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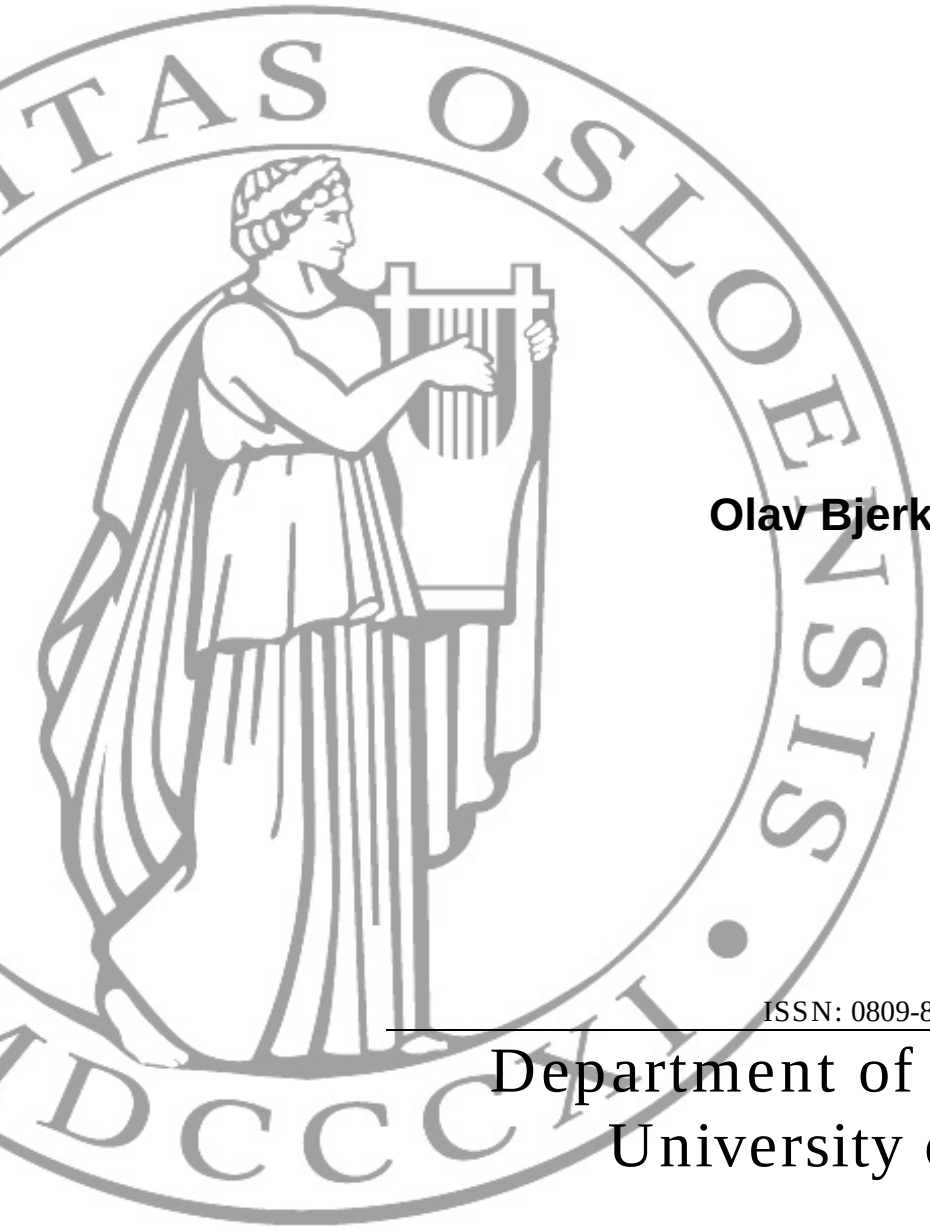
Ragnar Frisch's contribution to business cycle analysis



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Ragnar Frisch's contribution to business cycle analysis

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Abstract

Business cycle analysis, i.e. investigations into the more or less regular fluctuations in economic activity, emerged around the mid-nineteenth century. During Ragnar Frisch's formative years as a young economist the field came to the centre of attention. Frisch was highly concerned about the inability of modern economies in the midst of plenty to prevent economic fluctuations from playing havoc with the livelihood of millions. He first directed his attention towards methods for analysing time series data and increasingly shifted it to the question of the nature of a proper theoretical explanation of economic fluctuations. Thus Frisch's orientation and contribution was not so much business cycle analysis in the substantive sense, understood as explaining why and how economic activities varied cyclically, but the appropriate methods for analysing and explaining cycles. He is best known for the model he presented in his *Propagation and Impulse* essay in the festschrift for Gustav Cassel (Frisch, 1933b). It became Frisch's most ambitious research project before World War II, but the intended main publications from the project never appeared. 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 macroeconomic model Frisch presented. His real message was to demonstrate his overall paradigm for macro analysis in economics. This article looks in more detail at the genesis of the propagation and impulse model. The presentation is non-technical and includes some biographical details.

Keywords: Ragnar Frisch, business cycle analysis, propagation and impulse.

1. Introduction

Ragnar Frisch is difficult to classify in the history of economics. He did not attempt to build theoretical edifices, he did not have a theoretical program. He was primarily

concerned with a two-faced methodological program: how to "quantify" theory using mathematics as a tool, and how to confront theory with data, particularly in the analysis of simultaneous equations and time series data.

As these problems to Frisch were, more than anything, *econometric* problems, the most apt characterization of him may well be the one used by Kenneth Arrow in the special festschrift issue of Econometrica in 1960, namely, *Ragnar Frisch, Econometrician*. He was a practitioner of *econometrics*, as understood not with the more limited connotation the term has today, but with the meaning that Frisch originally gave to it and then became institutionalized by the foundation of the Econometric Society.

To Frisch *econometrics* primarily meant *scientific* economics, rather than a sub-discipline of economics. Frisch saw it as a necessity for economics to advance to a more scientific level to cope with the challenges of modern society. Like many of his generation Frisch was very impressed and influenced by the striking advances of physical sciences in the first quarter of the twentieth century. He set out to contribute towards making economics more scientific, inter alia through adapting methods and procedures from physical sciences for use in economics.

The key term in Frisch's conception of scientification in economics was *quantification*, a term he most likely had borrowed from Irving Fisher and used frequently since 1926. When he together with Fisher founded the Econometric Society in 1930, Frisch drafted the key formulations in the Society's constitution determining its main object to be 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."[1] As Frisch saw it there was a need for *theoretical quantification*, i.e. to make theory operational as a preparatory step in confronting theory and data. And there was a need for *empirical quantification*, both methods for confronting theory with data for determination of empirical relationships, and compilation of data for economic studies. Frisch, regarding himself as a statistician as much as an economist, emphasized that both for theoretical and empirical quantification, statistical insights were needed. Indeed, the Econometric Society adopted requirements both for membership and fellowship that required *econometricians* to know economics, statistics and mathematics.

Business cycle research, in the sense of investigations into the more or less regular fluctuations in economic activity, emerged around the mid-nineteenth century, particularly through the influential work of Juglar in 1862, see Morgan (1990: 41-44). During Frisch's formative years as a young economist the field came to the centre of attention as the A-B-C method emanated from Harvard and business cycle research institutes mushroomed in Europe, while post-World War I shocks reverberated throughout the industrial world, see Morgan (1990: 44-68). As a socially conscious young man in the 1920s Frisch, like many others, was highly concerned about the inability of modern economies in the midst of plenty to prevent economic fluctuations from playing havoc with the livelihood of millions.

[1] The constitution was stated on the inside back cover of Econometrica for several decades.

Frisch at first directed his interest in the cyclical character of the fluctuations towards methods for analysing time series data. Increasingly he shifted his attention to the question of the nature of a proper theoretical explanation of economic fluctuations. His interest in the theory of science had led him to regard "cause" largely as a metaphysical term and he showed little interest in the ad hoc discussion of "causes," so frequent in business cycle discussions. Thus Frisch's orientation and contribution was not so much business cycle analysis in the substantive sense, understood as explaining why and how economic activities varied cyclically, but the appropriate methods for analysing and explaining cycles. To Frisch the explanation and determination of cyclical fluctuations was a prime example of an econometric problem.

Frisch's achievement in 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). The essay's propagation-impulse explanation of economic cycles, underpinning the rocking horse simile of Wicksell received great attention.[2] Even after 50 years it is widely discussed in the literature.[3] Frisch (1933b), which figures as a pioneering contribution also in the histories of macroeconomic modelling and national accounting, stands out as Frisch's only contribution to business cycle analysis. The Cassel essay was part of Frisch's most ambitious research project before World War II, but the intended main publications from the project never appeared. Frisch certainly did not regard the *Propagation and Impulse* essay as his final word in business cycle analysis and not even as a major outcome. For this reason the project must be deemed an unsuccessful one, and questions may be raised as to what his overall ambitions were.

Frisch (1933b) introduced the "propagation problem" as that of working out the cyclical properties of a given "swinging system" when it is started in some initial situation. The "impulse problem" was to explain how the damped cycles found as the solution of the propagation problem, could give rise to maintained swings as observed for economic systems. Frisch put forward that there were "several alternative ways in which one might approach the impulse problem and try to reconcile the results of the determinate dynamic analysis with the facts" (Frisch 1933b: 197). 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.[4] Frisch credited his hero Wicksell as having been the first "who has been definitely aware of the two types of problems in economic cycle analysis – the propagation problem and the impulse problem – and also the first who has formulated explicitly the theory that the source of energy which maintains the economic cycles are erratic shocks." (Frisch 1933b: 198). He credited Slutsky for having "definitely established that some sort of swings will be produced by the accumulation of erratic influences" (Frisch (1933, p.199), adding that the exact and general law telling us what

[2] Frisch cared about the distribution as he ordered 500 reprints, i.e. more than the number of members of the Econometric Society at the time.

[3] See e.g. Andvig (1981, 1986), Boumans (1999), Klein (1998), Le Gall (1993), Thalberg (1998), Velupillai (1992) and Zambelli (1992,2007).

[4] See e.g. Morgan (1990), Ch.3.2 "Frisch's rocking-horse model of the business cycle", and Louçã (1997), Ch.8 "The Rocking Horse: The Propagation-Impulse Metaphor for Business Cycle Theories".

sort of cycles that a given accumulation of shocks will create was not discovered. Such cycles could be called *accumulation cycles* as distinct from pure *intrinsic cycles*.[5]

Due to Frisch's incomplete publication of his work the *Propagation and Impulse* essay may have been interpreted in a way that differed from Frisch's own view with regard to what his achievement was. It may have been interpreted with too much emphasis on the content and properties of the macroeconomic model as Frisch's business cycle model per se.[6] Frisch was brilliantly showing off in the festschrift article by 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). This article aims at corroborating this interpretation of Frisch's *Propagation and Impulse* essay, by looking in more detail at the genesis of the propagation and impulse model. The presentation is non-technical and includes some biographical details.

Ironically, Frisch erred in his presentation. The model, which had been studied more than any other model of business cycles, did not generate cycles. This was shown by Zambelli (1992), which attracted minimal attention, and then restated in Zambelli (2007). Frisch's structural explanatory paradigm survives unscathed by Zambelli's findings, contrary to what is asserted in Zambelli (2007).

Section 2 briefly sketches Frisch's early career, and the roots of the propagation-impulse paradigm in his early work on cyclical fluctuations. Section 3 sets out some of Frisch's idea and tenets in his theory of science approach to economics, as set out in elementary form to Yale students in 1930. Section 4 deals with Frisch's presentation of his paradigm from the his first presentation to a Nordic meeting in Stockholm in June 1931 until the Cassel festschrift appeared in October 1933, and section 6 with some remaining issues and speculates on reasons for Frisch's inability to complete his project and publish his results.

2. The making of an econometrician

Frisch was an only child and destined to take over the Frisch family's jeweller shop in Oslo. He excelled in school and while he was in training to become a silversmith he also followed, at his mother's suggestion, the two-year program in economics at the University of Oslo. When he became journeyman his father made him partner in the business, which made Frisch relatively well off and allowed him to be a silent partner while he pursued his scientific interests by studying. He was abroad for almost three years, mostly in Paris, where he delved deeply into mathematics and statistics. Among

[5] Frisch's theory is generally presumed to require that the deterministic dynamic model must display cycles. But pure accumulation cycles could arise from deterministic models without cyclical behaviour. Frisch's assistant, Trygve Haavelmo, pursued the idea, concluding: "It seems possible to explain observed cyclical movements by the combination of a structure which is non-|cyclic, but which contains inertial forces, and outside influences of random events. This possibility should not be excluded; it should be left to be determined by fitting to data." Haavelmo (1940: 321).

[6] Also Boumans (1999) highly interesting discussion of Frisch approach his contribution from this angle.

the economists he learnt to appreciate at an early stage was Irving Fisher, whose 1891 dissertation he studied in a French edition (Fisher, 1917), acquired in Paris in 1922. He admired Fisher and was inspired by his work in several ways.

In his very first essay in economics, *Sur un problème d'une economie pure* (Frisch, 1926a), Frisch attempted to "réaliser le rêve de Jevons" of measuring utility. Using household data from a cooperative association in Paris data Frisch made assumptions that allowed estimation of the demand function for sugar in a way that also resulted in estimates of the marginal utility of income (as a function of income) for the consumers under consideration.[7] Although Frisch referred to Jevons, the impetus for his study had more likely come from Irving Fisher. An important part of Fisher's dissertation revolved around the issue of the measurability of utility. In the dissertation he had shown how the problem could be solved theoretically and the added: "To do this statistically is of course a more difficult, though by no means hopeless proceeding."[8]

In the preface to the reprint of his dissertation in 1925 Fisher wrote at the end: "The suggestion in this booklet that so-called "marginal utility" may be measurable statistically is now being followed up. Within a few months, I expect to publish the first attempt of this sort and to discuss its possible practical use in deciding on the proper rate of progression for an income tax." Fisher was in fact for his contribution to a festschrift for John Bates Clark working on a method for estimating marginal utility from statistical data. But he was not the only one! Frisch, who until then had never been in touch with Fisher, had just had his Paris study published in a mathematical journal in Norway (Frisch, 1926a) and sent Fisher a reprint. Fisher got it while he was proofreading his own paper and recognized immediately that Frisch had hit upon a much more promising method. He hastily wrote a note to be attached to his own paper when it was distributed as a reprint to notify the readers of Frisch's achievement.

Frisch's estimation technique in 1926 was perhaps not impressive in retrospect, but the main point he wanted to bring home by his empirical demonstration of econometrics was not the quality of the estimates as much as the estimability of marginal utility when guided by theory. This was, indeed, *empirical quantification*, adhering to Frisch's main tenet: the primacy of theory. Analysis of economic data was futile unless it was inspired and led by theory. As he liked to state on various occasions: "The observation material is and remains a dead mass until it is animated by a constructive theoretical speculation."[9]

Frisch's achievement in the *Sur un problème* essay was, however, not only his solution to the challenge set by Jevons and Fisher. The essay also demonstrated how the elusive concept of a utility function could be subjected to *theoretical quantification* through an axiomatization of the choice acts of the consumer. Also this idea was inspired by Fisher's dissertation.

Frisch defended a doctoral dissertation in 1926, it was the first dissertation in theoretical statistics at the University of Oslo. In the draft of a chapter he left out at the end, leaving

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[7] See Bjerkholt (1995: xxiv-xxv).
[8] Fisher (1925: 20). In Frisch's copy of the French edition he had underlined the corresponding sentence: "Quant à l'établissement de cette courbe en conformité de données statistiques, c'est une autre affaire et bien plus difficile, quoiqu'il n'y ait nullement lieu de désespérer de sa réussite." (Fisher 1917: 24)
[9] Frisch (1931a), translated by the author.

only some remarks at the conclusion, he touched upon the issues that were to occupy him throughout the 1930s, namely, the limitations of our knowledge of the outer world when observations have been contaminated by stochastic disturbances (Frisch, 1926c).

Soon after, in February 1927, Frisch went to the U.S.A. with a Rockefeller fellowship. He completed two treatises during that year, both on methods for analysis of empirical data. One was *The Analysis of Statistical Time Series* (Frisch, 1927b) and the other the *Correlation and Scatter* essay on the analysis of multidimensional economic data (Frisch, 1929a).

Both treatises were critical of current practices, and both were written with the use of so much mathematics that it probably prevented them from being studied by some of those he wanted to reach. Frisch (1929a) was a profound and inventive introduction to data analysis, that provided the basis for Frisch's later work on simultaneity and confluent relations, which indeed was a component of his overall paradigm for business cycle analysis. Frisch may have been the first one who really became aware of the problems related to empirical analysis of economic time series data due to their non-experimental character and simultaneity of economic relationships. It was on the basis of the insights he gained from Frisch (1929a) that found ammunition for his frequently repeated attacks on "geschäftige Korrelationsberechner."[10]

Frisch (1927b) was a treatise on methods of time series analysis, sharply criticizing the current methods in use for of determining trends and cycles in economic time series data. Frisch argued against "total methods" for determining the components of a time series, such as Fourier analysis and periodograms, in favour of "differential methods" to be applied in the environment of a single point, see Morgan (1990: 83-90). Frisch's point was that economic cycles were changing in their characteristic properties and "total methods" were bound to fail. The treatise had been written without access to computational resources, implying that Frisch had had limited opportunities for testing the workability of his methods.

Frisch gave "condensed statement" of the main ideas of Frisch (1927b) in an article for a Scandinavian journal (Frisch, 1928), in which he introduced the term *changing harmonics* to express the idea of changing cyclical properties, using an analogy from mechanics to make his point:[11]

> 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. (Frisch, 1928: 231).

[10] Frisch (1929a), which was poorly distributed and thus never got the attention it deserved, comprised e.g. the basic theory of principal components, the discovery of which was credited to Hotelling some years later. I owe this observation to Michael Leznik.

[11] Thus the idea of 'changing harmonics' originated in Frisch (1927b), while he coined the term later.

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 or analogy is still another example of Frisch following in the footpath of Fisher whose dissertation was amply illustrated by pictures of Fisher's constructions of mechanic or hydraulic models. Obviously, 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 (1926b: 302-303).

This chain of pendula analogy is highly relevant in the genesis of the propagation-impulse model. It was used also in Frisch (1927b). The pendula system was the deterministic model, as a kind of reduced form according to cyclical properties. Frisch noted that if friction was present "a stream of impulses will be necessary to keep the system going. It may be impulses of a more or less accidental character distributed more or less irregularly over the interval of time considered, and over the various pendula." (Frisch, 1927b: 15-16). In the same context he also used the term "propagate" about how the effect of movements spread from one pendulum to the entire system.

The time series treatise (Frisch, 1927b) was never properly published. Its presence in the history of econometrics literature is due to the fact that the Rockefeller office in New York at Wesley Mitchell's suggestion distributed mimeographed copies to a considerable number of economists and statisticians in the USA. Frisch was shortly afterwards offered to have it published by the Frankfurter Gesellschaft für Konjunkturforschung, but declined the offer as he had already revised some of his ideas and wanted to prepare a more elaborate publication with numerical results (which never appeared).

At the joint meetings of the American Economic Association and other association in December 1927 Frisch took part in panel discussion on the present status and future prospects of quantitative economics. The panel was chaired by F.C. Mills who invited Frisch to submit his statement, which he did, but in the end the editor decided to publish only the statements of the invited panelists who include W.C. Mitchell, E.B. Wilson and others, Mill et al. (1928). Frisch's prepared statement is nevertheless of interest as evidence of his scientific attitude. He took the opportunity to argue in favour 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. … I cannot get rid of the impression that very often 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 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. (Frisch/Mills, 21 February, 1928, National Library of Norway).

The appearance of Slutsky (1927) during Frisch's visit to the U.S.A. in 1927 came to have crucial inspirational influence on his thinking about how cycles were created and maintained, namely. The St. Petersburg school in statistics had a strong standing in the 1920s and Frisch had several acquaintances among Soviet statisticians and mathematicians (Klein 1997: 276ff; Klein 1999). One of them put Frisch and Slutsky in touch with each other by correspondence in 1925. They retained connections for about ten years but never met.

In early 1927 Frisch sent Slutsky his time series treatise (Frisch, 1927b), and Slutsky, who at the time worked at the Conjuncture Institute in Moscow, sent in return, as soon as it was published, his paper on artificially created cycles (Slutsky, 1927) and also made Frisch aware of Yule's related work.[12] The paper was in Russian, a language Frisch could not read, but had a five page English summary from which Frisch immediately got 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 waves may show a certain regularity being an imitation, in a lesser or greater degree, of the strictly periodical fluctuations" (Slutsky, 1927: 156).

Slutsky and Frisch shared a number of scientific concerns; Frisch had introduced Slutsky to the idea (which had come from François Divisia) of an econometric association and a journal and Slutsky had given very constructive response. Frisch knew that Slutsky was, like Frisch himself, highly adept at combining statistical theory with calculations and numerical analysis. The paper promised a continuation with analyses of statistical data, and already after one month Frisch asked about the sequel, which never appeared. Frisch mentioned and embraced on a number of occasions the practical implications of Slutsky's assertions. But the use he would make of Slutsky's paper was in fact a reinterpretation. Slutsky asserted that manipulation, such as smoothing of statistical data by moving averages, would amount to a summation of random shocks, which would generate artificial waves lasting for a length of time. Frisch realized that an economy represented by a (linear) dynamic structural model exposed to stochastic shocks might from the same mechanism experience cycles in addition to those inherent in the structural model, as the (linear) dynamic model would provide weights for summation of the shocks.

Thus we can trace back to 1927 major ideas underlying the propagation-impulse model, mostly from his own time series studies but with Slutsky's striking result as an additional influence.

The Rockefeller fellowship was for three years, of which Frisch had planned to spend the last two in Europe. But shortly after Frisch's arrival in Europe Frisch's father became seriously ill and later 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 was in a

[12] As soon as Frisch became editor of Econometrica in 1932 he pursued the idea of having Slutsky's paper published in the new journal. It eventually appeared in a revised version as Slutzky (1937). The time delay seems to have been due partly to Slutsky's revision of original article and partly to slow communication.

dilemma as to whether he could continue his scientific career. Decisive was an offer from Irving Fisher to arrange an invitation from Yale University as Visiting Professor (and in its entirety paid for by Fisher). Thus after being in Norway for two years, finding a satisfactory arrangement for the management of the family business, Frisch left for USA again in February 1930, a visit that would last for almost one year and a half.

In a paper presented at a joint American Statistical Association /American Mathematical Society meeting in December 1930 Frisch followed up on his critique of "usual time series technique" from his 1927 visit, for not being powerful enough to deal with situations of interference of several components and inadequate when components were changing. Thus the conclusion of a "break in the trend" was in most cases rather "a breakdown in the trend method." There was nothing fundamentally new relative to Frisch (1928), but the presentation of the use of linear operators to distinguish different components had been improved and corroborated by numerical work undertaken in Frisch's seminar in time series analysis at Yale. The overall perspective was still mainly descriptive decomposition, but the paper expressed the need for explaining how the components got into the series, i.e. the theory of the economic phenomena under consideration, see Frisch (1931c).

While still in USA 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 Rockefeller Foundation about the establishment of a research institute at the University of Oslo was initiated and brought to fruition. The annual support of $5000 (admittedly with prospects for additional support) was tiny compared to what was received by many of the other institutions within economics supported by the Foundation at that time, but it sufficed for him to choose Oslo. From 1932 Frisch had the Institute of Economics as his laboratory with research assistants and computing equipment as he could afford.

One more Frisch experience from the 1920s ought to be mentioned, namely that of Johan Åkerman's dissertation. Åkerman was a Swedish economist who throughout his career was a business cycle analyst, and Frisch was opponent at his dissertation defence at Lund University in November 1928, see Åkerman (1928). Åkerman, who had spent one year at Harvard with Warren M. Persons and also studied the work of W.C. Mitchell at the National Bureau of Economic Research, was a follower of Knut Wicksell, who had commented an early draft of Åkerman's dissertation. Frisch as a self-confessed Wicksellian praised in his opposition Åkerman's theoretical credentials as a follower of "the brightest and most profound economic theoretician ever from Scandinavia" (Frisch (1931a).

Frisch's opposition to Åkerman's dissertation gave him an opportunity to articulate his view, partly as a reaction to flaws he spotted in Åkerman's reasoning. Before touching upon Frisch's criticism in more detail we note, firstly, that Frisch got the reference to Wicksell's rocking horse from Åkerman's dissertation, see Åkerman (1928: 98). Frisch knew Wicksell's view on business cycles from Wicksell (1907), but had never come

across the rocking horse metaphor.[13] Secondly, Frisch at the defense expounded the propagation-impulse mechanism as something that already permeated his thinking about business cycles.

Frisch commended Åkerman at the outset for his attempt at a "synthesis between the abstract economic theory and the concrete observation material found in economic statistics," and for being the first Scandinavian to attempt this in business cycle analysis. This starting point gave Frisch another opportunity to lambast the American conception of quantitative economics, amounting to nothing more than economic statistics, and also to bring out his favourite maxim of the primacy of theory. "Economic statistics tells us nothing if not viewed on the background of a broad theoretical-economic analysis." He also appreciated in Åkerman that he was one of few who realized the necessity of distinguishing components of different wave lengths, not just the crude peak-to-peak analysis of Mitchell and others, which Frisch saw as one of the most common mistakes in business cycle analysis. But that was the only praise Åkerman got from Frisch.

Frisch used the propagation-impulse mechanism to reveal flaws and logical weaknesses in Åkerman's approach, particularly regarding the search for the cause of each phase of the cycle. Frisch found that Åkerman was not really able to identify and separate components in the time series. He even used his own methods to analyse some of Åkerman's data and stated to Åkerman's embarrassment that he had got his suspicions confirmed. The criticism of Åkerman's empirical work was devastating but milder formulated than in Frisch's usual style. Surely this was an experience also for Frisch who might not have studied so closely the empirical work of any business cycle analyst before, and it comes through that he learnt a lot from it.

Åkerman's search for causes initiating the cycle was in vain in Frisch's opinion, because the cyclical movement had to be understood as a continuous process. This criticism was in fact hitting at a wide range of business cycle analysts of that time. Frisch argued that many of the contradictory business cycle theories and hypotheses could, in fact, be systematized and harmonized if one only distinguished between *impulse* phenomena and *rhythm* phenomena. Åkerman's firmly held opinion that the cycle components of different lengths had to be multiples of the seasonal components, implied that the business cycles were bound rather than free oscillations. Frisch was convinced on methodological grounds that a search for free oscillations at the outset was the superior approach, counting Wicksell on his side on that issue.

Åkerman had studied Frisch but he had hardly conceived Frisch's demanding methodology. They were on different tracks. The interaction between Frisch and Åkerman is discussed more fully in Boianovsky and Trautwein (2007).

3. Frisch on methods in economics as of 1930

When Frisch lectured to Yale students as a visiting professor in the autumn of 1930 in a lecture series titled "A Dynamic Approach to Economic Theory," he opened with a lecture which reflected his conviction that for economics to become a science it was

[13] Wicksell used the rocking horse simile in a review of a book by Petander. Åkerman's somewhat incomplete reference to it, caused Frisch's mistake in referring to it in Frisch (1933b).

necessary to adhere much closer to the methods and procedures of the natural sciences. Later in the lecture series he dealt with most of the methodological elements that were particularly pertinent for his contribution to business cycle analysis. He spoke on and discussed his methodological views in various places, but they were never properly published in English. In setting out Frisch's theory of science views we draw on these unpublished lecture notes.

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 it has not been that kind of theory which is built 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: 1).

Perhaps this could be read, in fact misinterpreted, as another attempted revival of Newtonian science as an ideal for economics, but that is not at all what Frisch is after. He contrasts 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 19th century an overwhelming statistical and historical material of economic facts had been compiled.

> "But these observations 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 did not progress so rapidly as, for instance, astronomy, physics and biology, even though theoretical thinking had been applied to it some economists would lose confidence altogether in theoretical thinking in this field and plunge themselves into a pure empirical fact collection in the hope that such a blind grappling with facts should reveal something of the nature of the complicated phenomena with which the economist is faced. 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. (Frisch 1930: 1-2).

Rather than such cycles what was needed economics was a "new fusion between theory and observation" in economics. To achieve that it was required with a "theoretical structure which is such that it is capable of being connected directly or indirectly with actual numerical observations."[14] What was needed? Frisch had answers:

> "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. This will be stimulating, not only for the observational work, but also for the abstract speculations themselves. I know of no better check on foggish thought in economic theory than to have the theorist specify his notions in such a

[14] These were remarkably modern viewpoints, and may not have been more welcomed in the profession than Wassily Leontief's sharp criticism of "speculative economic theory" 40 years later, Leontief (1971).

> way as if he were to apply the notions immediately to some actual or hypothetical statistical material." (Frisch 1930: 2).

Frisch regarded himself as much as a statistician as an economist, and he had already coined a term for the kind of scientist he called for, namely – *econometrician*. It had by then been decided between Frisch, Irving Fisher and Charles Roos that invitations would be sent out for a meeting to establish the Econometric Society during the joint meetings of scientific associations in December 1930. Frisch's viewpoints, as set out briefly above, illuminates the even briefer formulation he used in the constitution he drafted for the Econometric Society, as quoted above. The "theoretical quantification" Frisch had written into the constitution as one of the Society's aim, did not merely mean mathematization as it often was rendered and some still interpret it that way. The use of mathematics in the formulation of theory was just a tool, the key idea behind "theoretical quantification" was indeed to formulate notions to get a possibility for connecting theory with actual observations.

The motivation behind Frisch's view on the need for "scientification" of economics and how to approach it did not merely reflect a profound interest in science as a human activity. He impressed upon his students that the fusion between theory and observation was an issue of far-reaching consequences for mankind's future, in fact 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, which occur again and again in the production cycle; huge productive forces, machinery, and labor being idle at the same time as there are millions of people who want very badly a great variety of things which could be produced by the idle machinery and labor. Not only has man been able to create a big economic machine which 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: 3).

Frisch referred to the cyclical pattern of idle production capacity in the economy, but it may have been not least the current situation in 1930, both in the U.S.A. and in Europe, that impelled him to emphasize to the students the social importance of an econometric approach. He did not leave the students in any doubt about his overall view with regard to the wide range of activities he included under the econometrician's domain, see Box 1.[15]

Box 1. Five types of mental activities in which the scientific worker has to engage

1. The descriptive procedure.

[15] For a more comprehensive treatment of Frisch's methodological and scientific views, see Dupont-Kieffer (2003)

One sort of question which 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, like 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 sort of questions which 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 bring some reasonable order into the happening 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? In order that this sort of questions shall 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 are exclusively of an intellectual character. On the contrary the sort of questions here considered is of an ethical or moral sort. It cannot be answered unless we adopt some sort of 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 different individuals. 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 such happenings or such 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.

Although each of the five categories of activities might have its theoretical components, economic theory as such belonged under the "understanding procedure." The essential feature of theory was according to Frisch to "bring a rational order into things." But what was the nature of theory? Frisch approached this issue in the following way:

> "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 for 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 1930: 5).

Frisch then tried to convey the essentials of the art of modelling. The model world builder is a sovereign in the model world. He can decide which features and characteristics the model world shall have and the relations between various phenomena in the model world, as 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 to 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."

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." (Frisch 1930: 6).

The model world sovereign is guided, naturally, by observed empirical laws, which in idealized form may be incorporated in the model world, like downward sloping demand curves. But empirical generalizations might be enough:

> " … often the investigator will equip his model world with something more than this. By a heroic guess, he will add something which is entirely outside the body of observation at his disposal. It is exactly in this kind of heroic guesses, transgressing the observational facts, that the great constructive minds distinguished themselves from the average scientific worker." (Frisch 1930: 7).

What then was the nature of such "transobservational notions"? Frisch suggested two kinds of such notions. One could be an object, not resembling anything from actual observation. Another was a new relation between phenomena which by themselves were well known from actual experience, but had never been observationally related because "the phenomena are of such kind that they cannot be observed together directly with the given technique of observation." Frisch's example of a transobservational object was drawn from physics, the classic example of the atom. But as a transobservational relation he suggested the relation "between the diminishing return of land and the fact that rent exists." Both facts were known long before Ricardo, but "the relation between them was not seen until revealed by the abstract speculations of Ricardo," contradicting the explanations given by the Physiocrats and Adam Smith.

Frisch thus introduced his theory of scientific investigation, and the idea of economic modelling, in an intuitive non-formalized way. He downplayed the distinction he had introduced between "object" and "relation" as conventional, not one of principle. Something which is a relation in a "microcosmic" model world might be a relation in a "macrocosmic" model world. He then elaborated upon the need for exploring the model world. Despite the fact that we have ourselves created it we cannot overlook all consequences, systematic investigations of the model world are needed. Within this framework Frisch defined empirical laws vs. rational laws, touched upon induction and deduction, and elaborated what was meant by "explanation" within a model. He also discussed probability, sorting out various concepts of probability in this pre-Kolmogorov period. He regarded 'abstract probability' as the model world's counterpart of empirical frequency and hinted at how the model itself could be formulated in a probabilistic way, leading to probabilistic rather than necessary rational laws being derived from it. He

finally discussed at some length the concept of 'cause', although it was "perfectly possible to do without altogether:"

> "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. The scientific (as distinguished from the scholastic) answer to the question: what is a cause, does not read: it is such and such a phenomenon. If any scientific answer is possible it must read: it is such and such way of thinking." (Frisch 1930: 13).

Before we look closer at Frisch's concepts and ideas of particular importance for his approach to the analysis of cyclical fluctuations, we note that the ideas set out above from the introduction of Frisch's lecture series at Yale, are quite relevant for understanding Frisch's approach to the analysis of business cycles. His emphasis on modelling the economic phenomena under consideration may seem commonplace today, but it was certainly not commonplace at that time. Frisch took little interest in discussion of 'causes' of business cycles, to explain business cycles by 'causes of upturns' and 'causes of downturns', meant to him just outmoded ways of thinking and a cul-de-sac with regard to a real understanding of the phenomena under consideration. But at the time his methodological approach put him apart from most people who regarded themselves as business cycle analysts.

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

> "…are only apt to capture the pure empiricist and keep him at some laborious and sterile tasks of fact getting. 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 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." (Frisch 1930: 20-21).

Frisch mentioned Albert Einstein's theory of relativity as a "grandiose example" of a heroic guess and also 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. (Frisch 1930: 22).

Business cycles had been the topic of the day for all the years Frisch had taken an interest in economics. It was unavoidable that he as a methodologist took a keen interest in how this field was conducted. It was not merely a methodological interest either, it meant most likely to Frisch one of those life and death races that man would lose unless constructive theoretical thinking got a good grip on the issue. And in Frisch's eyes not much in that direction had happened so far, since the business cycle analysis really took off in the early 1920s. A lot of data had admittedly been collected. But in quite a few of these laborious investigations a great amount of work had been "wasted because the investigations have not been animated and directed by constructive theoretical thinking." And even when it was not directly wasted it did not have the relevance for understanding economic fluctuations which the empiricists engaged in the compilation of fact thought it had. Frisch exemplified and corroborated his view that data collection for economic analysis needed to be guided by theoretical insights.

Although he did not touch upon his propagation-impulse model in these lectures the thought is close at hand that when he spoke abut "heroic guesses" and "transgressing the observational facts" he had in mind his own forthcoming contribution to the explanation of business cycles. It seems likely that he had already solved in his own mind the problem of how approximately regular, maintained cycles could be explained. He had got the logical solution but not yet written out the model that would illuminate his theory.

The young visiting professor at Yale, Ragnar Frisch, ended his introductory lecture with a scathing attack on economists adhering to other schools, well knowing that they were represented on the faculty. He knew, however, that on this issue he saw eye to eye with Irving Fisher. They had both worked in different ways to promote a more scientific economics and were now looking forward to see their efforts brought to fruition with the foundation of an econometric society for which they had great hopes.

> "I have tried in this lecture to develop my view on theory and of its necessity in the construction of a really useful science of economics. As I see it, there are only two issues possible for the historical, institutional, ands similar schools in economics. Either they have to accept theory and its far-reaching rational implications; they have to make theory serve their purpose, and by doing so rise to the dignity of a science, or they have to alienate rationality and rest forever a pure narrative art – the art of collecting and filing data. The idea that it should be possible to "explain" the things of this world by any other mental process than the one built on rationality, that is, on "theory", seem to me a fundamental naivity of the extremists of the historical and institutional schools in economics." (Frisch 1930: 25).

Thus Frisch emerges from these pages of lecture notes as an economic theoretician – or perhaps 'rationalist' – of high ambition. Frisch may not have been aware that he might have appeared also as highly arrogant. But the view he conveyed reflected also that Frisch over the last few years had gathered together the pieces he needed to introduce his explanation of cyclical fluctuations. It was the cumulative result of ideas he had worked on for years, during which had also benefited from interaction with others. In the continuation of the lecture series he elaborated on some of the ideas of his overall paradigm.

4. The propagation and impulse model 1931-1933

The propagation and impulse model as known from the Cassel festschrift is the last of several presentations 1931-33. The different presentations overlap in content but have somewhat different emphasis. Only the Cassel festschrift got published in English and thus has dominated the perception of Frisch's ideas.

His first presentation was prepared for a Nordic meeting of economists in Stockholm in June 1931. These Nordic meetings were triennial gatherings and comprised usually one lecture from each country (Denmark, Finland, Norway and Sweden), which were published with discussions in volumes of proceedings. Frisch received while he was at Yale in the beginning of 1930, a request from Oslo to take responsibility for the Norwegian contribution at the forthcoming meeting. Frisch accepted and titled his contribution *Business cycles as a statistical and theoretical problem.*[16]

Frisch prepared for the meeting at the University of Minnesota where he spent the last three months of his stay, hosted by Alvin Hansen. He submitted a paper in English for the meeting. It was translated by Johan Åkerman into Swedish, distributed to the participants and published in the proceedings.

While he was preparing for the Stockholm meeting Alvin Hansen invited him to give a talk in the Campus Club at the University of Minnesota and he chose to speak under the same title, as if it were a trial run of his Stockholm lecture.

Frisch came to the Stockholm meeting directly from the U.S.A. and had prepared a handout in which he briefly stated 14 points on the issue, see Box 2.[17]

Box 2. Frischs's handout at the Nordic meeting of economists, June 1931

Economic-theoretical part

1. The connection between the economic-theoretical and the statistical parts of the problem. Necessary for the business cycle theoretician to handle modern statistical tools.
2. Cycles of different kinds: short run cycles, long run cycles, etc.
3. Free and bound oscillations.
4. Impulse problems and propagation problems with a free oscillation. Perturbations and half-free oscillations.
5. The business cycle must for the most part be dealt with as a free oscillation.
6. The business cycle theory (understood as a theory for a free oscillation) must be *determined*. It must contain exactly the same number of conditions as variables.
7. None of the business cycle theories proposed until now, have been determined.
8. In a determined business cycle theory at least one of the conditions must be *dynamic*. From this follows *inter alia* that a system of Walrasian equations never can lead to a

[16] Frisch (1931b). Frisch's presentation is dealt with at length in Andvig (1981, 1986) but not much discussed internationally as it was published in Swedish and Norwegian.
[17] Frisch (1931b), App. IIIb., translated by the author. The number of points was a reference to President Wilson's Fourteen Points, still in fresh memory for the people who met in Stockholm.

business cycle theory. A dynamic condition is a condition which connects the values of a certain variable at two (or more) points in time.

9. A complete business cycle theory comprises three problems:
   a) The specification problem: The specification of the relevant variables.
   b) The determination problem: Analysis of the number and the independence of the posed conditions and comparison with the number of variables.
   c) The shape problem: Clarify that the posed conditions really lead to cyclical movements. This depends not only upon which variables enter which condition(s), but also of the numerical relationship. Certain values of the numerical parameters characterizing the conditions will lead to cyclical movements, while other values of the same parameters do not lead to cyclical movements. The numerical character of the conditions is thus essential.
10. An attempt at erecting a determined scheme for a business cycle theory [here followed as an addendum to point 10 a "draft of a dynamic equilibrium theory of business cycles", in fact a sketch of a macroeconomic framework with 38 endogenous variables and 37 equations].

Statistical part

11. The decomposition problem for statistical time series.
12. The distinction between *prim*-relations and *confluent* relations (*phase*-relations). Inflated and deflated phase-relations.
13. Pitfalls to watch out for when trying to determine the numerical character of the economic-theoretical laws by means of statistical data. In principle the phase-relations can always be determined, but the prim-relations can only be determined in certain cases.
14. Conditions under which it is possible to determine prim-relations statistically. Even if prim-relations cannot be determined statistically, there is a loophole: a systematic "interview" method which very likely will give useful results.

The handout may seem to have signs of having been prepared at short notice, perhaps aboard the steamship from New York to Gothenburg, as the logical structure seemed somewhat arbitrary to make it fit with 14 points, and the "determined scheme for a business cycle theory" in point 10 was neither determined nor very convincingly formulated.

In the Stockholm lecture Frisch referred to the points in the handout as a research program or "lines of advance" in business cycle analysis, necessary to achieve more fruitful and above all more definite results than achieved through the many different and partly contradictory business cycle theories.

Perhaps the most striking point in this handout is that the "economic-theoretical part" does not expound a theoretical argument for explaining, indeed, "causing" business cycles. Frisch argues convincingly that business cycle analysis must comprise a model, but says nothing about the content of the model. The "mechanisms that may generate cycles are just absent. Frisch's presentation dealt only with the formal requirements of the theory (points 6-8), rather than with the economic explanations of real phenomena. Thus it is the overall explanatory paradigm, the logic and the components of the explanation,

which is Frisch's message. In Frisch's view the real challenge in business cycle analysis was how theory must be formulated to explain cycles in a satisfactory way. Frisch was thus thinking, not in terms of theoretical explanations, but rather in terms of "theoretical structures" that with appropriate parameter values could generate cycles.

There existed potentially many such mechanisms. The idea of his very loosely sketched "model" (point 10) was not really to suggest a model, but to sketch a theoretical framework that encompassed most such theoretical structures that were of actual interest. The "equations" of his model scheme were not mathematically formulated but just a verbal description of content.

When Åkerman in the discussion stated that the only justification for such a theoretical structural framework had to an intension of turning it into a numerical model, Frisch protested mildly: "Even if we did not succeed in turning the scheme into a numerical mode, it would still be useful to show what is possible and what is impossible in logical terms. Such a system may prevent us from trying what is logically impossible. A certain tendency to try the logically impossible is often present in economic discussions."[18]

His model scheme may not have been very helpful, but the logic of it was clear. To settle whether a given structure would generate cycles the problems set out in point 9 had to be confronted, and that was Frisch's home ground. He also had a short annex to illuminate the "shape problem."

In the handout he made very little out of what would be his most famous contribution to the literature, the propagation-impulse mechanism (point 4). Also in the lecture he passed over this very briefly, just using the pendulum as an example. He noted in passing that in Schumpeter's terminology the impulse was the dynamic element, but that his own terminology was totally different and also his recent discovery that the effects of shocks could be calculated as the weighted sum of the shocks with weights given by the time shape of the time curve that would have been the solution of the determinate dynamic system in case the movement had been allowed to go on undisturbed.

Another notable characteristic of the Stockholm presentation is points 12-14, expressing Frisch's concern with the limitation simultaneity might imply for the possibility for getting at the structural or "prim-relations" (of an unspecified model!). In the lecture he elaborated on the meaning of the different kind of relations using an illuminating mathematical example, borrowed from the Yale lectures. But his points must have been hard to comprehend for the participants, not only for the special terminology that Frisch introduced, but also because Åkerman in his translation of Frisch's submitted paper had skipped the last two of the 12 pages of the submitted paper and thus omitted one of Frisch's key ideas. The missing pages contained in fact the first use by Frisch of *autonomous* and *autonomy*, in exactly the same sense as these concepts were used by Haavelmo (1944). Frisch was brief and lefts open why the autonomy/confluence aspect is an integral part of his business cycle analysis, whether the ultimate rationale was to clarify if the model could be used for social engineering, or other reasons.

Thus neither the lecture nor the handout was as comprehensive as Frisch's full conception of analysis of business cycle problems at the time. He aimed at a fairly

[18] Frisch (1931b: 165). Andvig (1986) discussed Frisch's theoretical scheme and called it "Gargantuan."

elementary presentation, although several of his concepts must have been completely new to most of the audience.

The lecture given at the University of Minnesota exactly one month earlier had been different. Frisch had started out by a brief survey of business cycle theory from Marx and Tugan-Baranowsky to Irving Frisch. The remainder had been a discussion around the following six briefly formulated theses:[19]

1. Free oscillations.
2. The propagation must be made by an equilibrium theory.
3. Fluctuations explained not by discovering some bacillus cyclicus, but by a dynamic theory.
4. Necessity of distinguishing between short cycles and long cycles.
5. Usual technique of decomposition inadequate.
6. The fundamental pitfall in the attempt at determining theoretical parameters from time series data.

As one can see there are overlapping points. In Minnesota Frisch did not discuss the role of "impulses" and thus left out a quite important part. On the other hand in Minnesota he made a point out of the equilibrium property of the propagation part, while that was not made explicit in Stockholm. It was during Frisch's stay in Minnesota that he wrote his criticism of J.M. Clark, Frisch (1931d), in close contact with Hansen, whom it seems he had brought over on his side. The controversy with Clark has been interpreted as a theoretical controversy, but Frisch's aim was mainly to point out that Clark had not fulfilled the "determination" condition the Stockholm handout's point 10.

In 1932 Frisch was invited by the Poincaré Institute at the University of Paris to give a series of lectures on *Problems and methods of econometrics*. Frisch knew the Poincaré Institute from his years in Paris in the early 1920s and was most likely aware that it had been the venue for quite influential lecture series. Frisch may well have engineered the invitation himself, perhaps through his friend and fellow econometrician Georges Darmois. It seems likely that Frisch may have seen this as a highly prestigious way of introducing the new discipline of econometrics, perhaps the most prestigious way he could think of. The lecture series was decided to take place in March-April 1933 and by agreement it would be published by the Poincaré Institute.

Frisch gave the eight lectures (in French) between 24 March and 5 April 1933. The Poincaré Institute was primarily a mathematical institution and the audience comprised mathematician and natural scientists in addition to economists and statisticians. Thus Frisch in his preparations of the lectures seemed to have taken care in explaining to people from other fields what econometrics was about. The eight lectures in the series had the following titles (translated by the author):

1. The philosophical foundations of econometrics. The axiomatic method. Utility as quantity.
2. Examples of static and semi-static econometric theories. Monopoly, polypoly. The concept of force.

[19] The lecture at the Campus Club, University of Minnesota was given on 15 May, 1931. A handwritten manuscript (16 pp.) is in the Frisch Archive.

3. What is a "dynamic" theory? Properties of determined and undetermined systems.
4. Examples of dynamic econometric theories. Oscillations in closed systems. The theory of crises.
5. The creation of cycles by random shocks. Synthesis between a probabilistic point of view and the point of view of deterministic dynamic laws.
6. The statistical construction of econometric functions. Autonomous and confluent equations. The danger of analysis of many variables.
7. Time series techniques. Decomposition of series. Linear operations and their inversion problem.
8. Conclusion: The significance of social and mechanical laws. Invariance and rigidity. Remarks on a philosophy of chaos.

As one may gather from the titles Frisch's paradigm for explaining business cycles figured prominently, particularly lecture nos. 4 and 5 were devoted to that, while also the topics lecture nos. 3, 6 and 7 played some role within the paradigm. But the lectures were never published, neither by the Poincaré Institute nor by Frisch.[20] The reason why is not obvious, but a likely hypothesis is that Frisch did not get around to finalize the manuscript until too much time had passed or changed his mind about it. Perhaps was Frisch's also affected by the fact that some of the material in the lectures became published through other channels. The Cassel festschrift drew particularly upon lecture no. 4, in fact long passages are practically identical in content, while another lecture practically coincided with Frisch's contribution to the Westergaard festschrift also published in 1933.

The Poincaré lecture no. 4 comprised the model of the Cassel festschrift and also the parameter values chosen by Frisch. Apparently he had assistants working on the model in Oslo and had hoped to be able to present the complete solution of the model, but instead he passed on the following message to the audience:

> "One of my assistants at the University of Norway is just about to do the necessary calculations for determining the solution of the model. He has invented a graphical method for the solution of the characteristic equation, which is very convenient. I had hoped to be able to bring you the exact, numerical results, but I have just received a letter from my assistant, in which he tells me that the results he has achieved are not the correct ones as a mistake has slipped into the calculation and therefore I cannot give you any results." (Frisch (1933a), translated by the author).

Before he came to the macrodynamic model part of lecture no. 4 had gave a very pedagogic and instructive discussion of a microdynamic scheme, the core of which was the following model:

$$x = a_0 - a_1 p + a_2 \dot{p} - a_3 X \quad \text{(demand)}$$
$$y = b_0 + b_1 p - b_2 \dot{p} + b_3 X \quad \text{(supply)}$$
$$u = c_0 + c_1 p + c_2 \dot{p} - c_3 X \quad \text{(production)}$$

[20] The manuscript for the Poincaré lectures was typewritten, probably in 1932, but when Frisch gave the lecture series he had rewritten most of the pages in the margins of the original manuscript and on additional sheets of paper, with many passages hard to decipher. The lectures are now being prepared for publication by Routledge in 2008.

The dynamics here is given by the appearance of $\dot{p}$ in addition to p in the expression for *x*, *y* and *z*, but also via the stock of goods *X*, as $\dot{X} = u - y$. Furthermore, an equilibrium condition in this model is that $x = y$. Frisch's presentation, discussion and solution, both numerically and graphically, are all very instructive.

While preparing the Poincaré lectures Frisch was in December 1932 invited to contribute to a festschrift to celebrate the 70th birthday of Gustav Cassel. The invitation came from Karin Kock who was the editor of the festschrift.[21] Frisch accepted and promised a publication. He was no friend of Cassel and hardly ever referred to him. The deadline for the contribution was in late spring, but Frisch did not meet it. Kock instructed him in June to send the MS directly to the publisher in London. But when Frisch finally submitted his contribution it was way above the editor's suggested length. Kock insisted that it had to be cut down. Frisch yielded and allowed what was originally section 2 titled "An example of micro-dynamic analysis" to be deleted.[22] Frisch got the proofs in late August and returned it a few days later after adapting the manuscript to the deletion of section 2. The deleted microeconomic part was taken from the Poincaré lectures and comprised the same microeconomic model, see above. The "Tableau Economique" of the Cassel paper had also been lifted from the Poincaré lectures. The Cassel festschrift appeared in October 1933.

Having sent off the proof pages he must have rushed into preparing his presentation of the paper "Some problems in economic macrodynamics" at the Third European Econometric Society meeting in Leyden 29 Sept. – 2 Oct.[23] At the Leyden meeting Frisch clearly presented on the basis of the Cassel paper. Marschak, who wrote the report, did not use the "propagation" and "impulse" terms to explain Frisch's mechanism and gave the impression that the model was, indeed, Frisch's business cycle model.

Thus Frisch worked to solve three equation difference-differential model for the Cassel festschrift after he returned from Paris presentation. This was achieved but the Cassell festschrift presentation became tilted in the direction of too much emphasis on the numerical solution of the model, giving the impression that Frisch was strongly wedded to this model. But reading carefully Frisch's discussion of possible economic mechanisms in the Cassel paper one cannot avoid the impression that the choice of mechanism for demonstrating the paradigm is just a choice of convenience.

One aspect of Frisch's paradigm is the introduction of the *macro* aspect. The *micro/macro* distinction in Frisch's work can be traced to Frisch (1929b: 323, footnote 3). He used it also as *microcosmic/macrocosmic* in his Yale lectures (Frisch 1930), then *macrodynamic* took over as the favourite term as in the title of his paper at the Leyden meeting, *Some Problems in Economic Macrodynamics* (Marschak, 1934: 189). Frisch did not use *micro/macro* in the Poincaré lectures. He introduced (in Norwegian) "micro-

[21] Karin Kock who at the time worked as assistant to Erik Lindahl, became the first female professor of economics in Sweden.

[22] In the end Frisch's contribution was still 50 % longer than any other chapter in the festschrift. The late delivery of a too long manuscript happened on more than one occasion with Frisch.

[23] It was not only macrodynamics he prepared for Leyden. The entire first day of the meeting was devoted to "colloquium-lectures" by Frisch on the algebra of linear transformations and quadratic forms. Frisch lectured in altogether 8 hours, from 10:00 until 22:00 with two two-hour breaks!

/macroeconomics" in lectures to Norwegian students (Frisch 1934c). Micro/macro were of course used in other sciences from which Frisch had adapted them. His contribution was not so much the term but that he pointed to the need for macroeconomic reasoning and, indeed, a need for macroeconomic data, as in the Cassel paper:

> When we approach the study of business cycles with the intention of carrying through an analysis that is really dynamic and determinate in the above sense, we are naturally led to distinguish between two types of analysis: the micro-dynamic and the macro-dynamic types. The micro-dynamic analysis is an analysis by which we try to explain in some detail the behaviour of a certain section of the huge economic mechanism, taking for granted that certain general parameters are given. (…)
>
> The macro-dynamic, on the other hand, tries to give an account of the fluctuations of the whole economic system taken in its entirety. Obviously in this case it is impossible to carry through the analysis in great detail. … we must deliberately disregard a considerable amount of the details of the picture. We may perhaps start by throwing all kinds of production into one variable, all consumption into another, and so on, imagining that the notions "production", "consumption", and so on, can be measured by some sort of total indices. (Frisch, 1933b: 172-173).

Clearly, there could be many phenomena in the economic system that could be usefully described by a micro-dynamic formulation. Such mechanisms had the potential of contributing to an explanation of macro-dynamic cycles. To move from a micro-dynamic formulation to macro-dynamics was a step away from "getting concepts from the observational technique." Frisch seemed to be begging for national accounts aggregates in the last sentence of the quote, but national accounts were still at an embryonic stage. Frisch (1933b) clearly helped to pave the way for macroeconomic models. He took upon himself in 1937 to develop national accounts for Norway, but like so many of his projects he was not able to complete it (although in this case his pupils did).

Before he presented his overall paradigm he had also other conceptual clarifications. Frisch had found concepts like 'statics' and 'dynamics' and also that of 'equilibrium' used in imprecise and ambiguous ways in business cycle analysis. He found that the "distinction between statics and dynamics is confused with the distinction between the case where 'equilibrium is realized' and the case where 'equilibrium is not realized'." Some explication of concepts was called for as the situation was confusing:

> "In the great variety of cycle "explanations" which we have had in the course of time, there are very few which really attack the cycle problem as an <u>equilibrium</u> problem in the sense here envisaged. The great bulk of cycle theories have not yet emerged from the stage where the static theory was before the static equilibrium theories were developed: many of the current cycle "explanations" consist merely in chasing one unknown at a time in turn over on the left and side of the equation." (Frisch, 1930: 71).

His *statics/dynamics* distinction became well known and was quite original with Frisch. He first formulated his notion that these concepts should be used to characterize the methods of analysis and not the phenomena themselves, in a lecture in Copenhagen in 1928 (Frisch 1929b). In Frisch's terminology an economic system could be stationary or changing, while the method of analysis of such a system could be either static or dynamic.

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 showing that all four combinations of them may be perfectly meaningful. In the Poincaré lecture no. 3 he illuminated the issue by using both mechanical and economic examples (not too contrived) to drive his point home. (As it is not unusual still to come across confused opinions about the static/dynamic distinction, Frisch's examples may still serve a purpose.)

Frisch did not publish much on his conception of equilibrium, but dealt with it at some length e.g. 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 characteristics of the model world. Hence, it had no meaning 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 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: 72).

In the Yale lectures he also elaborated upon the concept of moving equilibrium which is implicit in the propagation model. He compared and distinguished his concepts from those of H.L. Moore, J. M. Clark and L. Robbins, finding himself rather close to Robbins, but finding faults with all of them. (Frisch 1930: 76-79).

6. Aftermath: promises, unfulfilled ambitions and a failed research program?

Frisch's ambitions in analysis of business cycles went far beyond what he had deal with in tha Cassel festschrift and became his research program until World War II interrupted. He was also in demand to present his views.

In the spring of 1934 Frisch visited Cambridge and London. He gave two lectures at the London School of Economics on *Dynamic economics and the theory of business cycles* (Frisch, 1934b), opening the first lecture by speaking about "the four stages of business cycle theory". The first "primitive stage" covering everything that had taken place until the 1920s, was followed by the "stage of determinate dynamic analyses", as put forth by Evans, Roos and Tinbergen as contributors of microdynamic analyses and Frisch, Kalecki, Amoroso and Vinci as contributors of macrodynamic analyses. Then there was the "stage of comparative studies, the multiplicity of schemes leading to cycles", perhaps a characterization of the situation at that time with the Leyden meeting in fresh memory. The final stage was the "reformulation of the problem whereby the general theory of changing harmonics appears as the necessary complement to the structural-economic theory," was merely a characterization of Frisch's own approach. Thus at LSE Frisch pointed to structural-economic theory and the changing harmonics as the two key elements in his approach.

The structural-economic theory – or macroeconomic model – was a mathematical formulation of the theory causing the ups and downs in economic activity. With explanations of business cycles not explicitly formulated as a system of equations there were many examples, Frisch asserted, of explaining the upswing or the downswing by "stretching a hand out to pull a new variable into the model." Frisch was ahead of his time in his insistence upon a structural model, representing an equilibrium situation, typically a stable equilibrium.[24] On the other hand Frisch did not seem to be equally much interested in the theoretical content of the structural-economic schemes. His lectures on macrodynamics and monetary theory showed that he was very familiar with the theoretical literature, and excelled in putting verbally formulated reasoning into equations. When Keynes' *General Theory* appeared he was disappointed with its lack of macrodynamics, and thus by implication its inability to explain cyclical fluctuations according to Frisch's methodological prescriptions.

In the LSE lecture in 1934 he even included a list of structural-economic schemes:

1. Simplified systems not leading to cycles
2. Systems built on the wearing-out-period of capital goods. Primary investment and reinvestment.
3. Systems involving the construction period for capital goods. The discrepancy between capital starting and carry-on activity.
4. Disproportionality between industrial groups and branches. Marginal productivity interpreted as a force defining a gravitational field.
5. Pure circulation cycles. Intrinsic tendencies towards contraction and expansion in an exchange system.
6. Cycles created by profit accumulation.
7. Monetary cycles. The discrepancy between savings and investment. Credit expansion and contraction, the inflation and deflation mechanism.
   (Frisch, 1934b).

Here the construction time explanation of cycles (no. 3) originated in Aftalion and the monetary cycles (no. 7) in Wicksell. The mathematical implications of replacement investment (no. 2) had been investigated in Frisch (1927a), and "pure circulation cycles" (no. 2) had been discussed in Frisch (1934d).

What then was the changing harmonics as "the necessary complement to the structural-economic theory" as Frisch had stated it in London? Frisch had introduced this term in his 1928 article as a description for a cycle that was more or less regular over time. By 1933 he had achieved at a full understanding of how random shocks created changing harmonics from damped cycles of a (linear) structural model. In Frisch (1933b) he defined a changing harmonic as follows: "By a changing harmonic I understand 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."

[24] Frisch (1939) seems to have been written to correct a misunderstanding stated by Gerhard Tintner, namely that the propagation-impulse mechanism was a case of an unstable equilibrium in the deterministic model resulting in large cyclical fluctuations when disturbed by small erratic fluctuations. It must have been disheartening for Frisch that a veteran econometrician like Tintner held such a misconceived opinion.

The propagation-impulse mechanism was a qualitative result with a strong intuitive appeal, the shocks energized the damped cycles of the "structural-economic theory" and prevented them from dying out. But Frisch wanted more than that. Also the effect of the shocks could be quantified, that is, as the effect of the stream of shocks upon the cycles generated by the model.

Slutsky's work had showed that if one applied a moving average to erratic data, there would emerge cycle of length 3.2 and 0.7, relative to the length of the period of moving average. The existence of these cycles could be proved both theoretically and experimentally. This was a phenomenon of the same nature as the explanation of the maintenance of economic cycles and the creation of new cycles. It thus all went back to Slutsky, the maintenance and the creation of these cycles followed definite laws that could be analysed and explained a priori.

The characteristics of the maintained cycles could be explained partly by the weight system 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.[25] 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 1934b).

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.

What Frisch had showed in the Cassel festschrift contribution was 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. He referred to a forthcoming publication on many occasions, and indeed, also in the Cassel festschrift, which made references to a forthcoming article in

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

Econometrica, promising "detailed mathematical analysis" and "detailed proof, together with extensive numerical computations."[26]

One reason for this outcome may be that Frisch had raised the ambition in his project and was not able fully to complete the enhanced project. One such raised ambition was the two-fold nature of the stochastic shocks he wanted to incorporate. This is reflected in Frisch's discussion in the Cassel festschrift of how the Schumpeterian theory of innovations could be included in his paradigm. The interaction with Schumpeter about this had started in 1931. Frisch introduced Schumpeter to his propagation-impulse paradigm in 1931 and argued that the more or less unpredictable innovations that figured so prominently in Schumpeter's theory, should be regarded as impulses, like peas pelting Yule's pendulum. Schumpeter disagreed, arguing that the innovations "come from the inside, they create economic phenomena. … If you class innovations simply among impulses you arrive at some such position as Pigou, and miss what seems to me the very heart of the matter: You catch only the 'vibrations' incident to the impact of the 'impulse' and not the phenomena attaching to the impulse itself." (Schumpeter/Frisch, 24 June 1931).

That was the beginning of an elongated exchange. The regular random Wicksellian rocking horse shocks caused no changes to the model. 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*.[27] The innovations were not randomly distributed, but might still energize the swings of an economic system or even cause a "secular or supersecular movement" by changing the economic mechanism (Frisch, 1933b, p.205). Frisch was bent on solving these problems in a satisfactory way and it led him at first to construct in his mind physical analogy of Schumpeter's innovations:

> Suppose you have a pendulum exposed to friction, so that its motion would die down if it were left to itself. Now build a container of water on top of the pendulum. Further build a pipe down through the length of the pendulum and arrange an outlet for the water at the very lowest point of the pendulum. This outlet shall be of the following very peculiar sort: Its opening points to the left and is equipped with a valve that is regulated by the velocity of the pendulum. The regulation of the valve is such that the opening is largest when the pendulum moves towards the right, and in particular the opening is largest when the speed of the pendulum (towards the right) is at its maximum. We can imagine that the opening is some simple function of the speed. When the pendulum moves

[26] Frisch (1933b:199-203). Frisch asked E.B. Wilson, a highly respected statistician at Harvard University and member of the Econometric Society, to referee the article he wanted to publish in Econometrica The MS was titled "Changing Harmonics studied from the point of view of linear operators and erratic shocks". Wilson found Frisch's treatise unsuitable for Econometrica and that finished it. In Wilson's view, no economists would read it and if written for mathematicians, it could be considerably condensed!

After Econometrica was given up, another plan was to have the treatise published as a Cowles Commission Monograph, titled "Changing harmonics a study of the effects of linear operations performed on erratic shocks." But it never appeared. The residue of Frisch's huge project and efforts is still residing in boxes in the Frisch archive at the Department of Economics, University of Oslo.

[27] "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 cumulation cycles will emerge." (Frisch, 1938).

> towards the left the opening is nearly closed, the opening being at the smallest when the speed towards the left is the largest. This variation of the opening both under the movement to the left and the right can of course be represented by one and the same functional relationship. The only difference between the two movements is, indeed, that the variable on which the opening depends in one case is positive and in the other is negative. The amount of water flowing out of the valve will depend not only on the size of the opening as here discussed but also on the pressure, that is on the level of the water in the container above the pendulum. Now let the container be alimented with water from some source which we may consider as a datum in the problem. (Frisch/Schumpeter, 5 July 1931).

If the system was left to itself it would evolve in cycles whose length will be determined, partly by the length of the pendulum, partly by the friction and partly by details of the device for opening the valve. The interpretation was obvious. The water represented the new ideas, inventions etc. They were not utilized immediately but were stored until the next period of prosperity (or even longer). When they finally were utilized they provided the additional surplus of energy necessary to maintain the swings and prevented them from dying out. The amount of energy released depended upon whether there was a large amount of potential innovations stored and also on the velocity of the upswing.

> This picture may now be completed by taking into account random disturbances of the type which I had originally in mind: Imagine a series of random impulses, working either to the right or to the left and being distributed in time and size according to some sort of chance law. (…)
>
> As I see it now there are two aspects to the impulse (or "energy") problem: On the one hand the more or less random irregularity of inventions and progress in the arts. This idea can be followed back to Knut Wicksell's "Hack-teori". On the other hand the periodic release in the actual utilization of "stored" inventions, which is your idea. Which one of the two which is actually the most important in the sense of representing the largest source of "energy" for the maintenance of the economic swings I think nobody can say today. This can only be found out by painstaking studies that are econometric in the best sense of the word. (Frisch/Schumpeter, 5 July 1931).

Frisch's elaborate mechanical device showed, as he stated, his physical approach to thinking about dynamic problems. It may be read in support of an interpretation of his business cycle work as based on a rather mechanistic view of economic cycles. The raised ambition in incorporating Schumpeterian shocks, as if Frisch wanted to pay tribute to his greatest admirer in the Econometric Society, made it far more difficult to complete his elaborate theory.

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 either by the pure Slutsky effect (accumulation cycles) or 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 1936b:16).

Frisch credited Divisia for having been the first to suggest that the shocks might be measured quantitatively by "errors" in the deterministic system.[28] But what was really a shock? Frisch had given a definition in a discussion at the Namur meeting in 1935 (Frisch, 1936a), which as recapitulated a few years later went as follows: "A shock is any event which contradicts the assumptions of some pure economic theory and thus prevents the variables from following the exact course implied by that theory", see Frisch (1939: 639). The effect of a shock was to make the elements of the system behave a little differently from what was assumed in theory, pushing the point ever so often out of position.

Considerable efforts were exerted at Frisch's Institute 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 very definite and comprehensive results. The motivation for the search for the solution to the inversion problem was that the ultimate purpose was to provide a much better basis for forecasting. Instead of attempting more or less mechanical extrapolation 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." The general idea of the forecasting problem, as rephrased from the Rockefeller report, ran as follows:

First, determine the weight system by which the erratic shocks were accumulated. Then assess those erratic shocks in the past which it was necessary to know to have sufficient knowledge of the further evolutionary tendencies of the system. That had to be done by utilizing all information available up to a given point in time. Then a forecast could be made on the assumption that all future random disturbances would be zero.

> This conception of the forecasting problem gives a natural classification of those features that are known and those that are not. The whole setup further introduces in a very plausible way an increasing amount of indeterminateness as we try to forecast further and further into the future. One essential feature of a good forecasting system must of course be that it brings this increasing indeterminateness into clear relief. (Frisch, 1936b: 18).

Frisch added in the report to the Rockefeller Foundation that attempts at forecasting by the outlined method had been made with "very simplified constructed models" with a fair degree of success, but was still only in its beginning. Not much real progress had apparently been achieved by the time World War II broke out and after the war it was no longer on the agenda.

The history of Ragnar Frisch's approach to business cycle analysis leaves several puzzles. Why did the success of the propagation-impulse model not lead Frisch towards the path that Tinbergen chose of constructing real models of the macroeconomy and finding ways of testing the model, perhaps better than Tinbergen's, against statistical data? Did his way of testing a model against data focus too much on the properties of observed vs. modeled cycles, or was he just fascinated by the challenge of cracking "the inversion problem", without realizing that except for very simple models it would be too much for the equipment he had available? His involvement with random shocks was profound, but his ambition of determining them turned out to be futile. Perhaps the most

[28] Frisch (1939: 639). According to Frisch Divisia had made this point in a discussion at the Leyden meeting in 1933, although it is not in the report from the meeting.

positive outcome of Frisch's effort was that he may have inspired his chief assistent and student, Trygve Haavelmo, to turn the problem around some years a later in his *Probability Approach* (Haavelmo, 1944).

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