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Ragnar Frisch (1895–1973) has arguably a stronger claim than anyone else on having originated the meaning of *econometrics*. He coined the term (in French) in his very first essay in economics, "Sur un problème d'économie pure" (Frisch 1926b).[1] The term itself may naturally be read from its Greek etymological origin as "measurement in economics." The opening lines of his essay ran as follows:

> Intermediate between mathematics, statistics, and economics, we find a new discipline which for lack of a better name, may be called *econometrics*. Econometrics has as its aim to subject abstract laws of theoretical political economy or "pure" economics to experimental and numerical verification, and thus to turn pure economics, as far as is possible, into a science in the strict sense of the word. (Frisch [1926] 1971)

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1. It was discovered in 1934 that the term had been coined in German—*Oekonometrie*—by Pavel Ciompa in 1910, but in the sense of bookkeeping; see Bjerkholt 1998, 33, and page 95 of vol. 4 of *Econometrica*.

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The definition was thus motivated by a desire to "turn pure economics . . . into a science." That the aim of econometrics was to "subject abstract laws . . . to experimental and numerical verification" could reasonably be read as a plea for (better) econometric methods, in the current meaning of the term. But that was only part of Frisch's purpose. He was equally concerned with how theory should be formulated in economics to fulfill positivistic scientific requirements. That meant that concepts should be given empirical meaning through operational definitions, and that statements of economic relationships should be formulated such that they (at least under ideal conditions) could be tested against observations and actually measured. Frisch's conception of econometrics in his 1926 essay might, however, be said to reside not so much in his verbal definition as in what he actually attempted to do in his essay and in the following ten years, during which he launched most of his very innovative research ideas.

Our aim is to elaborate the heuristic definition of *econometrics* developed by Frisch in the interwar years in order to see Frisch as a major step in the development of econometrics, a development that started in the late nineteenth century. Our article is part of an attempt to reconstruct Frisch's econometric agenda. We try to show how he was led to define *econometrics*. The loosely formulated idea of a connection between economic theory, statistics, and mathematics was not new at the time, but Frisch gave it a more specific definition; see Dupont-Kieffer 2003.

After the publication of his 1926 essay, Frisch introduced his new term and its proposed connotation to fellow economists, mathematicians, and statisticians, resulting eventually in the founding of the Econometric Society in 1930 (see Bjerkholt 1998). Frisch drafted the key formulations in the Econometric Society's constitution, stating that its goal was to "promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences."[2] Frisch had adopted from physics *quantification* as a new key word.[3]

He was not alone in this regard. The emerging scientific community of econometricians in the first part of the twentieth century frequently made

2. The key phrases of the constitution were stated on the inside back cover of *Econometrica* for several decades.

3. The term can also be traced back to Frisch's writings in 1926 and was first used in an article written in Norwegian (Frisch 1926a).

references to physics and appealed to physical analogies, as Marcel Boumans (1992, 2005) shows for the case of Jan Tinbergen, who borrowed from physics not only analytical tools but also a definition of *scientific investigation* for economics. But as set out in Le Gall 2007, Louçã 2001, Mirowski 1989 and 1990, and Rima 1995, early econometric work, rooted in the nineteenth century, had grown on the basis of analogies and transfers from the natural sciences, not only physics and mechanics but also biology, meteorology, and geology; see Klein 1995 and 1997 and Klein and Morgan 2001. The references to the natural sciences changed over time and explain the different meaning given by econometricians to *quantification* and *measurement*, and to the precise content of the articulation between the "theoretical-quantitative" and the "empirical-quantitative" aspects of econometrics.

In the Econometric Society's constitution, Frisch had pointed to the natural sciences as an ideal for economics to emulate with regard to "rigorous thinking," and his concept of a science was definitely physics. Remarks he made in various places indicated the view that for economics to become a science it should adapt and adhere to the principles and procedures of modern physics, which in Frisch's formative years had made spectacular advances, not least the theory of relativity.[4]

As explained by Claude Ménard (1981), the analogy is fruitful, implying as it does the transfer of methods and analytical and practical tools, as well as the appropriation and transformation of concepts that are then adapted to economic analysis and objects. But the concepts and laws imported from physics become different when applied to economics, and it is the distance between and distortion of the concepts and laws as they exist in physics and as they, in an adapted form, exist in economics that is interesting from an epistemological point of view. In Frisch's case he referred to the traditional framework of physics, involving reductionism and mechanism (Dupont-Kieffer 2003, chap. 5), but he also considered that modern physics was fragmentary as well (Israel 1996, 19), with an appropriately chosen mathematization applied only to parts of the universe and with a probabilistic perspective added.

4. Frisch attended a lecture series given by Albert Einstein at the University of Oslo in 1920, shortly after observations of the solar eclipse in 1919 had confirmed in a spectacular way the predictions of the general theory of relativity. It was Einstein's first lecture series outside Germany about the general theory of relativity and must have been a source of inspiration for the young Ragnar Frisch (Johansen 2005). Newspaper clippings about Einstein in Frisch's archival remains confirm his continued interest in and fascination with the great physicist.

Ragnar Frisch's role in the founding of econometrics in the 1930s is well known, and his scholarly contributions in this period are dealt with at length in the history of econometrics.[5] Among the early econometricians Frisch became known for his *measurement of marginal utility*, the main topic of his 1926 essay and his 1932 monograph *New Methods of Measuring Marginal Utility*. In the history of econometrics he is above all recognized for his *propagation and impulse* explanation of business cycles (Frisch 1933b) and for his *confluence analysis* cum bunch-map method for analyzing simultaneous economic relationships (Frisch 1934b).[6] Frisch is acknowledged for having paved the way for structural econometrics. He challenged econometrics to discover the mechanical and causal laws of the socioeconomic world, assigning it an approach resembling that of the science of Galileo and Newton, a science to control and to be controlled. Frisch's underlying conception of science can be understood with reference to Georgio Israel's *Jardin au noyer*: science should describe the mechanisms of the universe and reveal the laws ruling that universe (Israel 2000, 30). By discovering the laws, humankind can predict the future behavior of the universal mechanism and thereby control it. Structural econometrics is rooted in this ambition of determining causal, simple, and universal laws explaining the behavior of economic phenomena in a mechanist perspective.

The evolution toward structural econometrics at the beginning of the twentieth century with Frisch's work in particular is related to the evolution of a concept of "causality" among statisticians and economists. Economics had in the nineteenth century turned increasingly from the explanation of particular and historically dependent facts to the investigation of general laws. Several scholars have shown how economists increasingly raised the issue of causality, for instance, in the statistical investigation of economic cycles, as reflected in the development and use of specific statistical instruments such as correlation and regression; see Klein 1997 and Desrosières 1993. Theodore M. Porter (1986) has shown how statistically

5. See, e.g., Morgan 1990 and Hendry and Morgan 1995; cf. the first assessment of Frisch's lifetime work in Arrow 1960.

6. Despite his achievements and active role in the early econometric community, it may be argued that Frisch's overall econometric conception is insufficiently well known in the history of economics literature. Some of his key papers were published only in Norwegian (even only as mimeographs), others in publications that were hard to obtain or inaccessible. This was partly due to the fact that leading journals refused to publish papers heavily loaded with mathematical reasoning. On several occasions Frisch promised forthcoming major publications which did not appear, although they seem to have been virtually completed at the time.

oriented economists in the nineteenth century turned from "error" to "variability," how the law of error was slowly understood as the normal distribution, and how the focus moved from the "mean" to the "variability," and then slowly toward the relations between variables. Ian Hacking (1990) explained the rise of autonomous statistical laws induced by the use of correlation and regression, allowing observers to measure economic phenomena and formulate law-like relationships between them. Mary S. Morgan (1997, 73) elaborates on how economic analysis in the nineteenth century moved from the identification of sequences of causes rooted in a historical context toward "attempts to specify and explain lagged relationships in economics." Economists started to "think [of] the correlation statistic as a measure of the strength of an atemporal causal relation between variables" (75). The antagonism between the search for "causal laws" versus "historical laws" in business cycle analysis was still vivid in the 1930s, as in the debate between Frisch and John Maurice Clark (and indirectly Wesley C. Mitchell); see Dupont-Kieffer 2001, 2003. The debate reflected different views on the very nature of economic research and empirical investigation, and it was fed by the belief in a "hidden" or "underlying" structure of the economic system or mechanism. The belief reached "its zenith in econometrics in the 1940s" (Morgan 1997, 76).

The meaning of the term *econometrics* later shifted away from the connotation given to it by Frisch to a narrower meaning, as the study of statistical methods for the determination of economic models. Indeed, the shift was not least due to the wartime work of Frisch's pupil and assistant Trygve Haavelmo, who carefully titled his treatise "The Probability Approach in Econometrics" to indicate that it was an approach *within* the Frischian conception of econometrics. At the Cowles Commission the probability approach, as outlined by Haavelmo, became from 1944 the core and essence of econometrics, from then on a subdiscipline of economics.[7]

A major problem for historians dealing with Frisch's work is the lack of explicit references to his own conception of science, an explicit formulation

7. The Haavelmo revolution in econometrics is rendered in the history of econometrics as Haavelmo's profound contribution after he was no longer under the sway of Frisch and after he had been converted to probability reasoning by Jerzy Neyman. The story is not as simple as that; it may be closer to the truth to say that Haavelmo developed his probability approach within the framework of the econometric ideas of Frisch; see Bjerkholt 2005, 2007b. Haavelmo's concern with the estimation of simultaneous equations and the autonomy of relations was taken over from Frisch. Although Frisch never sketched anything in the direction of Haavelmo's probability approach, he was deeply concerned with probability in economics and has somewhat unfairly been depicted in the literature as an "anti-probabilist"; see Bjerkholt 2007a.

of the heuristic roots of his structural modeling approach. The authors have drawn on key documents from Frisch's archival remains shedding light on Frisch's overall conception of econometrics. Among these are a series of eight lectures on the "theory and methods of econometrics" given by Frisch at the Poincaré Institute at the University of Paris in 1933 (Frisch 1933a; a translation by the present authors was published in 2009 by Routledge), as well as extensive lecture notes from Yale University in 1930 (Frisch 1930). The Poincaré lectures offer essential insights into Frisch's world. They constitute the only integrated presentation of his work (statistical as well as theoretical) and the main entry point for understanding how Frisch wanted to articulate economics, statistics, and mathematics. He ended the lectures by stating in a somewhat compressed, even cryptic, form his own philosophy of science and chaos and his conception of the purpose of scientific investigation. The two hitherto unpublished sources constitute the backbone of the interpretation of Frisch offered in this article.

We start by considering in section 1 the historical context and Frisch's biography in order to identify influences and understand the path he chose in his early academic years in developing connections between empirical quantitative economics and theoretical quantitative economics. Section 2 elaborates briefly on the importance to Frisch of the social responsibility of the econometrician. Deeply rooted in Frischian econometrics is the conception that "reality," as represented by statistical data, could not be understood without a theoretical framework and that the foremost theoretical approach is the axiomatic one, as set out in section 3, followed by other innovative theoretical tools, such as strategic types and dynamics, in section 4. Frisch's explanation of business cycles and the modeling approach for bridging economic theories and observed data are dealt with in section 5. The issue of the empirical determination of economic relationships of structural models, in view of simultaneity and ensuing identification problems, led to confluence analysis, as discussed in section 6. Finally, in section 7, we discuss briefly his conception of the limits of knowledge and the chaotic outer world in the Poincaré lectures, with implications for his appeal to the social responsibility of the econometrician. Section 8 concludes.

1. The Making of an Econometrician

Frisch's scientific agenda and his ambition to reform research in economics competed for a time with other pursuits in his rich, busy, and colorful life. His main pursuits may have made his life coherent rather than pulling it in different directions. A biographical perspective can give us clues about the influences that structured his work and help us understand some of his scientific choices.

Frisch was an only child and destined to take over the family jewelry shop in Oslo. While he was trained to become a silversmith, he also completed school, qualifying for entrance to the university with excellent marks. His mother suggested he enroll in the two-year program in economics at the University of Oslo. After completing the program and at almost the same time his apprenticeship as a silversmith, his father made him partner in the family business. This made Frisch relatively well off and allowed him to be a silent partner while pursuing his scientific interests. He was abroad for almost three years in 1921–23, mostly in Paris, where he, without being enrolled in a formal program of study, delved deeply into mathematics and statistics and mingled with French mathematicians, statisticians, and economists such as François Divisia and Georges Darmois.[8]

At the end of his stay in Paris Frisch may have regarded himself more as a statistician than as an economist. He published some papers in mathematical statistics in the early 1920s and took part in an international mathematical congress in 1925. After his return to Oslo he submitted a doctoral dissertation in mathematical statistics and defended it at the end of 1926, the first dissertation in statistics at the University of Oslo (Frisch 1926c). Frisch had planned a final chapter in the dissertation that he in the end left out, with only a trace in the conclusion of what he had wanted to discuss in it:

> The inverse problem: how to reconstruct from an empirical distribution the scheme, which has given birth to the observed distribution, is a problem of a rather different kind. To deal with it in depth one cannot

8. Frisch gave a detailed account of his activities in his report to the university (Frisch Archive, Department of Economics, University of Oslo). He stated that he had intended to extend his mathematical knowledge by studying function theory but continued to study mathematics along with economics throughout his stay in Paris. He paid particular attention to the literature on verification of theoretical results in value theory but found little. He worked on methods for measuring the marginal utility of money and apparently completed parts of the essay that finally appeared as Frisch 1926b. He also worked on methods for analysis of time series. He followed lectures by Auguste Deschamps on "l'histoire des doctrines économiques," by Germain Martin on "économie industrielle," by Charles Rist on "la révolution monétaire internationale," by Elie Faure on "théorie de la statistique," by Henri Lévy-Bruhl on "affaires de banquet," and by Charles Gide on "la cooperation."

> avoid entering into philosophical issues and in particular into the theory of knowledge. It seems to us that too often scholars in statistics and mathematics have refused to enter into these philosophical issues, instead confining themselves only to technical questions. That is the reason in our opinion why the critical interpretation of the foundation and the methods of statistics has not kept in step with the development of techniques and the increasing range of applications of our discipline in the social as well as in the natural sciences. (Frisch 1926c, 86; our translation)

His remark suggested a key role for statistics not only in the analysis of empirical data, but also in the theoretical understanding of the real world.[9] Frisch was clearly concerned from the beginning of his career about profound methodological issues but dealt with them at times by throwing around observations like "theoretical economics is about to enter the phase of development at which the natural sciences, particularly theoretical physics, long have been, the phase in which the *theory gets its concepts from the observational technique*" (Frisch 1926a, 302; our translation). He added on various occasions that experiments or observations that served as a foundation for the definition of concepts did not have to be workable at present; for the logical definition it would suffice that they were workable in principle, existing as a logical construction, similar to the light signal experiments in the theory of relativity; see Frisch 1933a, lecture 1. Was there perhaps an influence from contemporary contributions in physics and already here a concern with the limitations of our knowledge of the outer world when observations have been contaminated by stochastic disturbances, which surfaced in the last Poincaré lecture (see section 7)?

Inspired by Vilfredo Pareto's work and by Irving Fisher's dissertation, Frisch attempted in his first essay in economics in 1926 to develop an operational approach to measuring the marginal utility of income.[10] In retrospect Frisch's estimation technique in the essay was perhaps not impressive with regard to statistical techniques, but the most important point he wanted to bring home was the importance of approaching the

9. As we shall see in section 5 an "inversion problem" in the sense suggested here arose in his approach to business cycle analysis and became an obstacle to further progress and publication.

10. Frisch had in Paris studied Fisher's 1891 dissertation in a French edition (Fisher [1892] 1917), sharing with Fisher the idea that to achieve a more scientific economics it was necessary to adapt the principles and methods of the natural sciences. On Fisher's conception of science, see Boumans 2005, chap. 6.

measurability of marginal utility *guided by theory*.[11] The primacy of theory was already one of Frisch's main tenets: the analysis of economic data was futile unless it was inspired and led by theory.

A further step in the making of an econometrician was the American experience. In February 1927 Frisch went to the United States on a Rockefeller fellowship. There he met with Irving Fisher for the first time. He asked him and Allyn Young for lists of people who shared his interest in what he had defined as econometrics and tried to meet with them all. There were not that many.[12] Naturally, he took the opportunity to promote the idea of an econometric association, which originally had arisen in correspondence with Divisia in 1926. It was a European idea but its realization would in the end take place in the United States.

During the year he stayed in the States he completed two treatises; both reflected his emphasis on statistics as the key to a more scientific economics. One was "The Analysis of Statistical Time Series" (Frisch 1927a) and the other was the "Correlation and Scatter" essay on the analysis of multidimensional economic data (Frisch 1929a). Both were critical of current practices. Frisch may at the time have been one of the very few aware of the problems related to the empirical analysis of economic time series data due to their nonexperimental character and the simultaneity of economic relationships.

Frisch 1927a was a treatise on the methods of time series analysis, critical of the current methods in use for determining trends and cycles in economic time series data. Frisch argued against Fourier analysis and periodograms for determining the components of a time series, and in favor of "local methods" to be applied in the environment of a single point (see Morgan 1990, 83–90).[13] Frisch's point was that economic

11. Frisch's theoretical assumptions were more restrictive than he realized at first, as they implied homotheticity, as shown by Abraham Burk (1936). The full story is told in Chipman 1998, 59–67. The interest in the "measurement of marginal utility," which in Frisch's version implied cardinality, waned after the rediscovery of Slutsky's 1915 article and the demand revolution in the mid-1930s.

12. Allyn Young provided a list of "practically everyone in the field of mathematical economics"; the list contained eight names. See Bjerkholt 1998, 33.

13. Thomas F. Cargill (1974) and Mary Morgan (1990) showed that early statistical economists used and relied on the frequency domain methods but temporarily abandoned them when faced with the difficulty of identifying regular and frequent cycles. But the issue of frequency still drove the investigation of business cycles. Spectral methods have reappeared since 1964 with the work of Oskar Morgenstern, Clive Granger, and Michio Hatanaka. One should have in mind Frisch's concern with identifying the superimposition of different and frequent cycles in a time series until 1927 and his dissatisfaction with the general methods in use compared to his own approach. Frisch (1927a, 1927b, 1933a, 1933b) introduced a less mechanical vision of the cycles than that induced by spectral methods.

cycles were changing in their characteristic properties and "total methods" were bound to fail. He later summarized the main ideas in Frisch 1928, introducing the term *changing harmonics* to express the idea of changing cyclical properties, using an analogy from mechanics to make his point:[14]

> Suppose we have a chain of *n* pendula: To a long pendulum with a great mass is attached a much shorter pendulum with a much smaller mass, and so on. Suppose the whole system is in movement in a field of gravitation whose intensity is slowly changing. The length of the individual pendula may also be slowly changing. The fluctuations of the lowest pendulum measured from the vertical through the point of suspension of the system, is given. The problem is to determine the individual components, i.e. determine the fluctuations of each pendulum measured from the vertical through its own point of suspension. (231)

If the interval of observation was long enough to encompass a significant change in the intensity of the gravitational field or in the length of the pendula, no kind of curve fitting with constant period sine functions would be successful. In particular the harmonic components determined by ordinary harmonic analysis would have no real significance.

The use of a physical model was another example of Frisch following in the footsteps of Irving Fisher, whose dissertation was amply illustrated by pictures of mechanic or hydraulic models.[15] They had similar inclinations to think in terms of physical analogies to visualize an economic mechanism. When drawing on analogies with physics, Frisch hastened to add that "the methods of natural science cannot unreflectedly be copied for use in economics" (Frisch 1926a, 302–3; our translation).

14. Thus the idea of changing harmonics originated in Frisch 1927a, although he did not coin the term until later.

15. On the long tradition in economics of depicting economic relationships by recourse to hydraulics, Morgan and Boumans (2004, 369) underline that "this metaphor lies behind a small but influential practice of hydraulic modelling in economics" and show how the hydraulic metaphor enriched the formulation of theoretical propositions in order to represent and clarify "the governing economic relationships between internal flows and stocks" (380). The authors stressed on the basis of the development by Bill (not Irving) Fisher of a three-dimensional hydraulic machine, the limits and the implications of the recourse to a metaphor and of borrowing concepts from a different scientific field. The economist has to find and specify the counterparts of the hydraulic machine in the economic sphere; see the list of ten elements in Morgan and Boumans 2004, 381. Bill Fisher was "constrained not only by the laws of hydraulics, but also by the modeller's commitments to his account of the economic world being modelled" (381). In some cases the metaphor could lead to misinterpretation and confusion due to the difficulty of translating outcomes from hydraulics to economics.

Frisch argued for the fruitfulness of the use of physical analogies in economics (316–17). He explained how the mechanical concept of "force" and the economic concept of "marginal utility" played similar roles in explaining the equilibrium (317). As we shall see in section 3, the consumer's choice could be and should be modeled as a vector field in physics. Frisch explained that the agent slides along in his budget plane influenced by a force with direction given by the marginal utility vector, until he hits upon a "point where the marginal utility vector is vertical upon the budget plane . . . and there is no longer any force exerted upon the agent" (319–20; our translation). In Frisch 1926a and 1926b it appears that his reference to physics is approached through a reductionist and a mechanistic prism. In addition to taking over the concepts of force and equilibrium, he also suggested adopting kinematics.

As shown by historians of econometrics, Frisch was not the first to turn to physics in order to find tools and ways to push econometrics toward quantification. What we want to underline here is the way Frisch combined theoretical and empirical approaches in his attempt to quantify economic relationships.

An interesting observation in this regard has been preserved in Frisch's archive. At the joint meeting of the American Economic Association and other associations in December 1927 Frisch took impromptu part from the floor in a panel discussion on the "present status and future prospects of quantitative economics."[16] Frisch's statement is of interest, as he took the opportunity to argue in favor of an axiomatic approach:

> We speak of one statistical procedure as giving a better result than another. The idea underlying this distinction is evidently that a statistical procedure is considered as a sort of approximation by which we try to determine the numerical magnitude or intensity of a certain phenomenon or the character of a certain function. . . . we engage in this kind of approximation work without knowing exactly what we are trying to approximate. We engage seriously in target shooting without having any target to shoot at. The target has to be furnished by axiomatic economics. The axiomatic process of target making must necessarily be rather abstract, a fact which accounts, perhaps, for its lack of popularity in these days when it is considered quite a virtue to disregard

16. The panel was chaired by F. C. Mills, who invited Frisch to submit his statement for publication, which he did, but in the end the editor decided to publish only the statements of the invited panelists. The panelists included Wesley C. Mitchell and E. B. Wilson, among others; see Mills et al. 1928.

> abstract thinking in economics. It is abstract, but neither in the sense of a logic game nor in the sense of metaphysical verbiage, of which we have had some in economics, at times. Axiomatic economics will construct its quantitative notions in the same way as theoretical physics has constructed its quantitative notions. (letter from Frisch to F. C. Mills, 21 February 1928, National Library of Norway)

Frisch's statement conveyed his critical attitude toward empirical studies of relationships that were not well defined theoretically. Or as he stated it more spectacularly on another occasion: "The material under observation is and remains a dead mass until it is animated by constructive theoretical speculation" (Frisch 1931b, 282; our translation).

The Rockefeller fellowship had been awarded for three years, the last two of which Frisch had planned to spend in Europe. But shortly after Frisch's arrival in Europe, his father became seriously ill and soon died. This put Frisch's entire career in jeopardy. He surrendered the fellowship to take care of the family business, which was in dire straits. He confided to Irving Fisher in the spring of 1929 that he was considering giving up his scientific career because of the precarious economic situation of and the responsibility he had for his family. Fisher responded by arranging for an invitation to Frisch from Yale University to spend time in New Haven as a visiting professor.[17] Frisch arrived at Yale in February 1930 and stayed until June 1931, with decisive importance for the development of his scientific ideas and for the emergence of econometrics, including the founding of the Econometric Society halfway through his stay in the United States.

In the spring of 1931 Frisch was by a special act of the Norwegian Storting (Parliament) called to a new chair in economics and statistics at the University of Oslo to prevent him from accepting an offer—generously equipped with research resources—from Yale University. After his return to Oslo negotiations with the Rockefeller Foundation about the establishment of a research institute at the University of Oslo were initiated and brought to fruition. From 1932 Frisch was director of his own Institute of Economics at the University of Oslo, thanks mainly to Rockefeller fund-

17. It is nevertheless hard to believe that Frisch, after his achievements up to 1929, would even consider giving up his scientific career. Fisher was quite wealthy; he discreetly gave Yale $3,500 for inviting Frisch and then offered Frisch a similar amount in addition to be his personal adviser and assistant. Fisher gave his commitment before the stock market crash, which eventually wiped out his entire wealth.

ing, with as many research assistants and as much computing equipment as he could afford—an econometric laboratory, it might well be called. The institute was designated from the beginning as a place to develop an integrated approach to empirical and theoretical investigations.

2. The Responsibility of the Econometrician

The scientific ambition of Frisch for econometrics was embedded in intellectual and heuristic requirements: the formulation of quantitative laws was not a matter of intellectual challenge alone but a matter of social importance. Toward the end of his life he reiterated this message when he warned the community of econometricians of letting it become "playometrics" (Frisch 1970a, 163).

"The Responsibility of the Econometrician" was the title of Frisch's article marking his return as editor in *Econometrica* after his wartime isolation and incarceration (Frisch 1946). The article reflected Frisch's almost euphoric enthusiasm of being at the center of events: whereas months before he had been clandestinely involved with Norway's preparations for postwar reconstruction, he was now at the head of a growing movement to apply econometrics to all sorts of present-day challenges. In the article Frisch impressed upon his readers that econometrics was indeed a tool for solving social and economic problems.

The motivation behind Frisch's demanding a more scientific economics had indeed not been driven purely by an intellectual interest in science as a human activity. When he lectured as a visiting professor at Yale in the fall of 1930, he impressed upon his students that the need for econometrics was an issue with far-reaching consequences for humankind's future; in fact, Frisch intimated, it was a matter of "life and death":

> Man has proved sufficiently intelligent to create a huge economic machine capable of producing a great variety of useful things. But he has not been sufficiently intelligent to understand how to *handle* this big machine. He stands beside his big machine, not knowing how to steer it, only hoping that the running of the machine will be not too disastrous to him. . . . We may only think, for instance, of the situation that occurs again and again in the production cycle: huge productive forces, machinery, and labor are idle at the same time as there are millions of people who want very badly a great variety of things that could be produced by the idle machinery and labor. Not only has man been able to create a big economic machine that he cannot handle, but he is

> making it bigger and bigger and more complicated all the time. He is constantly getting more handicapped in his attempt to steer it. . . . It is a race of life and death, and man is certain to lose if he does not succeed in developing economics into the state of a true science, that is, a study based not only on fact collection, but also on constructive theoretical thinking. (Frisch 1930, chap. 1.1)

Frisch was not the only one who had such concerns. The disastrous economic situation in 1930, both in the United States and in Europe, gave a good opportunity for emphasizing to the students the social importance of an econometric approach, for a better understanding of what was happening in the economy and to do something about it. But Frisch was not opportunistic. His conception of the econometric domain was a broad one. He presented it in his lecture in simple language as "five types of mental activity"; see table 1.

We note in passing that Frisch spread his life's work as an econometrician over all five activities. Frisch was not a theory builder, nor did he have a theoretical program, but he was highly concerned about the nature of theory. He was not a data collector, but he designed national accounting systems. He was not a policy maker, but he had strong views on how to make policy, etc. Thus we find Frisch covering all five activities, but primarily concerned with the methodology of each activity.

Although each of the five categories had its theoretical components, economic theory as such belonged under "the understanding procedure." The whole point of theory was to understand the real world. The essential feature of theory was to "bring a rational order into things," by which Frisch meant to make a model of the phenomenon under consideration. This may sound commonplace today, but it was not common around 1930 to model an economic system as a system of equations.

At the end of the nineteenth and at the beginning of the twentieth century, economists turned to the definition of sets of equations in order to account for the interrelations of economic phenomena (as in Walras's work) and the multiple causes at stake in business cycles (as in Moore's work on cycles). As shown by Morgan (1997), the development of equation frameworks in economics has to be related to the investigation of causal mechanisms and the combinations of causes in the economic sphere, and to the belief in an underlying or hidden structure or mechanism that only mathematics can identify and render.

Frisch (1930, chap. 1.1) introduced the idea to his student audience in the following way:

1. The descriptive procedure. One set of questions the scientist has to answer is, What happened? What is the situation? What course did the events follow? In order to answer these questions he has to engage in descriptive, historical, and experimental work. In some sciences, such as economics, direct experiment is more or less impossible and the scientist must rely largely on the descriptive and historical answers to the questions here considered.
2. The understanding procedure. Another set of questions that the scientist has to answer is, Why did it happen? Why did this situation exist? Why did the events follow the course they did? The answers to these questions constitute the rational part of the investigation. By the power of his mind the scientist tries to discern or impose some reasonable order onto what happened and the things he observed.
3. The prediction procedure. The questions here are, What will happen? What will the course of events be in the future? For these sorts of questions to have a meaning, the phenomenon must be such that it cannot easily be controlled by man. If it can be fairly completely controlled, no forecasting problem really exists.
4. The human purpose decision. Here the questions are, What do we wish shall happen? What do we wish the situation to be? The three first sorts of questions were exclusively of an intellectual character. On the contrary the sorts of questions here considered are ethical or moral. They cannot be answered unless we adopt a standard of social values. If the answer to such a question shall be socially significant, it must, of course, in some way or another weigh the opinions of the several individuals who make up the society. It is not a question of what you or I personally think in this matter, but of what is a socially fair position.
5. Social engineering. The question here is, What can we do to produce certain outcomes or certain situations? This last sort of question is the most complicated we can ask. In order to give a significant answer to this sort of question, we have to build on an analysis of all the first four sorts of questions.

Table 1 Five types of mental activities in which the econometrician has to engage. Source: Frisch 1930, chap. 1.1.

> The observational world itself, taken as a whole in its infinite complexity and with its infinite mass of detail, is impossible to grasp. Taken in its entirety, in its immediate form of sense impressions, it resembles, so to speak, a jelly-like mass on which the mind cannot get a grip. In order to create the points where the mind can get a grip, we make an intellectual trick: In our mind we create a little *model world* of our own, a model world which is not too complicated to be overlooked, and which is equipped with points where the mind can get a grip, so that we can find our way through without getting confused. And then we analyse this little model world instead of the real world.

Frisch then went on to convey the essentials of the art of modeling: "When we create the model world it is up to ourselves to decide which features and characteristics the model world shall have and what kind of relations shall exist between the various phenomena and groups of phenomena in the model world. This we can do because we are sovereigns in the model world, so long as we do not break the rules of formal logic." But are the decisions regarding the constitution of the model world then "ruled completely by free fantasy or caprice?" No, because the model world shall serve a purpose; it shall help us adopt a way of thinking that will "ultimately be useful in our fight for control over nature and social institutions." The model world shall picture "those indefinable things in the real world which we might call 'essentials' . . . with regard to our own ends" (chap. 1.1).[18]

But are there criteria to judge if the model world conforms to this ideal? No, there are no criteria that can be formulated as a definite logical rule:

> We have nothing except a mysterious, inborn "sense of smell" which as a rule will guide us so that we finally get on the right track. This is precisely the reason why the scientist is to be considered as a logical sovereign in his model world. He is just like a wise, absolute monarch. *He knows that this is the only way of ultimately obtaining his ends.* He listens to the suggestions of facts but takes care to consider them as non-obligatory. (chap. 1.1)

The model-world sovereign is guided, naturally, by observed empirical laws, which in idealized form—think of downward sloping demand curves—may be incorporated in the model world.

18. Frisch's conception of science may be viewed as close to what Israel (1996, 2000) names the Galileo-Newton paradigm, meaning that laws ruling the universe uncovered by science lead to control over phenomena, but the science is also assumed to be controllable itself.

Frisch thus introduced his theory of scientific investigation of the economy by means of mathematical modeling. He stressed the need to explore the model world. Despite the fact that we had ourselves created it, we could not overlook (examine) all the consequences; systematic investigations of the model world were needed.

In the Yale lectures Frisch distinguished *empirical laws* from *rational laws*, touched upon induction and deduction, and elaborated further upon what was meant by "explanation" within a model. He also discussed probability, sorting out various concepts of probability, and he hinted at how the model itself could be formulated in a probabilistic way, leading to probabilistic laws being derived from it. He finally discussed at some length the concept of "cause," although it was "perfectly possible to do without altogether":[19]

> If we strip the word "cause" of its animistic mystery, and leave only that part which science can accept, nothing is left except *a certain way of thinking*, an intellectual trick, a shorthand symbol, which has proved itself to be a useful weapon, legitimate or illegitimate, in our fight with nature and social institutions. As I see it, the scientific (as distinguished from the scholastic) problem of causality is essentially a problem regarding our way of thinking, not a problem regarding the nature of the exterior world. (chap. 1.1)

At the end of his introduction he returned again to the indispensability of theory for a comprehensive and accurate analysis of the phenomena under consideration. The phenomena often make much noise and attract attention to things that are inessential for a real understanding.

> The key to the phenomena is very often furnished by some feature which seems utterly unimportant from the empirical point of view. This is why the purely empirical and so-called institutional approach

19. Morgan (1997) shows how the concept of "cause" changed over the nineteenth century and the beginning of the twentieth century within economics, especially with the increasingly widespread use of correlation and linear regression. From the identification of *sequences* of causes, economists slowly turned to a more complex conception of causality by aiming at the identification of *combinations* of intertwined and multiple causes at stake: "We find multiple causes being used both before and after the introduction of correlation theory, but the ways in which they were conceptualized in each period differed. . . . The problem is not just that these statisticians and economists lacked the resources provided by multivariate analysis to cope with multiple causes; the deeper problem is that they also lacked what I suppose we could now call systematic or structural models capable of linking up causes in more complex ways, and thus facilitating analysis of their combination, interaction, and time sequencing" (Morgan 1997, 74).

> to economics is so dangerous. If we go to our economic or social investigations under the motto that we shall "let the facts speak for themselves," what we will hear will very often be childish talk. When it comes to really understanding a phenomenon, to gain an insight into its nature, not only to be familiar with its appearances, then the *discrimination*, this mysterious sense of smell which the real theorist uses in the construction of his model world, becomes basic. The only road to wisdom has been and will ever be to hear all things and *believe little*. That is why in the deeper problems of science the crucial contribution towards a real understanding of the phenomenon is always furnished by one of these heroic guesses transgressing observational facts. (chap. 1.1)

There was no recipe for theoretical advance, and we certainly cannot advance through generalizing from an observed fact to a theory. Frisch's "mysterious sense of smell" suggested an element of elitism and so did his exemplification, as Frisch mentioned Albert Einstein's theory of relativity as a "grandiose example" of a heroic guess, as was Isaac Newton's explanation of the orbit of the moon:

> In his imaginative mind he constructed a model world where bodies attracted each other with a force proportional to the masses of the bodies and inversely proportional to the square of their distances. He started exploring this model world and found that certain bodies would move in certain orbits, and one of these orbits that could be computed from the law of his model world was the orbit of the moon. The real discovery was brought about by a brain, not by a staff of patient observers. All the observational material would have been a dead mass if not animated by a theorist of genius. (chap. 1.1)

Frisch had at this time developed but not published his epoch-making solution to the problem of how approximately regular, persistent business cycles could be explained (see section 5). One may wonder whether he also considered his own work when he spoke about "heroic guesses" and "transgressing the observational facts." Business cycles had been the topic of the day throughout the years Frisch had taken an interest in economics. Large amounts of data had been collected and many analyses had been conducted in business cycle institutes. In Frisch's view, most of these laborious investigations had been "wasted because the investigations [were not] animated and directed by constructive theoretical thinking" (chap. 1.1). And even if they had not been wasted, they did not have the assumed relevance for understanding economic fluctuations, because the data collection for economic analysis needed to be guided by theoretical insights.

3. Axiomatics

Frischian econometrics was about "quantification," as stated in Frisch 1926a; econometrics should intertwine theoretical measurement and empirical quantification. It is in this perspective that we can understand why Frisch started his research by focusing on utility measurement. And it then clearly appears that Frisch could not imagine quantifying economic concepts without the help of a solid theoretical framework. This heuristic aim was recalled in the opening of the Poincaré lectures in 1933:

> The attempt at quantification in econometrics comprises two aspects of equal importance. First, we have the axiomatic aspect, i.e., an abstract approach that consists in establishing as far as possible logical and quantitative definitions and constructing from the definitions a quantitative theory of economic relations. Then we have the statistical aspect, and here we use empirical data. We try to fill the boxes of abstract quantitative relationships with real numerical data. We try hard to show how the theoretical laws manifest themselves at present in this or that industry or for this or that consumption category, etc. The true unification of these quantitative elements is the foundation of econometrics. (Frisch 1933a, lecture 1)

Frisch's axiomatic approach had first been set out in Frisch 1926b. Frisch's axioms for *homo economicus* were simple. The preference comparisons were made between displacements from an initial situation. With suitable mathematical regularity conditions added, the axioms determined marginal utility as an ordinal or cardinal concept, depending on whether the displacements compared were from the same or from different positions. Frisch was fully aware that only ordinal utility could be derived from observable choice, but empirical observations could be extended with interview data that would provide a foundation for cardinal utility. Frisch adhered to cardinal assumptions, often with an appeal to "everyday experience."

While Frisch's monograph on the measurement of utility (Frisch 1932c) got considerable attention, his axiomatic approach in Frisch 1926b was by and large bypassed in the journal exchange about utility issues.[20]

Frisch put the axiomatic approach up front when he was invited to give lectures at the Poincaré Institute in the spring of 1933. In the first of

20. Of the seminal contributions on the measurement of utility in the mid-1930s such as Lange 1934, Samuelson 1937, and Alt 1936, only Lange 1934 referred to Frisch 1926b, but without mentioning the axioms.

eight lectures, "The Philosophical Foundations of Econometrics, the Axiomatic Method, Utility as Quantity," he presented a set of axioms that encompassed and extended the 1926 axioms. The lecture and the axiom system were never published, and to fully convey his ideas we therefore devote somewhat more space to the axiomatic approach than to some of Frisch's other ideas.

The axiomatization of marginal utility thus became a prime example of the quantification of theoretical relationships, an entirely abstract approach consisting in establishing as far as possible logical and quantitative definitions and constructing from the definitions a quantitative theory of economic relations.

In the lecture Frisch tried to dispose of the need to account for psychological factors. He chose the example of a banking crisis, created by the distrust of depositors. Certainly there would be psychological factors at play in a run on the banks, but the whole phenomenon was driven by the fact that the public had observed over a period of time the development in objective and measurable circumstances of the economic situation. It was in fact a regular phenomenon that could be predicted as occurring at a certain stage in the economic cycle, when the objective conditions were ripe. Frisch (1933a, lecture 1) focused on the consequences of the choices and not on the determinants of the choices:

> The essential condition that must be fulfilled for the econometrician to be able to formulate his quantitative laws, is not that the psychological element is present, but that it manifests itself with a certain *regularity* in the empirical phenomena he observes, whether psychological or not. Regularity—or lack of regularity—is not necessarily linked to the absence or presence of a psychological factor.

The axioms were implicitly based on the assumption that the utility for a given individual could be determined by a series of *choice questions* "that we *suppose* have been posed" to the given individual. An essential element in the structuring of the choice questions was, as in 1926, the distinction between *choice situation* (or *choice position*) and *choice object*. The choice object was something subject to a choice, while the choice situation on the contrary was *given* in the setting of the choice and could not be altered before deciding between the choice objects. The nature of the choice questions was defined by a series of choice axioms. From the answers to the choice questions Frisch aimed to show how to deduce *choice coefficients*, representing marginal utilities. It was then only a short step to defining utility as a function of vectors of consumer goods.

The *choice situation* could be considered as a point in an abstract space with axes representing the various characteristics of choice situations. The *choice objects* were points in another abstract space, normally assumed to be a Euclidean space. In the statement of the axioms, which are in table 2, the notation is as follows: capital Latin letters (P, Q, R, . . .) denote vectors of choice situations; small Latin letters (a, b, c, d, . . .) denote vectors of finite changes; small Greek letters (α, β, γ, δ, . . .) denote vectors of infinitesimal changes; and ω denotes a vector of marginal utilities.

The justification for the list of axioms, Frisch told the audience, should not "be judged purely by a priori considerations. It is not called for to argue that one could, by reference to actual life situations, find situations more or less bizarre where this or that axiom is not satisfied. It is always necessary to remind ourselves that in economics we are interested in the general phenomena and not in the isolated cases." The scientific attitude behind the axiomatic structure was to investigate which consequences we could deduce adopting this or that axiom and then see whether the consequences agreed with the observations: "It is by the subsequent agreement of the consequences of the axioms with reality that we can judge the plausibility of them" (Frisch 1933a, lecture 1).

Frisch showed that the local axioms allowed the definition in any choice position of the *maximum direction* determining the relative marginal utilities associated with any choice position. That was the *local point of view.* Then from the interlocal axioms also the magnitudes of the utilities, characteristic for the individual in question, could be determined apart from a constant, as components of a vector associated with each point in the abstract space of choice positions. That was the *interlocal point of view.*

In Frisch's approach the choice positions and choice objects were at the outset inhomogeneous concepts that could not be compared. In most analyses of the concept of utility, the idea that the objects and the choice positions were comparable was adopted or taken for granted from the very beginning of the analysis by assuming that the two concepts could be represented by points in the same quantitative space with identical axes. Frisch insisted on the distinction to avoid "the false impression that commensurability between object and choice situation is necessary for establishing the concepts of *maximum direction* and marginal utility."

He then "let into the picture the idea of a certain *affinity* between the object space and the choice position space, i.e., considering the two spaces

1. Local axioms

1.1. Determination

$(P, a) > (P, b)$ or $(P, b) > (P, a)$ or $(P, a) \simeq (P, b)$

1.2. Transitivity

If $(P, a) > (P, b)$ and $(P, b) > (P, c)$, then $(P, a) > (P, c)$

1.3. Additivity

If $(P, \alpha) > (P, \beta)$ and $(P, \gamma) > (P, \delta)$, then $(P, \alpha + \gamma) > (P, \beta + \delta)$

2. Interlocal axioms

2.1. Determination

$(P, a) > (Q, b)$ or $(Q, b) > (P, a)$ or $(P, a) \simeq (Q,b)$

2.2. Transitivity

If $(P, a) > (Q, b)$ and $(Q, b) > (R, c)$, then $(P, a) > (R, c)$

2.3. Additivity

If $(P, \alpha) > (Q, \beta)$ and $(P, \gamma) > (Q, \delta)$, then $(P, \alpha + \gamma) > (Q, \beta + \delta)$

3. Affinity axioms

$P + a = Q$

3a. Contact axiom

$\omega_i (\omega_{jk} - \omega_{kj}) + \omega_j (\omega_{ki} - \omega_{ik}) + \omega_k (\omega_{ij} - \omega_{ji})$

3b. Potential axiom

$\omega_{ij} = \omega_{ji}$

4. Connectivity axiom

If $(P, a) > (Q, b)$ and $(P + a, \alpha) > (Q + b, \beta)$, then $(P, a + \alpha) > (Q, b + \beta)$

5. Reversibility axiom

$(P, a - a) = (P, 0)$

6. Roundabout indifference axiom

If c is a closed curve, then $(P, c) = (P, 0)$

7. Point determination axiom (or position axiom)

If $(P \rightarrow R) > (P \rightarrow S)$, then $(Q \rightarrow R) > (Q \rightarrow S)$

8. Combination axiom

If $(P, a) > (Q, b)$ and $(R, c) > (S, d)$, then $(P, a) + (R, c) > (Q, b) + (S, d)$

Table 2 The axiom system for quantifying utility. Source: Frisch 1933a, lecture 1.

as being to some degree characterized by commensurable concepts." That was a new logical element not entering into the definition of the marginal utilities but needed to do a systematic study of the way the marginal utility vector changed from one point to another in the choice position space, for example, to compare the rate of the change of the marginal utility vector with the change of the position. This was above all necessary to push the analysis all the way to a problem implying the idea of total utility.

The idea of affinity expressed that the acceptance in a given choice position of a certain object changed in an unequivocal way the choice position. Thus in a choice position *P*, accepting a certain object *a* moved the individual to another choice position *Q*, unequivocally defined by *P* and *a*.

With affinity, the choice positions might be represented geometrically in the same quantitative space in *n* dimensions as the choice object with axes measuring the quantities of goods. Each point in the space represented a choice position while any movement away from it represented a choice object. To each point in this space a vector ω—marginal utilities—was associated. The overall picture had become analogous to the vector field in physics. It could be called a *choice field*. Thus Frisch again affirmed his inspiration from physics.

In this vector field some special cases could be distinguished. First, there was the *local contact* case, meaning that the maximum direction changed from one point to another in the field such that the normal planes, so to speak, could be stuck together to make a family of surfaces traversing the choice situation space. The criterion for this case was that

$$\omega_i\,(\omega_{jk} - \omega_{kj}) + \omega_j\,(\omega_{ki} - \omega_{ik}) + \omega_k\,(\omega_{ij} + \omega_{ji}) = 0,$$

where ω_i was the *i*th component of the marginal vector and $\omega_{ij} = \partial\omega_i/\partial x_j$, as expressed by the contact axiom.

By accepting the interlocal axioms, we could consider an even more special case where the variation in the length of the vector fulfilled the integrability criterion expressed by the potential axiom:

$$\omega_{ij} = \omega_{ji}.$$

When the potential axiom was satisfied, the vector integral taken along some path is, as we know, defined by the end points. Then there existed a *potential*, that is, a function whose partial derivatives were exactly the components of the vector. In that case the contact axiom was a fortiori satisfied, and the contact surfaces now got a certain numerical meaning defined by the field vector and its length.

Frisch emphasized that the concept of a potential function was not the same thing as an indicator of the total choice of the individual. The analysis had so far not introduced any axiom permitting one to establish the consequences of total choice. It was admittedly true that if the potential axiom was satisfied, one could integrate the vector, but that did not imply an indicator for the total choice. Frisch (1933a, lecture 1) warned about false prophets:

> We are here at a crucial point where it seems to me that most authors who have dealt with the ophelimity problem have committed inexactness or even a fundamental error. They have assumed more or less implicitly that if the integrability criterion is satisfied in such a way that a potential exists, then that potential can be taken as total ophelimity.

But with integrability fulfilled, the potential is not necessarily a measure of total utility. To be able to interpret the vector integral as a fundamental choice index it would be necessary to use the *connectivity* axiom.

While the affinity axiom said that the acceptance of an object defines a well-determined change in position, Frisch argued that it was necessary to make precise what choice characteristics were attached to the new situation. That was exactly the content of the connectivity axiom, which defined a sort of transitivity between position and object. If the individual preferred the object a in the position P to the object b in the position Q, and if he further preferred the object α in the position $P + a$ to the object β in the position $Q + b$, then he would also prefer the object $a + \alpha$ in position P to $b + \beta$ in position Q. The object $a + \alpha$ was here defined simply as the object obtained by joining the curves (P, a) and $(P + a, \alpha)$ end to end.

The connectivity axiom thus presupposed the affinity axiom. Adopting the connectivity axiom implied that the vector integral of utility along a given path could be taken as a "total choice coefficient" (= total utility). The connectivity axiom allowed us thus to define the total utility quantitatively. It should be noted that this definition was completely independent of the integrability condition. The only difference was that if the potential axiom held, then total utility became a point function. If the potential axiom was not fulfilled, total utility became a functional, that is, a curve integral.

The decisive point about the existence of a total utility indicator was not that the marginal utility had a certain functional form as expressed by the potential axiom. The crux of the matter was rather to know whether any conclusions about total aspects could be reached by starting from marginal considerations. This is just what was expressed by the connectivity axiom. On this point Frisch parted with Pareto, who had attached the question of the existence of a total utility indicator to the integrability condition.

How then could total utility be defined, departing from the connectivity axiom? It could be defined by virtue of the connectivity axiom alone, only as far as the paths along which the integration was taken were such that the increase in the ophelimities, that is, $\omega\delta x$, remained nonnegative all along the path. But if the *reversibility axiom* was adopted, the definition of total utility as a vector integral along a given path would apply also in the general case where the increase in the utility $\omega\delta x$ did not remain nonnegative.

The remaining axioms were of less importance. The *roundabout indifference axiom* stated that the individual was indifferent with regard to the transaction taking him back to the starting point. The *point determination axiom* stated that if position R was preferred to S when starting from P, then R would be preferred to S also when starting from another position Q. With this axiom fulfilled, all the points in the field could be ranked in a unique order (allowing for multiplicities of indifference). Finally, the *combination axiom* expressed the possibility of enlarging the notion of an object.

Frisch then came to the natural question of whether the axioms were consistent and independent. Rather than systematic discussion of the issue he went straight to the core example of interest. Connectivity, even reversible, did not imply the roundabout indifference axiom. For this, one could simply imagine the case where the individual acted as if his marginal choice was determined by a vector field not derived from a potential and that his total choice was determined by the integration of that vector field along the transaction path. If connectivity was established, then the roundabout indifference axiom became equivalent to the point determination axiom.

If we had connectivity, then it would also be the case that the point determination axiom and the integrability axiom were equivalent. But if connectivity was not satisfied, the integrability axiom and the point determination axiom expressed two different things. Here Frisch proceeded to a formal proof before he ended his attempted clarification of the issues of integrability and the existence of a total utility indicator. The further discussion of this issue by other scholars followed alternative routes.

4. Innovations in the Formulation of Theory: Modeling, Static/Dynamic, Conjectural, and Micro/Macro Concepts

Frisch's further effort at "quantifying theory" beyond axiomatics and the general idea of modeling involved innovations in the formulation of theoretical statements. The latter are very important in the sense that they help to illuminate Frisch's mechanical conception of economics. Soon after dealing with utility measurement Frisch embarked on the design of a theoretical framework able to account for various market forms. The concepts of "forces" and "equilibrium" were already paramount in the two articles of 1926. It appears clearly that the "equilibrium" concept is a keystone in Frisch's theoretical framework, and it drives him to develop innovative concepts regarding market structure as well as to introduce time into economic analysis and, last but not least, to distinguish between *macro* and *micro* phenomena. All these new tools, Frisch believed, would enable econometricians to better understand and quantify fluctuations in economic activities.

We first consider Frisch's attempt to formalize market structures and introduce conjectural actions in markets, and then we deal with his better-known approach to give a more precise meaning and usage to the concepts of "static" and "dynamic." For both of these elements we draw on Frisch's discussion in the Poincaré lectures. Finally, we make some remarks on the term and concept of "macroeconomic," which may reasonably be regarded as having its origin in Frisch.

A very interesting idea for quantifying theory and involving equilibrium concepts was Frisch's attempt at formalizing market structures. As part of that he introduced *conjectural* action, a concept with strong overtones of game theory.[21] As Frisch did not follow this line of thought further, except in unpublished lecture notes, it cannot be called more than an attempt. He presented these ideas in the Poincaré lectures.[22]

Frisch introduced and discussed "strategic types" in the market; they are listed in table 3. Under the elementary adjustment the *quantity adjuster* needs no comment. Now if quantity is given instead of the price—say, if the buyer asks the producer at what price can he deliver a certain quan-

21. Frisch's work on conjectural markets is, indeed, included in the history of game theory; see Dimand and Dimand 1992.

22. Frisch 1933a, lecture 2. Frisch offered soon after a paper (in French) based on this lecture when asked for a contribution to a Festschrift for the Danish statistician Harald Westergaard, published in 1933. It was republished in English in the 1950s.

1. Elementary adjustment
A. Quantity adjuster
B. Stochastic price adjuster
C. Option receiver
D. Option issuer
2. Parametric action
A. Autonomous action
B. Conjectural action
C. Superior action
3. General negotiation

Table 3 Strategic types in markets. Source: Frisch 1933a, lecture 2.

tity of goods at a specified quality—it is a situation involving a tender of goods. The individual has one parameter, that is, the price, at his disposal, but he cannot be sure that he can finalize the transaction. He is a *stochastic price adjuster*. Frisch offered some clues on the shape and position of the stochastic supply curve implied by this strategic type. The *option receiver* is in a take-it-or-leave-it situation: he is offered both price and quantity, while the *option issuer* is someone who is in a position to force other agents to act as option receivers.

Frisch introduced the term *polist* for an agent in a market with limited competition. In the case of *parametric action* each agent controls a number of parameters, the total number of which may be large. Each polist's benefit depends upon the parameters set by himself and by the others. The distinction between the three kinds of parametric action is the attitude of the polists in this generalized competitive or game-theoretic situation. *Autonomous action* rules when each polist acts as if a small change in his own parameters will not induce any change in the parameters of the others. Hence, it is a generalization of a Cournot market.

Then *conjectural action* occurs when each polist takes into account the possibility that a change in his own parameters will induce a change in the parameters of the others,

> namely, that each polist acts as if the possible changes in the parameters of others will be (differentiable) functions of the changes in his own parameters. We introduce the elasticities
>
> (1) $$z_{ij}^{hk} = \frac{\partial z_i^h}{\partial z_j^k} \cdot \frac{z_j^k}{z_i^h}$$

> expressing the change in the parameter *i* of polist no. *h* which polist no. *k* believes will be induced when he changes his own parameter *j*. These coefficients do not necessarily express what will actually happen when polist no. *k* changes a little his parameter *j*, but rather what polist no. *k* *believes* will happen. For this reason I call these coefficients *conjectural coefficients* or *conjectural elasticities*, as different from elasticities expressing what will actually happen. (Frisch 1933a, lecture 2)

Finally, in the case of *superior action*, one group of polists acts autonomously, while another group acts conjecturally among themselves, while playing, so to speak, upon the behavior of the first group. The conjectural element entering into the considerations of the second group is only among this group.

On the basis of the defined kinds of parametric action, Frisch defined various derived concepts, particularly the *attraction force* of the various parameters, to analyze equilibria, friction, and cyclical oscillations in autonomous and conjectural regimes. One of the applications Frisch had particularly in mind and that he developed further in Norwegian lectures was the labor market with the interaction of trade unions and employers.

Frisch's definition of *statics/dynamics* was motivated by the imprecise use of these terms and the lack of precise definitions of concepts. The confused use of statics and dynamics often reflected unclear notions about equilibrium, say, that a situation was dynamic if it was out of equilibrium. The Frischian distinction between dynamics and statics may likely have been the outcome of the opposition between the "analytical dynamics" of Frisch and the "historical dynamics" of Wesley C. Mitchell; see Dupont-Kieffer 2001, 2003.

Frisch first formulated his idea that the concepts of "statics" and "dynamics" should be used to characterize the methods of analysis and not the phenomena themselves in a lecture in Copenhagen in 1928 published in Norwegian (Frisch 1929b). Perhaps recognizing that his innovation had not been too widely distributed, he chose it as one of the topics for the Poincaré lectures. In Frisch's terminology an economic relation of an entire economic system could be *stationary* or *changing*, while the model for analyzing such a system could be either *static* or *dynamic*:

> What will then be the difference between a static theory and a dynamic theory? I propose the following distinction: a relation with all variables entering into it referring to the same point in time is a static relation, while a relation comprising variables referring to different points in time is a dynamic relation. A static theory will be an analysis with only

> static relations, according to this definition. Likewise, a dynamic theory will be an analysis comprising at least one relation that is dynamic in the sense I have indicated. (Frisch 1933a, lecture 3)

Was static analysis then to be used for stationary phenomena and dynamic analysis for changing phenomena? No, not at all. Frisch counterpoised these two dichotomies by attempting to show that all four combinations of them might be perfectly meaningful, that is, (1) static analysis of stationary phenomena, (2) static analysis of changing phenomena, (3) dynamic analysis of changing phenomena, and (4) dynamic analysis of stationary phenomena.

In the Poincaré lecture he illuminated the issue by using mechanical and economic examples for all four combinations to drive his point home. The mechanical example was of a rod of iron fixed at one end to a pivot and at the other end to an iron ring, inside which there was a small ball that could slide inside the ring, while the economic example was a slightly contrived (for a modern reader) simple market case; see table 4.

Summing up in a pragmatic way these simple cases, he also gave the reason for his insistence on a dynamic model for his business cycle approach:

> These examples show you how it can be justified from the specific concrete conditions and also according to the underlying object of investigation sometimes to apply a static theory and sometimes a dynamic theory. The justification of a simple analysis by a static theory is often the fact that the phenomenon under investigation adjusts very rapidly to the changing factors it depends upon. And likewise if the adjustment speed is not very great the static analysis can still be justified if the changes in the factors do not happen very frequently and if we only are interested in long-run effects. But if the adjustment of the phenomenon under various conditions is not rapid, because there are frictions or inertia, and if, even more, the conditions the phenomenon depends upon change frequently, then a static analysis will not have a *raison d'être*. That is precisely the situation where it is absolutely necessary to develop a dynamic theory if we want a true analysis of the phenomena under consideration. (Frisch 1933a, lecture 3)

Frisch did unfortunately not publish much on his conception of equilibrium, but he dealt with it at length in the Yale lectures, distinguishing between *assumption-equilibrium* and *situation-equilibrium*. The *assumption-equilibrium* referred to the model world; it was a definition of the

	Static analysis	Dynamic analysis
Stationary phenomena	1. Consider the demand for a given good, assuming that the price immediately finds its new level after a change in the quantity. If we have found by observation that the price remains stationary during a certain time, then it will be natural to introduce the idea of a static demand curve. In other words we can try to explain the constancy of the price by the constancy of the quantity in the market. We will then have explained *a stationary phenomenon by a static theory.*	4. Finally, if we want to explain the final level of the price and its constancy in a more profound way, e.g., why economic forces tend toward a stable equilibrium represented by the asymptotic level of the price, then it will be necessary to enter into the picture a dynamic theory to show the interplay between forces, namely how the dynamic elements of the situation tend to generate a stable level. We would thus have to analyze *a stationary phenomenon by means of a dynamic theory.*
Changing phenomena	2. On the other hand, if we have observed during some time a systematic development in the price and at the same time a systematic development in the quantity, it may seem plausible to try to express this by means of a static demand curve. Then, we will have analyzed *a changing phenomenon by a static theory.*	3. If the market does not adjust rapidly, the quantity may depend not solely upon the price, but also on the rate of change of the price (and other dynamic elements). Such a theory would explain the movement of the price, the rapidity of its fall before it starts to increase again, etc. This will be an example of *a dynamic theory of a changing phenomenon.*

Table 4 Static/dynamic analysis vs. stationary/changing phenomena. Source: Frisch 1933a, lecture 3.

characteristics of the model world. Hence, it meant nothing to ask whether it was fulfilled or not. The *situation-equilibrium* on the other hand referred to the real world, as a characterization of a situation.

> A stationary equilibrium is not the same as a static equilibrium, not any more than a rainstorm is the same as that part of meteorology which is concerned with rainfall. The stationary equilibrium is something characterized by a particular kind of situation that might arise under certain circumstances, the emergence of which it is the object of theory to explain, and this explanation may be attempted either by a static theory (involving the idea of static assumption equilibria) or by a dynamic theory (involving the idea of dynamic assumption equilibria). (Frisch 1930, chap. 1.4)

The Yale lectures also elaborated upon the concept of moving that was implicit in Frisch's business cycle model. As showed by Philippe Le Gall (1999), the notion of a "moving equilibrium" was already envisaged by Moore (1914) in his approach to economic cycles. The main innovative distinction provided in the Cassel Festschrift model between impulse and propagation phenomena is rooted in the Frischian investigation of time in the economic analysis started in 1927 (Frisch 1927a, 1927b, 1928, 1931a). This distinction allows him to reconcile the theoretical statement on the economic system automatically driven to an equilibrium position with the empirical observations of recurrent and persistent cycles of different duration and magnitude. The impact of his contribution is wider, as shown by Andvig (1981, 1984, 1985), Bjerkholt (1995), Dupont-Kieffer (2003), Morgan (1990), and Le Gall (1993, 1994): by defining *macrodynamics* he allowed the investigation of economic phenomena at an aggregate level with a dynamic approach. This possibility was offered in a way by recourse to mechanical metaphors, including the Le Corbeiller and Van der Pol theoretical frameworks accounting for pendulum movements and oscillations (Le Gall 1994).

The second major improvement in the analysis of business cycles brought about in Frisch's contribution to the Cassel Festschrift was the distinction between micro and macro analyses. Frisch dealt at length with this distinction in the Poincaré lectures, as different from the Cassel paper, where a drafted section on a microdynamic analysis had to be dropped at the behest of the editor, as Frisch was far beyond the page limit. The micro/macro distinction can be traced back to Frisch's work on statics/dynamics (Frisch 1929b, 323) and the Yale lectures, before it appeared in the Poincaré lectures. The seminal distinction between micro and macro was first applied by Frisch to economic objects, rather than economic relations. Frisch then conceived the idea of modeling the entire economy at a macro level, ignoring details of the functioning of the economic system, but facilitating the understanding of the nature and causes of the fluctuations of economic activity:

> I shall instead now deal with the study of global systems. In such systems it is impossible to introduce as much detail as we do in the partial systems. Of course, we could always introduce all sorts of details in a formal way, i.e., to introduce a system of symbols, themselves defined by all sorts of indices indicating the various producers, the various consumers, the various traders, etc. For such a procedure we could naturally in a certain sense keep count of the equations and variables and assure ourselves that the problem was determined. However, that would be a totally formal procedure which could serve as a general overview of the nature of the interdependence. To study the temporal pattern of a global phenomenon in a profound way it will be necessary to limit the study. We cannot envisage so much detail as we can do in the partial system case. The most important variables we can envisage in a global system are indicated in . . . *tableau économique* of the same kind as developed by the physiocrats. Our table is, however, adapted to our special purpose of studying the movement of a determined system of dynamic equations. (Frisch 1933a, lecture 4)

The *tableau économique* accompanying the text was the French original of the graph in Frisch 1933b.

Frisch gathered these innovative tools in order to provide an explanation of the fluctuations in economic activity. From the perspective of the history of econometric ideas, the Cassel model comprised three points of interest: first, it provided an explanation of the nature of the business cycle with the introduction of the accelerator principle within the macroeconomic framework;[23] second, it explained cycles as the outcome of interacting differential and difference equations; and last but not least it

23. Frisch had undertaken an investigation of reinvestment cycles in 1927, inspired by the work of Norwegian economists E. Einarsen and K. Schønheyder, as well as Albert Aftalion. The investigation led to a confrontation with John Maurice Clark in the *Journal of Political Economy* (Frisch 1932a, 1932b), referred to by Frisch in the Poincaré lectures, stating that he linked the origin of the propagation cycles to lagged investments and then explained it by the accelerator principle defined by Aftalion (Frisch 1933a, lecture 4).

put forward the structural modeling approach as the way to reconcile theoretical and empirical quantification.[24]

5. Probabilistic Shocks and Deterministic Laws: The Explanation of Business Cycles

Frisch's business cycle analysis is best known for the model he presented in his "Propagation and Impulse" essay in the Festschrift for Gustav Cassel (Frisch 1933b) and widely discussed in the literature (Morgan 1990). The model, also known as the *rocking horse model*, is based on an idea developed by Knut Wicksell in 1907, making the distinction between the impulse effects, which occur when the rocking horse is hit by a wooden stick, and the propagation effects, the movement of the rocking horse.[25]

But the influence cannot be restricted to Wicksellian economics. The appearance of Slutsky 1927 during Frisch's visit to the United States in 1927 came to have a crucial inspirational influence on his thinking about how cycles were created and maintained. Slutsky wrote in Russian, but the five-page English summary conveyed the main point of the paper, namely, "to show that cyclic processes may originate owing to a summation of the mutually independent chance causes, and that these chance

24. Due to Frisch's incomplete publication of his work, the "Propagation and Impulse" essay may have been interpreted with too much emphasis on the content and properties of the specific macroeconomic model as Frisch's business cycle model per se. Frisch was showing off in the Festschrift article with his "discovery" that his model generated the two most common cycle lengths in the conception of business cycle analysts at the time, and in addition predicted a third shorter cycle. His real message was to demonstrate his overall paradigm for macro analysis in economics (applicable in micro settings as well) and to corroborate that his quite sophisticated paradigm offered the adequate structure for a scientifically appropriate explanation of more or less regular fluctuations; see Morgan 1990, 99. Ironically, Frisch erred in his presentation. The model, which has been studied more than any other business cycle model, did not generate cycles; see Zambelli 2007.

25. Frisch credited Wicksell with the idea of erratic shocks hitting a system, accounting for the source of energy which maintains the economic cycles, erratic shocks. Wicksell should have conceived, according to Frisch, more or less definitely of the economic system as being pushed along irregularly. These irregular jerks may cause more or less regular cyclical movements. Frisch (1933a, 79) illustrated it by this illustration: "If you hit a wooden rocking-horse with a club, the movement of the horse will be different from that of the club." Frisch derived this illustration from Wicksell's article of 1907, referenced by Frisch as Wicksell, "Krisernas Gåta," *Statsøkonomisk tidsskrift* 21:255–86. In fact, it seems that the first reference of Wicksell to this idea has to be sought in Wicksell's 1918 article "Karl Petander: Goda och dårlige tider," *Ekonomisk tidskrift* 11:71.

waves may show a certain regularity being an imitation, in a lesser or greater degree, of the strictly periodical fluctuations" (Slutsky 1927, 156). Slutsky's assertion that manipulation, such as the smoothing of statistical data by moving averages, could generate artificial waves appealed immediately to Frisch, thinking of an economy as represented by a (linear) dynamic structural model exposed to stochastic shocks.

Frisch 1933b stands out as his only contribution to business cycle analysis. It was preceded by the Poincaré lectures, which discussed the explanation of cycles, particularly lecture 5, "The Creation of Cycles by Random Shocks, Synthesis between a Probabilistic Point of View, and the Point of View of Deterministic Dynamic Laws." The explanation of business cycles was part of Frisch's most ambitious research project before World War II, but the intended main publications from the project never appeared. For this reason the project must be deemed an unsuccessful one.

The "impulse problem" was to explain how the damped cycles, which were found to be the solution of the propagation problem, could give rise to the sustained swings that were observed in economic systems. The Wicksell-Slutsky-Frisch paradigm was that a stream of "erratic shocks" energized the damped swings of the economic system, the famous "rocking horse" model.[26]

The propagation-impulse mechanism was a qualitative result with a strong intuitive appeal: the shocks energized the damped cycles and prevented them from dying out. But Frisch wanted more than that. Also the effect of the shocks could be quantified, in their effect upon the cycles generated by the model. This was the *changing harmonics*, which together with the *structural-economic theory* constituted the two key elements in Frisch's approach. Frisch (1933b, 342) defined a changing harmonic as "a curve that is moving more or less regularly in cycles, the length of the period and also the amplitude being to some extent variable, these variations taking place, however, within such limits that it is reasonable to speak of an *average* period and an *average* amplitude."

Slutsky's work had showed that if one applied a moving average to erratic data, there would emerge cycles with length related to the length of the period of the moving average. The existence of these cycles could be proved both theoretically and experimentally. This was a phenome-

26. See, e.g., Morgan 1990, chap. 3.2, "Frisch's Rocking-Horse Model of the Business Cycle."

non of the same nature as the explanation of the persistence of economic cycles and the creation of new cycles.

The characteristics of the sustained cycles could be explained partly by the weight system (the structural-economic model) and partly by the distribution characteristics of the shocks. Already by 1934 Frisch had arrived at the following general conclusions:

> The fundamental characteristics regarding the time shape, as (1) the *average length of the cycles*, (2) *their relative average intensity*, i.e. average amplitudes, and (3) *the beating effect*, could all be explained only knowing the structural-economic solution, i.e. the weight function.[27] The *absolute intensity of the cycles* required knowledge of the average standard deviation of the shocks, but not the actual distribution. Finally, the exact timing, the *phase of a given cycle*, i.e. whether at a given moment it shall be in maximum or minimum, depended upon the actual distribution of shocks. (Frisch 1934a)

Thus the dynamic structural-economic theory only furnished one part of the explanation—the other and equally important part was the superstructure of the general theory of changing harmonics.

> I showed [in my Cassel Festschrift contribution] that if a set of variables are defined by a linear system (i.e. one whose structural dynamic equations are linear in the unknown time functions), the time shape of one of the variables, *when hit by shocks*, is obtained by extending to the shock series a moving summation whose weight system is exactly the same sort of curve as that which would have given the time evolution of this variable, if no shocks had occurred. (Frisch 1939, 640)

Or shorter and more succinctly: Economic theory furnishes the weight system, statistical theory does the rest! But Frisch's full presentation of his model with all ramifications never materialized. A manuscript for a major publication seems to have been ready for publication in 1933–34, but never appeared.

One reason for this outcome was that Frisch had raised the ambition in his project to encompass two kinds of shocks. The regular random Wicksellian rocking-horse shocks caused no changes to the model.

27. The beating effect was the variation in amplitudes and frequency that would occur in the fluctuations of a time series that comprised more than one sinus component.

Schumpeterian shocks on the other hand would change the model. Frisch introduced different terms for these two kinds of shocks, calling them *aberrations* and *stimuli*, respectively. Thus a new problem would be to account for the effect of stimuli.[28] The innovations were not randomly distributed, but might still energize the swings of an economic system or even cause a "secular or super-secular movement" by changing the economic mechanism (Frisch 1933b, 205).

But there was also another hard-to-reach ambition, namely, "the inversion problem," as stated by Frisch in 1936. Suppose a statistical time series was given, produced by the propagation-impulse effect of energizing a theoretically explained cycle.

> Is it possible to determine the weight curves by which the random disturbances have been accumulated? And, second, is it possible to measure the random disturbances themselves. This was the structural decomposition problem. (Frisch 1936, 16)

Considerable efforts were exerted at Frisch's Institute of Economics pursuing these targets. The method of attack was a combination of theoretical analysis and the construction of numerical models, but apparently this never led to definite and comprehensive results. The motivation for the search for the solution to the inversion problem was to provide a better basis for forecasting. Instead of attempting more or less mechanical extrapolations of statistical curves, "one is led to consider forecasting as an extremely complex problem the solution of which depends on a previous successful solution of the structural decomposition problems" (Frisch 1936, 17).

Frisch did not succeed in solving the "inversion problem," and worse than that he published practically nothing more after 1933 about his immense effort and prestigious project on business cycles. His involvement with random shocks was profound, but his attempt to determine them turned out to be futile. Perhaps the most positive outcome of Frisch's effort was that he may have inspired his chief assistant and student, Trygve Haavelmo, to turn the problem around some years later in his "Probability Approach" (1944).

28. "The existence of stimuli entails much more far-reaching consequences. The total time shape will now be more or less transformed, for instance, damped cycles will become undamped in the long run, but will have a disturbing effect over shorter intervals. The timing between the cycles may be changed from what it is in the stimulus-free system, and entirely new, pure accumulation cycles will emerge" (Frisch 1938, 15).

6. The Empirical Determination of Economic Relations

One of Frisch's assertions was that astronomy as a field of study was more "scientific" than any other field of study. This was so because

> in astronomy the fusion between theory and observation has been realised more perfectly than in the other fields of study. The astronomical observations are *filled into* the theoretical structure. It is this unification that raises astronomy to the dignity and significance of a true science. Also in economics we have had theoretical speculations, but most of the time [they have] not been [made] with a view to being verified by observations. Economic theory has not as yet reached the stage where its fundamental notions are derived from the technique of observations. (Frisch 1930, chap. 1.1)

Perhaps this could be misinterpreted as another attempted revival of Newtonian science as an ideal for economics, but that was not at all what Frisch was after. He contrasted the "unification" he praised in astronomy with the situation in economics. Not that there was a lack of observational data in economics; on the contrary, since the nineteenth century an overwhelming amount of statistical and historical economic facts had been compiled. Nonetheless, economics did not really take off as a science.[29]

> Observations [in economics] have not been guided and animated by constructive theoretical thinking in the same way as the astronomical observations. Theory and observations in economics have gone along in a more or less disconnected way. There have been cycles of empiricism and rationalism. At times when it became too obvious that economics

29. As pointed out by Porter (2001, 5), "From the late eighteenth century to the early twentieth, methodologically reflective pioneers of social science generally saw their charge not in terms of following the one true path of science toward mathematical exactitude but in terms of a consequential choice between mechanical and organic models, requiring more or less modification to be applied to society." And for a long time, references to historicist and organic approaches were favored to the detriment of physics, and thought experiments were more valuable than actual manipulations. Porter showed, along with Collins and Hacking, how important laboratories and experiments that used practical measurements had been in the development of modern science and the paramount importance given then to mathematics and statistics in the social sciences, while neoclassical theory did not encourage measurement in practice. However, more and more "practical demands" coming from the administrative world and civil society pushed economic "measurers" toward developing tools to put numerical values to economic objects and relationships, favoring then the analogies between the economic sciences and mathematics, the natural sciences, and the engineering sciences.

> did not progress so rapidly as, for instance, astronomy, physics and biology . . . some economists would lose confidence altogether in theoretical thinking in this field and plunge themselves into a pure empirical fact collection. . . . Then again it became obvious that such a pure empiricism did not lead anywhere, theoretical speculations in economics had a revival and the abstract-minded type of people ruled the ground for a while. (chap. 1.1)

Rather than "cycles of empiricism and rationalism," what was needed in economics was a "new fusion between theory and observation." To achieve that, economics required a "theoretical structure which is such that it is capable of being connected directly or indirectly with actual numerical observations." In other words, as Frisch explained,

> The true theorist in economics has to become at the same time a statistician. He has to formulate his notions in such a way that he gets a possibility of ultimately connecting his theory with actual observations. I know of no better check on foggish thought in economic theory than to have the theorist specify his notions in such a way as if he were to apply the notions immediately to some actual or hypothetical statistical material. (chap. 1.1)

Frischian structural modeling should then be understood as a new methodology for connecting theoretical and empirical quantitative economics, a perspective well identified by Morgan and Morrison (1999, 10–11):

> Models are one of the critical instruments of modern science. . . . they function as *instruments* of investigation. . . . It is because they are neither one thing nor the other, neither just theory nor data, but typically involve some of both (and often additional "outside" elements), that they can mediate between theory and the world.

Frisch 1929a was a profound and inventive introduction to data analysis that provided the basis for his later work on simultaneity and confluent relations. It remained almost neglected in the literature, notwithstanding the fact that it foreshadowed both principal component analysis, credited to Harold Hotelling (1933), and generalized least squares, credited to Alexander C. Aitken (1934). Frisch's treatise also scored on new concepts and methods of presentation, including the first use of linear algebra for econometric analysis (often credited to Aitken).

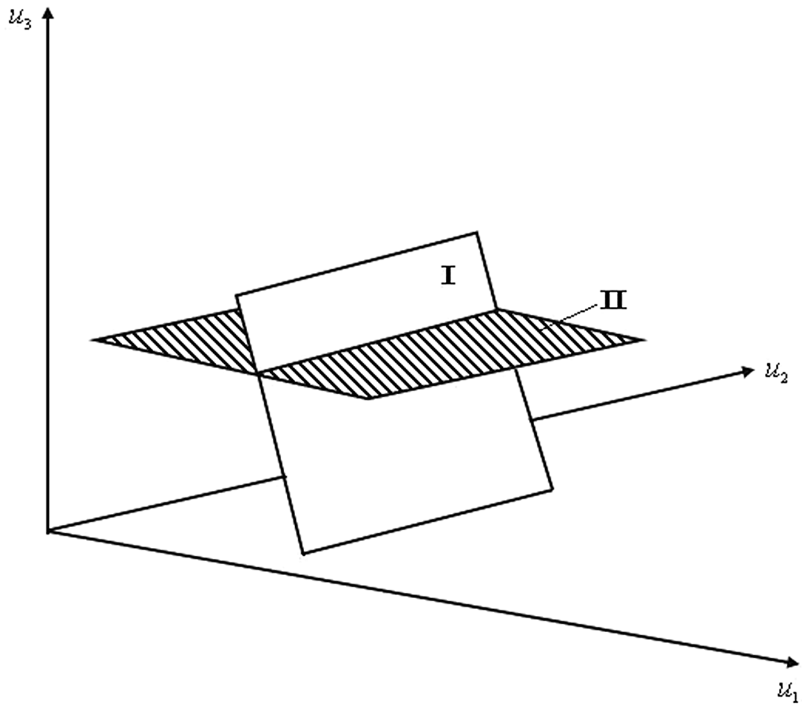

Figure 1 Two simultaneous relations in three variables

It is really in Frisch 1929a that we find the origin of the ideas that led to the focus on *simultaneity* and *autonomy* in Haavelmo's probability approach. Frisch studied in a completely nonprobabilistic setting how observations scattered and clustered in an observation space when relations were represented by geometric planes of different orders, noting the mathematically trivial fact that when two relations were assumed to be fulfilled at the same time the observations would be scattered around or close to the interface of the two planes representing the relations (see figure 1). This led him to the reflection that when we analyze a part of the real world, which we then would think of as represented by a structural equation system, we would not in general be able to determine from data the individual relationships, as the observations all would be found along the interfaces of the jointly fulfilled relations. He developed this idea in the late 1920s, without fully working it through in all consequences, but clearly he was thinking at that time in terms of autonomous and confluent relationships as concepts for describing this situation.[30]

Frisch's main project was still the explanation of business cycles. What bothered him was the limits set by simultaneity to the extent we could identify (without using this term) the model from data. Hence, the problem of simultaneity was closely connected to his business cycle approach, although in the history of econometrics (e.g., Morgan 1990), Frisch's business cycles and his confluence analysis are dealt with literally in separate chapters. To Frisch they were closely connected.

Frisch's confluence analysis book was published in 1934. In Poincaré lecture 6, "The Statistical Construction of Econometric Functions, Autonomous and Confluent Equations, the Danger of Analysis of Many Variables," he introduced the key issues, briefer but also in a more attractive way than in the 1934 book. This is not least true for his key distinction between structural (autonomous) and confluent relations. He introduced the idea by means of an example, which is presented in table 5.[31]

Thus for the structural relation and also for the deflated confluent relation, the coefficients have a well-defined meaning; in the case of the inflated confluent relation, however, this is not the case: only the *sum* of the two constants has any significance. Frisch (1933a, lecture 6) commented:

> Suppose that we have by a priori considerations reached the conclusion that the area of a rectangle must be of the form (5) [i.e., equation 5 in table 5] where b_1 and b_2 are certain constants, and assume that we have at our disposal a certain number of rectangles, given as pieces of homogenous cardboard, and that we weigh each piece. If there is an assortment of rectangles of different length and height, we shall find that we can determine b_1 and b_2 as equal to unity, with a small error of no importance. But, on the other hand, let us suppose that the empirical material at our disposal only comprises squares. If we then try to determine b_1 and b_2 by calculating the regression of y with regard to x_1 and x_2 by the

30. The terms *autonomous* and *autonomy* were in fact not used in print in the econometrics sense before Haavelmo's "Probability Approach" in 1944. Haavelmo referred to Frisch 1938, which was an unpublished mimeograph, while Frisch had coined the terms in 1931 in a document that was never published.

31. The example originated in the Yale lectures. His terminology changed over time. He had coined *autonomous* in 1931 but hardly ever used the term in ensuing works. He did not use "inflated/deflated confluent relations" in Frisch 1934b, which was entirely devoted to computational methods.

Let y be the log of the area of a rectangle, x_1 the log of the base, and x_2 the log of the height.

(1) $y = x_1 + x_2$.

Consider rectangles with base equal to height, i.e.,

(2) $x_1 = x_2$.

For this class of rectangles we shall have

(3) $y = 2x_1$.

Another formula for the same case, not very much used but nevertheless absolutely correct, is

(4) $y = \frac{3}{7}x^1 + \frac{1}{7}x^2$.

When $x_1 = x_2$, then (4) expresses correctly the area of the square, more generally,

(5) $y = b_1x_1 + b_2x_2$ (with $b_1 + b_2 = 2$).

The *structural relation* (1) can be verified with no regard for whether other relations are satisfied; it is an *autonomous* relation, holding identically in the right-hand-side variables.

The *deflated confluent relation* (3) is distinguished from the structural relation by being satisfied only when relation (2) is true. Also (3) is true identically in the right-hand variables.

The *inflated confluent relation* (4), contrariwise, is satisfied only if (2) is true, and it is not identically true in the variables.

Table 5 Three kinds of relations: (1) structural relations, (2) inflated confluent relations, and (3) deflated confluent relations. Source: Frisch 1933a, lecture 6.

least squares method, one will find that b_1 is of the form error/error and the same for b_2. Suppose someone attacks the problem by means of a theoretical scheme that implies at the same time the hypothesis that the area can be written as in (5) and the hypothesis that x_1 is equal to x_2; then the matter becomes serious. The theoretical position of the problem itself is then such that if the statistical data really fulfilled the theoretical conditions that have been assumed, then this will prevent the available data from giving any information about the parameters of the theory adopted.

Recognizing the problem of simultaneity for the identification of economic relationships, Frisch tried to establish a battery of methods to mitigate the problem, not least investigate how closely the unidentifiable coefficients could be approximated by making suitable assumptions. One line of reasoning as set out in the sixth Poincaré lecture went as follows:

Let the relationship under consideration be

$$(1) \quad y = b_0 + b_1 x_1 + \ldots\ldots + b_n x_n = \Sigma_i b_i x_i$$

but with the rank of the "swarm" of observations equal to ρ less than n. Then there exists a set of ρ variables, e.g., x_1 ,..., x_ρ, such that all variables can be expressed as linear functions of those ρ variables.

$$(2) \quad x_i = \sum_{j=0}^{\rho} a_{ij} x_j \qquad (i = 0, 1,\ldots, n)$$

for $i \leq \rho$ we have

$$a_{ij} = e_{ij} = \begin{cases} 0 & i \neq j \\ 1 & i = j \end{cases}$$

Since the variables $x_1,\ldots,x_\rho$ are linearly independent, the coefficients a_{ij} for $i > \rho$ may be determined by the statistical material. Thus all the a_{ij} coefficients may be considered as known. Inserting (2) in the structural condition (1) we see that the variable y also must be expressed as a linear form in the basic set, i.e., that we have a relation of the form

$$(3) \quad y = \sum_{j=0}^{\rho} B_j x_j.$$

Since the ρ variables of the basis constitute a linearly independent set, the coefficients B in (3) may be determined by the statistical material, provided that the swarm of observations $(y, x_1,\ldots,x_\rho)$ is not degenerate. We are going to utilize the a and B coefficients to express the nature of the information that the data furnish on the structural coefficients b entering relation (1).

From the construction of (3) we have

$$B_j = \sum_{i=0}^{n} b_i a_{ij}.$$

In other words

$$(4) \quad B_j = b_j + R_j \qquad (j = 0,1,\ldots,\rho)$$

where $R_j = \sum_{i=\rho+1}^{n} b_i a_{ij}$.

We shall call the B_j's the *confluence coefficients* and the b_i's the *structural coefficients.*

We see from Frisch's example that the confluence coefficients, which can be determined from the statistical data, are equal to the corresponding structural coefficients apart from a residual R, which again is a linear form in the structural coefficients. The a_{ij} coefficients are known, while the coefficients $b_{\rho+1}, \ldots, b_n$ are not. The latter are just arbitrary parameters indicating *how indetermination enters into the problem*. Within Frisch's nonprobabilistic framework this was a quite natural way of formulating what he called "indetermination," but we recognize it as an identification problem. Within Frisch's reasoning he considered that there might exist supplementary information bearing upon just the structural coefficients $b_{\rho+1}, \ldots, b_n$. If we knew the upper limits on the absolute values of (any of) the structural coefficients, we might indicate an upper limit to the error committed by setting the confluent coefficients equal to the structural coefficients. In other cases it might be possible to evaluate the limit of the domain of variation of the parameters $b_{\rho+1}, \ldots, b_n$, and in that case the information one might derive on the structural coefficients $b_1, \ldots, b_\rho$ would be even more precise. For the case of only one arbitrary parameter, that is, $n - \rho = 1$, Frisch expressed the nature of such a limitation graphically as in figure 2. From the assumed domain of variation for b_n given by D, one can read the corresponding domains for b_1 and b_2, as determined by the given data.

According to Frisch, the solution to identifying autonomous versus confluent relations goes through different stages: (1) determining at the theoretical level the structure of the economic system; (2) estimating from the statistical data the value of the structural constants; and (3) establishing the nature of the observed variations by distinguishing between systematic variations and disturbances. Frisch's methodology relies on an assumed *gap* between the theoretical-ideal variables, given by the theoretical structure, and the actual ones, given by the empirical-statistical investigation, measured by a *scatter diagram*, which allows for redefinition of the former structure and causal relationships. The goal of the process is to determine if the assumed structural relations are actually structural relations; if not, they have to be redefined as confluent ones. At the end of the process, Frisch is able to build a numerical model based on *autonomous* (or *structural*) and *confluent* relations. Structural modeling formulates "mixed statements" that are built from both theoretical investigation and empirical experimentation matched by a process of trial and

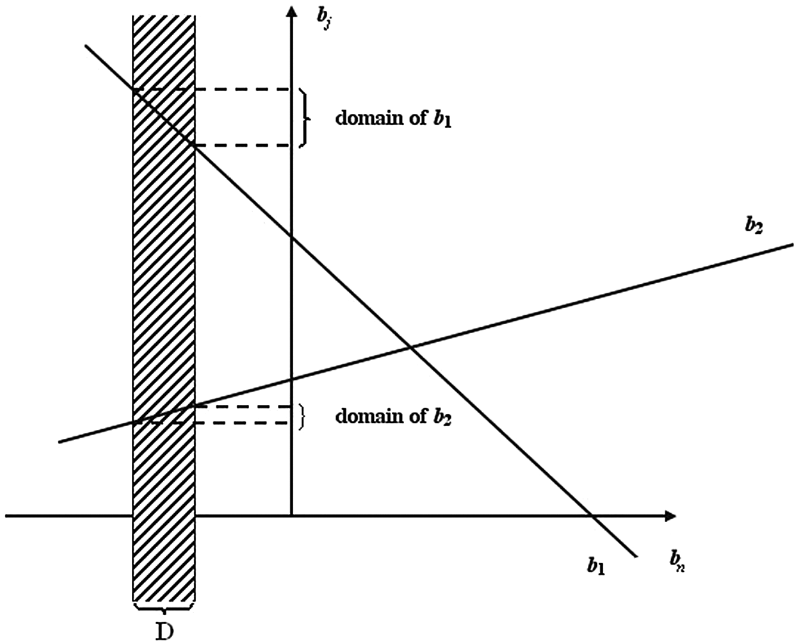

Figure 2 Domain of variation of structural coefficients in the case of only one arbitrary parameter ($n - \rho = 1$)

error. Modeling can then be interpreted as resembling an artisanal craft, a laborious and strenuous process:

> Model building is like baking a cake without a recipe. The ingredients are theoretical ideas, policy views, mathematization of the cycle, metaphors and empirical facts. . . . Each recipe is a manual for building a business-cycle model, but in each case the set of ingredients is different. The integration of a new set of ingredients demands a new recipe, otherwise the result will fall apart. However a recipe is not unique in the sense that it is the one and only way to integrate a certain set of ingredients. Thus a new recipe is a manual for a successful integration of a new set of ingredients. (Boumans 1999, 67)

The Poincaré lectures presented a major and indeed paramount statement by Frisch on his conception of knowledge and how to do econometrics, especially when dealing with business cycles. He did not only demonstrate to the emerging community of econometricians that econometrics could not be reduced to investigations of statistical methods and the determination of empirical laws; he also explained his motivations for his entirely new approach of dealing with economic phenomena. Econometrics was not only a sophisticated technical matter; it also had to do with scientific requirements and social issues. The only other moment of his career when Frisch was so explicit about his overall conception of science was in his Nobel Prize speech.

7. The Limits of Knowledge: Frisch's Philosophy of Chaos

In the last of the Poincaré lectures, which was titled "The Meaning of Social and Mechanical Laws, Invariance and Rigidity, Remarks on a Philosophy of Chaos," Frisch touched upon fundamental cognitive issues related to the probabilistic nature of the outer world. He began with the notion of a scientific law as a mathematical relationship in one coordinate system, which by a nonsingular transformation can be transformed with all its complexity preserved into another coordinate system. Trivial as this may seem, the situation becomes much less trivial if there are small irregularities in the observations:

> In that case the nature of the transformation itself begins to exert *an independent influence upon the complexity of the outcome of observations*. . . . In fact, if the systematic law that we try to identify among irregular fluctuations depends upon the system within which it is observed, what is then left of the concept of a law itself? (Frisch 1933a, lecture 8)

Frisch then outlined the topic of the last Poincaré lecture:

> I will speak about three aspects of this question. First, I shall make a somewhat technical remark on *the invariance of statistical laws*, then say something on *the reversibility of the phenomena of the external world*, and, finally, speak on the most important aspect, namely, the *rigidity of observable laws and the question is then to know to what extent we have constructed the observed laws ourselves.* These three aspects of the question will lead us toward the same general conclusion, namely, the absolute relativity of all our observations and all our conclusions concerning the external world. We conclude with the conception of an external world as being ultimately essentially chaotic.

His technical remark about the invariance of statistical laws was as follows. Let a scatter of observations be given in (x, y). A nonsingular linear transformation will transform the scatter from the two-dimensional space (x, y) to the two-dimensional space (u, v). If the scatter is not completely disorganized, we may choose a regression method and determine a regression line through the scatter. Frisch then posed the question of the relation between the regression line in (x, y) transformed to (u, v) and the regression line determined by the same method in (u, v). It is mathematically trivial but perhaps epistemologically uncomfortable that there is no invariance of most regression methods under a simple linear transformation.

Thus Frisch cast doubt on the invariance of economic laws by pointing out the lack of invariance of regression methods. This was in fact an old topic for him, as Frisch 1929a had explored invariant regressions.

On the reversibility of the phenomena of the external world, Frisch (1933a, lecture 8) made a visit to thermodynamics and may have been on shaky ground when he asserted that

> instead of saying that there exists a universal law of increasing entropy, one may arrive one day at proving a principle of conservation of entropy in the same way as one has a principle of conservation of energy.

On the third issue about the rigidity of laws, however, Frisch formulated his assertion as the following mathematical theorem:

> For any given scatter of points in (x, y) that are not perfectly correlated, there exists a nonsingular transformation into (u, v), such that the correlation can be chosen arbitrarily close to 1 and furthermore the regression coefficient of the transformed mass of points can also be chosen freely.

The proof was mathematically trivial but Frisch elaborated persuasively upon its implications, which we here quote at some length:

> In a large number, if not the majority, of the problems we meet with in social, in biological, and above all in physical series, it is nothing which imposes the choice of one coordinate system rather than another. But where are we then? By a change of variables we can make the appearance of a law disappear, or we can create a law and give it the appearance we like.
>
> But what is then the object of science? The incessant preoccupation of science is to find theoretical schemas, new coordinate systems that fit better and better the so-called facts. If science finds a discrepancy, it modifies its theoretical scheme, it introduces other variables, and in

> short it makes a transformation. Having done that, it declares triumphantly that now it has succeeded in finding a scheme fitting even better with experience. What does that mean? It means that science has made transformations closer to singular transformations than before. You probably find such a view of science disgusting; you would rather regard scientific activity as a disinterested search for objective truths that are perpetually outside us.

And he continued with invincible logic toward the inevitable conclusion:

> We have arrived at the point where it is necessary to draw the final consequences of the perspective I have presented for you here. It is necessary to translate this perspective to the biological plane. Let us suppose that we have a biological being of some sort that was first equipped with sense organs that could register the influences (x, y). It lives in a chaotic world and it will have neither the means of looking ahead nor the means of serving itself from the forces of nature. Thus it will most likely be eliminated. But other biological beings will develop, perhaps some supplied with sense organs influenced by (u, v). These beings will live in a very beautiful world; they will develop natural sciences for discovering the laws of the world. Their science will research the same genre of singular transformations according to which the biological transformation has taken place. But there is more. Science, encouraged by its success, will probably engage in speculations and in research of new singular transformations that go beyond by far the biological transformations, which have defined the sense organs, and in that new domain, at the same time both abstract and empirical, there are infinite possibilities of discovering new so-called laws of nature.
>
> Most likely these new adventures of science will have a repercussion on life and perhaps even on the biological development of the species. There will be interdependence between biological evolution and scientific development, very similar to the relationship between a demagogue and the people. Under the influence of this mutual interdependence the biological and the scientific life will continue their evolution. During this evolution science will certainly from time to time register new fundamental discoveries. But the world that science in that way will discover will be very, very far from being an objective world.
>
> Why then do science? Because we can perhaps by that hope to soften at least a little the pain of the species that develops, for evolution will always be accompanied by pain; that is the only universal and eternal principle whose existence we will never have to question.

After being awarded the Nobel Prize in economics in 1969, Frisch chose to recapitulate this ominous concluding part of the unpublished Poincaré lectures in his Nobel address, suggesting that it was still a topic close to his heart thirty-six years later:

> It is quite clear that the chances of survival of man will be all the greater the more man finds regularities in what seems to him to be the "outer world." The survival of the fittest will simply eliminate that kind of man that does not live in a world of regularities. . . . Science considers it a triumph whenever it has been able by some partial transformation here or there, to discover new and stronger regularities. . . . If "the ultimate reality" is chaotic, the sum total of the evolution over time—biological and scientific—would tend in the direction of producing a mammoth singular transformation which would in the end place man in a world of regularities. How can we possibly on a scientific basis exclude the possibility that this is really what has happened? This is a crucial question that confronts us when we speak about an "ultimate reality." Have we created the laws of nature, instead of discovering them? (Frisch 1970b, 218–19)

8. Conclusion

By focusing on the technical and innovative aspects of the various works of Frisch, one may miss the coherence of his research program. By reading Frisch and especially the Poincaré lectures in their entirety, econometrics cannot be seen anymore as a set of tools for analyzing and forecasting. Those tools are only the vehicles of a new way of defining the nature and the subject of scientific research in economics. The complexity of the work of Frisch is reflected indeed in the specificity and the diversity of his research topics, as can be seen in the Poincaré lectures. The complexity of his work is the consequence of the difficulty of articulating a field of research (at the crossroads of empirical quantitative economics and of theoretical economics), developing various tools of analysis and measurement, and a methodology able to link theories with facts.

The Poincaré lectures help us to understand that Frisch is defining a new research field based on the intertwining of the empirical and theoretical quantification of economic phenomena, but he is also attempting to articulate economic rationality and political rationality. Indeed, Frischian econometrics has to be read in a broader perspective: according to Frisch (lecture 8, but also Frisch 1926b and the Nobel Prize lecture [1970b]), science aims to discover or at least formulate laws in order to avoid chaos. Laws and heuristic aims are subordinated to philosophical and political ones: avoiding chaos implies a reliance on scientific discoveries in order to create social order and economic growth.

The work of Frisch is clearly twofold: one part structural modeling and one part economic policy. But to what extent are the two really separate? Are they not in fact interdependent? Does Frisch's definition of *econometrics* as a new research field in "a more scientific perspective," that is to say in "a more quantitative perspective," point to the political side of the econometrician's task?

This interrelation of the heuristic aims and the political ones in Frisch's work has already been underlined by Andvig (1985) and Louçã (1999, 2001), both of whom relate the development of a set of planning tools and models by Frisch to the social consequences of the Great Depression and the paramount task of rebuilding economies after World War II. Poincaré lecture 8 helps establish that this intertwining of heuristic and political ambitions in Frisch's economics can be sought in the definition of *econometrics* as the unification of empirical quantitative economics and theoretical quantitative economics.

This interdependence, be it explained by the historical context or by the epistemology of Frisch, has some consequences for the nature of the tools for analysis and forecasting but also for planning. By investigating the way to set up these tools and how he combines them, the ambivalence of Frischian econometrics can be better understood, and econometrics can be seen as being at the crossroads of normative and positive ambitions.

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