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

BY

JUAN CARVAJALINO

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Edwin B. Wilson and the Rise of Mathematical Economics in America, 1920-1940

Juan Carvajalino¹

Abstract: In the paper, Edwin B. Wilson's influence on the rise of mathematical economics in America between the 1920s and 1940s is explored. The focus is laid on showing how on the grounds of his foundational ideas about science Wilson worked at the organizational and educational fronts to modernize economics, at this at three levels. First, the paper shows the ways in which around 1930 Wilson was key, at the nationwide level, in the constitution of the first organized community of American mathematical economists, which he established within the well-recognized scientific community of the American Association for the Advancement of Science. In the way, the paper traces Wilson's crucial role in the origins of the Econometric Society and bears new lights on the constitution of the econometric movement. Second, the paper reconstructs Wilson's leadership in promoting and establishing the first program in advanced mathematical and statistical economics at the more local level of Cambridge at Harvard. He offered two courses to economists, Mathematical Economics and Mathematical Statistics, in which he respectively taught Gibbs's thermodynamics systems and numerical mathematics and analytical statistics. In his courses, willing to interconnect Vifredo Pareto's and Wesley Mitchell's economics, Wilson emphasized that a sound scientific attitude required connecting economics with data, if only in idealized conditions. Finally, the paper argues that Wilson's lasting influence in economics took shape at a more personal level, through his influence on Paul Samuelson, one of his students at Harvard. Samuelson wrote his thesis (1940) and subsequently *Foundations of Economic Analysis* (1947) in a Wilsonian style.

Key words: E. B. Wilson, Samuelson, Gibbs, Mathematics is a Language, AAAS, Econometric Society, Harvard.

JEL Classification: B10, B20, B23, B31.

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Introduction

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 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

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 while praising Wesley Mitchell’s more quantitative approach. Fisher was another student of Gibbs at Yale; at the national level, Wilson played a central role in the constitution of the community of American mathematical economists. At the local level of Harvard, he promoted and established the first program in modern mathematical economics. At a personal level, he greatly influenced the mathematical and statistical thought of one of his students, Paul Samuelson, whose famous *Foundations of Economic Analysis* (1947) was framed within a

² Yann Giraud (2007), Roger Backhouse (2017, 2015, 2014, 2013), Bruna Ingraio 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.

Wilsonian world. Through the study of Wilson's influence in the development of mathematical economics, new light is also shed regarding the origins of the Econometric Society, of which Wilson was a founding member, but from which he rapidly distanced himself.

Behind Wilson's activism in economics lay his belief that mathematics was like a language. This implied for him defining mathematics as intrinsically interconnected with science and meaning. For him, mathematics without its corresponding ties with science remained a futile exercise, and science without its corresponding mathematical structures could not be regarded as truly scientific. Further, Wilson thought, intuition, experience and personal judgment of the mathematician/scientist were as important as, or even more important than, mathematical rigor, logic, and consistency. He preferred scientific contributions in which intuition and meaning relative to a given subject matter filled the possible mathematical gaps over contributions that were only concerned with mathematical consistency. For him, the former kinds of contributions were fortunate examples of *much science with little mathematics*;³ on the contrary, the latter kinds of contributions were unfortunate examples of *much mathematics with little science*.⁴ In his words:

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

Also underpinning Wilson's activism in economics was his belief that American mathematics and science must better reflect American national values—which should frame intuition and meaning in American sciences—and in this vein more significantly serve national security and prosperity. Wilson was confident that subtle planning of science would yield democratic science in America. He also believed that matters of pedagogy and epistemology were connected. Central to Wilson's thought was his perennial belief that in science, as in society, progress was 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

³ See Wilson 1928b.

⁴ On Wilson's Gibbsian definition of mathematics, See (Carvajalino 2016), first chapter.

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.⁵ The spirit in 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.”⁶

Using archival material, this paper will reconstruct Wilson's activism “to push” mathematical economics in America. Three levels of Wilson's influence in the rise of mathematical economics are distinguished: the nationwide level of American scientific academies and associations, the local level of Cambridge at Harvard, and the personal level through the Wilson-Samuelson connection.

1. 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.⁷ Wilson's career illustrates this process of marginalization, and corollary process of incursion into other fields. First, in 1907, he became associate professor of mathematical physics at Massachusetts Institute of Technology (MIT). Second, in 1922, Wilson accepted the chairmanship of the department of vital statistics at the newly founded Harvard School of Public Health. In parallel spheres, from 1914, when the *Proceeding of the*

⁵ Henderson was a Harvard physiologist and the main figure of the Pareto Circle during the 1930s (see Heyl 1968).

⁶ E. Wilson to L. Henderson, Jan. 10, 1933 (PEBW, 21). On Wilson-Henderson connection, see (Carvajalino 2016), first chapter.

⁷ See Wilson 1903c.

National Academy of Science 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 mathematics and defining, in an original and unique way, mathematics as language or, in other words, as intermediate and constrained.⁸

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 Sumner and the mathematical economist Irving Fisher.⁹ Wilson helped Fisher setting up business and stock market barometers.¹⁰

These interests persisted and around 1910 Wilson promoted Fisher's and Pareto's mathematical economics; in reviews of their work, addressed to the readership of the *Bulletin of the American Mathematical Society* and *Science*, Wilson presented economics as a field where mathematics could be naturally applied.¹¹ 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,

⁸ See (Carvajalino 2016), first chapter.

⁹ E. Wilson to R. Vance, Dec. 19, 1922 (PEBW 3).

¹⁰ E. Wilson to G. U. Yule, May 6, 1924 (PEBW, 7).

¹¹ 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).

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

At the beginning of the 1920s, Wilson felt that he was “further away from those mathematical interests which the persons in control [of the American Mathematical Society (AMS)] define to be mathematics.”¹² Believing German mathematics was “essentially foreign to [American] nature” 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 1923.¹³ He had just joined the Harvard School of Public Health, 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.”¹⁴

During the 1920s, Wilson also became more involved with the functioning of the National Academy of Science (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 1924.¹⁵ 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.

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

¹² E. Wilson to J. Coolidge, March 30, 1922 (PEBW, 4).

¹³ E. Wilson to J. Whittemore, May 23, 1924 (PEBW, 7).

¹⁴ E. Wilson to R. Richardson, 3 Nov. 1928 (PEBW, 13, italics added).

¹⁵ See Cochrane 1978.

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.”¹⁶

2. Becoming a mathematical economist in America

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.

Section K: the American origins of the Econometric Society

In 1928, Wilson, the editor of the *Proceeding of the National Academy of Science*, 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 and economics at the national level.¹⁷ Wilson’s idea for Section K consisted of gathering in joint meetings people from the AAAS, the AMS, the ASA and the American Economic Association (AEA). The difficulty was to coordinate annual meetings of these

¹⁶ E. Wilson to J. Lipka, 17 Dec. 1923 (PEBW, 5).

¹⁷ Wilson’s 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 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 in a roundtable during the December meeting of AEA on *The Present Status and Future Prospects of Quantitative Economics* (Mills et al. 1928).

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.¹⁸ However, both Mitchell and Fisher were highly interested in Wilson's project.¹⁹ Mitchell was "glad that the AAAS [had] put Section K in [Wilson's] hands."²⁰ From the community of mathematicians, Ronald G. D. Richardson, Wilson's friend and secretary of the AMS, found Wilson's suggestion interesting.²¹ 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."²²

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.²³ 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."²⁴

¹⁸ 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.

¹⁹ I. Fisher to E. Wilson, June 28 1928 (PEBW, 13).

²⁰ W. Mitchell to E. Wilson, Apr. 27, 1928 (PEBW, 13).

²¹ R. Richardson to E. Wilson, Oct. 19 1928 (PEBW, 13).

²² E. Wilson to W. Mitchell, May 3, 1928 (PEBW, 13).

²³ 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.

²⁴ C. Roos to E. Wilson, June 14, 1931 (PEBW, 16).

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. In the same spirit, Wilson conducted the affairs of Section K in such a way as to avoid discord between the different associations. As he wrote to Mitchell, “I don’t like secessions, they often turn into civil war.”²⁵

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.”²⁶ 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.²⁷ Wilson, a charter member, was elected fellow of the Society, member of the Council of the Society and, later, member of the Advisory Editorial Committee of *Econometrica*.²⁸ Among the elected American members, there were Evans, Fisher, Hotelling, Moore, Mitchell, Roos, Schultz and Wilson.²⁹ All of them, with the exception of Moore, had been closely related to Section K. Overall, as Wilson noted to Frisch, the number of American members was larger than that of any European country.³⁰ 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 the event of national intrigues coming up between Europeans. Possibly the America group may act as a safety valve.”³¹

²⁵ E. Wilson to W. Mitchell, Oct. 9, 1928 (PEBW, 13). In the same letter, Wilson insisted that Section K 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]).

²⁶ C. Roos to E. Wilson, Oct. 21, 1932 (PEBW, 19).

²⁷ I. Fisher, R. Frisch, C. Roos to E. Wilson, Nov. 29, 1930 (idem).

²⁸ See Memorandum in re the Econometric Society, Signed by J. Schumpeter and R. Frisch at Bonn, Sep. 28, 1931 (PEBW, 16) and letter I. Fisher to E. Wilson, Dec. 16, 1931 (PEBW, 16).

²⁹ Memorandum in re the Econometric Society.

³⁰ E. Wilson to R. Frisch, Oct. 31, 1932 (PEBW, 18).

³¹ R. Frisch to E. Wilson, Nov. 24, 1932 (idem).

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.”³² 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.³³ For Wilson, the (European) 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 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 due only to his “greater interest in science” rather than pure technique.³⁴

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

³² E. Wilson to J. Lipka, 17 Dec. 1923 (PEBW, 5).

³³ 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]).

³⁴ E. Wilson to C. Roos, Sep. 16, 1936 (PEBW, 27).

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 about 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.³⁵

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 [Econometric] Society," Wilson felt that negotiations should be held by Americans, in more American ways.³⁶ If 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

³⁵ E. Wilson to F. Edgeworth March 12, 1923 (PEBW, 4).

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

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.”³⁷

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.³⁸ 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.”³⁹ 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 encourage [Americans] to dig into mathematics [was] to convince them that there [was] some practical use for the mathematics.”⁴⁰

Under such circumstances, in which Wilson disliked Frisch's strong (European) influence within the Econometric Society in America and in which he also disliked (European) econometricists' 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. During this process of self-marginalization from the econometric project, Wilson wrote *What is a Proof?* (1935b). This short paper was read during a joint meeting of Section K and the Econometric Society in December, 1934. In the paper, Wilson suggested that even if mathematics was a necessary condition for the development of economics as a science, econometricians should understand that there was “little correlation between the amount of formal mathematics used by the investigator and the substantial validity of the proof offered” (Wilson 1935b, 373). In America, econometricians, he insisted, should understand that “proofs, truth and facts must be

³⁷ E. Wilson to I. Fisher, Dec. 18, 1931 (PEBW, 16).

³⁸ 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]).

³⁹ E. Wilson to W. Nelson, Nov. 13, 1933 (idem).

⁴⁰ E. Wilson to R. Frisch, Nov. 15, 1933 (PEBW, 20).

relative to our [American] culture and conditioned by it” (Wilson 1935b, 373). For him, econometricians should endeavor to offer *much economics with little mathematics*, not the other way around. Because he must have felt that they were offering *little economics with (too) much mathematics*, in 1935 Wilson asked to no longer be part of the Council of the Econometric Society.⁴¹ Furthermore, Wilson 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 time 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.⁴²

Increasing concerns about economic policy

During the 1920s, Wilson adhered to the progressive and institutionalist American idea of regarding social science and economics as social engineering.⁴³ 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

⁴¹ E. Wilson to C. Roos, Jan. 21, 1935 (PEBW, 25).

⁴² See next section.

⁴³ See Leonard Forthcoming, Rutherford 2011.

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

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 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).

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). 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.⁴⁵ 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.⁴⁶ Because it was not yet known if the *remedies* would or would not stabilize economic fluctuations, the New Deal policies were, for Wilson, the social problem itself.⁴⁷

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.⁴⁸ More significantly for Wilson, when studying

⁴⁵ E. Wilson to W. Mitchell, Jan. 4, 1932 (PEBW, 19).

⁴⁶ E. Wilson to I. Fisher, May 25, 1932 (PEBW, 18).

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

⁴⁸ E. Wilson to I. Fisher, May 13, 1932 (PEBW, 18).

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.”⁴⁹ 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 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

⁴⁹ E. Wilson to I. Fisher, July 25, 1934 (PEBW, 23).

time. This is all very carefully pointed out with its implications for social science by Pareto in his *Manual of Political Economy*.⁵⁰

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 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.

3. Retreat to Cambridge

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

⁵⁰ E. Wilson to C. Snyder, June 2, 1934 (PEBW, 24).

turned his efforts towards Cambridge at the more local level of Harvard, where he assumed effective leadership regarding statistical and mathematical economics.⁵¹

A program of mathematical economics at Harvard

In 1928, Wilson had declined an offer to teach statistical economics made by Harold Burbank, chairman of the department of economics at Harvard.⁵² 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.”⁵³ 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,” working on statistical data and hoping that some “students might really be considering economics more than statistics and getting into a position where they could handle a statistical economic thesis.”⁵⁴ Despite low attendance, “the course met all expectations and needs” of the department.⁵⁵ At the end of 1932, invited by Burbank, Wilson started attending departmental meetings.⁵⁶

With the idea of establishing an American school of mathematical economics, in November 1932, Wilson presented to Burbank the possibility of alternately offering a course on theoretical statistics (in the sense of mathematical statistics) and a course on empirical statistics (in Mitchell's sense). 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. 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.⁵⁷ 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

⁵¹ At Harvard, Henderson, Wilson's close friend, had become one of the most influential figures (Isaac 2012).

⁵² E. Wilson to H. Burbank, May 29, 1928 (PEBW, 12).

⁵³ L. Crum to E. Wilson, Jan. 4, 1930 (PEBW, 15).

⁵⁴ E. Wilson to H. Burbank, March 23, 1932 (PEBW, 18).

⁵⁵ H. Burbank to E. Wilson, Apr. 13, 1932 (idem).

⁵⁶ H. Burbank to E. Wilson, Oct. 29, 1932 (idem).

⁵⁷ While Schumpeter supported Wilson's program, Wilson was rather unsympathetic with Schumpeter, who, Wilson felt, emphasized too much pure economic theory (E. Wilson to L. Henderson, Oct, 22, 1930, [PEBW, 15]).

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.⁵⁸ For Wilson, the aim of such a program consisted of developing, through instruction, the necessary conditions so that young 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.”⁵⁹

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.⁶⁰ The following academic year, 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.”⁶¹ 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.”⁶²

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

⁵⁸ Report, Meeting of the Committee (Wilson, Crum, Schumpeter) on Instruction in Mathematical Economics, Tuesday, April 18 (idem).

⁵⁹ E. Wilson to W. L. Crum, May 1, 1933 (PEBW, 20).

⁶⁰ E. Wilson to H. Burbank, Apr. 12, 1934 (PEBW, 22).

⁶¹ H. Burbank to E. Wilson, March 30, 1934 (PEBW, 22).

⁶² E. Wilson to H. Burbank, May 17, 1932 (PEBW, 22).

“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 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.”⁶³

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 and in 1937.⁶⁴ During the

⁶³ E. Wilson to J. Schumpeter, May 29, 1934 (PEBW, 24).

⁶⁴ J. Schumpeter to E. Wilson, Apr. 24, 1936 (PEBW, 27); 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]).

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.”⁶⁵ Until 1943, Wilson offered each one of these courses, alternating them every two years, while Leontief kept teaching the introductory mathematical course.

The courses

During the 1920s, Wilson had established a close relationship with Henderson, who was then developing his ideas about stable equilibrium as applied to social science and economics.⁶⁶ 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.”⁶⁷

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

⁶⁵ W. Leontief to E. Wilson, Feb. 3, 1933 (PEBW, 21).

⁶⁶ On Henderson and equilibrium in social science, see Russett 1966.

⁶⁷ Royce Club, PEBW, HUG4878.214, Box 1, Folder : Book reviews, letters to the Editor, p. 5.

purpose, use will be made of archival material found in Wilson's, Lloyd Metzler's and James Tobin's archives.⁶⁸ Wilson's published papers in economics, his and others' writings on dynamics and Gibbs thermodynamics will be of help, too.

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. He wanted to emphasize the greater level of generality that could be attained in economics with mathematics, when adequately applied. 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."⁶⁹

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.⁷⁰ In 1935, Wilson introduced the course by covering Arthur Bowley's *The Mathematical Groundwork of Economics* (1924).⁷¹ 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 the notion of consumer stable equilibrium.⁷² Over the years, Wilson complemented his lectures, leaving however a rather disordered track of this evolution in his

⁶⁸ Metzler attended Wilson's course in mathematical economics, probably during the spring of 1939; Tobin attended Wilson's course in mathematical economics during the spring of 1941.

⁶⁹ E. Wilson to F. Taussig, May 17, 1934, (PEBW, 24). On Evans's mathematical economics, see Weintraub 1998.

⁷⁰ E. Wilson to C. Roos, Oct. 6, 1934 (PEBW, 23).

⁷¹ E. Wilson to J. D. Black, July 14, 1936 (PEBW, 26).

⁷² 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.

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."⁷³ 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.⁷⁴ 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 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.⁷⁵

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.

⁷³ J. Schumpeter to E. Wilson, May 19, 1937 (PEBW, 28)

⁷⁴ E. Wilson, *Notes on Economics*, PEBW, HUG4878.214, 1.

⁷⁵ JTP, 7, Folder "Ec 104b E.B. Wilson", p. 204-6.

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.⁷⁶ Wilson's published papers in economics would have emerged from this part of his lectures.⁷⁷ 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* (1935a), 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.

In the section of the course in which 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.⁷⁸ 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.

⁷⁶ 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 (Carvajalino 2017).

⁷⁷ 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]).

⁷⁸ Wilson's *Gibbs' lectures on thermodynamics* (1936) probably illustrate the kind of insight about the Gibbs's mathematical style that he gave during his lectures to economists.

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” and, third, in both systems some inequalities, which were called by Wilson the Gibbs conditions, characterized the static stable equilibrium position.⁷⁹ 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.”⁸⁰

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.”⁸¹

Topics in Statistical Theory

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.⁸² 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.⁸³

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, 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

⁷⁹ LMP, 7, Folder Econ-theory: Harvard courses Notes 1938-1939, Wilson p. 6.

⁸⁰ JTP, 7, Folder “Ec 104b E. B. Wilson,” p.187.

⁸¹ LMP, 7, Folder Econ-theory: Harvard courses Notes 1938-1939, Wilson p. 10.

⁸² E. Wilson to W. Mitchell, Nov. 16, 1936 (PEBW, 27).

⁸³ E. Wilson to I. Fisher, May 25, 1932 (PEBW, 18).

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.⁸⁴ By explaining to Burbank that Alvin Hansen, who was appointed at Harvard in 1937, agreed with him on these matters, Wilson believed that this sort of study required “a dreadful lot of statistical work.”⁸⁵ In December 1938 in a meeting to coordinate the courses in mathematical economics, Leontief would have encouraged Wilson to cover dynamical economics to deal with business cycles.⁸⁶

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.⁸⁷

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.⁸⁸ 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.”⁸⁹ 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*.⁹⁰

⁸⁴ 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).

⁸⁵ E. Wilson to H. Burbank, May 14, 1937 (idem).

⁸⁶ E. Wilson to H. Burbank, Dec. 20, 1938 (PEBW, 30).

⁸⁷ E. Wilson, *Notes on Mathematical Statistics*, PEBW, HUG4878.214, 2.

⁸⁸ E. Wilson to F. Mills, Oct. 30, 1935 (PEBW, 25).

⁸⁹ E. Wilson to F. Mills, May 28, 1938 (PEBW, 31).

⁹⁰ 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).

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* (1911) and Edmund Taylor Whittaker and G. Robinson's 1924 *The Calculus of Observations* numerous. In brief, Wilson covered difference equations, gradation—Wilson's contemporary term to talk about curve fitting and other smoothing techniques—, sample theory, the theory of moments as well as certain distribution functions, Gram-Charlier series—as a method of approximating (asymptotically) probability distributions—, logarithm transforms, the method of maximum likelihood as well as interpolation—how to construct a curve or a function from a finite number of discrete points or values of a given variable.

His course, Wilson thought, was “not of very great advantage to a person who works with actual statistical material.”⁹¹ He probably felt that way 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 Wilson's feeling that a taxonomy of the dynamics of the aggregate economic system as found in business cycles based on postulational thinking was needed first; 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” was missing.⁹² His analytical statistics appeared therefore to be a discrete alternative to the econometric movement, developed for and presented only in Harvard classrooms.

4. Lasting influence

Wilson retired in 1945. His activism had been important for the rise of mathematical economics in the American academic national sphere through his leadership in Section K, which was key for American origins of the Econometric Society around 1930. However, as Wilson subsequently retreated from the econometric movement, mathematical and statistical economics of which would play a preponderant role in the subsequent evolution of economics, his influence in this academic nationwide sphere was rather punctual. Wilson's truly lasting influence in

⁹¹ E. Wilson to J. Schumpeter, May 30, 1936 (PEBW, 27).

⁹² JTP, 7, Folder "Ec 104b E.B. Wilson", p. 158.

economics was most of all embodied at the personal level, through the way his Harvard lectures framed the mathematical and statistical thought of one of his students: Paul Samuelson. The latter attended Wilson's *Mathematical Statistics* course during the spring of 1936 and his course in *Mathematical Economics* during the spring of 1937. Wilson subsequently strongly supported Samuelson's career (Backhouse 2014).

A few months after the publication of *Foundations of Economic Analysis*, which was an extension of his 1941 thesis, Samuelson expressed his indebtedness to Wilson's lectures. In his words:

"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."⁹³

In his *Foundations*, in a Wilsonian spirit, Samuelson used his operational knowledge of mathematics and statistics 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, following Wilson, 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 adopted Wilson's skepticism of classical statistics and probability, indeed of econometrics, Samuelson did not use statistical tests or regressions; regarding mathematics as a vernacular language, he rather attempted to *translate* the continuous into the discrete, which was more cogent with data. In consumer theory, Samuelson defined the stable equilibrium position as a discrete time-independent inequality. More generally, Samuelson presented the notion of equilibrium—at the individual (consumer theory and cost and production theory) and aggregate (business cycles) levels—as intuitive, treated it as a conventional theoretical idea and gave it certain mathematical structure, that could be defined in discrete terms. In this way, he developed a *taxonomical* framework to study the economy as a system, in

⁹³ PASP, Box 77, Folder E. B. Wilson.

which the microeconomic and the macroeconomic levels could be analyzed separately while being understood, intuitively, as interconnected. Under this Wilson-Samuelson perspective, *Foundations* can be regarded as a mathematical and statistical proposition tentatively providing an alternative to the contemporary econometric movement, from which Samuelson, like Wilson, tried to keep a distance.⁹⁴

In the last analysis, Wilson's lasting influence in the rise of mathematical economics was embodied in Samuelson's *Foundations*, which was framed within Wilson's ideas about mathematics and statistics and which would subsequently played a significant role in the rise of mathematical economics.⁹⁵

5. Conclusion

As Samuelson pointed out in the quotation opening this paper, Wilson knew everything and everybody. He successfully promoted the “no man's land” of inter-disciplinarity in America. Wilson was a community-builder. At the American nationwide level, 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 America. Wilson's punctual support enabled American mathematical economists to hold the scientific label with legitimacy, a legitimacy 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

⁹⁴ On Wilson-Samuelson connection, see (Carvajalino Forthcoming).

⁹⁵ Samuelson's commitment to Wilson's mathematics and statistics did not imply that he naively engaged with Wilson's own national and political prejudices. Samuelson was a Democrat and he would even become the economic advisor of President John F. Kennedy.

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's truly American attitude 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 also focused on analytical statistics rather than on pure probability theory, as Frisch and his contemporary econometricians, Wilson felt, tended to do. In this spirit, in his courses at Harvard, Wilson offered 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 was fully committed to Wilson's ideas about mathematics and who eventually wrote *Foundations of Economic Analysis* within a Wilsonian framework. *Foundations* would subsequently become a major contribution for the rise of mathematical economics. Indirectly, *Foundations* embodied in economics Wilson's efforts of connecting mathematics and science. It illustrated Wilson's argument that mathematical and statistical economics were different from pure mathematics. It also represented Wilson's efforts to convince (American) economists that mathematics and statistics could be beneficial for the development of economics as a science. During his whole career, these had been precisely Wilson's lines of work to modernize, in his terms, the disciplines of mathematics, physics, statistics, and other fields of social science.⁹⁶

If based in his attitude, he thought, modern economics, like modern science, could eventually serve to control and plan the economy and matters of society. Modern economics and modern science would eventually yield to modern society, and vice versa.

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⁹⁶ See (Carvajalino 2016).

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