 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.

[30] These ideas about conventionalism and indeterminism had led Wilson, when he turned his efforts to statistics during the 1920s, to propose a method dealing with statistical inference as probable and approximate. Wilson had then defined something that he might have called a range of meaningfulness, the construction of which vaguely referred to the notion of statistical likelihood and which implied the concept of the not-yet-well-established notion of confidence interval (Wilson 1927). Wilson treated probable inference as an operational way of quantitatively determining the most likely working hypothesis regarding a specific set of data (Wilson 1926b, 296). Statistical inference, for him, therefore consisted of determining the best approximate correspondence between theoretical and empirical entities.

3.3.1. Introductory chapters

3.3.1.1. Methodology

Samuelson started the thesis by criticizing how, in the economics, "bad methodological preconceptions" (Samuelson 1941a, 2) had left the field without sound scientific foundations. During his career, Wilson had systematically 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. [31] Samuelson precisely aimed at establishing a methodological balance between economic theory and data representing "empirical human behavior" (Samuelson 1941a, 2).

As a consequence of unsatisfactory methodological approaches, Samuelson believed, disagreement among economists about applied and theoretical concerns was the rule rather than the exception. Following Wilson, Samuelson believed that consensus was a necessary condition for any scientific practice. The purpose of the thesis was thus to achieve minimal consensus about the basic working hypotheses at the foundations of economics.

Also for the same methodological reasons, Samuelson held that economics lacked unity; its different branches remained unsatisfactorily connected. In order to offer a unifying approach in economics, it was necessary, Samuelson claimed, to build on the high level of generality provided by mathematical thinking. In his courses to economists, Wilson emphasized the greater level of generality that could be attained in economics when the mathematics was properly applied.[32]

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[31] See chapter I.

[32] See chapter II, in particular section 2.4.2., of the present thesis. In the introductory chapters of his thesis, Samuelson quoted Eliakim H. Moore's principle of generality by abstraction: "The existence of

In this spirit of generality, Samuelson suggested that he had begun by studying separately different subfields of economics and their related concept of equilibrium: he had ended by studying economics and its (general) aggregate equilibrium framework. He also claimed that in his research in various subfields, he had "found out" that certain *discrete* inequalities acted as a formal analogy connecting these different subfields. Wilson's ideas about (mathematical) generality shaped Samuelson's work in two ways. First, Samuelson's approach consisted of using (Wilson-Gibbs) matrix analysis to study the economy as an aggregate system, *individual units* of which could be regarded as being interconnected. At the same time that (matrix) generality enabled the study of the economy as a general system, interconnectivity between *individual units* implied that the different branches of economics were also interrelated and interdependent. Second, Samuelson aimed at developing formulas composed by finite differences that corresponded to formulas defined at the margin; according to Wilson, when it came to defining an equilibrium position, the discrete was more general than the continuous. Illustratively, the discrete inequalities that Samuelson established embodied, he argued, the necessary and sufficient conditions for obtaining definite mathematical results in economics, namely, the conditions for reaching a point of equilibrium. Such discrete inequalities corresponded to formulas already established in the standard treatment of individual's optimization problems with marginal calculus and of dynamical and stable aggregate economic systems with differential and functional calculus.

analogies between central features of various theories implies the existence of a general theory which underlines the particular theories and unifies them with respect to those central features" (Samuelson 1941a, 1). Because Samuelson must have felt that Moore's principle explained clearly what Wilson had taught him, he used Moore, rather than Wilson or Gibbs. Moore was then probably the most important figure in the recent history of American mathematics (Parshall 1984). Samuelson's use or Moore was also a rhetorical argument of authority.

3.3.1.2. Basic working hypotheses

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

In the thesis, 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. Samuelson argued that this first general working hypothesis, which ensured a correspondence between conditions of stable equilibrium in a system and an individual's optimizing behavior, did not assume normative statements about individual's behavior. It rather supposed a naturalistic assumption, reflecting, in idealized conditions, how individual elements adapted themselves to their natural and institutional environments. Such an approach consisted of defining equilibrium with respect to specifically demanded and/or supplied quantities that corresponded to an optimal individual's position. Such quantities, as for the optimizing individual was concerned, implied therefore simultaneously concepts of optimality and stability of equilibrium. A simple summation of all individuals' optimal quantities yielded in this way the corresponding quantity at the aggregate level, at a given moment in time.

In this vein, Samuelson did not make normative statements about individuals' behavior. He made, rather, normative statements about how economists should study, scientifically, consumer and firm theory: as "mathematics is a language," economists should be able to learn how to connect these theories with some sort of mathematical structures (of optimization) and with observable quantities, if only in idealized conditions.

Samuelson also argued that his system-framework was *useful* for the scientific practice of distinct branches of economics, which should be approached from the perspective of optimization under constraint and its correspondence with specific observable quantities. In the general system-framework Samuelson employed, the different branches of economics *could* be studied as being interconnected, as the variables of one problem could be regarded as the parameters of another. Such interconnections yielded relations of interdependence between variables, as stable equilibrium conditions implied that all variables were simultaneously determined; the only causal relationships, he claimed, resulted from changes of parameters. In this way, Samuelson dealt with optimizing economic behavior and its corresponding notion of stable equilibrium as a problem of comparative statics; discrete local equilibria could be meaningfully connected with discrete values of variables and parameters.

Not all meaningful economics, however, Samuelson underlined, emerged from studying stability conditions of equilibrium as corresponding to individual's optimizing behavior. A comprehensive analysis of stability, he believed, required analyzing dynamical considerations of aggregate systems as found in business cycles. Individual's optimality and stable equilibrium, Samuelson insisted, did not necessarily imply optimality and stability at the aggregate level.

In this direction, Samuelson argued that the second general working hypothesis of his thesis consisted of assuming that the aggregate system of the economy, namely the interaction over time of aggregate variables, was in dynamical stable equilibrium.[33] This second 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.

With his two general working hypotheses, Samuelson tied together the different chapters of the thesis using the notion of *system*; with it, he presented the microeconomic level, which dealt with individual's optimizing behavior at given moments in time, and the macroeconomic level, which was related to the interactions between aggregate macroeconomic variables through time, as being distinct problems which could be studied as interconnected, as they shared, he emphasized, a similar formal structure.[34] In both cases, certain inequalities were regarded as the necessary and sufficient conditions of achieving stable equilibrium positions, which Samuelson claimed, implied the existence of operationally meaningful theorems.

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[33] In the introductory chapter of the thesis, Samuelson suggested that he was following Ragnar Frisch's (1936) and Jan Tinbergen's (1935) recent work on economics dynamic systems. As will be discussed, he was also significantly influenced by Wilson's ideas about dynamical systems in physics.

[34] However, Samuelson did not clearly establish the *necessary and sufficient conditions* of the formal interconnection between the microeconomic and the macroeconomic levels.

3.3.1.3. Operationally meaningful theorems:

Samuelson's reference to operationally meaningful theorems was another important instantiation of his commitment to Wilson's ideas about mathematics *and* statistics.[35]

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. The represented a Wilsonian way of mathematically structuring economic thinking; of attributing meaning to mathematical structures connectedly with 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.

Emblematically of his thesis, Samuelson took as a conventional working hypothesis the notion of stable equilibrium as related to the microeconomic and macroeconomic levels. He then made them correspond to certain mathematical structures, which represented the "structural characteristics of the equilibrium set" (Samuelson 1941a, 15). Illustratively, in his consumer and firm theory, Samuelson *translated* the problem of the individual consumer and firm into a problem of constrained optimization, rendering thus equivalent the notions of individual's equilibrium and individual's optimality. In his dynamics, Samuelson *translated* the intertemporal interrelations between aggregate variables into a problem of functional analysis, making correspond the notion of aggregate steady state and possible scenarios (explosive of stable) of the future evolution of the economy.

Whereas the use of marginal calculus in optimization problems and of differential equations in business cycles analysis was already standard in Samuelson's time,

[35] Samuelson's reference to operationally meaningful knowledge was *also* another rhetorical argument of authority as it resonated well with Percy Bridgman's comprehensive philosophy of knowledge (Bridgman 1927). It remains however difficult to establish how Bridgman's ideas directly framed and limited Samuelson's early mathematical economics.

standard mathematical and statistical economics, Samuelson believed, remained as operationally meaningless as it did empirically empty. In this vein, Samuelson most of all sought to connect his working hypotheses, structured by postulational thinking, with some sort of data. The problem in economics, he believed, was that there was not yet enough available economic data, as detailed quantitative empirical information. In the thesis, the emphasis was placed on observable, not observed, data.

Whether or not economic data was missing, Samuelson seemed to have adopted Wilson's skepticism of Pearsonian and Fisherian statistical estimation procedures. In his course on mathematical economics, indeed, Wilson argued that his analysis was original relative to the general relevant literature, particularly Pareto's economics, as it was more general because it was made "with finite differences [rather] than [only] with derivatives."[36] Also, in his course on mathematical statistics, having in mind economic spectral analysis, Wilson taught the fundamental elements in calculus laying being lag operators, emphasized analytical statistics and numerical mathematics and used his *Advanced Calculus* (1911a) and Edmund T. Whitaker and G. Robinson's 1924 *The Calculus of Observations* as main references. Wilson did not cover standard inference theory, of which he was rather critical. Reflecting Wilson's ideas, Samuelson wrote:

> 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.

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

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. Wilson believed that extant statistical methods in the emerging (econometric) quantitative movement remained arbitrary.[37] In Samuelson's thesis, there were no standard statistical tests. 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, which represented what Samuelson meant by operationally meaningful theorems, did not imply the use of probability theory, of which Wilson was more than skeptical.

3.3.2. 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* (1911a) and to Whittaker's and Robinson's *The Calculus of Observations* (1924), both covered by Wilson in his 1936 course on Mathematical Etatistics. In these papers, Wilson's deep influence on the way Samuelson approached mathematical economics was not yet evident. Wilson's presentation of Gibbs's thermodynamical systems that "have profoundly altered

[37] See footnote 12.

[Samuelson's] views in corresponding fields of economics" (see footnote 4) 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. In this vein, 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's 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.[38]

3.3.2.1. 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 claimed to have established new foundations for consumer theory by establishing its empirical implications (1938a; 1938c; 1938d).[39] When Samuelson sent to Wilson the last of the three cited papers for suggestions, the latter responded explaining that he had actually refereed the work for publication in *Econometrica*. In general terms, Wilson believed,

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[38] Consequently, for Samuelson, the dynamics, as the evolution through time, 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 could explain why Samuelson did not introduce in the thesis his work on capital and investment theory.

[39] In 1937, Samuelson also attended Haberler's International Trade course. On Haberler's significant influence on Samuelson's consumer theory, particularly through his ideas about index numbers, see Backhouse, Forthcoming, Chapter 9: Making connections.

> "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."[40]

"Samuelson [had] followed almost all the leads [that Wilson had] gave"[41] him, and, further, had brought significant improvements to the analysis.

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 on "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 these invariants in science that Wilson regarded as necessary for the applicability of mathematics; determining its operational meaningfulness, required then properly connecting it with some mathematical structures and 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

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

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

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.[42] In a Wilsonian spirit, 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.[43]

In his course on Mathematical Economics, Wilson presented consumer theory analogically to thermodynamics by explaining that certain discrete inequalities, which he called the Gibbs conditions, characterized the static and stable equilibrium position of thermodynamics *and* economics systems.[44] 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. In his lectures, Wilson's consumer analysis was indeed time independent: "With time introduced, everyone recognizes that preferences change."[45]

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[42] He certainly also had in mind Harold Hotelling (1932; 1935).

[43] Samuelson in particularly showed that the auxiliary assumptions of independence of utility and of constancy of the marginal utility of income —that were needed to derive the standards negatively inclined demand curves from the cardinal utility analysis— were operationally meaningless, because their empirical implications could not be properly derived from idealized price and quantity behavior.

[44] See chapter II.

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

In this Wilsonian spirit, 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, Samuelson thought, 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 in different moments in time, but simultaneously. Samuelson focused on expenditure, evaluated with the following summation: $\sum_{i=1}^{n} p_i^1\, x_i^1$, for the first situation. 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, with the following summation $\sum_{i=1}^{n} p_i^1\, x_i^2$. From this little thought experiment (no real data involved) 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.[46] 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

[46] Samuelson also offered a similar thought experiment, which yielded similar results in terms of the price and quantity relationships. He supposed this time the individual to be constrained to move along the same indifference locus; this case enabled Samuelson to analyze the case of a compensated change of one price.

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).[47]

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. On this basis, he was then able to deduce a specific correspondence between such information/relation and demanded quantity behavior, expressed by the demand function. To accomplish this, and building on his theorem, Samuelson deduced the following relationships:

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

In the last formula, Samuelson argued, the operationally meaningfulness of his theorem could be understood, as it "contained almost all the meaningful empirical implications of the whole pure theory of consumer's choice" (Samuelson 1941a, 138–39). In this vein, from this formula, Samuelson was able to deduce known and *empirical* restrictions upon the demand functions. In particular, he was able to derive the "valid qualitative restrictions upon the slopes [and curvatures] of the demand functions" (Samuelson 1941a, 139).

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[47] In terms of preferences, Samuelson's consistency postulate implied that if $x^1 \prec x^2$ and $x^2 \prec x^1$ do not hold simultaneously, then $x^1 \prec x^2$ implied $x^2 \nprec x^1$.

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.[48] In this way, he connected his consistency postulate, grounded on observable data—not observed data—and the notion of equilibrium, with some structural characteristics of optimization under constraint.[49]

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.[50] In his words:

> Integrability conditions "reflect differential properties of our demand functions which are hard to visualize and hard to refute. For our empirical data consists of isolated points. These must be smoothed in some sense before our relations can be tested; the smoothing, even by the best known statistical methods, is to a degree arbitrary, and so refutation and verification are difficult.

[48] 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: $\sum_{i=1}^{n} \sum_{j=1}^{n} \left(\frac{\partial x_i}{\partial p_j} + \; x_j \frac{\partial x_i}{\partial I} \right) dp_i dp_j \; \leq 0.$

[49] 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.

[50] 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. In standard procedures of maximization of a utility function, the symmetry of the cross-price substitution effects, namely the symmetry of the substitution matrix or Hessian matrix, was assumed. On integrability, see Hands 2006.

> 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).

All in all, in consumer theory, Samuelson felt that he had developed something new, grounded on the old. Because he firmly believed that with his operationally meaningful theorem he had successfully *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 questions within his framework, precisely because he had not been able to establish such a continuous-discrete connection. In this process of *translation*, Samuelson did not take the relationship between prices and quantities as a structure existing in the market place; he took it as the conventional working hypothesis that prevailed in the Marshallian tradition. It was this working hypothesis that primarily needed to be connected with some sort of data and simultaneously with certain mathematical structures of optimization. Samuelson did not attempt to find invariant regularities by using statistical or probabilistic methods to estimate significance of parameters related to demand information. He used a thought experiment in which the emphasis was placed on discrete data concerning an idealized consistent individual's price and quantity behavior, at a given moment in time; he then interpreted this data as providing information about his individual's optimizing behavior (he must be minimizing expenditure) and derived a discrete inequality relationship, which not only was also inferred from this individual's preferences but also corresponded to the conditions of stable equilibrium that were found in standards procedures of optimization.

From our Wilsonian perspective, it can be argued that the novelty of Samuelson's consumer theory in respect to his contemporaries 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 empirically, theoretically and mathematically grounded, emphasizing more one aspect or the other, depending on the part of the thesis.[51] 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 empirically intuitive, theoretically grounded and mathematically elaborated.

3.3.2.2. 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.

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[51] In the most accomplished of his work on consumer theory, published in Econometrica, Samuelson had also offered a generalization of his approach into group demanded-quantity behavior, which corresponded to the aggregation, as a sum, of individual's demanded quantities (1938d). The aggregate demand function had the same characteristics of individual demand functions. Ironically, Samuelson's consistency approach was not fully consistent. Stanley Wong (1978) has underlined a major logical flaw in Samuelson's work in consumer theory. For the purpose of this chapter, it suffices to note that if in Samuelson's thought experiment, four different sets of prices and income and their respective batch of goods were considered, and if the respective costs were successively compared two by two, then, some inconsistencies would appear. Samuelson's consistency postulates does not explain why certain bundles that implied the same cost were not bought.

First, Wilson's criticism of wrong methodological approaches must not be very far from Samuelson's thinking when arguing, as an introduction of his forth chapter, that, in his words:

> 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,[52] two connected problems that had been kept separated in economics, he noticed. In his new unifying (not yet dual) approach, cost and production were part of the same technological relation, as embodied in the production function. In the study of optimal behavior of the firm, he explained, minimization of costs given a level of production 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 insisted, 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. Further, optimization, in the consistent behavior of the firm regarding its demand of inputs, Samuelson argued, was independent of the market structure of the firm. In this spirit, Samuelson solved first the constrained problem of minimization of cost determining optimal demand for inputs and then the unconstrained problem of maximization of profits establishing the optimal supply of outputs.[53]

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

[53] In this chapter, it is difficult to determine the references specifically related to theory of the firm on which Samuelson was developing his ideas. He mentioned the lectures of Jacob Viner, his professor in

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."[54] These problems raise new questions about stability when dealing with systems, equilibrium of which depended on "prescribed values of […] 'conjugate variables',"[55] or parameters. They led Samuelson to study thermodynamics,[56] where, he claimed, analogical problems were found, and which implied optimizing with a greater number of constraints. 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.[57] 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 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 his words, his

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Chicago, certain misconceptions of marginal analysis by Joan Robinson (1933), and Harold Hotelling's work (1932).

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

[55] *Idem*, p. 1.

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

[57] See chapter II.

> “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.”[58]

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 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.”[59]

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.”[60]

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

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

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

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:

> "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."[61]

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

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. He 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.

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).

3.3.3. 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.[62]

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.[63] 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 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" on dynamical systems (Weintraub 1991, 58). 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

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[62] 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 or less theoretical economics.

[63] See for example Hicks 1939.

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.[64]

Samuelson's dynamics were also informed by his personal research experience on business cycles[65] and population dynamics,[66] in which he reflected, to a certain

[64] 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's lectures on thermodynamics*. In the second volume, Paul Epstein commented on *Gibbs's Methods in Statistics*. Epstein's argument vaguely 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 a 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.

[65] Samuelson embarked on research in business cycles, only after having attended Alvin Hansen's Harvard seminar during the spring of 1938 (see Backhouse 2012). Particularly, Samuelson combined the multiplier and the acceleration approaches, reducing then the Keynes-Hansen macroeconomic system of equations to a second-order linear difference equation (Samuelson 1939a). The analysis came then to study the behavior of a polynomial and finding the roots of the characteristic equation of the system. Depending on the values of the roots, themselves defined by two parameters, the marginal propensity to consume (α) and the factor of proportionality (β), Samuelson was able to offer an analytical typology of the different dynamical possibilities of the cycle. National income would either approach a certain steady state asymptotically or by cyclical and decreasing oscillations; or, it would diverge from a certain steady state asymptotically or by cyclical increasing oscillations (for the last case, called the pump-priming case, see also Samuelson 1940). Rapidly, Samuelson associated the notion of stationary equilibrium with the notion of full employment (Samuelson 1939b). Convergence towards such stationary equilibrium required as necessary and sufficient formal conditions certain discrete inequalities, involving only the marginal propensity to consume and the factor of proportionality. The assumed values of such parameters determined therefore the evolution of the system. Samuelson summarized the four possibilities and the boundaries of the qualitative behavior of national income in a chart, axes of which were the values of α and β, and which simulated a possible graphical representation of Gibbs's phase systems (Samuelson 1939a, 78; 1939a, 792).

[66] At the same time that he worked on business cycle analysis, Samuelson embarked, in collaboration with Marion Crawford, his wife since 1938, on the study of population dynamics; they coauthored two unpublished papers. It is highly probable that Samuelson again consulted Wilson, a specialist on population; Samuelson and Crawford concluded, in one of their papers, that empirical formulas, such

extent, the spirit of Wilson-Gibbs mathematics. Illustratively, in business cycles, Samuelson presented his work as a way of mediating between what he regarded as being wrong methodological approaches when it came to understand the cycles, particularly the inter-relations between investment and consumption, as well as the role of government expenditure in the functioning and performing over time of the economy. Illustratively, he believed, “there would seem to be some ground for the fear that, this extremely simplified mechanism [—the multiplier analysis—] is in danger of hardening into a dogma, hindering progress and obscuring important subsidiary relations and processes” (Samuelson 1939a, 75). In business cycles, Samuelson aimed at establishing a certain consensus in the way interrelations between aggregate variables should be studied, by reconciling certain aspects of Ragnar Frisch’s econometric approach (1931; 1932a) and certain theoretical economic ideas of Maynard Keynes (1936), as well as connecting his Harvard professors’ economics, in particular Gottfried Haberler (1937) and Alvin Hansen (1938), with some aspects of Jan Tinbergen’s quantitative approach (1935).

More generally, in business cycles and population dynamics, 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; such was the case in the chapter on stability of aggregate systems of the thesis, wherein Samuelson aimed at deriving “meaningful theorems of observational significance such as could even ideally be empirically refuted under any conceivable circumstances” (Samuelson 1941a, 191).

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as the normal or the logistic curves, were of poor help when predicting future behavior of population (See Samuelson and Crawford “The structure of a population growing according to any prescribed law,” 1939, Ps archives), a conclusion that was coherent with Wilson’s claims on the question (Wilson 1925; Wilson and Puffer 1933).

Samuelson's emphasis, however, 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). With this in mind, Samuelson did not present business cycles and population dynamics in a topical way. Instead, he used some examples of business cycles as well as examples of aggregate supply-and-demand dynamical systems to illustrate his general ideas about stability.

Samuelson's approach was general; 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.[67]

In this general framework, Samuelson 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

[67] Given Wilson's skepticism of Frisch's structuralist econometrics, it must not be a coincidence that Samuelson called functionals 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, sctructuralist approaches illustrated a sort of universalizing scientific approach; from the same perspective, functionals embodied only a possible operational way to deal with complex systems.

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 some of which he did not yet have finite results for.

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."[68]

Wilson liked the thesis; it embodied his program for mathematical economics. Wilson had always wanted to show economists how his kind of mathematics could be of help in making economics more scientific. Notwithstanding this, Wilson believed that, in

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

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:

> "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."[69]

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.[70] *Foundations of Economic Analysis*, as he titled the extended version of his thesis, wasn't published until 1947, due to publishing delays.

3.4. *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 mainly 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

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

[70] "My Wells Prize dissertation has finally been handed in to the University Press" Samuelson wrote to Wilson on February 27, 1945 (*idem*).

transportation) undergraduate students as of 1943.[71] 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.[72] Already in July of 1943, he "was engaged in some part-time, technical war work," probably at MIT.[73] In view of this experience "in testing anti-aircraft,"[74] 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.[75]

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."[76] In the framing of some of these expansions, Wilson was still highly influential.[77]

On dynamics, Samuelson further developed the difficult problems in higher mathematics that he had encountered; these involved studying stability issues of

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

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

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

[74] P. Samuelson to K. Compton, 21 March1944 (PASP, 19).

[75] See Samuelson's correspondence with F. Loomis (PASP, 61, Folder Radiation Lab). For Samuelson's work at the Radiation Laboratory, see Backhouse and Maas 2016.

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

[77] Between 1940 and 1943, when Samuelson seemed to have finished the mathematical expansions for *Foundations*, 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 insistently 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, HUG4878.203, 9, Folder U-Z, HUA]), which Wiener obtained in 1926. Wilson believed that Wiener was the best young American mathematician of his time.

linear and non-linear systems. In this analysis, Samuelson called his correspondence between dynamics and comparative statics the Correspondence Principle. He introduced the catchy term in *Foundations*, already in the introductory chapters. As with Epstein,[78] Samuelson thought that the relation between old and new economics, as had been the case regarding the relation between classical and modern quantum mechanics, could be clarified: there were possible translations between the discrete and the continuous.[79]

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.[80] 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."[81]

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 instruction of mathematical and statistical economics by sending him some of his

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[78] See footnote 53.

[79] See Samuelson 1942.

[80] See for example Samuelson 1941; 1942b; 1942a; 1943c; 1943a.

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

MIT studentsand letting them write papers (for final examination) "in a cooperative fashion,"[82] in which Samuelson and Wilson would agree on the subject covered. [83]

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 Proceedings of the National Academy of Science (PNAS), which Wilson still edited, prevented him from sponsoring particular papers,[84] he made "an exception to the general rule and [took] them under [his] own sponsorship."[85] The papers appeared in the December 1943 volume of the PNAS.[86] Samuelson was happy about their publication: he "could make reference to them in connection with other work on the fire,"[87] related probably to his war work on ballistics and/or to his appendix on difference equations.

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

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

[83] 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*]).

[84] E. Wilson to P. Samuelson, 27 July 1943 (*idem*).

[85] E. Wilson to P. Samuelson, 2 Nov. 1943 (*idem*).

[86] See Samuelson 1943; 1943c.

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

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.

3.5. Conclusion

As suggested by Wilson and Samuelson in the opening quotations of this paper, Samuelson's doctoral projects, thesis and *Foundations* reflected his active commitment to Wilson as regards mathematics, statistics and science.

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 then offered intermediating solutions, balancing and reconciling different approaches with the explicit purpose of establishing consensus on the sound way of studying the theoretical and/or empirical problem at hand. In this effort, 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 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.

3.6. References

Backhouse, Roger. Forthcoming. *Maker of Modern Economics: An Intellectual Biography of Paul A. Samuelson*. Vol. 1: Becoming Samuelson. 2 vols.

———.. 2012. “Entering Hansen’s New World: Paul Samuelson, the Young Keynesian, 1938-47.” *Unpublished Draft*.

———.. 2014. “Paul A. Samuelson’s Move to MIT.” *History of Political Economy* 46 (suppl 1): 60–77.

———.. 2015. “Revisiting Samuelson’s Foundations of Economic Analysis.” *Journal of Economic Literature* 53 (2): 326–50.

Backhouse, Roger, and Harro Maas. 2016. “Taming Maxwell’s Demon: The Bowman Committee and the Planning of Science for the Public Good.” *Unpublished Draft*.

Barnett, William Arnold, and Paul A. Samuelson. 2004. “An Interview with Paul A. Samuelson.” In *Inside the Economists’ Mind*. United States of America: Cambridge University Press.

Bridgman, Percy. 1927. *The Logic of Modern Physics*. New York: Macmillan.

Corry, Leo. 2004a. *David Hilbert and the Axiomatization of Physics (1898-1918) : From Grundlagen Der Geometrie to Grundlagen Der Physik*. Dordrecht: Kluwer Academic Publishers.

———. 2004b. *Modern Algebra and the Rise of Mathematical Structures*. 2nd ed. Basel, Switzerland: Birkhäuser Verlag.

Epstein, Paul. 1936. "Gibbs' Methods in Quantum Statistics." In *Commentary on the Scientific Writings of J. Willard Gibbs*, edited by Frederick Donnan and Arthur Haas, 2:521–84. New Haven: Yale University Press.

———. 1937. *Textbook of Thermodynamics*. New York: J. Wiley & sons, inc.

Fenster, Della Dumbaugh, and Karen Parshall. 1994. "A Profile of the American Mathematical Research Community: 1891-1906." In *The History of Modern Mathematics: Images, Ideas, and Communities*, edited by Eberhard Knobloch and David Rowe, 3:179–227. Boston: Academic Press.

Fisher, Irving. 1927. "A Statistical Method for Measuring 'Marginal Utility' and Testing the Justice of a Progressive Income Tax." In *Economic Essays in Honor of John Bates Clark*, edited by Jacob Hollander, 157–93. New York: The Macmillan Company.

Frisch, Ragnar. 1931. "The Interrelation Between Capital Production and Consumer-Taking." *Journal of Political Economy* 39 (5): 646–54.

———. 1932a. "Capital Production and Consumer-Taking: A Rejoinder." *Journal of Political Economy* 40 (2): 253–55.

———. 1932b. *New Methods of Measuring Marginal Utility,*. Tübingen: Mohr.

———. 1936. "On the Notion of Equilibrium and Disequilibrium." *The Review of Economic Studies* 3 (2): 100–105.

Haberler, Gottfried. 1937. *Prosperity and Depression: A Theoretical Analysis of Cyclical Movements*. Geneva: League of Nations.

Hands, D. Wade. 2006. "Integrability, Rationalizability, and Path-Dependency in the History of Demand Theory." *History of Political Economy* 38 (Suppl 1): 153–85.

Hansen, Alvin Harvey. 1938. *Full Recovery Or Stagnation?* New York: W. W. Norton.

Hicks, J. R. 1939. *Value and Capital: An Inquiry into Some Fundamental Principles of Economic Theory*. 2nd ed. Oxford University Press, USA.

Hotelling, Harold. 1932. "Edgeworth's Taxation Paradox and the Nature of Demand and Supply Functions." *Journal of Political Economy* 40 (5): 577–616.

———. 1935. "Demand Functions with Limited Budgets." *Econometrica* 3 (1): 66–78.

Hunsaker, Jerome, and Saunders Mac Lane. 1973. "Edwin Bidwell Wilson, 1879-1964, A Biographical Memoir." *National Academy of Sciences (US)*, 283–320.

Keynes, John Maynard. 1936. *The General Theory Of Employment, Interest, And Money*. London: MacMillan & Co.

Lange, Oskar. 1934. "The Determinateness of the Utility Function." *The Review of Economic Studies* 1 (3): 218–25.

Louçã, Francisco. 2007. *The Years of High Econometrics: A Short History of the Generation That Reinvented Economics*. London and New York: Routledge.

Maas, Harro. 2014. "Making Things Technical: Samuelson at MIT." *History of Political Economy* 46 (suppl 1): 272–94.

Parshall, Karen. 1984. "Eliakim Hastings Moore and the Founding of a Mathematical Community in America, 1892–1902." *Annals of Science* 41 (4): 313–33.

Parshall, Karen, and David Rowe. 1994. *The Emergence of the American Mathematical Research Community, 1876-1900: J.J. Sylvester, Felix Klein, and E.H. Moore*. United States of America: American Mathematical Society.

Robinson, Joan. 1933. *The Economics of Imperfect Competition*. London: MacMillan & Co.

Rukeyser, Muriel. 1941. *Willard Gibbs*. New York: Dutton.

Russett, Cynthia Eagle. 1966. *The Concept of Equilibrium in American Social Thought*. New Haven, Cann.: Yale University Press.

Samuelson, Paul. 1937a. “A Note on Measurement of Utility.” *The Review of Economic Studies* 4 (2): 155–61.

———.. 1937b. “Some Aspects of the Pure Theory of Capital.” *The Quarterly Journal of Economics* 51 (3): 469–96.

———.. 1938a. “A Note on the Pure Theory of Consumer’s Behaviour.” *Economica* 5 (17): 61–71.

———.. 1938b. “Welfare Economics and International Trade.” *The American Economic Review* 28 (2): 261–66.

———. 1938c. “A Note on the Pure Theory of Consumer’s Behaviour: An Addendum.” *Economica* 5 (19): 353–54.

———. 1938d. “The Empirical Implications of Utility Analysis.” *Econometrica* 6 (4): 344–56.

———. 1938e. “The Numerical Representation of Ordered Classifications and the Concept of Utility.” *The Review of Economic Studies* 6 (1): 65–70.

———. 1939a. “Interactions between the Multiplier Analysis and the Principle of Acceleration.” *The Review of Economics and Statistics* 21 (2): 75–78.

———. 1939b. “The Gains from International Trade.” *The Canadian Journal of Economics and Political Science / Revue Canadienne d’Economique et de Science Politique* 5 (2): 195–205.

———. 1939c. "A Synthesis of the Principle of Acceleration and the Multiplier." *Journal of Political Economy* 47 (6): 786–97.

———. 1940. "The Theory of Pump-Priming Reëxamined." *The American Economic Review* 30 (3): 492–506.

———. 1941a. "Foundations of Analytical Economics: The Observational Significance of Economic Theory." Cambridge Mass.: Harvard University.

———. 1941b. "Conditions That the Roots of a Polynomial Be Less Than Unity in Absolute Value." *The Annals of Mathematical Statistics* 12 (3): 360–64.

———. 1942a. "A Note on Alternative Regressions." *Econometrica* 10 (1): 80–83.

———. 1942b. "The Stability of Equilibrium: Linear and Nonlinear Systems." *Econometrica* 10 (1): 1–25.

———. 1942c. "A Method of Determining Explicitly the Coefficients of the Characteristic Equation." *The Annals of Mathematical Statistics* 13 (4): 424–29.

———. 1943a. "Fitting General Gram-Charlier Series." *The Annals of Mathematical Statistics* 14 (2): 179–87.

———. 1943b. "A Simple Method of Interpolation." *Proceedings of the National Academy of Sciences of the United States of America* 29 (11): 397–401.

———. 1943c. "Efficient Computation of the Latent Vectors of a Matrix." *Proceedings of the National Academy of Sciences of the United States of America* 29 (11): 393–97.

———. 1947. *Foundations of Economic Analysis*. Cambridge, Mass. Harvard University Press.

———. 1998. "How Foundations Came to Be." *Journal of Economic Literature* 36 (3): 1375–86.

———. 2004. “A Portrait of the Master as a Young Man.” In *Wassily Leontief and Input-Output Economics*, edited by Erik Dietzenbacher and Lahr. Michael, 3–8. Cambridge, UK ; New York: Cambridge University Press.

Schultz, Henry. 1938. *The Theory and Measurement of Demand*. Chicago: The University of Chicago Press.

Shoesmith, Beulah. 1916. “Mathematics Clubs in Secondary Schools.” *School Science and Mathematics* 16 (2): 106–113.

Tinbergen, Jan. 1935. “Annual Survey: Suggestions on Quantitative Business Cycle Theory.” *Econometrica* 3 (3): 241–308.

Weintraub, Roy. 1991. *Stabilizing Dynamics Constructing Economic Knowledge*. Cambridge, England: Cambridge University Press.

Whittaker, E. T., and G. Robinson. 1924. *The Calculus Of Observations: A Treatise On Numerical Mathematics*. London: Blackie And Son Limited.

Wilson, Edwin. 1901. *Vector Analysis: A Text-Book for the Use of Students of Mathematics and Physics*. United States of America: Yale University Press.

———. 1903. “The So-Called Foundations of Geometry.” *Archiv Der Mathematik Und Physik* 6: 104–22.

———. 1904. “The Foundations of Mathematics.” *Bulletin of the American Mathematical Society* 11 (2): 74–93.

———. 1905. “On Products in Additive Fields.” In *Verhandlungen Des Dritten Internationalen Mathmaticker-Kongresses in Heidelberg*, 202–16. Leipniz.

———. 1911. *Advanced Calculus*. Boston, New York: Ginn and company.

———. 1920. “Space, Time, and Gravitation.” *The Scientific Monthly* 10 (3): 217–35.

———. 1925. “The Logistic or Autocatalytic Grid.” *Proceedings of the National Academy of Sciences of the United States of America* 11 (8): 451–56.

———. 1926. “Statistical Inference.” *Science* 63 (1629): 289–96.

———. 1927. “Probable Inference, the Law of Succession, and Statistical Inference.” *Journal of the American Statistical Association* 22 (158): 209–12.

———. 1940. “Methodology in the Natural and the Social Sciences.” *American Journal of Sociology* 45 (5): 655–68.

Wilson, Edwin, and Ruth Puffer. 1933. “Least Squares and Laws of Population Growth.” *Proceedings of the National Academy of Sciences of the United States of America* 68 (9): 285–382.

Wong, Stanley. 1978. *Foundations of Paul Samuelson’s Revealed Preference Theory: A Study by the Method of Rational Reconstruction*. London: Routledge & Kegan Paul.

CONCLUSION

In this thesis, we focused our attention on Edwin B. Wilson and Paul Samuelson, and traced an intellectual genealogy linking the former, an American polymath who was committed to Gibbs's kind of constrained and intermediate mathematics, with the latter, an American mathematical economist who acknowledged being Wilson's disciple and whose early work was deeply influenced by Wilson's Gibbsian attitude towards mathematics, statistics and science.

The thesis went from the general to the specific. It started with a comprehensive analysis of Wilson's work and career in America between 1900 and the 1940s; the scope was then narrowed and emphasis was strictly laid on Wilson's direct role in the history of American economics between the 1920s and the 1930s. Finally the focus was put on Wilson's indirect role in the history of economics through his influence on Samuelson's early mathematical and statistical thinking, which eventually shaped Samuelson's thesis and *Foundations of Economic Analysis*.

In the first chapter, Wilson's career and work were presented as illustrating a process of scientific migration from mathematics towards other fields. Through such a process, between 1900 and the 1940s, Wilson worked to connect mathematics with various subject matters, including mathematical physics, statistics, economics and social science. In such efforts, Wilson was inspired by his unique and original Gibbsian ideas about the foundations of mathematics and science, which he developed as he rejected the Hilbertian structuralist kind of mathematics; in his foundational discussions, Wilson defined (mathematical) rationality and used his definition as the only valid invariant, in a strict sense, in science. It was invariant because mathematics was like a language: everybody could learn its techniques and reason following its precepts. Such foundational ideas embodied essential inter-relations between mathematics and subject matters; they also embodied essential connections between science and society. As Wilson regarded intuition and judgment as fundamental elements of his foundational ideas, he let his personal (national, political, social and cultural) biases enter into his epistemology. In this way, historical and institutional contexts framing his daily activities did not only provide a general framework for his professional career; they acted as epistemic factors conferring meaning or lack of it to scientific statements. At the same time, as he defined his epistemology connectedly with pedagogy, Wilson determinedly sought to reform high education of the various subject matters with which he engaged. His aim was to instruct mathematicians and (natural and social) scientists into Gibbs-Wilson's attitude towards mathematics and science in order to frame their thought and discipline their practices as scientists in such a way that they would adopt the Wilsonian constrained and *inter-mediate*, "really scientific," approach in their professional practices.

In the second chapter**,** emphasis was placed on the specific aspect of Wilson's professional career that greatly impacted economics, as his influence on the rise of mathematical economics in America between the 1920s and 1930s was studied. His influence was effective at the organizational and educational fronts; he was central in establishing an organized community of American mathematical economists within the well-recognized scientific community of the American Association for the Advancement of Science. In the community that Wilson helped to establish, scientists and mathematicians interested in social science and economics *inter-acted* with social scientists and economists working with mathematical and statistical techniques. That organized community, which included Wilson as an active member, subsequently played a central role in the origins of the Econometric Society. Concerned with the increasing European structuralist approach, particularly that of Ragnar Frisch, on the econometric movement, as well as with the political platform of the Democratic Party in power during the 1930s, Wilson turned his attention to reform the curriculum of economists (and social scientists) at Harvard. In this way, he succeeded in establishing the first program in advanced mathematical economics at the department of economics at Harvard. His program, largely inspired by Pareto's mathematical economics, included a course on advanced mathematical economics *and* another course on mathematical statistics. In these courses, Wilson taught Gibbs's thermodynamics systems and used the notion of equilibrium of such systems to analogically present the consumer equilibrium. He also taught numerical mathematics and analytical statistics, which he thought was first necessary for developing a taxonomy of the economy when being studied as a system in stable equilibrium. The main message of Wilson's courses emphasized that a sound scientific attitude in (mathematical) economics required connecting economics with data, if only in idealized conditions.

Finally, in the third and last chapter, emphasis was placed on Wilson's catalytic influence on Samuelson's thesis and *Foundations of Economic Analysis*. By reconstructing such influence, the chapter described the ideas that Samuelson must have had in mind when claiming that mathematics was a language and when arguing that he offered operationally meaningful theorems of observational and empirical usefulness: he used mathematical techniques to establish correspondences between the notion of equilibrium, which he took as a convention in economics at the static individual and dynamical aggregate levels, and the mathematical structural characteristics of optimization problems under constraint and of functional analysis. At the same time, he sought to give this sort of mathematics of the continuous empirical and observational meaningfulness by connecting his work with some sort of data. As he seems to have adopted Wilson's skepticism of classical statistics and probability, Samuelson did not use statistical tests or regressions; he rather attempted to *translate* the continuous into the discrete. Wilson himself defined the stable equilibrium position in consumer theory as a discrete time-independent inequality. Samuelson explicitly aimed at establishing consensus among economists regarding the notion of stable equilibrium; conventions were a necessary epistemic Wilsonian condition for science. Using mathematics as a language in a Wilsonian spirit, Samuelson presented the notion of equilibrium as intuitive, treated it as a conventional theoretical idea and gave it certain mathematical structures. In this way, he developed a (taxonomical) framework to study the economy as a system, in which the microeconomic and the macroeconomic levels could be analyzed separately while being understood, intuitively, as interconnected. However, he did not explicitly explain how these levels were *formally* interconnected. Finally, from this Wilsonian perspective, *Foundations* can be regarded not only as a mathematical exercise, but also as a statistical proposition tentatively providing an alternative to the contemporary econometric movement, from which Wilson and Samuelson tried to keep a distance.

Limits of this research

The adopted approach in this doctoral research consisted of exploring the historical development of economics and other disciplines in America from the perspective of Wilson's career and work, and from the perspective of Samuelson's commitment to those ideas, or at least from our understanding of Wilson's and Samuelson's ideas.

A narrative approach in the history of science, which consists of following and reconstructing particular scientists' professional lives, has the advantage of providing the specific and local circumstances and influences that framed and limited the thinking of protagonists. Incidentally, intellectual biography as a historiographical category in the history of science has an essential limit in that it does not provide comprehensive accounts about general movements, which altogether framed particular transformations of scientific disciplines and practices, as well as participated, with conflict or not, in negotiations leading to the establishment of general discourses and practices.[1]

In this vein, the scope of this thesis is limited by its own approach. The first chapter can only purport to be an account about Wilson's particular influence in the development of scientific interdisciplinarity in America and his definition of mathematical rationality, not of *the* development of interdisciplinarity in America and of rationality at large. Likewise, the second chapter can only pretend to offer a new perspective, Wilson's perspective, about the rise of mathematical economics in America; it cannot intend to be an account about *the* mathematization of economics. Similarly, the third chapter can only pretend to offer new insight of Samuelson's *Foundations of Economic Analysis*, by reconstructing the Wilsonian framework within which Samuelson was working when writing his thesis and the other chapters that he subsequently added and which composed the body of the book. This last chapter cannot be regarded as a general account tracing *all* the influences that framed and limited Samuelson's early thought.

This thesis is not an account of how American sciences, particularly economics, became mathematized. This thesis is an account of how Wilson, a professional mathematician disenchanted with his profession, which he thought was misleadingly following the Hilbertian structuralist kind of mathematics, decided to leave his initial research community and approach other American scientific communities, participating and contributing hence, in various ways, to the process of mathematization of some American scientific fields, particularly economics. Samuelson's commitment to Wilson's mathematics illustrated such influence.

[1] See Hacohen 2007.

Open doors

In his *How Economics Became a Mathematical Science* (2002), Roy Weintraub uses a historiographical category found in the literature of the history of modern mathematics. Such a category consists of distinguishing, as the historian of mathematics Leo Corry suggests (Corry 2004b), between the body and the image of knowledge. In mathematics, the body of mathematics reflects on questions directly related to the discipline of mathematics, namely to the professional practices of mathematicians trained in modern departments of mathematics; it is connected to the language *of* mathematics, namely to the rules that objectively determine the truth or falsity of mathematical statements in modern mathematics. The image of mathematics, instead, reflects on meta-mathematical questions, involving normative and methodological ideas about the body of mathematics, which are contingent to cultural, institutional and historical factors. The image of mathematics is therefore connected to the language *about* mathematics.

Within the body-image framework, professional mathematical practitioners are regarded as being concerned with the body of mathematics only; it is also assumed that their image of mathematics does not have any epistemic role, as it does not provide any kind of element that enters in the set of rules determining truth or falsity of mathematical statements. The image of mathematics, however, as embedded in cultural context, is thought of as framing and limiting the practices of mathematicians, individually or in community, as well as influencing their choices regarding the approach to be adopted, the problems to be solved, the curriculum to be established and the kind of connections with other scientific disciplines and with society that should be developed. In the body-image framework, professional historians of mathematics endeavor to reconstruct the image of mathematics of mathematicians in the recent past, individually or in community, not the body of mathematics.

Whereas the body-image framework has been useful in the history of economics in establishing the present understanding of a large variety of significant historical interactions between economics and mathematics, it is of little help for the reconstruction of the interwoven histories of mathematics, statistics and economics as regarded from the perspective of the Wilson-Samuelson connection. Any account in the history of economics underlining Samuelson's influence in the process of *mathematization* of economics should therefore be taken with skepticism, if it is made within the body-image framework.

Wilson's professional life was the story of a mathematician who left the American community of modern mathematicians. Throughout his whole life, however, he kept struggling with the right definition of mathematics within different research and academic American communities; the sites where he engaged in negotiations with modern mathematicians were scientific academies and associations, university departments, clubs and groups of intellectual discussion, college and university courses as well as the *Proceedings of the National Academy of Science*, which he edited for fifty years. All of these sites were located outside departments and institutions of mathematics. Institutionally and socially, Wilson's work and career developed outside the circles of professional researchers in mathematics. At the same time, his epistemology contrasted with the epistemology of his mathematician colleagues. In Wilson's foundational thinking, the body and the image of mathematics were interconnected, not separated; for him, intuition, belief, visualization and personal judgment were as important as rigor and logic when establishing meaningfulness (truth or falsity) of mathematical *and* scientific statements. Consequently, in the historical reconstruction of his legacy (in economics), emphasis has to be laid on the interconnections between the body and the image of his mathematics, or in other words, between his language *of* and *about* mathematics.

Wilson's struggles with modern mathematicians for the right definition of mathematics and science led him to adopt an *intermediary* role between mathematics and various subject matters, and to define mathematics as *intermediate* and constrained. His significant influence in the rise of mathematical economics during the 1920s and the 1930s should be therefore understood from that perspective. That being the case, even though Wilson's mathematical efforts and ideas around 1930 were (institutionally, socially and epistemologically) disconnected from, indeed in contrast to, what was going on in American modern departments of mathematics, his impact in shaping American modern economics was of a mathematical nature: it embodied his eternal struggle for the right definition of mathematics and its interconnections with science, as well as the establishment of the right practices of future mathematicians and scientists, of the right scientific discipline that they should adopt.

Eventually, our thesis opens three new perspectives for the historical study of the mathematical turn of economics in America as related to the Wilson-Samuelson connection. These perspectives involve theoretical, methodological and historiographical discussions.

First, Samuelson's work in consumer theory as well as cost and production theory have been interpreted as a *direct* response to what has been called the 1930s Harold Hotelling and Henry Schultz impasse (Hands and Mirowski 1998; Mirowski and Hands 1998). In these accounts, Hotelling and Schultz are convincingly shown to follow a physics metaphor. In their demand theory, they arguably reflect their belief in the existence of a structural relationship between prices and quantities in the market place that could be translated into mathematical and statistical language: "Both passionately believed that science was transparently mathematical, that it was firmly based upon the collection and statistical processing of empirical data, and that physics was the embodiment of the success of the scientific method" (Hands and

Mirowski 1998, 345). However, it is explained, the physics metaphor failed when Hotelling and Schultz sought unsuccessfully to statistically test their demand theory. Samuelson's consistency postulate, which introduced in the forefront of the analysis an empirical emphasis that was not subject to statistical tests for validity, is then presented as providing, with Samuelson consciously and purposely working in this direction, a new (but only temporary) lease to Hotelling's and Schultz's (neoclassical) program and their physics metaphor.

Given Wilson's ideas about the interconnections between the *image* and the *body* of mathematics, namely his emphasis on the connection between mathematical structures, *conventional* working hypotheses—which were never to be taken as structures existing in nature—and some sort of data, and further given Samuelson's commitment to these ideas, the Hotelling-Schultz-Samuelson connection appears to be less evident than Wade Hands and Philip Mirowki claim. More precisely, their argument about the significant relevance of Hotelling's and Schulz's work for the way Samuelson framed certain theoretical problems at the individual level is enlightening. The questioning that our research brings to the foreground relates to their interpretation of Samuelson's early work as mainly, if not merely, an extension of Hotelling's and Schultz' (neoclassical) research program, implying suggestively at the same time Samuelson's adoption of Hotelling's and Schultz's epistemic positions.

Physics and some of its mathematical structures were undeniably a significant source of inspiration for all of them. However, these structures as well as other elements, such as conventions found in subject matters, seem to have played very different roles in the creative practices of each one of these individuals. While structures (mathematical or economic) were not as relevant for (the young) Samuelson as for Hotelling and Schultz, conventions in economics were central for (the young) Samuelson, probably less for Hotelling and Schultz.

From our Wilsonian perspective, the Hotelling-Schultz-Samuelson connection appears at least ambiguous, as was Wilson's relationship with Hotelling. In the early 1930s, Wilson had promoted Hotelling's (and Schultz's) career; but Hotelling's subsequent work in mathematical economics and in (Fisherian) statistics was precisely the kind of mathematics and statistics that Wilson despised. For Wilson, mathematical economics did not consist of copying and pasting formulas found in pure mathematics and physics into economics, as he probably felt Hotelling did. Applicability of these formulas regarding shared conventions and data in the subject matter was his watchword. In his courses to economists, he never mentioned Hotelling's work. In this vein, this time regarding statistics, Wilson believed, in his words:

> "the recent developments in the field of probabilities due to Fisher, Pearson, Hotelling and others were meritorious. What none of these persons understands is that probabilities and statistics are different things. It is perfectly easy to prove either with the theory of inverse probabilities which I have never liked or with the theory of likelihood which I prefer that provided (1) the theory of likelihood is to be regarded as a sound fundamental axioms and (2) that the universe from which the sample is selected is normal, then (3) certain consequences follow. This is a mathematical theorem. It can't be wrong. But in practice when one comes to apply it one may seriously question each of the two axioms at the basis of the conclusion. But there is a still further difficulty which is at the heart of the difference between statistics and probabilities and the corresponding difference between pure mathematics and science. The sample we have generally comes from a universe which even if essentially normal is unknown and any particular investigator selecting a sample out of that universe even with the greatest intelligence, and many exercise none, will probably get a sample which is biased. In fact, even if he gets a great many samples they may all be somewhat biased because he hasn't all the variables or rather the parameters which define the

> universe under control indeed generally doesn't know what these parameters are."[2]

Wilson's and Hotelling's ideas about mathematics and statistics seemed to differ significantly. It is therefore reasonable to expect that Samuelson's and Hotelling's mathematical economics were not mainly or merely *singular*. With all these elements in mind, and particularly taking into account Samuelson's Wilsonian understanding of operationally meaningful knowledge, Samuelson's connection to Hotelling and Schultz is left as an open door for future research. This would certainly require a full and comprehensive analysis involving, among other things, an analytical and technical study of Samuelson's work, and a comparison of his work that he felt was operationally meaningful to the other that he felt was not.

Second, Samuelson's famous "operationally meaningful theorems," which he presented as being of observational and empirical significance as they could be tested in idealized conditions, have often been studied by historians, philosophers, and methodologists of economics within the *ex-post* framework of rational reconstructions. In these accounts, Samuelson's ideas about operational and meaningful knowledge are analyzed in reference to comprehensive philosophies such as operationalism, logical positivism, empiricism, falsificationism, descriptivism or behaviorism, or in reference to the methodological positions of other economists who themselves explicitly adopted some of these philosophical positions (Hands 2001; Mongin 2000; Cohen 1995; Hausman 1992; Caldwell 1982; Blaug 1980; Wong 1973, 1978). In these accounts, Samuelson's ideas about operational, meaningful, observational and testable knowledge appear philosophically naïve and unintelligible because they do not properly fit with these rationalizing references points. Often, if not systematically, the works that are used in order to reconstruct Samuelson's early

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[2] E. Wilson to C. Roos, Sep. 16, 1936 [PEBW, 27].

methodological ideas are comments that Samuelson wrote during the 1960s (Archibald, Simon, and Samuelson 1963; Samuelson 1964; Samuelson 1965).

Those comments were related to a controversy over the realism of hypotheses in economics;[3] in them, Samuelson invited us to look backwards to his thesis and *Foundations* and to interpret them as consistently related to some of the mentioned comprehensive philosophies. These comments are surely enlightening for our understanding of Samuelson's (naïve) philosophical pretentions in the 1960s, and probably for contextualizing in methodological terms his work in economics in the 1960s and thereafter. They are, however, misleading regarding his earlier work: there is no well-documented evidence that around 1940 Samuelson seriously engaged with any of these comprehensive philosophies.

Around 1940, instead, Samuelson was committed to Wilson's ideas about mathematics and science. In their turn, Wilson's foundational ideas could be qualified as philosophically naïve; they do not seem to properly fit with any of the mentioned comprehensive philosophies of knowledge serving as rationalizing references. Wilson had his own original—indeed peculiar, but nonetheless deep, sophisticated and complex—set of ideas about the foundations of mathematics and science. In return, his commitment to Wilson's thinking around 1940 placed Samuelson in a peculiar position regarding his epistemic ideas.

How Samuelson's epistemology and practices as a mathematical economist changed, or not, and namely how the interaction between his *image* of and the *body* of his mathematical economics evolved between the 1940s and the 1960s, is a question that remains open for future inquiry.

A possible way of exploring this evolution is connected to the third door that our thesis left open. We have insisted that Samuelson fully committed to Wilson's

[3] See also Friedman 1953; Nagel 1961; 1963; Machlup 1964; Garb 1965; Massey 1965; Lerner 1965.

American attitude toward mathematics, which contrasted with Hilbert's attitude towards mathematics. Incidentally, John von Neumann, one of the worldwide leading professional mathematicians who coauthored with the economist Oskar Morgenstern the *Theory of Games and Economic Behavior* (1944) and who contributed in significant ways to connect mathematics and economics, worked in a Hilbertian spirit.

While it cannot be claimed that Samuelson's professional career mainly developed as he rejected von Neumann's mathematical economics, it can be observed that his mathematical economics significantly contrasted with von Neumann's. Samuelson and von Neumann met once in March 1942,[4] when the latter was presenting his work at the department of economics at Harvard. At that occasion, the young mathematical economist and the well-established mathematician engaged in a disagreement about the right kind of mathematics to be used as the foundations of modern economics.

Such disagreement embodied Wilson's lifelong conflicted relationship with Hilbert's mathematics, which, through the encounter between Samuelson and von Neumann in a conference room at Harvard, started gaining relevance in economics. More significantly, almost simultaneously, Samuelson's *Foundations*, finished in 1945, and von Neumann's *Theory of Games*, published in 1944, presented contrasting foundations for economics, and each in its own way became hugely influential in the subsequent evolution of the field. Each book attached central importance to the fact that it was using a kind of mathematics that was both different and new in social science. The first was written under the influence of Wilson, the second was written in the spirit of the Hilbertian structuralist mathematics.

In the last analysis, the present research opens the door to a new historiographical category in the history of economics. The field still lacks a detailed study of the

[4] J. von Neumann to G. Haberler, 31 Oct. 1931, Library of Congress.

mathematical turn of economics as regarded from the point of view of a conflict between different and contrasting mathematical attitudes, as represented by the Wilson-Samuelson and Hilbert-von Neumann traditions. Both Samuelson and von Neumann participated in significant ways in establishing a particular discourse and specific practices, seemingly harmonized, that importantly contributed to shaping the mathematical transformation of economics during the 1940s and thereafter.

As an extension of this doctoral thesis, the next step will consist of studying in detail the conflict between Samuelson and von Neumann, as the genealogy of an intellectual conflict begun by Wilson's rejection of Hilbert's mathematical style and his adoption of Gibbs's attitude towards mathematics, and to use it as a point of departure for a historical exploration of two contrasting traditions in mathematical economics.

REFERENCES

Archibald, G. C., Herbert A. Simon, and Paul Samuelson. 1963. "Discussion." *The American Economic Review* 53 (2): 227–36.

Arrow, Kenneth. 1983. "Contributions to Welfare Economics." In *Paul Samuelson and Modern Economic Theory*, 15–30. New York: McGraw-Hill.

Aubin, David, Hélène Gispert, and Catherine Goldstein. 2014. "The Total War of Paris Mathematicians." In *The War of Guns and Mathematics: Mathematical Practices and Communities in France and Its Western Allies Around World War I*, edited by David Aubin and Catherine Goldstein, 125–78. USA: AMS.

Backhouse, Roger. Forthcoming. *Maker of Modern Economics: An Intellectual Biography of Paul A. Samuelson*. Vol. 1: Becoming Samuelson. 2 vols.

———. 2012. "Entering Hansen's New World: Paul Samuelson, the Young Keynesian, 1938-47." *Unpublished Draft*.

———. 2013. "Scientific Welfare Analysis: The Origins of Bergson-Samuelson Welfare Economics, 1936-1947." *Unpublished Draft*.

———. 2014. "Paul A. Samuelson's Move to MIT." *History of Political Economy* 46 (suppl 1): 60–77.

———. 2015. "Revisiting Samuelson's Foundations of Economic Analysis." *Journal of Economic Literature* 53 (2): 326–50.

Backhouse, Roger, and Harro Maas. 2016. "Taming Maxwell's Demon: The Bowman Committee and the Planning of Science for the Public Good." *Unpublished Draft*.

Barber, William. 1985. *From New Era to New Deal: Herbert Hoover, the Economists, and American Economic Policy, 1921-1933*. Cambridge; New York: Cambridge University Press.

Barnett, William Arnold, and Paul A. Samuelson. 2004. “An Interview with Paul A. Samuelson.” In *Inside the Economists' Mind*. United States of America: Cambridge University Press.

Bellhouse, David. 2009. “Karl Pearson's Influence in the United States.” *International Statistical Review* 77 (1): 51–63.

Bjerkholt, Olav. 1998. “Ragnar Frisch and the Foundation of the Econometric Society and Econometrica.” In *Econometrics and Economic Theory in the 20th Century: The Ragnar Frisch Centennial Symposium*, edited by Strøm Steinar, 26–57. United States of America: Cambridge University Press.

Blaug, Mark. 1980. *The Methodology of Economics*. http://www.cambridge.org/gb/knowledge/isbn/item1143239/?site_locale=en_GB.

———. 2003. “The Formalist Revolution of the 1950s.” *Journal of the History of Economic Thought* 25 (02): 145–56.

Bowley, Arthur. 1924. *The Mathematical Groundwork of Economics: An Introductory Treatise*. Oxford: Clarendon Press.

Bridgman, Percy. 1927. *The Logic of Modern Physics*. New York: Macmillan.

Brown, Carry, and Robert Solow, eds. 1983. *Paul Samuelson and Modern Economic Theory*. New York: McGraw-Hill.

Caldwell, Bruce. 1982. *Beyond Positivism: Economic Methodology in the Twentith Century*. London: George Allen and Unwin.

Cochrane, Rexmond. 1978. *The National Academy of Sciences: The First Hundred Years, 1863-1963*. Washington: National Academy of Sciences.

Cohen, Joshua. 1995. "Samuelson's Operationalist-Descriptivist Thesis." *Journal of Economic Methodology* 2 (1): 53–78. doi:10.1080/13501789500000003.

Corry, Leo. 2004a. *David Hilbert and the Axiomatization of Physics (1898-1918) : From Grundlagen Der Geometrie to Grundlagen Der Physik*. Dordrecht: Kluwer Academic Publishers.

———. 2004b. *Modern Algebra and the Rise of Mathematical Structures*. 2nd ed. Basel, Switzerland: Birkhäuser Verlag.

Cot, Annie. 2011. "A 1930s North American Creative Community: The Harvard 'Pareto Circle.'" *History of Political Economy* 43 (1): 131–59.

Crowe, Michael. 1985. *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System*. New York: Dover.

Curran, Jean. 1970. *Founders of the Harvard School of Public Health: With Biographical Notes, 1909-1946*. New York: Josiah Macy, Jr., Foundation.

Davis, Harold Thayer. 1941. *The Theory of Econometrics*. Bloomington, Ind: The Principia press.

Düppe, Till, and Roy Weintraub. 2014. *Finding Equilibrium: Arrow, Debreu, McKenzie and the Problem of Scientific Credit*. Princeton, NJ: Princeton University Press.

Dupree, Hunter. 1986. *Science in the Federal Government: A History of Policies and Activities*. Baltimore: Johns Hopkins UP.

Epple, Moritz. 2003. "The End of the Science of Quantity: Foundations of Analysis, 1860-1910." In *A History of Analysis*, edited by Hans Niels Jahnke, HMath 24:291–323. Providence, RI: American Mathematical Society, London Mathematical Society.

Epstein, Paul. 1936. “Gibbs’ Methods in Quantum Statistics.” In *Commentary on the Scientific Writings of J. Willard Gibbs*, edited by Frederick Donnan and Arthur Haas, 2:521–84. New Haven: Yale University Press.

———. 1937. *Textbook of Thermodynamics*. New York: J. Wiley & sons, inc.

Evans, Griffith Conrad. 1930. *Mathematical Introduction to Economics*. New York: McGraw-Hill Book Company, Inc.

Fenster, Della Dumbaugh, and Karen Parshall. 1994. “A Profile of the American Mathematical Research Community: 1891-1906.” In *The History of Modern Mathematics: Images, Ideas, and Communities*, edited by Eberhard Knobloch and David Rowe, 3:179–227. Boston: Academic Press.

Fisher, Irving. 1927. “A Statistical Method for Measuring ‘Marginal Utility’ and Testing the Justice of a Progressive Income Tax.” In *Economic Essays in Honor of John Bates Clark*, edited by Jacob Hollander, 157–93. New York: The Macmillan Company.

———. 1930. *The Stock Market Crash - And After*. First Edition edition. New York: MacMillan & Co LTD.

Friedman, Milton. 1953. *Essays in Positive Economics*. Chicago and London: University of Chicago Press.

Frisch, Ragnar. 1931. “The Interrelation Between Capital Production and Consumer-Taking.” *Journal of Political Economy* 39 (5): 646–54.

———. 1932a. “Capital Production and Consumer-Taking: A Rejoinder.” *Journal of Political Economy* 40 (2): 253–55.

———. 1932b. *New Methods of Measuring Marginal Utility,*. Tübingen: Mohr.

———. 1936. “On the Notion of Equilibrium and Disequilibrium.” *The Review of Economic Studies* 3 (2): 100–105.

Garb, Gerald. 1965. “Professor Samuelson on Theory and Realism: Comment.” *The American Economic Review* 55 (5): 1151–53.

Gibbs, Josiah. 1902. *Elementary Principles in Statistical Mechanics*. New York: Dover Publication, Inc.

Giocoli, Nicola. 2003. *Modeling Rational Agents: From Interwar Economics to Early Modern Game Theory*. Cheltenham: Edward Elgar.

Giraud, Yann. 2007. “L’économie est-elle une discipline visuelle ? (1932-1969).” Paris: Université Paris X Nanterre.

Gray, Jeremy. 2008. *Plato’s Ghost: The Modernist Transformation of Mathematics*. Princeton, N.J.: Princeton University Press.

———. 2013. *Henri Poincaré: A Scientific Biography*. Princeton, N.J.: Princeton University Press.

Haberler, Gottfried. 1937. *Prosperity and Depression: A Theoretical Analysis of Cyclical Movements*. Geneva: League of Nations.

Hacking, Ian. 1980. “The Theory of Probable Inference: Neyman, Peirce and Braithwaite.” In *Science, Belief and Behaviour: Essays in Honour of R. B. Braithwaite*, edited by David Mellor. Cambridge: Cambridge University Press.

Hacohen, Malachi. 2007. “Rediscovering Intellectual Biography—and Its Limits.” *History of Political Economy* 39 (Suppl 1): 9–29.

Hahn, Frank. 1983. “On General Equilibrium and Stability.” In *Paul Samuelson and Modern Economic Theory*, 31–56. New York: McGraw-Hill.

Hale, George. 1915. *National Academies and the Progress of Research*. Lancaster: New Era Printing Co.

Hands, D. Wade. 2006. "Integrability, Rationalizability, and Path-Dependency in the History of Demand Theory." *History of Political Economy* 38 (Suppl 1): 153–85.

———. 2014. "Paul Samuelson and Revealed Preference Theory." *History of Political Economy* 46 (1): 85–116.

Hands, Wade. 2001. *Reflection Without Rules: Economic Methodology and Contemporary Science Theory*. Cambridge [etc.]: Cambridge University Press.

———. 2013. "Foundations of Contemporary Revealed Preference Theory." *Erkenntnis* 78 (5): 1081–1108.

———. 2014. "The Individual and the Market: Paul Samuelson on (Homothetic) Santa Claus Economics." *The European Journal of the History of Economic Thought* 23 (3): 425–52.

Hands, Wade, and Philip Mirowski. 1998. "Harold Hotelling and the Neoclassical Dream." In *Economics and Methodology: Crossing Boundaries*, edited by Roger Backhouse, Daniel Hausman, Uskali Mäki, and Andrea Salanti, 322–97. New York: Macmillan and St. Martin's.

Hansen, Alvin Harvey. 1938. *Full Recovery Or Stagnation?* New York: W. W. Norton.

Hausman, Daniel M. 1992. *The Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press.

Heyl, Barbara S. 1968. "The Harvard 'Pareto Circle.'" *Journal of the History of the Behavioral Sciences* 4 (4): 316–334.

Hicks, J. R. 1939. *Value and Capital: An Inquiry into Some Fundamental Principles of Economic Theory*. 2nd ed. Oxford University Press, USA.

Holton, Gerald. 1981. “The Formation of the American Physics Community in the 1920s and the Coming of Albert Einstein.” *Minerva* 19 (4): 569–81.

Hotelling, Harold. 1932. “Edgeworth’s Taxation Paradox and the Nature of Demand and Supply Functions.” *Journal of Political Economy* 40 (5): 577–616.

———. 1935. “Demand Functions with Limited Budgets.” *Econometrica* 3 (1): 66–78.

Hunsaker, Jerome, and Saunders Mac Lane. 1973. “Edwin Bidwell Wilson, 1879-1964, A Biographical Memoir.” *National Academy of Sciences (US)*, 283–320.

Hunter, Patti. 1999. “An Unofficial Community: American Mathematical Statisticians before 1935.” *Annals of Science* 56 (1): 47–68.

Huntington, Edward. 1904. “Sets of Independent Postulates for the Algebra of Logic.” *Transactions of the American Mathematical Society* 5 (3): 288–309.

Ingrao, Bruna, and Giorgio Israel. 1990. *The Invisible Hand: Economic Equilibrium in the History of Science*. Cambridge Mass.: The MIT Press.

Isaac, Joel. 2012. *Working Knowledge: Making the Human Sciences from Parsons to Kuhn*. Cambridge Mass.: Harvard University Press.

Israel, Giorgio. 1981. “Rigor and Axiomatics in Modern Mathematics.” *Fundamental Scientiae* 2 (2): 205–19.

———. 1998. *La Mathématisation Du Réel*. Paris: Seuil.

Johnson, Harry. 1977. “The American Tradition in Economics.” *Nebraska Journal of Economics and Business* 16 (3): 17–26.

Johnson, Norman L., and Samuel Kotz. 2011. *Leading Personalities in Statistical Sciences: From the Seventeenth Century to the Present*. John Wiley & Sons.

Kevles, Daniel. 1968. "George Ellery Hale, the First World War, and the Advancement of Science in America." *Isis* 59 (4): 427–37.

Keynes, John Maynard. 1936. *The General Theory Of Employment, Interest, And Money*. London: MacMillan & Co.

Kingsland, Sharon. 1982. "The Refractory Model: The Logistic Curve and the History of Population Ecology." *Quarterly Review of Biology* 57 (1): 29–52.

———. 1985. *Modeling Nature: Episodes in the History of Population Ecology*. 1st ed. Chicago: University of Chicago Press.

Krüger, Lorenz, Lorraine Daston, and Michael Heidelberger, eds. 1987. *The Probabilistic Revolution*. Vol. 1: Ideas in History. 2 vols. Cambridge, Mass: The MIT Press.

Krüger, Lorenz, Gerd Gigerenzer, and Mary Morgan, eds. 1987. *The Probabilistic Revolution*. Vol. 2: Ideas in the Sciences. 2 vols. Cambridge Mass.: The MIT Press.

Landreth, Harry, and David Colander. 2004. "Pluralism, Formalism and American Economics." Middlebury College Working Paper Series 0409. Middlebury College, Department of Economics. https://ideas.repec.org/p/mdl/mdlpap/0409.html.

Lange, Oskar. 1934. "The Determinateness of the Utility Function." *The Review of Economic Studies* 1 (3): 218–25.

Leonard, Robert. 2010. *Von Neumann, Morgenstern, and the Creation of Game Theory: From Chess to Social Science, 1900-1960*. 1st ed. Cambridge: Cambridge University Press.

Leonard, Thomas. Forthcoming. *Liberal Reformers*. Princeton UP.

Lerner, Abba P. 1965. "Professor Samuelson on Theory and Realism: Comment." *The American Economic Review* 55 (5): 1153–55.

Livingston, Burton E. 1929. “The American Association for the Advancement of Science: Accounts of the Sessions of Sections and Societies at the Fifth New York Meeting.” *Science*, New Series, 69 (1779): 107–31.

———. 1930. “The American Association for the Advancement of Science: Reports of the Des Moines Meeting of the American Association for the Advancement of Science and Associated Societies.” *Science*, New Series, 71 (1832): 137–68.

———. 1931. “The American Association for the Advancement of Science: Reports of the Fourth Cleveland Meeting of the American Association for the Advancement of Science and Associated Societies.” *Science*, New Series, 73 (1884): 137–68.

Louçã, Francisco. 2007. *The Years of High Econometrics: A Short History of the Generation That Reinvented Economics*. London and New York: Routledge.

Maas, Harro. 2014. “Making Things Technical: Samuelson at MIT.” *History of Political Economy* 46 (suppl 1): 272–94.

Machlup, Fritz. 1964. “Professor Samuelson on Theory and Realism.” *The American Economic Review* 54 (5): 733–35.

Massey, Gerald J. 1965. “Professor Samuelson on Theory and Realism: Comment.” *The American Economic Review* 55 (5): 1155–64.

McCraw, Thomas K. 2010. *Prophet of Innovation: Joseph Schumpeter and Creative Destruction*. 1 edition. Cambridge, MA: Belknap Press.

Menand, Louis. 2002. *The Metaphysical Club: A Story of Ideas in America*. 1st ed. Farrar, Straus and Giroux.

Mills, Frederick C., Jacob H. Hollander, Jacob Viner, Edwin Wilson, Wesley C. Mitchell, F. W. Taussig, T. S. Adams, John D. Black, and John Candler Cobb.

1928. “The Present Status and Future Prospects of Quantitative Economics.” *The American Economic Review* 18 (1): 28–45.

Mirowski, Philip. 1989. *More Heat than Light: Economics as Social Physics, Physics as Nature’s Economics*. Cambridge, England: Cambridge University Press.

———. 2002. *Machine Dreams: Economics Becomes a Cyborg Science*. Cambridge, England: Cambridge University Press.

Mirowski, Philip, and D. Wade Hands. 2006. “Introduction to Agreement on Demand: Consumer Theory in the Twentieth Century.” *History of Political Economy* 38 (Suppl 1): 1–6.

Mirowski, Philip, and Wade Hands. 1998. “A Paradox of Budgets: The Postwar Stabilization of American Neoclassical Demand Theory.” *History of Political Economy* 30 (Supplement): 260–92.

Mongin, Philippe. 2000. “La Méthodologie Économique Au Xxème Siècle. Les Controverses En Théorie de L’entreprise et La Théorie Des Préférences Révélées.” In *Nouvelle Histoire de La Pensée Économique*, edited by Béraud and Gillbert Faccarello, 3:340–78. Paris: Éditions La Découverte.

Moore, Eliakim. 1903. “On the Foundations of Mathematics.” *Bulletin of the American Mathematical Society* 9: 402–24.

Morgan, Mary, and Malcolm Rutherford. 1998. “American Economics: The Character of the Transformation.” *History of Political Economy* 30 (Supplement): 1–26.

Nagel, Ernest. 1961. *The Structure of Science: Problems in the Logic of Scientific Explanation*. New York: Harcourt, Brace & World.

———. 1963. “Assumptions in Economic Theory.” *The American Economic Review* 53 (2): 211–19.

Neyman, Jerzy. 1952. *Lectures and Conferences on Mathematical Statistics and Probability*. USA: Graduate School, U.S. Dept. of Agriculture.

Parascandola, John. 1992. "L. J. Henderson and the Mutual Dependency of Variables: From Physical Chemistry to Pareto." In *Science at Harvard University: Historical Perspectives*, edited by Clark Elliott and Margaret Rossiter, 146–90. United States of America: Associated UP.

Parshall, Karen. 1984. "Eliakim Hastings Moore and the Founding of a Mathematical Community in America, 1892–1902." *Annals of Science* 41 (4): 313–33.

Parshall, Karen, and David Rowe. 1994. *The Emergence of the American Mathematical Research Community, 1876-1900: J.J. Sylvester, Felix Klein, and E.H. Moore*. United States of America: American Mathematical Society.

Perrin, Jean Baptiste. 1903. *Traité de chimie physique*. Paris: Gauthiers-Villars.

Pierpont, James. 1899. "On the Arithmetization of Mathematics." *Bulletin of the American Mathematical Society*, 8, 5: 394–406.

Poincaré, Henri. 1903. "Poincaré's Review of Hilbert's 'foundations of Geometry.'" *Bulletin of the American Mathematical Society* 10 (1): 1–23.

Porter, Theodore M. 1996. *Trust in Numbers*. Reprint edition. Princeton, N.J.: Princeton University Press.

Ramsden, Edmund. 2002. "Carving up Population Science: Eugenics, Demography and the Controversy over the 'Biological Law' of Population Growth." *Social Studies of Science* 32 (5/6): 857–99.

Reich, Karin. 1993. "The American Contribution to the Theory of Differential Invariants 1900-1916." In *The Attraction of Gravitation: New Studies in the History of General Relativity*, edited by John Earman, Michel Janssen, and John Norton, 5:225–47. Boston, Basel, Berlin: Birkhäuser.

———. 1994. *Die Entwicklung des Tensorkalküls: Vom absoluten Differentialkalkül zur Relativitätstheorie*. 1994 edition. Basel; Boston: Birkhäuser.

Robinson, Joan. 1933. *The Economics of Imperfect Competition*. London: MacMillan & Co.

Ross, Dorothy. 1992. *The Origins of American Social Science*. United States of America: Cambridge University Press.

Rukeyser, Muriel. 1941. *Willard Gibbs*. New York: Dutton.

Russett, Cynthia Eagle. 1966. *The Concept of Equilibrium in American Social Thought*. New Haven, Cann.: Yale University Press.

Rutherford, Malcolm. 2011. *The Institutionalist Movement in American Economics, 1918-1947: Science and Social Control*. Cambridge: Cambridge University Press.

Samuelson, Paul. 1937a. "A Note on Measurement of Utility." *The Review of Economic Studies* 4 (2): 155–61.

———. 1937b. "Some Aspects of the Pure Theory of Capital." *The Quarterly Journal of Economics* 51 (3): 469–96.

———. 1938a. "A Note on the Pure Theory of Consumer's Behaviour." *Economica* 5 (17): 61–71.

———. 1938b. "Welfare Economics and International Trade." *The American Economic Review* 28 (2): 261–66.

———. 1938c. "A Note on the Pure Theory of Consumer's Behaviour: An Addendum." *Economica* 5 (19): 353–54.

———. 1938d. "The Empirical Implications of Utility Analysis." *Econometrica* 6 (4): 344–56.

———. 1938e. “The Numerical Representation of Ordered Classifications and the Concept of Utility.” *The Review of Economic Studies* 6 (1): 65–70.

———. 1939a. “Interactions between the Multiplier Analysis and the Principle of Acceleration.” *The Review of Economics and Statistics* 21 (2): 75–78.

———. 1939b. “The Gains from International Trade.” *The Canadian Journal of Economics and Political Science / Revue Canadienne d’Economique et de Science Politique* 5 (2): 195–205.

———. 1939c. “A Synthesis of the Principle of Acceleration and the Multiplier.” *Journal of Political Economy* 47 (6): 786–97.

———. 1940. “The Theory of Pump-Priming Reëxamined.” *The American Economic Review* 30 (3): 492–506.

———. 1941a. “Foundations of Analytical Economics: The Observational Significance of Economic Theory.” Cambridge Mass.: Harvard University.

———. 1941b. “Conditions That the Roots of a Polynomial Be Less Than Unity in Absolute Value.” *The Annals of Mathematical Statistics* 12 (3): 360–64.

———. 1942a. “A Note on Alternative Regressions.” *Econometrica* 10 (1): 80–83.

———. 1942b. “The Stability of Equilibrium: Linear and Nonlinear Systems.” *Econometrica* 10 (1): 1–25.

———. 1942c. “A Method of Determining Explicitly the Coefficients of the Characteristic Equation.” *The Annals of Mathematical Statistics* 13 (4): 424–29.

———. 1943a. “Fitting General Gram-Charlier Series.” *The Annals of Mathematical Statistics* 14 (2): 179–87.

———. 1943b. “A Simple Method of Interpolation.” *Proceedings of the National Academy of Sciences of the United States of America* 29 (11): 397–401.

———. 1943c. “Efficient Computation of the Latent Vectors of a Matrix.” *Proceedings of the National Academy of Sciences of the United States of America* 29 (11): 393–97.

———. 1947. *Foundations of Economic Analysis*. Cambridge, Mass. Harvard University Press.

———. 1964. “Theory and Realism: A Reply.” *The American Economic Review* 54 (5): 736–39. doi:10.2307/1818572.

———. 1965. “Professor Samuelson on Theory and Realism: Reply.” *The American Economic Review* 55 (5): 1164–72.

———. 1998. “How Foundations Came to Be.” *Journal of Economic Literature* 36 (3): 1375–86.

———. 2004. “A Portrait of the Master as a Young Man.” In *Wassily Leontief and Input-Output Economics*, edited by Erik Dietzenbacher and Lahr. Michael, 3–8. Cambridge, UK ; New York: Cambridge University Press.

Schultz, Henry. 1938. *The Theory and Measurement of Demand*. Chicago: The University of Chicago Press.

Schumpeter, Joseph Alois. 2000. *Briefe /Letters*. Edited by Ulrich Hedtke and R. Swedberg. Tübingen: Mohr Siebeck.

Servos, John. 1986. “Mathematics and the Physical Sciences in America, 1880-1930.” *Isis* 77 (4): 611–29.

Shoesmith, Beulah. 1916. “Mathematics Clubs in Secondary Schools.” *School Science and Mathematics* 16 (2): 106–113.

Stigler, Stephen. 1996. “The History of Statistics in 1933.” *Statistical Science* 11 (3): 244–52.

Tinbergen, Jan. 1935. “Annual Survey: Suggestions on Quantitative Business Cycle Theory.” *Econometrica* 3 (3): 241–308.

Walter, Scott. 1999. “The Non-Euclidean Style of Minkowskian Relativity.” In *The Symbolic Universe: Geometry and Physics, 1890-1930*, edited by Jeremy Gray, 91–127.

Weintraub, Roy. 1991. *Stabilizing Dynamics Constructing Economic Knowledge*. Cambridge, England: Cambridge University Press.

———. 1998. “From Rigor to Axiomatics: The Marginalization of Grif.th C. Evans.” *History of Political Economy* 30 (Supplement): 227–59.

———. 2002. *How Economics Became a Mathematical Science*. Durham: Duke University Press.

Weintraub, Roy, and Evelyn Forget. 2007. *Economists Lives: Biography and Autobiography in the History of Economics*. Durham: Duke UP.

Whittaker, E. T., and G. Robinson. 1924. *The Calculus Of Observations: A Treatise On Numerical Mathematics*. London: Blackie And Son Limited.

Wilson, Edwin. 1900. “The Decomposition of the General Collineation of Space into Three Skew Reflections.” *Transactions of the American Mathematical Society* 1 (2): 193–96.

———. 1901. *Vector Analysis: A Text-Book for the Use of Students of Mathematics and Physics*. United States of America: Yale University Press.

———. 1902a. “Some Recent Books on Mechanics.” *Bulletin of the American Mathematical Society* 8 (8): 341–49.

———. 1902b. “Some Recent Books on Mechanics.” *Bulletin of the American Mathematical Society* 9 (1): 25–39.

———. 1902c. “Some Recent Books on Mechanics.” *Bulletin of the American Mathematical Society* 8 (9): 403–12.

———. 1903a. “Mach’s Mechanic’s.” *Bulletin of the American Mathematical Society* 10 (2): 80–86.

———. 1903b. “Review: Émile Borel, Leçons Sur Les Fonctions Méromorphes.” *Bulletin of the American Mathematical Society* 9 (9): 506–7.

———. 1903c. “Review: Höhere Analysis Für Ingenieure, by John Perry.” *Bulletin of the American Mathematical Society* 9 (9): 504–6.

———. 1903d. “The So-Called Foundations of Geometry.” *Archiv Der Mathematik Und Physik* 6: 104–22.

———. 1904a. “The Foundations of Mathematics.” *Bulletin of the American Mathematical Society* 11 (2): 74–93.

———. 1904b. “The Theory of Waves.” *Bulletin of the American Mathematical Society* 10 (6): 305–17.

———. 1905. “On Products in Additive Fields.” In *Verhandlungen Des Dritten Internationalen Mathmaticker-Kongresses in Heidelberg*, 202–16. Leipniz.

———. 1906. “The Foundations of Science.” *Bulletin of the American Mathematical Society* 12 (4): 187–93.

———. 1907. “Bryan’s Thermodynamics.” *Bulletin of the American Mathematical Society* 14 (3): 139–44.

———. 1908a. “Note on Statistical Mechanics.” *Bulletin of the American Mathematical Society* 15 (3): 107–15.

———. 1908b. “On the Differential Equations of the Equilibrium of an Inextensible String.” *Transactions of the American Mathematical Society* 9 (4): 425–39.

———. 1908c. “On the Theory of Double Products and Strains in Hyperspace.” *Transactions of the Connecticut Academy of Art and Sciences* 14: 1–57.

———. 1908d. “Review: Emile Picard, Sur Le Développement de l’Analyse et Ses Rapports Avec Diverses Sciences: Conférences Faites En Amérique, and Emile Picard, La Science Moderne et Son État Actuel.” *Bulletin of the American Mathematical Society* 14 (9): 444–48.

———. 1908e. “Symbolic Logic.” *Bulletin of the American Mathematical Society* 14 (4): 175–91.

———. 1909a. “Economics.” *Bulletin of the American Mathematical Society* 15 (4): 169–86.

———. 1909b. “Thermodynamic Analogies for a Simple Dynamical System.” *Annals of Mathematics* 10 (4): 149–66.

———. 1911a. *Advanced Calculus*. Boston, New York: Ginn and company.

———. 1911b. “Mathematical Physics for Engineers.” *Bulletin of the American Mathematical Society* 17 (7): 350–61.

———. 1912a. “Mathematical Economics.” *Bulletin of the American Mathematical Society* 18 (9): 462–74.

———. 1912b. “The Fourth Dimension as a Text.” *Science Conspectus* 2 (4): 104-07.

———. 1913a. “Let Us Have Calculus Early.” *Bulletin of the American Mathematical Society* 20: 30–36.

———. 1913b. “Review of The Purchasing Power of Money; Its Determination and Relation to Credit, Interest and Crises by I. Fisher.” *Science* 37 (959): 758–63.

———. 1914a. “Review of A. Einstein and M. Grossmann: ‘Entwurf Einer Verallgemeinerten Relativitâtstheorie Und Einer Theorie Der Gravitatio.’” *Bulletin of the American Mathematical Society* 20 (5).

———. 1914b. “Review of The Theory of Relativity by Robert D. Carmichael.” *Science* 39 (998): 251–52.

———. 1914c. “The Price Level.” *Bulletin of the American Mathematical Society* 20 (7): 377–81.

———. 1915. “Some Books on Calculus.” *Bulletin of the American Mathematical Society* 21 (9): 471–76.

———. 1918. “Insidious Scientific Control.” *Science*, New Series, 48 (1246): 491–93.

———. 1920a. *Aeronautics: A Class Text*. New York: John Wiley and Sons.

———. 1920b. “Space, Time, and Gravitation.” *The Scientific Monthly* 10 (3): 217–35.

———. 1923a. “First and Second Laws of Error.” *Journal of the American Statistical Association* 18 (143): 841–51.

———. 1923b. “Keynes on Probability.” *Bulletin of the American Mathematical Society* 29 (7): 319–22.

———. 1923c. “The Statistical Significance of Experimental Data.” *Science* 58 (1493): 93–100.

———. 1925. “The Logistic or Autocatalytic Grid.” *Proceedings of the National Academy of Sciences of the United States of America* 11 (8): 451–56.

———. 1926a. “Empiricism and Rationalism.” *Science* 64 (1646): 47–57.

———. 1926b. “Statistical Inference.” *Science* 63 (1629): 289–96.

———. 1927. “Probable Inference, the Law of Succession, and Statistical Inference.” *Journal of the American Statistical Association* 22 (158): 209–12.

———. 1928. “Too Little Mathematics--and Too Much.” *Science* 67 (1725): 52–59.

———. 1930. “Mathematics and Statistics.” *Journal of the American Statistical Association* 25 (169): 1–8.

———. 1934a. “Are There Periods in American Business Activity?” *Science* 80 (2070): 193–99.

———. 1934b. “The Periodogram of American Business Activity.” *The Quarterly Journal of Economics* 48 (3): 375–417.

———. 1935. “Generalization of Pareto’s Demand Theorem.” *The Quarterly Journal of Economics* 49 (4): 715–17.

———. 1936. “The Contributions of Gibbs to Vector Analysis and Multiple Algebra.” In *Commentary on the Scientific Writings of J. Willard Gibbs*, edited by Frederick Donnan and Arthur Haas, 2:127–60. New Haven: Yale University Press.

———. 1938. “How to Study the Social Aspects of the Depression.” *Journal of the American Statistical Association* 33 (203): 505–12.

———. 1939. “Pareto Versus Marshall.” *The Quarterly Journal of Economics* 53 (4): 645–50.

———. 1940a. “Methodology in the Natural and the Social Sciences.” *American Journal of Sociology* 45 (5): 655–68.

———. 1940b. “Social Progress.” *Proceedings of the National Academy of Sciences of the United States of America* 73 (15): 469–72.

———. 1940c. “What Is Social Science?” *Science* 92 (2382): 157–62.

———. 1943. “Pareto on Marshall’s Demand Curve.” *The Quarterly Journal of Economics* 58 (1): 141–45.

———. 1944a. “Hicks on Perfect Substitutes.” *The Quarterly Journal of Economics* 59 (1): 134–40.

———. 1944b. “On Notation for Utility Theory.” *The Quarterly Journal of Economics* 58 (4): 647–50.

———. 1945. “Consumption in Fixed Proportion.” *The Quarterly Journal of Economics* 59 (4): 635–39.

———. 1946. “Notes on Utility Theory and Demand Equations.” *The Quarterly Journal of Economics* 60 (3): 453–60.

———. 1962. Interview with Dr. Edwin Wilson Interview by Bruce Lindsay and James King. Niels Bohr Library & Archives. American Institute of Physics, College Park, MD USA. http://www.aip.org/history/ohilist/5065.html.

———. 1966. *History of the Proceedings of the National Academy of Sciences: 1914-1963*. Washington: National Academy of Sciences.

Wilson, Edwin, and Gilbert Lewis. 1912. “The Space-Time Manifold of Relativity. The Non-Euclidean Geometry of Mechanics and Electromagnetics.” *Proceedings of the American Academy of Arts and Sciences* 48 (11): 389–507.

Wilson, Edwin, and Clarence Moore. 1916a. “A General Theory of Surfaces.” *Proceedings of the National Academy of Sciences of the United States of America* 2 (5): 273–78.

———. 1916b. “Differential Geometry of Two Dimensional Surfaces in Hyperspace.” *Proceedings of the American Academy of Arts and Sciences* 52 (6): 269–368.

Wilson, Edwin, and Ruth Puffer. 1933. “Least Squares and Laws of Population Growth.” *Proceedings of the National Academy of Sciences of the United States of America* 68 (9): 285–382.

Wong, Stanley. 1973. “The ‘F-Twist’ and the Methodology of Paul Samuelson.” *The American Economic Review* 63 (3): 312–25.

———. 1978. *Foundations of Paul Samuelson’s Revealed Preference Theory: A Study by the Method of Rational Reconstruction*. London: Routledge & Kegan Paul.