

Statistics Norway
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Ragnar Frisch 1895-1995

Documents



Introduction*

In 1926 Ragnar Frisch published *Sur un problème d'économie pure* (Frisch, 1926a) in a not very widely distributed publication issued by the Norwegian Mathematical Association at the University of Oslo, the only university in Norway at that time. It was Frisch's first published paper in economics and had been prepared largely during his stay in Paris in the preceding years. Since 1923 Frisch had published some papers in theoretical statistics, but he was on a course to become - for lack of a better term - a mathematical economist, firmly determined to give his scientific contribution within economics. He had prepared himself for the task by far-reaching studies in mathematics and statistics in addition to his readings of economic literature. The two professors of economics in Norway at this time belonged to the Faculty of Law, and there was no research institution or separate university department in economics. Hence, colleagues with related scientific interests had by and large to be found abroad, and Frisch had established connections, mostly within Europe, by travels and correspondence.

The *Sur un problème* essay made a forceful impression on the small number of mathematically inclined economists who - like Irving Fisher - happened to be introduced to it. It was not translated and published in English until 1972 at the very end of Frisch's life. The paper would deserve a place in the history of economics, even for no other reason than the opening sentences, which coined the first of Frisch's many - and by far the most important - contributions to the international terminology of the discipline:

'Intermédiaire entre les mathématiques, la statistique et l'économie politique, nous trouvons une discipline nouvelle que l'on peut, faute de mieux, désigner sous le nom de *l'économétrie*. L'économétrie se pose le but de soumettre les lois abstraites de l'économie politique théorique ou l'économie 'pure' à une vérification expérimentale et numériques, et ainsi de constituer, autant que cela est possible, l'économie pure en une science dans le sens restreint de ce mot' (Frisch, 1926a, p.1).

In English translation:

'Intermediate between mathematics, statistics, and economics, we find a new discipline which for lack of a better name, may be called *econometrics*. Econometrics has as its aim to subject abstract laws of theoretical political economy or 'pure' economics to experimental and numerical verification, and thus to turn pure economics, as far as possible, into a science in the strict sense of the word' (Frisch, 1971b, p.386).

From then on Frisch started to use 'econometric' and 'econometrics' in his communication with fellow economists. A few years later the newly coined term gave name to an organization and a journal, which jointly would exert a strong influence on the development of economics as a science.¹

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The contribution of Frisch's 1926 paper was, however, more than the coinage of a new term. It was a powerful demonstration of the meaning of econometrics: mathematical precision in the formulation of theoretical concepts and relationships so as to make them quantifiable, and erudition and ingenuity in the application of statistical methods to available data. The paper drew up further perspectives for the development of the newly coined discipline, ending in the following statement:

'I believe that economic theory has arrived at a point in its development where the appeal to quantitative empirical data has become more necessary than ever. At the same time its analyses have reached a degree of complexity that require the application of a more refined scientific method than that employed by the classical economists' (ibid., p.417).

Clearly, Frisch had committed himself to contribute towards making this come true. In the role he came to play for the development of econometrics, in particular within the Econometric Society, he also exerted a strong influence on others both within his own generation, on budding talents, and even on some of the most prominent of his elders.

A life in perspective

Frisch was born as Ragnar Anton Kittil Frisch in Oslo on 3rd March 1895. His father, Anton Frisch, was a jeweller with his own gold- and silversmith firm in the centre of Oslo in a building which also housed the owner and his wife when Ragnar was born. The firm had been established in 1856 by Frisch's grandfather. As the name suggests the Frisch family had its origin on the Continent. One of the ancestors had been recruited in the reign of King Christian IV of Denmark-Norway as a mining specialist from Freiberg in Saxony to work in the silver mines at Kongsberg in Norway. Hence, gold and silver work was a long-lasting family tradition, and it was no wonder that the family expected Ragnar to continue in the same craft and take over the family business when his father retired.

Frisch's mother, Ragna Fredrikke Kittilsen, was apparently less sure that the obvious choice was also the best choice. In a brief autobiographical note Frisch wrote in connection with the Nobel Prize bestowed on him in 1969 he credits his mother with '... a great impact on my general outlook and view on life' (Frisch, 1970a, p.211). Frisch finished school in 1913 with excellent marks and began his apprenticeships with one of the larger gold and silversmith firms in Oslo. His mother '... got a strong feeling that the trade would not be satisfactory for me in the long run ... [and] ... insisted that at the same time as I completed my apprenticeship I should take up a university study' (ibid., p.211). Economics was chosen as 'the *shortest* and *easiest* study' (ibid., p.211). The first separate study of economics had been opened at the University of Oslo only recently in 1906 as a two-year study under the Faculty of Law. Frisch enrolled in 1916, while still apprenticed as a goldsmith, completed his economics degree in 1919 and in 1920 his apprenticeship with a coffee set in silver as probation work.

Frisch married Marie Smedal in 1920. Her importance in Frisch's life by making it possible for him to work over a long period of years as if the day had close to 24 working hours can hardly be exaggerated. It seems that she must have been totally devoted to him and a source for the superhuman energy and endurance he could mobilize in his pursuance of scientific quests.² Frisch's only child - Marie Ragna Antonette - was born in 1938, Frisch was 43. (Marie Smedal died in 1952, Frisch married Astrid Johannesen in 1953.)

Around 1920 Frisch must already have done the choice to leave the family business behind as a serious preoccupation in his life and instead pursue the course that would determine his career. '... I went abroad to study economics and mathematics in earnest. I visited France, Germany, Great Britain, the United States and Italy' (Frisch, 1970a, p.211). His stay in France lasted more than two years.

In 1925 he was appointed assistant professor by the University of Oslo.³ In 1926 he defended his doctoral thesis *Sur les semi-invariants et moments employés dans l'étude des distributions statistiques* (Frisch, 1926b). Around this time he established a long-lasting relationship with the Rockefeller Foundation when he received a three-year fellowship from the Laura Spelman Rockefeller Memorial from 1927. He spent 1927/28 in the United States, but visited also France and Italy. Later on he would exploit his good relationship with the Rockefeller Foundation to secure scholarships for selected pupils and sometimes for emigrés from Germany.

Frisch's life as an active economist stretched over a period of more than fifty years from when he graduated in 1919 till he died in 1973. He played a particularly prominent role in the international community of econometricians from the mid-1920s till the first postwar years. During the Second World War normal means of international communication were cut off, and Frisch was for a period put into a German internment camp in occupied Norway. Before his first paper on an economic topic in 1926 Frisch had published some papers in theoretical statistics. The last paper published in his lifetime was a speech addressed to Norwegian economists on cooperation between politicians and econometricians on the formulation of political preferences.⁴

Frisch's academic life can be roughly divided into two approximately equally long periods before and after 1945. In the first 20 years after the early 1920s Frisch travelled much and moved fast from the stage of an advanced student to that of an important contributor in several fields. He played a key role in putting the embryonic Econometric Society on the right track of development. He was an econometrician in the broad sense of the term, i.e. as he had introduced it, rather than in the more limited sense of statistical methods applied to economic theory which later took over the term. Perhaps it could be said that in those years he was not a stranger to any field that could be called econometric.

As Editor of *Econometrica* from its first issue and a very active participant at many of the meetings of Econometric Society he exerted a strong influence. Frisch was a leading figure in the small circuit of European econometricians after an impressive presence at the first Econometric Society

Meeting in Lausanne in September 1931. Frisch gave the Opening Address, the first ever at an Econometric Society Meeting, as well as the Closing Address, presented three out of nineteen papers, and had a large number of interventions at the meeting.⁵

Frisch maintained a huge correspondence and kept in touch with a large number of leading economists in many countries. Many of his contemporaries would remember him during his most active years as an abundant source of fresh and innovative ideas. The impression was underlined by his inventiveness with regard to suggesting new denotations, often combined with new or reformulated connotations. Some of the new terms he coined got defunct quickly, but others became standard terms in the discipline in many languages.⁶

Frisch became an *initiator* of new approaches, always solidly backed by theoretical corroboration, often also with impressive numerical illustrations, rather than someone to write up the final classical volume in any single field. From the mid-1920s he pursued a number of fields more or less simultaneously: production theory, time series analysis, utility measurement, dynamic modelling, econometric methods, national accounts et al. Typically, his main contributions are attempts at opening up new avenues and suggesting approaches hitherto untried. Perhaps, it could be ventured that he was more concerned about the methods of theoretical reasoning and statistical verification than in the resulting theories and estimates, although such a statement may easily be criticized as a too confining characterization of his activities. Frisch is difficult to classify in the history of economics, due to his diversity, his originality, and his lack of firm association with any "school". His persuasive enthusiasm was usually at par or even surpassing his erudition in the effort to promote his ideas. His views as to the fruitfulness of pursuing the new ideas may occasionally have been overoptimistic. His most widely studied contributions were relatively quickly superseded by the development within the discipline, a fact which may indicate the heavy impact of his contributions rather than the opposite. Important residue from the ideas he had introduced would, however, remain embedded in new theoretical foundations.

Ragnar Frisch left an impressive legacy of publications.⁷ He published original articles in four languages: English, French, German, and Norwegian. Some of the articles and monographs became instant classics, while other parts of his production remained relatively little known, partly due to language. It added to his inaccessibility that his publications often might include passages hard to penetrate for anyone less erudite in mathematics and statistics. Frisch was reluctant to release anything for publication in print unless it was meticulously edited and proofread by himself.

The ratio of unpublished to published papers was large. Some papers became influential and read by many, although they were never properly published. Many of Frisch's unpublished papers acquired a semipublished status as Memoranda from the Institute of Economics at the University of Oslo.⁸ Even for the Memoranda Frisch applied strict standards of approval! It was apparently difficult for him to complete his publications, as long as he considered there was more to do. There was always an urgency about Frisch, he seemed always busily absorbed in ongoing research and left a trace of unfulfilled promises to complete and deliver. Some parts of his early pioneering

efforts were never properly published and other parts delayed until long overdue. His streak of perfectionism was probably a stronger barrier for publication, than the fact that he also overbooked his time.

A number of Frisch's works, in particular his contributions to econometrics and economic dynamics, have been analyzed and discussed in contemporaneous and later articles, including in recent years, articles and more comprehensive presentations of the history of econometrics, e.g. Morgan (1990). Overall discussions of Frisch's contributions, although of a laudatory nature, are Arrow (1960), Johansen (1969), and Edvardsen (1970).

Although a prominent aspect of Frisch's life in the interwar period was his interaction with the international community of economists, with much travelling on long-distance trains and transatlantic liners and extended stays abroad as necessary sacrifices, he was firmly rooted in Norway and his home town of Oslo. His commitments at the University of Oslo were large. In 1928 Frisch was promoted to Associate Professor, and a new full professorship was created for him from 1931. The Institute of Economics at the University of Oslo [Universitetets Økonomiske Institutt] was founded in 1932.⁹ From the inception Frisch named himself the Institute's "director of research". From 1937 until he retired at seventy years in 1965 he was the plenipotentiary of the Institute and director of all activities.

The Institute became an important - and for a long time the only - centre for teaching and research of economics in Norway. A comprehensive five-year study of economics and statistics for a master degree (cand.oecon.) was adopted by the Storting (Parliament) in 1935. Frisch was the leading force in choosing a curriculum and teaching the new study. In his teaching Frisch had a tendency to take his research notes with him into the classroom. He may have overly strained the endurance and patience of his students in the interwar period with lectures on confluence analysis and complex roots of dynamic systems for which few students would have the mathematical prerequisites to understand. Some of those who may have benefitted most from Frisch's research oriented - rather than textbook oriented - lectures were foreign advanced students or research associates whom Frisch had invited or helped to arrange a stay for in Oslo. Among the most prominent students in this category were the Dutch economists Jan Tinbergen, Tjalling Koopmans and Pieter de Wolff with mathematical capabilities to match those of Frisch and the willingness to learn Norwegian, at least in a reading capacity.¹⁰ These early years of higher studies in economics in Norway laid the foundation for the tradition of teaching economics in Norway, and in the first postwar decade the number of students of economics boomed.

The Institute was for Frisch a laboratory, rather than a classroom. He would refer to it in various contexts as an economic laboratory, an econometric laboratory, as well as a statistical laboratory. Frisch's good relations with the Rockefeller Foundation had secured an essential financial support for the Institute which in the early years received little government support. The Institute would engage in ad hoc studies on request. The empirical research effort in the late 1930s was

concentrated in a project called "Plan for an economic structural survey of Norway", which included an ambition to establish fairly detailed national accounts for Norway.¹¹

A special feature of Frisch's research interests was his knack for numerical calculations. Some of his papers were almost superhuman in terms of numerical calculations. It is no exaggeration to state that Frisch had a passion for numerical analysis, particularly equation solving and optimization, and took a great interest in the mechanical and electric tools available for such calculations, e.g. the analogue computers used as "integrators" for solving differential equations. It may seem fitting in retrospect that Frisch's very first paper (Frisch, 1923) was a recipe for the most economical way of undertaking certain numerical calculations. It was the first of a great number of memoranda and notes, more like instructions, about how manual calculations should be conducted and about numerous newly invented "methods". For several years in the 1950s Frisch kept in close touch with the designers of Swedish calculators and posed calculation problems for them to solve, usually optimization problems drawn from his planning model exercises. The interest in numerical analysis methods was carried over to computers. He kept well informed about the computer development in the United States in the 1950s and became an intensive user of the computer equipment available in Norway in the last 10-15 years of his career. One may well wonder how he would have fared in today's world of immense computer resources at the fingertips!

Frisch took active part in politico-economic debates in prewar Norway, an activity which continued in the latter part of the 1940s.¹² He never meddled deeply into politics, but played an active role in promoting the modern economic view on appropriate policy measures. His sympathies lay in the 1930s with the Labour Party after it had shed its revolutionary allegiance and looked for reform policies to take Norway out of the depression. In 1933 he was an anonymous drafter of parts of a three year "Crisis Plan" launched by the Labour Party.¹³

In the second part of Frisch's academic life after the Second World War the emphasis shifted in his theoretical and econometric work. Frisch had always been concerned with the requirements of a rational macroeconomic planning. Now he made this the overriding aim of his research. In the early postwar period this was very much in line with the political programme of the Labour Party government which from 1945 until 1961 ruled with a comfortable parliamentary majority. The government never offered Frisch a formal role as a permanent advisor on economic policy and planning, but clearly held Frisch in high regard, although he may have been viewed as a man not constrained by the maxim of politics as the art of the possible. The shift from the interwar period was not abrupt in all respects, however. Frisch remained as the Editor of *Econometrica* until 1955, but he did not continue to play the innovative role in theory and econometric methods he had pursued before the war. The shift of emphasis in Frisch's agenda is set out programmatically in *The Responsibility of the Econometrician* (REF1946), the article which marked his return to the chair as Editor of *Econometrica* in 1945.

The teaching of economics became in the postwar period a more absorbing task for the Institute. The intake of students literally overflowed the capacity. Frisch tended to return to his habit of

combining lectures with ongoing research projects and leave most of the curriculum teaching to current and former assistants, not least Trygve Haavelmo, who was full professor from 1948. Frisch's teaching obligations were often interrupted by shorter or longer absence for travels. He would catch up by "double lectures" which on occasion might last for three hours without break! The preparations for these might at times be improvisatory. He would then take his lead either from the curriculum or from recent research and let the well-equipped students share in his extemporaneous reasoning. Nothing could be more inspiring for the research oriented students than these impromptu performances in research curiosity and erudition combined!¹⁴ Frisch also felt compelled at times to give the rank and file of students strongly worded talks about the need to pursue research curiosity, and not just to optimize with regard to the examination requirements.¹⁵

The importance of a good economic education programme naturally figured prominently in Frisch's priorities. High quality graduates in economics within government administration would be a *sine qua non* for implementing rational planning.

'We may predict that the science of which we try to be the humble and devoted servants will in the future life of the nations be an important factor in eliminating maladjustments between fundamental economic sectors and assure a smooth and progressive utilization of resources. ... One wants men with a knowledge of the characteristic features of the economic and social structure of their country and with a fundamental theoretical knowledge along modern lines' (Frisch, 1946, p.1).

The message was that econometrics was only for the chosen:

'It should be stated explicitly that such an increase in the number of men devoted to econometrics is desirable only on the condition of *quality*. ... There are so many chances of abusing it, of doing more harm than good with it, that it should only be put in the hands of really first-rate men. Others should be absolutely discouraged from taking up econometrics' (Frisch, 1946, p.4).

Frisch outlined an ambitious plan for training top level economic graduates to become mature econometricians (in the broad sense) through several years as associates at the Institute of Economics.¹⁶ The plan was never backed by resources, but Frisch managed to keep a number of his best students for a shorter or longer period as associates at the Institute. They would take part in one or more of Frisch's major research projects before they moved on to fill positions within research or administration. The better part of the vintages of economists from the mid-1940s until the early 1950s may perhaps be considered as the prime carriers of the Frischian heritage.

Frisch's effort at developing a theoretical foundation for economic planning at a national level led him to investigate a number of complicated and to some extent separate issues. One was the choice of model. Frisch constructed several prototypes at the Institute of Economics in a continuous effort at building models of the economy from the late 1940s until he retired in the mid-1960s.

Frisch became the first Chairman of the United Nations Economic and Employment Commission when it was established in 1947, and his first postwar attempt at macroeconomic modelling was

directly related to his work for the Commission. Frisch's model exercises were generally large and tended to stretch the limit with regard to data requirements and computer needs, with detailed representation of industries and social groups, sometimes with specification of large individual investment projects as well, and usually tied together by means of (extended) Leontief input-output matrices.¹⁷ Another area was the macroeconomic preference function. Frisch had an unshakable belief in the necessity of a preference function to complete the macroeconomic planning model, although Tinbergen, Bent Hansen and Frisch himself had done much to prove the usefulness of alternative approaches. A third issue was programming techniques for solving models such as linear programming and more advanced techniques for more complex optimization models. The programming techniques were inextricably connected with the computer issues in general, which Frisch gave much and eager attention as he observed the rapid changes from first to second and, then, third generation computers.

In the 1950s and 1960s Frisch got involved in planning for developing countries. He spent several months in India in 1954/55 and he established personal relations with Jawaharlal Nehru.¹⁸ His involvement with the United Arab Republic went much deeper. He spent extended periods there in 1957/58, 1958/59 and 1959/60 and also visited later in the 1960s. Frisch was highly revered in the UAR, also long after his work had ended there, and got on cordial terms with Gamal Abdul Nasser. He also established links with Soviet planning.¹⁹

Frisch's politics in the postwar period seemed to have developed towards discontent, due perhaps to a disappointment with the lack of interest in his models on the part of the ruling Labour Party. His verbal attacks on the Labour Party officials, although seldom publicly voiced, became bitter and sometimes venomous in the 1960s. He also occasionally vented his views in derogatory terms on the economic philosophy which ruled the western world, and, as he found, increasingly also Norway.

Frisch lived an absorbing life as an economist, and his endurance at his desk (in the office or at home) when he worked on a particular problem is legendary. He could literally wear out assistants half his age. It was not unusual for him to engage his assistants at odd hours for long work sessions by telephone on particularly urgent research matters.

He was a practitioner as well as a leading Norwegian self-taught theoretical authority in apiculture (honey bees) which he practised on a smallholding property he had inherited 75 kilometres from Oslo.²⁰ His specialty was queen rearing.²¹ His apicultural interests also resulted in publications and popular articles. But just to call it a hobby would be to underrate his engagement!²²

Another interest was mountaineering. To spend weeks in the central mountain massif of Southern Norway (known as Jotunheimen) was an annual summer event, particularly in his younger days, and much *Econometrica* correspondence was conducted from there. He even had printed some guides to peaks he had climbed, and meticulously marked the path to the top by small cairns.²³ He

mentioned his affinity to the Norwegian mountains as one of the reasons for not accepting a permanent position at a U.S. University offered him after he had visited Yale for one year.²⁴

Frisch was always a religious man, a devout Christian, who practised his religion primarily in the local parish of the Norwegian Lutheran-Evangelical Church and would occasionally write edifying articles for the parish magazine or in one of the daily newspapers. In the postwar period he would occasionally take part in peace initiatives and related causes.²⁵

A deep engagement from the early 1960s until he passed away was Frisch's participation in the popular front formed to keep Norway out of the European Economic Community. The engagement was non-academic, but certainly not non-economic. Frisch delighted in applying the term "unenlightened financialism" ("det uopplyste pengevelde"), which he had invented some years earlier, to characterize the underlying philosophy of the Rome Treaty on which the community was based. He had not much belief in the viability of the Community and no belief at all in the beneficial effects it might bring.²⁶ His views were summarized in sharply polemic pamphlets. Frisch lived long enough to take part in the referendum on Norwegian membership in the European Community in September 1972 and must have rejoiced and enjoyed the fruits of his efforts at persuasion when the Norwegian people voted 'No' to membership and closed the issue for more than twenty years. There may even have been echoes of his opposition also when the Norwegian people voted 'No' again in November 1994

The quantification of economics

In the way young and promising scholars often do, Frisch wrote and spoke at an early stage about the changes that had to come in the science he had chosen for himself. In 1926, the same year Frisch published his *Sur un problème* article and defended his doctoral dissertation, he also published a programmatic paper in the only Norwegian economic journal on the quantification of theoretical economics (Frisch, 1926c, excerpts translated by O.B.). In this paper he briefly reviews the discussion of some issues within theoretical economics - the *methodenstreit* and the controversy over the causes of value - and states his position on the current status of economics as a science. The discussions had, according to Frisch, revealed '... the rudimentary state, logically and systematically, of economic theory compared to the theory of other, more mature, empirical sciences' (ibid., p.300).

The paper also clearly reveals the major sources of inspiration for Frisch in his effort to lay a foundation for the quantification of economics. Frisch quotes at length and with great approval from Irving Fisher and Joseph Schumpeter - two giants whom he came to know particularly well among the founders of Econometric Society - as prominent representatives for two different groups working in a new direction towards 'revising the logical foundation of the theory' (ibid., p.300) . Schumpeter was Frisch's senior by twelve years and Fisher by twentyeight. As a trio they seemed

to have great mutual admiration and respect for each other. A bond to tie them together was consonant conceptions of what constituted appropriately formulated economic theory. Fisher was the pioneer in mathematical economics in the United States, and - like Edgeworth in England - he had done much to promote a greater role for mathematics in economic theory, although this is not apparent from his best known books which were written to be understood by a large public. Schumpeter had a fascination with mathematically formulated economics and preached and praised the use of mathematics in economics, but he was not an equally good algebraic manipulator and mathematical analyst himself, though admired Frisch who was.

Long before Frisch met Fisher during his first visit to the United States in 1926 he had been strongly influenced by Fisher's 1892 doctoral dissertation which he had acquired in the French edition in 1922 (Fisher, 1892, 1917). In Schumpeter (1908) Frisch found a clean break with the German tradition of *Begriff und Wesen* analysis and an appeal in somewhat different terms than Fisher's for precision in the formulation of theory and applications to real problems. Frisch started to correspond with Schumpeter in the mid-1920s, but did not meet him until some years later.²⁷

Frisch found that theoretical economics through the works of Fisher, Schumpeter and others had approached the state of development of physical sciences where

'the theory gets its concepts from the observation technique. ... For the logical definition it is enough that ..[observations] ... exist as a thought experiment. ... Nevertheless, this form of conceptualization has opened a possibility for realizing the connection between the abstract concepts of theoretical economics and economic life as it is reflected in the numerical data of economic statistics. - Although the observations that can corroborate the abstract quantitative definitions, are not possible in practice, they are even so the first step towards efficient observations. They pose a target where there used to be none. They show the point that the statistical technique of approximation shall try to hit" (ibid., p.302f).

Frisch named three areas within economic theory which had been more influenced than others by the development towards a more precise formulation of quantitative relations. The first was monetary theory (which was the term used for "macro" issues). Frisch mentions Fisher's works and also recent work by Francois Divisia. Another area was production theory. Frisch refers to the late 19th century works of John Bates Clark and Philip H. Wicksteed, and also to Knut Wicksell and other Scandinavian economists. In 1926 Frisch had also written in the form of mimeographed lecture notes the first draft of what would later become his textbook in production theory.²⁸ This also became an area in which Frisch's efforts did not reach English speaking readers until much later, his *Theory of Production* was not published until 1965.²⁹ Frisch's production theory with its broad and general conceptual apparatus and a high level of mathematical corroboration generated spin-offs into other areas, in population theory and optimal diet.³⁰ As a third area Frisch named value theory and elaborates on the quantification of marginal utility at some length. After having discarded conceptions of marginal utility which were not quantifiable, he sides unhesitatingly with Fisher over Edgeworth on the most fruitful approach to quantification, agreeing with Fisher that Edgeworth's hedonistic calculus is inappropriate for economic theorizing.

All the named areas became lifetime concerns of Frisch and many others as well. Earlier in 1926 Frisch had published *Sur un problème* as his first work in value theory, and in the quantification paper he made explicit reference to the "statistical method" applied there with great ingenuity to estimate the shape of the marginal utility schedule. He then proceeds to introduce another of his methodological innovations: the interview approach as a means for estimating functional relationships.³¹ Interview technique and the idea of interviews as a source of data for the econometrician became a lifetime pursuit of Frisch in spite of scant support from other economists. In the early years the interviewing was related primarily to the estimation of the flexibility of the marginal utility of income and in the 1950s and -60s to the construction of macroeconomic preference functions.³²

The article from 1926 was Frisch's first published reflection on the methodology of economics, a theme to which he would return on a number of occasions, but clearly more interested in arriving at conclusions within economic theory than at the meta level. Frisch reiterated some of these viewpoints in his Inaugural Lecture (Frisch, 1932c) for the chair in economics that had been created for him. In the lecture Frisch spoke of economics as an experimental science. Not only would theory draw 'its fundamental conceptions from the actual observation technique' (Frisch, 1932c, p.99), but also '... we shall obtain observations which are explicitly planned and carried out with the object of fitting them into the theoretical structure' (ibid., p.100).

In pursuance of his aims for the quantification of economics Frisch built on the efforts of his predecessors. His chosen fields for sharpening the formulation of economic theory by making concepts more precise and based on observability, were value theory, i.e. the measurement of marginal utility and demand analysis, production theory, i.e. the study of substitution and scale properties of commodity production, and - in lieu of monetary theory - macrodynamics, i.e. the representation and interplay of economic forces in a macroeconomic setting. At the time of his inauguration he was deeply engaged in all of them!

The concern of Frisch's research programme was, however, not only the 'theoretical quantitative formulation' (ibid., p.105), but as much the possibilities and practicability of the 'statistical quantitative verification' (ibid., p.105). Frisch refers in this context to the already large number of contributions towards statistical determination of supply and demand curves, and expresses his views that a much firmer methodological foundation is needed. 'The results arrived at in these spheres must, I think, at present be looked at in a critical light. They are extremely rough and certain eradication in the theoretical schedule which lies behind them will be necessary' (ibid., p.105). At this time Frisch had more than an inkling that there were problems underlying current practices in the estimation of economic relationships that had not got the attention of the eager young econometricians. He would shortly finish his *Pitfall* essay (Frisch, 1933a) which would expound how estimation could result in nonsensical results, and he had got the idea for an article on a constructive methodological approach to estimating economic relationships: the Confluence Analysis.

Utility measurement: the dream of Jevons

After the initial definition of *econometrics* in the *Sur un problème* essay Frisch stated his purpose: 'The econometric study that I shall present is an attempt to realize the dream of Jevons: to measure the variation in the marginal utility of economic goods. I shall give special attention to the variation in the marginal utility of money (Frisch, 1971b, p.386). In some regards this was an attempt at measuring what was considered by many to be unmeasurable. 'Marginal utility' was still an elusive concept and was being used in ways that were remote from operational meaning. Frisch's heroes in utility measurement certainly included Jevons, but more for having introduced the concept of a marginal utility schedule and posing the question - even as a dream - of actually measuring marginal utility. Jevons' catchphrase of a 'calculus of pleasure and pain' was a suggestive remark, but not very productive in promoting quantification. A much deeper influence on the approach Frisch took towards measurement, had been exerted by Fisher (1892) and Pareto (1909). Pareto had coined "la theorie des choix" as an appropriate name for the theoretical foundation of value theory, but Fisher had - without using the term - outlined the first choice theoretic approach.

The idea of incorporating the choice theoretic approach in an axiom system was, however, original with Frisch. His 1926 paper set out an axiom system (Frisch, 1971b, p.388ff) to determine marginal utility curves in cardinal interpretation. Throughout his career Frisch steadfastly defended the cardinal approach. His axioms may be somewhat lacking in details of mathematical regularity conditions, but in essence he formulated the prototype of the axioms to be reiterated by numerous successors. The cardinality entered through his 'Axioms of the Second Kind' (Frisch, 1971b, p.389) which dealt with the individual's preference between marginal changes starting from different commodity bundle positions.

Frisch's treatment was fairly mathematical using vector field terminology and geometric reasoning. He also made the first observations on the integrability issue. He stressed the parametric nature of marginal utility that emerged from the axiomatic choice theoretic approach and suggested, somewhat feebly, 'choice coefficient' (Frisch, 1971b, p.395) as a synonym for marginal utility, i.e. following the lead of Pareto's *ophemilité* and Fisher's *desirability* in avoiding the tainted marginal utility, but suggesting a more down-to-earth term.

After having laid the axiomatic foundation Frisch proceeded to the next step: the formulation of the theoretical relationship to allow numerical estimation from statistical data. The statistical estimation was also foreshadowed by Fisher; hence, Frisch may quite likely have got his cue from Fisher (1892).³³ However, the ingenuity he showed in making additional theoretical assumptions to allow estimation and using the data at hand for the purpose was certainly his own. The theoretical assumptions Frisch made were twofold. The first and most crucial assumption was that the marginal utility of money (or income) could be written as a function of the real income and the price level (rather than as in the general case as function of income and all prices). The second and more innocuous assumption was that one good could be selected as want independent of all the others. In

his actual estimation Frisch chose sugar! By easy manipulation of the first order condition of utility maximization (Gossen's Law) for the independent good Frisch arrived at a relationship between the quantity of sugar, real income, and the relative (to the price level) price of sugar. By assuming constant one variable at a time his relationship would express the Engel curve for sugar, the demand curve for sugar, and the marginal utility of money, respectively.

The next step was application to his data which were monthly observations of the average income and sugar consumption of members of a Parisian cooperative society. Frisch gives a fair amount of detail about the smoothing and massaging of the original data to arrive at the final data used in a regression equation to determine the marginal utility of income schedule. Why the three-dimensional data set was projected in that direction, rather than for the estimation of the Engel curve or the demand schedule has perhaps no other explanation than the challenge posed by Jevons. The experience of this advance into the econometric field must have generated in Frisch's mind an enormous amount of ideas about the econometric problems that had to be sorted out to achieve the quantification also in statistical terms of economic theory.

At the same time as Frisch completed his path-breaking article, Fisher returned to his ideas about measuring marginal utility in his contribution to the *Festschrift* in honour of John Bates Clark (Fisher, 1927). Fisher's approach, which did not include actual numerical estimation, is clearly more hypothetical in its data requirements than Frisch's method. Fisher became aware of Frisch's paper only after he had completed his own article, but in time for him to enclose with every reprint he distributed a letter crediting Frisch with the honour of being the first to publish anything on utility measurement, but perhaps as much to express his high hopes for the future of '... "economic statistics" with less of empiricism and more of rationale ... '.

The two men would later join forces while Frisch visited Yale in 1930-31 in an attempt to bring the analysis further using regional price data for major U.S. cities, but the effort did not succeed in publishable results.³⁴ Frisch could not drop the topic and continued his work in finding ways of measuring marginal utility, developing alternative methods to the "isoquant method" he had applied in *Sur un problème* and broadening the range of applications to include the issue of the true cost-of-living index, the supply of labour, and the income tax. The results were given in *New Methods of Measuring Marginal Utility* (Frisch, 1932a, see also Frisch, 1931a), a book widely studied among the proselytes of the recently founded Econometric Society.³⁵

Frisch's approach in attempting to measure marginal utility was scrutinized by younger econometricians interested in demand analysis. Frisch's work took place just prior to the revolution in demand analysis in the 1930s, and later developments in value theory took other directions. The interest in starting from cardinal assumptions in reasoning about the utility function waned and disappeared into hibernation after Samuelson's (1947) scathing attack on cardinal assumptions and '... the never lacking army of utility measurers' (ibid. p.181).³⁶ Some of the leading demand analysts of the 1930s reviewed Frisch's work in full-length articles at the cross-road between the Jevons-Fisher tradition, so vastly enlivened and invigorated by Frisch, and the modern demand theory of

John Hicks, Roy G.D. Allen, Henry Schultz, and others. Allen (1933) took an appreciative and yet critical view. Allen found that rather than the actual statistical results the importance of the book is the '... fact that almost limitless fields are opened up for discussion by economists interested in what is now called "econometrics" ...' (ibid., 1933, p.186). He took a critical view of Frisch's first assumption which took '... from the notion of money marginal utility most of its essential and distinctive meaning' (ibid., p.187). On the other hand he praised Frisch's more fundamental approach to the analysis of labour supply and justice in taxation. On the true cost-of-living index he found Frisch's analysis somewhat unconvincing, spurring Frisch to consider the index problem more in depth. Schultz (1933) reviews the main content of Frisch's partly impenetrable book and is more unreserved '... then we must conclude that the problem of measuring the flexibility of the marginal utility of money has been solved' (ibid., p.110). Burk (1936) dissects in more technical detail the first assumption of Frisch and shows that it has - as Allen suspected - strong and restrictive implications, perhaps a flaw in Frisch's reasoning.

Frisch did not comment in writing or publish any rejoinder to these criticisms. He worked on reformulations that would counter the restrictive nature of the assumptions, but nothing appeared eventually.³⁷ In the area of cost-of-living index applications, however, he continued his work. Frisch had already in 1930 published an article on index numbers following Fisher's lead (Frisch, 1930b). Index problems and the inequalities to delimit the true cost-of-living index attracted increasing interest in the early thirties. In a series of surveys of current economic theory in *Econometrica* he wrote a lucid survey - *The Problem of Index Numbers* (Frisch, 1936a) - which became a standard reference for a long time.³⁸ He followed up this survey by elaborating his rule for approximating the true cost-of-living index, the "double expenditure method", in another *Econometrica* article (Frisch, 1938b). His only postwar publication related to this topic was his *Complete Scheme* article (Frisch, 1959), which offered a useful and simple approach to estimating a complete set of demand functions of want independent goods. The article was not new, it was a translation of interwar lecture notes in Norwegian.

Statistical and econometric methods

The economics curriculum at the University of Oslo included a certain amount of mathematics and statistics when Frisch started his studies, but he must have decided early to pursue these disciplines to much greater depths by further studies both at the Department of Mathematics, University of Oslo and in Paris. The University of Oslo had a strong tradition in mathematics, not least applied mathematics. Frisch may also have been influenced by the strong Russian tradition in statistics, both Aleksandr Aleksandrovich Chuprov and Ladislaus von Bortkiewicz were frequent contributors to the *Nordisk Statistisk Tidsskrift* in the early 1920s.³⁹ Paris, of course, was a world centre of mathematics.

The depth of his involvement in mathematical statistics was hardly any less than his reading of economic literature. Some time after he returned home in 1924 he submitted his doctoral thesis to the University of Oslo, which conferred his doctor's degree upon him in 1926. The topic of the thesis (Frisch, 1926b) was "semi-invariants", which is a variety of a moment generating function, now usually known as "cumulants". He had published one or two papers on this topic earlier, and would later find out that this was an interest he shared with Harold Hotelling.⁴⁰ In 1927 Frisch gave a series of lectures at Yale University for which he had prepared a long manuscript (Frisch, 1927a) on the analysis of time series. The manuscript never got published but became well known as Frisch got help in distributing it from Wesley C. Mitchell and the Rockefeller Institution. He also used it as a basis for lectures several times, in particular at Yale and at Minnesota.

Analysis of economic time series for the determination of cycles of various lengths was an established field of investigation with Warren Persons as a leading contributor. Frisch was dissatisfied with the lack of rigour in this analysis, in particular, the inhomogeneity of the methods used to determine the various cyclical components. His second main point of criticism of existing practices was the (implicit) assumption of periodogram analysis of cycles of constant periodicity. Frisch wanted to allow for cycles varying in length as well as shape. Morgan (1990, chapter 3.2) gives a fair, but somewhat limited account of Frisch's approach to time series analysis and characterizes it as a failure in several respects. It was according to Morgan (1990) neither as original as Frisch claimed compared to e.g. Persons (*ibid.*, p.89), nor convenient enough to be adopted for practical purposes.⁴¹ H.T. Davis, who was the leading mathematician at the Cowles Commission in the early years, later came up with an easier method of dealing with moving components than Frisch had proposed (Morgan, 1990, p.88). Frisch presented his ideas to a larger public at the joint meeting of the American Mathematical Society and the American Statistical Associations in Cleveland in December 1930.⁴²

The only results Frisch got published from his time series studies was an article in 1928 which gave a summary of his methods (Frisch, 1928) and his presentation in the proceedings from the ASA Meeting, it was brief and non-technical but conveyed his overall views (Frisch, 1931d). Frisch never published again on methods of time series analysis, although he surely considered his work incomplete when he left it. He kept promising a monograph on the issue 'next year' and he also wanted Gini, as editor of *Metron*, to publish an article on his method of decomposing time series.⁴³ He took the time series studies up again in 1939-40, but again nothing was completed.⁴⁴ The explanation and behaviour of economic time series from a somewhat different point of view, was, of course, central in his macrodynamics. The impression made by Slutsky's work on the cyclical pattern that could be generated by random causes, is already apparent in contribution to the ASA Meeting (Frisch, 1931d, p.78). Presumably, competing claims on Frisch's time, new ventures such as the confluence analysis, and unsolved problems in his attempt at arriving at more general methods, combined to block completion and publication of his time series studies. The disturbances caused by the war and, of course, bee-keeping made other claims on Frisch's time in this period.

Frisch's main contribution to statistics and econometric methods was not his time series analysis, but his contribution towards developing tools for determining the interrelations between stochastic variables, in particular, his "confluence analysis" which had developed from ideas Frisch had nurtured in the 1920s. While Frisch was at Yale in early 1928, he completed his *Correlation and Scatter* (RF1929b), which was subsequently published in 1929. Rather than proposing distinct new methods *Correlation and Scatter* provided a more comprehensive framework for data analysis and for discussing and analyzing multiple regression methods. The paper introduced matrix methods as the formal tool to be used in the analysis, even providing a brief introduction to matrix algebra. In a Frisch like fashion several new concepts were introduced, such as the "scatter coefficient" (p.51) that gave name to the paper, but also the first use of "multiple collinearity" (p.89). The discussion of alternative ways of conducting a multiple regression was lively at this time, and Frisch tried to sober the discussion over the ideal regression by making the point that 'the difference between the various conceivable regressions is a difference in assumptions as to *how* the accidental element has actually manifested its presence in the material at hand, this difference in assumption entailing a difference in the technique by which the regression coefficients are determined' (p.95). He, particularly, wanted to hit at the frequent use - and misuse - of partial correlation coefficients.⁴⁵

After *Correlation and Scatter* Frisch published a simplified version of the main ideas in a paper, coauthored by one of his coworkers Bruce D. Mudgett, which abstained almost completely from matrix notation and focused on the concept of "cluster types" (Frisch, 1931e). The paper introduced what would be a pet theme for Frisch in the ensuing years and provide an important rationale for the *Confluence Analysis* (Frisch, 1934a), namely that careless use of regression methods might easily result in indeterminate 0/0 expressions, apart from random noise. This became the main theme in the *Pitfalls* article (Frisch, 1933a), in which Frisch had selected a method proposed by Leontief for distinguishing the demand curve from the supply curve in the estimation as the "Prügelknabe". Leontief had based his method on an assumption of independent shifts in the two curves and defended his approach eloquently (Leontief, 1934). This must have caught Frisch in a bad mood as he retorted in needlessly sharp formulations.⁴⁶

In 1934 Frisch had completed the *Confluence Analysis* (Frisch, 1934a). The article was meant for *Nordic Statistical Journal*, but grew out of hand and came to 192 pages when it was finally printed.⁴⁷ Frisch took it upon himself to ensure the distribution to a number of people in many countries. Hendry & Morgan (1989) make the point that confluence analysis was the first general statistical method especially designed for econometric analysis (*ibid.*, p.35). It was a difficult book to digest and most of those who received it may never have penetrated Frisch's heavy mathematics and style of presentation. The attempt to deal with errors-in-variables and multicollinearity by means of confluence analysis and bunch maps was extremely ambitious and set its mark on the discussion of econometric methods within the profession in the ensuing years.⁴⁸ The story of the rise and demise of confluence analysis is history and can be studied e.g. in Hendry & Morgan (1989), Morgan (1990).⁴⁹

Frisch enlisted his most gifted pupils in the further development of confluence analysis, foremost Trygve Haavelmo and Olav Reiersøl, but he tried to enroll visitors from other countries as well. Tjalling C. Koopmans followed Frisch's lectures at the Institute in 1935 (in Norwegian!) and was unwilling to accept Frisch's conviction of 1934 that economic observations could not be considered as a probability sample. He wanted to accommodate the confluence analysis with R.A. Fisher's sampling approach.⁵⁰ Haavelmo lectured on confluence analysis in the United States on a number of occasions as Frisch's emissary during the war, while Frisch was cut off from international communication in wartime Norway. Haavelmo's *Probability Approach* was published in *Econometrica* during the war (Haavelmo, 1944), and according to Morgan (1990, p.242n) Haavelmo's 'conversion to probability reasoning' occurred 'while trying to convert Jerzy Neyman to confluence analysis!' The impact of the *Probability Approach* through the forceful influence of the Cowles Commission econometricians was that confluence analysis came into disrepute in the early postwar period. The fact that it was not a very convenient tool, but extremely demanding in numerical calculations, may also have detracted from its popularity.⁵¹ It would, however, be grossly misleading to construe this as a cleft between Frisch and Haavelmo in their views on econometric methods.⁵²

During the 1930s Frisch also wrote an article with Frederick V. Waugh to resolve an old issue of whether detrending of economic data was appropriate or needed in regression analysis, by proving the equivalence of detrending and including a time variable in the regression (Frisch, 1933b). A rare article in applied econometrics from Frisch's hand deals with substitution in the chocolate industry (Frisch, 1935a). It may be more illustrative of Frisch's approach to production theory than to his econometric methods. Other empirical applications include a joint study by Frisch and Haavelmo on the demand for milk in Norway (Frisch, 1938a), and bread baking (Frisch, 1945b).

Finally, there is the famous *Autonomy* memorandum (Frisch, 1938c) which introduced this concept in econometric theory. The memorandum was written for the Business Cycle Conference in Cambridge in July 1938, convened to discuss the outcome of Jan Tinbergen's work for the League of Nations, published in two volumes the same year (Tinbergen 1938a,1938b). Frisch did not attend the conference, and his memo arrived too late.⁵³ Tinbergen's work relied very much upon Frisch by the use of bunch maps in Tinbergen (1938a). Frisch's critical viewpoints were directed primarily at Tinbergen (1938b) which was a pathbreaking attempt at constructing a macroeconomic model of the U.S. economy. Frisch's viewpoints were preliminary as he emphasized in the memorandum itself. He praised Tinbergen for having presented a work 'of paramount importance, perhaps the most important single step forward in Business Cycle Analysis of recent years' (Frisch, 1938c, p.1), but argued that Tinbergen had jumped to conclusions when assuming that the relations he had estimated were in fact the true theoretical relations.⁵⁴ After the war Frisch never published anything on statistical and econometric methods, but he had certainly neither lost belief in confluence analysis nor given up on surmounting the difficulties raised in the autonomy paper.⁵⁵

Macrodynamics and microdynamics

The use of concepts from physics, particularly mechanics, in economic analysis and attempts at defining analogues of physical laws as economic relationships was an old pastime among mathematically oriented economists. The impact of these efforts of developing economics as a science is, and may forever be, embedded in the use of concepts such as "static", "dynamic", "equilibrium" et al.

As referred to above, Frisch had early viewed physics as a model in certain respects for the development he wanted to promote in economics. He was, however, reluctant to go very far in finding economic analogues to laws of mechanics. He tended to emphasize the transferability to economics of certain basic concepts and some aspects of the method of analysis rather than - by analogy - the substantial content of scientific laws. In *Statics and dynamics* (Frisch, 1929a, later reissued in English as Frisch, 1992) he introduced his definitions of *static* and *dynamic* in economic theory. Frisch was certainly not the first to use these concepts in economics, on the contrary, his article was - without giving many references - directed towards correcting the misuse of this pair of concepts and making their meaning more precise. The main point made by Frisch was that *static* and *dynamic* should be used to denote *modes of analysis* rather than intrinsic properties of the phenomena under consideration. The phenomena might be 'stationary' or 'evolutionary', but the approach to study them can be either static or dynamic. Which approach is more appropriate for which phenomenon, depends upon the aim of the analysis. All four cross combinations are possible.⁵⁶

In *Statics and dynamics* Frisch formulated his celebrated definition of static and dynamic laws in economics: If the formulation of an economic relationship requires both a variable and its rate of change (or lagged value) it is a dynamic relationship; if not, it is static.⁵⁷ Frisch's next step was to define *static* and *dynamic equilibrium*, distinguishing between *instantaneous* and *total dynamic equilibrium*, characterizing in passing Walras' attempt of using these concepts as a 'complete misunderstanding'(!) (Frisch, 1992, p.397).

As *Statics and dynamics* was written in Norwegian it was not widely accessible. Frisch presented his idea, however, at Econometric Society meetings and in lectures at American universities, probably also in mimeographed lectures. In a later paper, which arose from an informal discussion at the Econometric Society Meeting in Namur 1935, Frisch elaborated on the equilibrium concepts, providing an informal definition of the stability of a dynamic system (Frisch, 1936b).⁵⁸ In the latter paper Frisch tried, furthermore, to make precise a concept of the "normal value" of an economic variable, *in casu* Wicksell's normal rate of interest, in a meaning different from the 'mechanic notion of a stationary state' (ibid.,p.101). The normal value is defined as the instantaneous equilibrium value, but of a modified system. The whole argument seems somewhat ad hoc, although Frisch refers in a general way to the use of the concept of "normal values" in economics.

It may have been offered primarily as a defence of the consistency in Wicksell's reasoning about the interest rate. In Frisch's view Wicksell was always right!⁵⁹

The problem of understanding and explaining the cyclical movements of the economy had been an important theme in economic speculation and analysis since Jevons launched his sunspot theory. In the interwar period there was a surge in business cycle studies. The sharp fluctuations in business conditions in several countries gave added impetus to conduct such studies. The higher availability of statistical data allowed a broader and more descriptive analysis than earlier and opened new ways for testing of theoretical hypotheses. The macroeconomic framework for formulating such hypotheses was by and large missing, however. The search for verification of cycles instead tended to have a mechanical character, even when formulated with an increasing degree of mathematical sophistication. The problem itself naturally caught Frisch's attention at an early stage, both as a scientific challenge and for its social urgency. Frisch may have been one of the first to recognize that this called for theoretical concepts and relationships at a macro level. He observed that 'in the medley of "explanations" of the business cycle put forward in the course of time, very few in my view contain any suggestion of dealing with this problem as a true equilibrium problem' (Frisch, 1992, p.399), adding that the cycle explanations offered consisted essentially in 'chasing one variable at a time over to the left hand side of the equation' (Frisch, 1992, p.399).

In 1931 Frisch got involved in an interchange with J.M. Clark about the relation between the culmination of capital production and the rate of consumption, criticizing earlier statements by Clark, A. Hansen and W. Mitchell (Frisch, 1931f).⁶⁰ The points to be made were fairly elementary as soon as the premises were agreed upon. Clark treated Frisch deferentially. They may both have experienced this primarily as a confrontation between a "literary" and a "mathematical" approach to theorizing. Clearly, Frisch considered his little skirmish with Clark as a victory, but it did not lead him very far in the direction of a suitable macroeconomic model, and perhaps not so far as a close reading of Clark's replies could have led him.

The *Propagation and impulse* article (Frisch, 1933c) became Frisch's most celebrated article, and the one for which he was awarded the Nobel Prize, although the article itself is not explicitly mentioned in the official announcement. Hence, Frisch's efforts in macrodynamics was ultimately rated higher than his contributions to utility measurement and econometric methods. The article has often been referred to as having caused a turning point in the understanding of the causes of cycles and the role of stochastic disturbances. It was also an important stepping-stone in development towards a macroeconomic formulation of economic interrelations.⁶¹ Frisch had dealt with the issue of analyzing cyclical movements in his time series analysis, and he drew upon the insight from that when he formulated his *Propagation and impulse* model and conducted numerical experiments. Frisch is explicit about the different sources which have led him in the direction pursued. He refers to his discussion with J.M. Clark (see above) to corroborate his specification of macro equations, i.e. the propagation mechanism. For the role and impact of the stochastic disturbances he referred to the apt rocking-horse example of Wicksell for the basic idea.⁶² A more immediate source may have been J. Åkerman's doctoral dissertation in 1928.⁶³ Frisch was an official opponent and wrote a long

review article later (Frisch, 1931b). Finally, he refers to recent work by Yule and Slutsky on the mechanism of transforming erratic shocks to cycles.⁶⁴

Frisch corroborated his theoretical analysis with impressive numerical calculations. More than fifty years later his numerical corroboration has been dissected by Thalberg (1990) and Zambelli (1992), who found the numerical calculations to be remarkably correct.⁶⁵ Frisch might have wanted to conduct stochastic simulations, after having solved for the propagation mechanisms, but that was definitely beyond the power of his human and mechanical computer resources. His point was brought home in his 1934 essay, in spite of the somewhat awkward model specification, and perhaps also thanks to the exuberant persuasiveness of this pathbreaking essay.⁶⁶

In the introduction to *Propagation and impulse* article Frisch also introduced the terms micro- and macro-dynamic and explained 'the complete macro-dynamic problem' as describing the relations between the categories in a Tableau Economique he had sketched graphically. Although it is a minor point in the context of the *Propagation and impulse* paper, the remark hinted at preliminary work Frisch had undertaken for establishing a conceptual national accounting scheme, a theme he would return to several times.⁶⁷

At the Leyden Meeting of the Econometric Society in October 1933 Frisch had presented a version of the *Propagation and impulse* article. One of those who immediately adopted Frisch's *macrodynamics* was M. Kalecki who presented his business cycle theory at the same meeting and when it was published in *Econometrica* in 1934 had renamed it a 'macrodynamic business cycle theory'. The central equation of the Kalecki paper, a mixed difference and differential equation, was later solved at Frisch's Institute and subsequently published (Frisch, 1935b).⁶⁸

Other contributions Frisch worked on at the same time and would include under "macrodynamics" were his *Circulation Planning* (Frisch, 1934d), the longest article ever to appear in a regular issue of *Econometrica*, and his time series manuscript *Changing Harmonics*. The theme of *Circulation Planning* was the 'incapsulating phenomenon' which Frisch had dealt with in his Norwegian lectures on macrodynamics, and which would turn up later in his early postwar papers on international trade problems (Frisch, 1947b,1948b). Threads from *Circulation Planning* would later appear in many other contexts, he referred to it as the original theoretical source of input-output analysis, and it may be related to other aspects of his postwar work on planning models.⁶⁹ *Circulation Planning* is an enigmatic paper, hard to penetrate, which does not seem to have received very much attention. The introductory part of the paper, on cycles generated by systems of difference equations, is related to the *Changing Harmonics* manuscript which was never published.⁷⁰ The study of the 'incapsulating phenomenon' can also be read as a microeconomic approach to the modelling of underemployment equilibria. Arrow (1960) makes the same point, but with reference to the postwar *Prolegomena* article (Frisch, 1949b), another enigmatic contribution. The *Prolegomena* essay may perhaps rather be read as a criticism of the way of conducting partial and general equilibrium analysis.⁷¹ The essay is clearly also related to policy issues of great concern to Frisch in this period: the international trade and liquidity situation (Frisch, 1947b,1948b).

A contribution from the same time period with an inherent dynamics of a different kind and *micro* rather than *macro*, is the *Polypoly* article (Frisch, 1951b), originally published (in French) in 1933. It has, in distinction from many other Frisch papers, more the character of an outline of promising approaches to pursue for the theoretical analysis of "polypolistic" (i.e. oligopolistic) markets.⁷² Another microdynamic contribution was the largely neglected dynamization of marginal utility Frisch undertook in *Statics and Dynamics* (Frisch, 1929a, sections 4-7, left out in the English translation in Frisch, 1992).

The editor at large

Ragnar Frisch was appointed by the Council of the Econometric Society as Editor of its journal in 1932. The first issue appeared in January 1933 and Frisch remained as Editor for a period of 22 years until 1955.

When the Econometric Society was founded in December 1930 nothing was mentioned in the minutes from the meeting (*Econometrica* 1, p.71) about a journal. The idea of an econometric journal had been mentioned, however, on many occasions in the discussions that led to the foundation of the *Econometric Society*. There is no doubt that Frisch deserves more credit for this idea than anyone else.⁷³ The name of the journal is in fact older than that of the Society. Frisch had suggested "Econometrica" as a suitable name for a journal already in 1926.⁷⁴ But doubts had also been expressed about the financial viability of such a journal, and there were differing views about the purpose of a new journal. In 1931 Frisch was of the opinion that the journal of the Econometric Society should not be in competition with other economic and statistical journals, but primarily report the proceedings and the progress of the work of the Society and publish 'an annotated current bibliography of econometric literature'.⁷⁵ The Society ought to encourage mathematical-economic articles in other regular journals rather than in its own. Irving Fisher, the first President, much as he would like to see a flourishing econometric movement, did not want a journal to be started only to see it collapse from lack of financial resources with nothing but membership fees to finance it.

Much to Fisher's delight, the unexpected happened. Alfred Cowles, 3rd, came to Fisher on October 18, 1931 and offered to finance a journal for the Econometric Society.⁷⁶ Fisher decided to put it all up to Frisch as the 'original founder' of the Society as to whether to accept Cowles' offer or not.⁷⁷ Fisher also suggested that Frisch become Editor, at which Frisch did not show much enthusiasm to begin with, instead suggesting others, like Hotelling, as editor. Towards the end of 1931 Frisch yielded under Fisher's pressure and was formally appointed in March 1932.

From the beginning there was the Editor and three Associate Editors, Alvin H. Hansen, Frederick C. Mills, and Harold T. Davis, representing economics, statistics, and mathematics, respectively. With Frisch in Oslo and the editorial office in Colorado Springs the Assistant (later Managing) Editor had an important and difficult function to fill. There was also an Advisory Editorial Board,

which Frisch later found to be so useless that he abolished it in 1947. Frisch was not inclined to use outside referees. He took much upon himself (often too much!), and expected the Associate Editors, the members of the Advisory Board and the Council to do the rest. Frisch's workload was overwhelming, especially as he not too seldom went into considerable editing and even rewriting of the submitted manuscript, such as changing the system of notation etc. A result of the burden on the Editor-in-Chief was inevitable delays!

Econometrica was meant to be a different kind of journal. As Editor Frisch generated innovative ideas about what Econometrica ought to contain. He took a number of initiatives to make the journal known and to fill its pages. In the first 10 years or so there was seldom a significant backlog of submitted articles in Econometrica, except in the first couple of years. It happened that some issues were published smaller in size than originally planned due to lack of approved manuscripts. The volumes started out large with 448 pp. in 1933 and grew to close to 500 pp. in 1935, but this was not superseded until 1952. Even with Cowles' generosity Econometrica worked on meagre resources for many years, and Cowles extended his financial contribution much beyond his initial commitment.

Frisch's ideas about what Econometrica ought to contain went in many directions, and were, of course, also influenced by ideas put forward by others. Some of the ideas were pursued only for a period, and the content of the journal changed over time as the whole community of econometricians changed. Frisch was naturally concerned that Econometrica should be a channel for the true econometric works, but he also wanted articles that would appeal to the uninitiated. One of his specific ideas was that Econometrica should solicit four survey articles each year on, respectively, (General) Economic Theory, Business Cycle Theory, Statistical Techniques, and Statistical Information. In the first volume Alvin Hansen & Herbert Tout wrote the first survey on Business Cycle Theory, W.A. Shewhart surveyed Statistical Technique, Jakob Marschak Statistical Information, while the first survey on General Economic Theory was written by Tinbergen in the second volume. In the third volume followed Hicks' survey of Economic Theory dealing with Monopoly Theory, and Tinbergen's survey of Business Cycle Theory, while Frisch contributed his index number article (Frisch, 1936a) as the survey of Economic Theory in the fourth volume. It turned out to be much too difficult to solicit four surveys each year. Frisch would even cheat a little bit and redefine submitted articles as surveys to uphold the idea, and after some years the surveys dwindled.

Another feature initiated by the editor was articles, both commemorative and theoretical, about the great predecessors in the econometric field. In the first volume Frisch's good friend Johan Åkerman wrote on Wicksell and René Roy on Cournot, the second volume comprised Bowley on Edgeworth, Schneider on von Thünen, Hicks on Walras, and the Jevons children (H. Winefrid and H. Stanley) on their father.⁷⁸ Frisch included in Econometrica also other pieces of historical interest, related to the forerunners of modern econometrics.⁷⁹ Frisch stayed on as editor until the historic pieces also included memorial articles on the origin of Econometric Society and Econometrica!⁸⁰

Frisch was also much concerned about the role of *Econometrica* as a medium for communication between members. For this purpose the programmes for coming meetings of the Econometric Society and the reports with proceedings from past meetings served an important function. A complete list of all members with addresses would be included at intervals. All new Fellows were presented with bibliographies of their econometric works.⁸¹ Frisch took also other initiatives which resulted in *Econometrica* information of presumed membership interest.⁸² In the April 1948 issue he introduced a "Criticism invited" column. At that time, however, serious readers rather preferred more space to cope with the postwar flow of econometric articles.⁸³

From the beginning *Econometrica* allowed articles in French and English and occasionally also in German, although manuscripts in German were usually translated. Language did not seem to have been much of a problem for the early econometricians, at least not on the European side of the Atlantic. Not everyone could - like Frisch - communicate by writing in three foreign languages and read a few more, but an ability to read the major Western European languages seems to have been rather common. Russian was, of course, another matter, in the early years of the journal Frisch took the initiative to translate and reissue articles earlier published in Russian.⁸⁴ Another idea which originated with Frisch was to establish the Monograph Series of Econometric Society,

Frisch was an Editor in the old style. He wrote much in the journal he edited and is credited with a record high number of 17 articles in *Econometrica* (not counting entries marked 'Editor'). The articles varied in length from 1 to 93 pages! One of the ways in which he set his mark on the journal was by writing comments, sometimes quite critical, to other articles he had accepted for publication. A more peculiar editorial habit was to insert editorial notes in articles by other authors, often to comment or relate the content or assertions in the article to his own work.⁸⁵ Another editorial oddity is Frisch's famous criticism of Kalecki's dynamic business cycle model which appeared in the issue prior to the one which contained Kalecki's article.⁸⁶

Frisch could be a sharp polemic, not everyone was happy about these editorial comments. When Frisch told Harold Hotelling he was prepared to append a tail to Hotelling (1938) on "General Welfare", the author retorted that he had worked on the topic for 6-7 years and brushed the Editor off rather brusquely: 'Under these circumstances, I do not think it likely that any criticism conceived within a few days and published immediately is likely to have much force'. Frisch was undisturbed, however, completed his comment, but acquiesced by not publishing it in the same issue.⁸⁷

Frisch's style as editor was very personal, and editorial affairs could sometimes be considerably disturbed by other business, in particular in the postwar years when a considerable backlog of manuscripts started to accumulate. The managing editor could be driven to despair waiting to get the Frisch's decision on manuscripts he was reviewing.⁸⁸ Altogether Frisch did a feat hard to surpass in establishing *Econometrica* as the leading journal for '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 ...'.⁸⁹

Perhaps he stayed on in the Editor-in Chief's chair for too long? While Irving Fisher was still President in 1935 he discovered a flaw in the appointment of Editor: nothing had been said about the expiration of the term in office.⁹⁰ The term was subsequently set at four years and Frisch was duly reelected by the Council again and again. There may have been voices raised to have Frisch removed long before 1955, especially after the marked shift from Europe to United States in the membership and even more in submitted papers. A committee to evaluate *Econometrica* and its editorial organization was appointed in 1953, perhaps with a view to engineer a change of Editor.⁹¹ With regard to editorial policy the committee advised that the proportion of space devoted to empirical studies should be increased. It also wanted the surveys reintroduced and that steps should be taken to see that all articles should contain 'a section which sets out its objectives and findings in a language which is generally intelligible'. There was no sharp criticism of Frisch's editorial policy in the committee's report, but Frisch later took issue with the demand for more empirical articles and had perhaps a more sophisticated view on the matter than the committee:

' ... roughly I would say that it would be rather too much if we devoted on the average something like one half of the number of pages to studies that are very definitely empirical. Many empirical results are of such a sort that they may be more or less forgotten in relatively few years, and we must try to make *Econometrica* a journal whose volumes have a value that will last for many years. Much depends on what is really meant by "empirical" studies. The most valuable articles in my mind are those that present new view points and new methods and actually use these methods on empirical data. It is this application to actual data that is the acid test of the methods and, therefore, the empirical part of such a study is fundamental. But the lasting value of such a paper will in general not consist in the figures obtained, but in the fact that a new viewpoint and a new method has been presented and tested empirically. ... I don't know whether such a paper should be classified as "methodological" or "empirical". It is of a hybrid sort and I would have no objection to seeing this hybrid sort taking up a very large part of *Econometrica* if really good manuscripts could be found'.⁹²

The committee also proposed an Editorial Board to review the editorial policy. Frisch was eased out of the Editor-in-Chief's chair in a gentle way by being offered to chair the Editorial Board, an offer he could hardly refuse.⁹³ *Econometrica* later honoured Frisch with a *Festschrift* issue to his 65th birthday.⁹⁴

Postwar efforts: planning, preferences and programming

After the Second World War Frisch became preoccupied with economic planning. He gave up to a large extent his wide ranging engagement in econometrics and economic theory to concentrate on the difficult issues - econometric, theoretical and computational - that had to be solved to put into effect the ambitious aims Frisch set for economic planning. An indispensable prerequisite for Frisch's planning models was detailed national accounts. Frisch had given much attention to national accounts since the 1920s⁹⁵ and detailed accounts were compiled by pioneering efforts in the early postwar years by the Central Bureau of Statistics along (but not exclusively on) Frischian lines.⁹⁶ In the ensuing years Frisch became an intensive user of the official national accounts which

included input-output tables and attempted occasionally to extend this data set at the Institute (with e.g. financial flows, household data, etc.) to suit his ambitious models.

The intended place of application for the planning models Frisch constructed was from the beginning his home country Norway. The lack of a formal position from which he could influence the design of Norwegian planning directly, meant that his influence on Norwegian planning methods and philosophy - and to a lesser extent on the design of models - was mediated more indirectly through his strong influence over the education of economists in Norway and, above all, through some of his pupils and former assistants who rose to occupy important positions in the political administration and planning apparatus. The main impact of Frisch's influence was perhaps that postwar planning and policy-making in Norway became very oriented towards the use of large macroeconomic models with the "official" models sharing important characteristics with Frisch's models.⁹⁷ The Frischian ideas were applied in a circumspect way, perhaps too circumspect to give much satisfaction to Frisch! The more ambitious ideas in the Frischian planning scheme, e.g. the specification of macroeconomic preference functions and mathematical programming, were not even attempted utilized in Norwegian planning. In his later years Frisch would occasionally drop scornful remarks about the Norwegian attempts at economic planning, in particular after considerable parts of the postwar regulations were lifted in the years around 1960.

But India and the United Arab Republic became new fields for application. The UAR, in particular, offered a chance to conduct fairly large-scale experiments in implementing his model ideas. Frisch brought several assistants with him to Cairo, and the UAR became a more important testing ground for his models than Norway.

In 1948 Frisch expressed his intention of developing large-scale quantitative economic models in his little note about *Repercussion studies* (Frisch, 1948c), which pointed at the connecting lines with the areas in which Frisch had worked before the war. Frisch's selfassurance comes through when after stating that what he is aiming at is 'too formidable to be practicable' adds '... we have been more or less specializing in unsolvable problems'(!). Frisch (1938c) reiterates (p.370) his reasoning about the difficulty of estimating autonomous relations set forth earlier. He suggests to counter this difficulty, firstly, by using interviews to get information about autonomous structural equations (p.370), and, secondly, by the confluence analysis approach.

Frisch's work in macroeconomic planning revolved around a sequence of models worked on from the late 1940s until the mid-1960s, known under nicknames such as the "submodel", the "median model", the "channel model", and the "refi model". Much of the work on these models, which consumed a large amount of assistant hours and computational resources at the Institute of Economics in addition to Frisch's wholehearted attention, was left unpublished.

The "submodel" was a result of work on a memorandum on postwar economic problems which Frisch wrote for the United Nations Sub-Commission on Employment and Economic Stability (Frisch, 1949a) and later published (Frisch, 1955). The model was fairly simple in theoretical

structure and was left with 14 degrees of freedom.⁹⁸ It served to define and demonstrate important notions about *decision models* and how they could be used.⁹⁹ The "submodel" was however, an incomplete model, Frisch had not yet incorporated the (extended) national accounting framework he would use in his later models. The numerical experiments Frisch conducted on this model were also experiences in dealing with fairly large models and must have contributed to his call for 'mass computation of solutions' and his interest in exploiting the first generation computers still under development in the United States (Frisch, 1948c, p.371f).

Frisch would often elaborate on his more general "model philosophy" beyond the technical details of model formulation. This would comprise Frisch's views on "stages in economic modelling", on his defence of the preference function as a *sine qua non* of a fullbodied model, on notation, his "selection and implementation" distinction, and many other issues often intermixed with political viewpoints bordering on naivité. His model philosophy was best expressed in the introduction to (often quite technical) model presentations (e.g. Frisch, 1962b,1963a).¹⁰⁰

Frisch's convictions about preference functions, both the need to include them in the macroeconomic model rather than reducing the ambitions to a target-instrument approach, and the possibility of estimating preference functions by sophisticated interviewing of decision makers, has gained little support from other econometricians. Frisch took the interviewing very seriously, but also in this area relatively little was published. The interviewing for the estimation of preference functions may, surely, have evoked memories of his early attempts at interviewing to determine the flexibility of the marginal utility of income, but establishing macroeconomic preference functions by interviews was a much more ambitious venture. The interviewing was undertaken as a sophisticated and, yet, pragmatic approach, widely different from the common pollster questioning.¹⁰¹ The raw data from interviewing would then have to be processed - 'smoothed' - to conform with regularity conditions. Frisch tested out his technique on various interview objects in the 1950s.¹⁰² The interview technique is set out for the case of a quadratic preference function in the del Vecchio *Festschrift* (Frisch, 1961b), while the smoothing process, still under refinement at the time of his death, was presented rather briefly in 1967 (Frisch, 1967).¹⁰³ One of the few assessments of Frisch's techniques for establishing preference functions is Johansen (1974), which points to some minor technical flaws and gives references to a number of Frisch's unpublished memoranda discussing the methods.

In his later years Frisch and the Institute of Economics had cordial relations with Soviet planners who expressed an interest in Frisch's models and programming techniques. There were a number of exchange visits in both directions. The closer contact with the intellectual resource pool of Soviet planning is the direct background for the paper *Rational price fixing in a socialistic society* (Frisch, 1966), announced to be a first half, but the other half never appeared.

An impressive piece of work, more didactic than political, is *Parametric solution and programming* (Frisch, 1963b) which gives the general solution of a small dynamic Hicksian model in a way which comes close to being a comprehensive course in difference equations and dynamic modeling.

Towards the end of the paper a programming approach is pursued and the model is used to discuss various approaches to the general planning problem.

Frisch's work on planning problems would not be completely described without mentioning his huge efforts at coming to grips with the mathematical programming tasks posed by overall national planning cast as an optimization problem. Mathematical optimization had always been a passion to Frisch and in between his postwar work he found time to publish (in French) a textbook on mathematical optimization (Frisch, 1960a, English edition Frisch, 1960b).

Most of Frisch's work on these problems took place in the period 1955-65 and was based on the use of the first and second generation computers available in this period. The early work in this area centered on efficient solutions to the linear programming problem. Frisch developed two gradient methods called the *multiplex* method and the *logarithmic potential* method, respectively, as alternatives to the simplex method. These methods were presented in non-technical terms in the Lindahl and Åkerman *Festschrifts*, respectively (Frisch, 1956,1961a), while a more complete elaboration of the multiplex method appeared in *Sankhya* (Frisch, 1957). Later Frisch undertook pioneering work to handle non-linear programming including cases with non-convex feasibility regions, and developed his *nonplex* method (Frisch, 1963c).

Looking back

When Frisch was awarded the Nobel prize in 1969 he was hospitalized with a broken leg and unable to attend the ceremony in Stockholm in December. His Nobel Lecture (Frisch, 1970c) was instead delivered in June 1970. The Lecture is an odd piece of document, very Frischian in spirit. He conveys the impression, which also pervades many of his best works, that even when he has been most deeply engaged in a specific problem there has always been a very broad perspective present in his mind, both socially and scientifically. The Lecture touches upon or hints at the tenets and mainstays of his earlier work and recapitulates in more concrete detail the history of the econometric movement by quoting from more than forty year old correspondence. Finally, he takes the audience with him into his on-going work and elaborates persuasively on his various approaches, until he abruptly interrupts the presentation with no conclusion at all.

When in a passage he says 'deep in the human nature there is an almost irresistible tendency to concentrate physical and mental energy on attempts at solving problems that *seem to be unsolvable*' (Frisch, 1970c, p.214), he is merely expressing in more high-flying words his attitude when attacking many of the problems he had grappled with throughout his life, as he also observed in *Circulation Planning*, 'we have here one of those cases - so frequent in economic practice - where it can be "proved" by abstract reasoning that a solution is not possible, but where life itself compels us nevertheless to find a way out' (Frisch, 1934d, p.274).

In Frisch's library the only really worn-out books were by his two great mentors: Alfred Marshall and Knut Wicksell.¹⁰⁴ He had learnt much from both and referred to them often. He expressed his appreciation for Marshall in Frisch (1950b) and for Wicksell in Frisch (1952a). Marshall's *Principles* had been the basic curriculum at the university when Frisch began to teach. Many of Frisch's lecture notes were written as notes on *Principles*.¹⁰⁵ His treatment of Marshall in Frisch (1950b) is, however, unemotional and to the point, opening up with: 'Like all human work, Alfred Marshall's theory of value had its definite shortcomings' (p.495). Wicksell had a more pervasive influence, as reflected in Frisch (1952a). Frisch was just old enough to have known Wicksell, but the two never met. Frisch had seen and heard Wicksell lecture in Oslo, but '... being an unassuming student at the time, I did not have the courage to talk to him' (p.654). No other economist influenced Frisch as much as Wicksell, and the penetrating study of Wicksell in the article is accompanied by warmth and a deep respect for Wicksell both as an economist and a human being.¹⁰⁶

In the last decade or so of his life Frisch grew critical to much of the recent developments in econometrics and economic theory. He had for a long time worked in a field where he was relatively alone in his belief in the worthwhileness of searching for methods for determining macroeconomic preference functions and for optimizing such functions with large planning models as constraints. He was certainly not ready to say as Marshall did at the end of his life: 'The Chinese worship their ancestors: an old student of economics may look with reverential awe on the work which he sees young students preparing themselves to do'.¹⁰⁷ At the First World Congress of Econometric Society in Rome in 1965 he created a small sensation by sharp and critical remarks about current tendencies in econometrics. His knack for coining words had never left him, and he offered *playometrics* as the term to express his disdain. His paper was not published, but he recapitulated the incident and his remarks in his contribution to the Harrod *Festschrift* (Frisch, 1970b).

Frisch hit specifically at 'turnpike type of growth theory' as an example of theorizing of little relevance for actual growth problem, and characterized as *epsilontic refinements* the practice of making assumptions 'more for the convenience of mathematical manipulation than for the reasons of similarity to concrete reality' (p.162). In the Harrod *Festschrift* he stated his scientific credo in commonsensical and down to earth terms:

'In the first place I have no objection in general to the application of rough approximation formulae. But ... [we] must have a good reason to believe that the conclusions to be drawn - and to be taken seriously - are of such a kind that they depend on the way in which the approximation *resembles* reality and not on the way in which the approximation incidentally *deviates from* reality.

In the second place there is no topic under the sun, even the most abstract or the most seemingly useless one, which I would remove from the list of subjects which might occasionally be made the object of a respectable scientific research. ... But I would strongly object to a situation where too many of us too often used too much of our time and energy on the study of keyholes in northern Iceland in the first half of the thirteenth century.

In the third place I have all my life insisted that factual observations alone - observations taken by themselves - do not have much sense. Observations get meaning only if they are interpreted by an underlying

theory. Therefore, theory, and sometimes very abstract theory, there must be. But at the same time I have insisted that econometrics must have relevance for concrete realities - otherwise it degenerates into something which is not worthy of the name econometrics, but ought rather to be called *playometrics*.' (Frisch, 1970b, pp.162-163).

The attack on *playometrics* can, however, be directly related to the editorial statement in the very first issue of *Econometrica*: 'The policy of *Econometrica* will be as heartily to denounce futile playing with mathematical symbols in economics as to encourage their constructive use' (*Econometrica* 1, January 1933, p.3).

When asked by Erik Lundberg, who sat on the first Nobel Prize Committee, in 1970 about his views on deserving future candidates for the Nobel Prize, Frisch was not lost for an answer. True to his usual style he was too busy to propose candidates officially, but on his ordered list of three were Wassily Leontief for input-output analysis, Gunnar Myrdal for his criticism of methods in economics and other social sciences, and Richard Stone for contributions to national accounting and economic planning techniques.¹⁰⁸ Frisch's "nominees" received the Nobel award in the same order in 1973, 1974 and 1984, respectively.

After being awarded the Nobel Prize Frisch reflected: 'When I think of the long list of problems of which I have in vain tried to find the solution, and think of the honours that have nevertheless been bestowed upon me, I understand with deep thankfulness to Whom all this is due: To the Lord Who has steered my steps over the years, and Who has been my refuge in the superior matters which no science can ever reach.'¹⁰⁹

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Notes

1. For a while it was not quite clear whether the Econometric Society had copyright on the use of 'econometrics'. Cowles Commission asked in 1934 the Econometric Society for permission to use 'econometric' in the title of the later famous Cowles Commission Monographs on Econometrics. Letter to Fisher 4th January 1934.

2. Haavelmo (1973) mentions that Frisch could work 'anytime and anywhere' and that he had once admitted having worked 56 hours continuously on a mathematical-statistical problem (p.1).

3. Frisch was always affiliated with the University of Oslo. Its official name was until 1939 Det kongelige Frederiks Universitet [The Royal Frederik's University], but Frisch would usually ignore this and refer to it, as in the affiliation at the end of *Econometrica* articles, as the "University of Norway". Until after the Second World War there was only one university in Norway.

4. In this speech he revisited his early interest in utility analysis and offered the following overelaborate example of his way of reasoning:

'Assume that my wife and I have had dinner alone as we usually do. For dessert two cakes have been purchased. They are very different, but both are very fine cakes and expensive - according to our standard. My wife hands me the tray and suggests that I help myself. What shall I do? By looking up my own total utility function I find that I very much would like to devour one particular one of the two cakes. I will propound that this introspective observation is completely irrelevant for the choice problem I face. The really relevant problem is: which one of the two cakes does my wife prefer? If I knew that the case would be easy. I would say "yes please" and take the other cake, the one that is her second priority. But here a problem of reliable data emerges. If I know exactly what she prefers, the case is resolved, but what if I am in doubt about that? The problem cannot be solved by asking her: "Which one do you prefer?" She would then say: "I am completely indifferent, take which one you prefer". Neither is the case resolved by saying: "You help yourself first", because then the same problem will arise for her. Hence, the simplest thing I can do is to utilise earlier experience and make the decision on that basis. In some cases my assessment of her preferences may be so vague and indeterminate that I to some degree must rely on my own total utility, i.e. make some compromise between the two preference scales.' (Frisch, 1973, p.4, translated by O.B.).

5. See *Econometrica* 1, 1933, pp.73-86. The small group attending the first and the successive meetings might have been even more impressed by observing that '... often Frisch came along with a synthesis at the next breakfast, evidently worked out during the night' (Tinbergen, 1974, p.3).

6. 'Econometrics' is of course the best known example, and Frisch coined 'macroeconomics' and 'microeconomics' in their current use as well. His terms would usually be introduced in English and Norwegian at roughly the same time, but some are to be found only in one of the two languages as 'consumer-taking' (= the rate of consumption) only in English and 'kryssløpsanalyse' (= input-output analysis) only in Norwegian.

7. The official Institute of Economics bibliography of Ragnar Frisch's published papers and books was originally prepared by Trygve Haavelmo for *Econometrica* 28 (April 1960), pp. 186-192. The list which then included 119 entries, was appended to Arrow (1960). After Frisch was awarded the Nobel Prize in Economics in 1969 the list was extended by Leif Johansen with more recent works, altogether 150 entries, and appended to Johansen (1969) and also to Edvardsen (1970) with minor typographical changes. The latter list extended with nine additional entries, altogether 159, was appended to Haavelmo (1973). (A conference proceeding paper from 1931 that had been overlooked earlier was entered at the correct chronological place as entry no. 25, while the original entry no. 25, a mimeographed paper was removed.) The list was meant to exclude political pamphlets (EEC), newspaper articles and publications on non-economic issues. (One paper on honey plants was nevertheless included, as well as a newspaper article.) The 1973 list was used by the Institute of Economics to produce in 1976 five bound volumes of all the publications. The 1973 list, which has not been revised or extended by the Institute, does not include e.g. Frisch (1931a),(1931b). Some translations into other languages are also missing.

8. Several of the more important memoranda ended up, finally, as published papers. As a source for the development of Frisch's thought the memoranda are invaluable. There were also other mimeographed papers that never got published, but some of which may have circulated widely, e.g. Frisch (1927a),(1930a),(1931c).

9. Although located at the University, the Institute was privately financed throughout the 1930s by means from the Rockefeller Foundation and Norwegian sources (in particular from Norsk Varekrigsforsikrings Fond on the initiative of the industrialist J. Throne Holst).

10. Tinbergen and Koopmans had both left their degrees as physicists behind when they went in for economics. Heckman (1992), p.880 honours - mistakenly - also Frisch with a degree in physics. Heckman's point that Frisch, as well as Tinbergen and Koopmans, mastered mathematics at a high level and applied it with originality in the new econometric discipline is certainly correct.
11. *Plan til en økonomisk strukturoversikt for Norge*, Oslo: Fabritius, 1936, written jointly by Frisch and his two co-professors at the Institute Ingvær Wedervang and Wilhelm Keilhau. The "structural survey" became an (over-)ambitious empirical project which died from lack of resources and interest during the war. It became in some ways a forerunner of Frisch's postwar attempts at using the Institute as a base for conducting large-scale empirical modelling projects. For the national accounting work, see Frisch (1939b),(1943) and Bjerve (1986).
12. Frisch's attempts at macroeconomic analysis before the Second World War and his politico-economic role has been impressively dealt with in a doctoral thesis (Andvig, 1986).
13. See Andvig (1986, pp. 374-384). In a wellknown episode in Norwegian political history Frisch helped the parliamentary leader of the Labour Party and future Prime Minister Johan Nygårdsvold in 1934 to get evidence directly from Keynes himself that a government appointed committee was misrepresenting Keynes' view when it argued, to undermine the "Crisis plan", that saving would have to be undertaken prior to expanding investment, Andvig (1986, pp.385-391).
14. Impressions communicated by Per Sevaldson.
15. See Frisch (1950a).
16. The plan was published as Frisch (1945a). Even today the doctoral programme in economics at the University of Oslo is a far cry from the education Frisch proposed of 5-7 years postgraduate research oriented studies.
17. Input-output analysis was congenial for Frisch's needs, although he emphasized the limitations of the Leontief framework compared to more general (but unmanageable) representations. For Frisch's approach to input-output analysis in lectures, see Frisch (1950c),(1952b),(1954).
18. Richard M. Goodwin (1992) seems to have regarded Frisch's work in India as eccentric and ineffectual.
19. Particularly with the Laboratory of Economic-Mathematical Methods of the Academy of Science, which in 1963 merged into the newly created Central Economic-Mathematical Institute (CEMI). The first director, V.S. Nemchinov, died in 1964. Frisch arranged for visitors from CEMI to study his methods at the Institute of Economics. He also visited the Soviet Union on some occasions.
20. As late as 1969-70 Frisch chaired a committee to evaluate applicants for a professorship in apiculture at the Norwegian Agricultural University.
21. To a colleague who considered taking up apiculture he gave the following advice: 'Let me advise you as strongly as I can not to go into queen rearing. You will use a lot of money and a lot of valuable time' (letter to Abdul G. Khan, Pittsburgh University, 8th February 1965).
22. '... above all it has been bee-keeping and queen-rearing in which I have been engaged for 57 years with emphasis on a genetic and statistical study with a view to improving the quality of the bee. If somebody asked me if I find this occupation pleasant and entertaining, I am not sure I could honestly say yes. It is more in the nature of an obsession which I shall never be able to get rid of' (Frisch, 1970a, p.212).
23. Frisch's climbs were recorded in protocols kept at Norwegian tourist huts, in particular at his favourite Eidsbugarden, to the hosts of which he dedicated one of his printed guides.
24. Letter to Fisher 21st March, 1931.
25. Perhaps, primarily after being rallied by friends and people he trusted. An initiative that may have originated with Frisch was to have the Nobel Peace Prize shared between J.F. Kennedy (posthumously) and N.S. Khrushchev in 1964. J. Neyman wrote to Frisch 27th July 1964 hoping to get his support for the Nobel Peace Prize to Martin Luther King. Frisch was moderately supportive, but countered with his Kennedy-Khrushchev proposal (letter to Neyman 4th August 1964) which Neyman found to be a more worthwhile cause, adding 'in this connection I am playing with the idea of a random sample survey among our professors so arranged that each selected individual could answer the question by letter without signature: because of the possibility that Goldwater will be elected (which I hope will not be the case), quite a

few people may be scared to fawn Khrushchev even in the company of Kennedy' (letter to Frisch 10th August 1964). Frisch was too late for the 1964 award and in 1965 the Chairman of the Nobel Peace Prize Committee at the time, Gunnar Jahn, former Director General of the Central Bureau of Statistics and former Governor of the Bank of Norway, told Frisch that the Peace Prize could not be awarded posthumously.

26. This was a view shared by many well-equipped economists in Norway at this time, especially at the universities, hardly in reverence to the master, but perhaps a result of his long-term paradigmatic influence on the formation of economists in Norway.

27. Frisch seems to have got on equally well with his two elders. He could compare notes on health diets with the somewhat puritan Fisher and share other delights with the suave and refined Schumpeter, with whom he came on much closer personal terms. Frisch included in *Econometrica* both his speech at the testimonial dinner for Fisher at eighty (Frisch, 1947a) and his warm tribute to Schumpeter in the obituary (Frisch, 1951a). According to Schumpeter (1948) Frisch surprised the audience when he spoke of Fisher's Ph.D. thesis as a work of "monumental importance". The thesis was an early and very important source of inspiration for Frisch, but was still undervalued in the United States at the time of Fisher's death.

28. The theory of production was issued several times as mimeographed lecture notes which gradually were converted into book format. When he came to number the editions Frisch, apparently, counted backwards and implicitly denoted his lecture notes from 1926 as the first edition. The 6th edition in 1941 was published as volume one, the second volume were to include i.a. "dynamic production theory" and the "reinvestment process", but eager students who had prepaid subscription to the second volume would never receive it! It appeared finally as Frisch (1953). The last edition of Frisch's production theory in Norwegian was the ninth edition (Frisch, 1962a), and this was still only the first volume! The theory of production was published in English as (Frisch, 1965a) following French and Spanish editions in 1963, and these editions included also the topics Frisch had reserved for the second volume. According to Frisch's preface in the 6th edition he had in 1931 'practically made an agreement that the book should be published by the Yale University Press'.

29. Frisch's approach had been studied at an early stage by Erich Schneider and Sune Carlson who both published production theories in the 1930s, much influenced by Frisch, see E. Schneider; *Theorie der Produktion*, Wien: Julius Springer, 1934 and S. Carlson; *A Study on the Pure Theory of Production*, London: P.S. King & Son, 1939.

30. The remarkable essay on optimum population (Frisch, 1940) is still not available in English. For Frisch's work on optimal diet (Frisch, 1941), see Sandmo (1993). The nutrition study was part of the "structural survey", see note 11.

31. Frisch even gives case results of the flexibility of the marginal utility of income by individuals and reports having interviewed husband and wife with almost identical results! (Frisch, 1926c, p.330). He renders the following example of his elaborate question phrasing:

"Imagine that your income for some reason in 1927 becomes 50% lower than usual, and in the following year 1928 50% higher than usual. ... From 1929 income is again as usual. Imagine that your needs and habits are unchanged. Try to disregard saving motives, acquisitions, and debt obligations, and take into consideration only the regular, consumptive use of income the year of accrual. You would then surely wish to even out by transferring some of the income from 1928 to 1927. Imagine now that an ordinary bank loan is not a possibility. Only I can help you out of this difficulty. I offer to place at your disposal 1000 kr in 1927. But this is not for free. I demand in return a larger amount in 1928. Do you accept if I demand 5000 kr in 1928? (No.) But if I only demand 1100 kr? (Yes.) 4000 kr (No.) 1500 kr (Yes.) etc." (ibid., p. 331).

32. In Vol.2/Ch.1 he mentions the interview method as the 'only possible way' (p.370) for getting data for structural equations, in view of the virtual impossibility of getting data for the estimation of truly autonomous equations from economic statistics.

33. Fisher (1892) showed how the marginal utility curve (for sugar) could be constructed from a series of hypothetical observations, adding: "To do this statistically is of course quite a different thing and more difficult though by no means hopeless proceeding" (Fisher, 1925, p. 20). In Frisch's copy (of the French edition) the latter part of the sentence is underlined!

34. See Frisch (1932a, p.6). The data Fisher and Frisch intended to use was the BLS family budget study 1918-19. The data, which were fixed basket price level data for a number of cities, were acquired in 1930 from Paul H. Douglas. Three years later Frisch voiced to Douglas his suspicion that something must have been wrong with the data, as neighbouring locations had too diverse prices. Douglas rechecked and found, indeed, that there were mistakes in the calculations! Letter from P.H. Douglas, 25th October 1933.

35. In *New Methods* (Frisch, 1932a) Frisch honours Fisher as the 'real pioneer ... in the field' (p.1). Fisher and Frisch were going to write a history of utility measurement, but it never materialized. See letter to E. Mackeprang 11th June 1930, Mackeprang was asked for an abstract in English of his treatment of utility measurement in *Matematisk Nationaløkonomi*, Copenhagen, 1924.
36. In recent years there has been increasing interest in reconsidering cardinality assumptions, and of course cardinality is inherent in the von-Neumann-Morgenstern utility functions, but the approaches taken by Frisch in *New Methods* can hardly be said to have been reawakened, yet.
37. Frisch offered in 1935 Keynes as editor of the *Economic Journal*, a paper generalizing the earlier methods and dispensed with the "income proportionality" criticized by Bergson (letter to Keynes 15th October 1935). Keynes turned it down in no unclear terms: 'I cannot persuade myself that this sort of treatment of economic theory has anything significant to contribute. I suspect it of being nothing better than a contraption proceeding from premises which have no clear application. I may misjudge the situation. But I am convinced that this mode of attack will only be justified for competent opinion if those who use it make it extremely clear what they are doing and do not take refuge in a mass of symbolism which covers up all kinds of unstated special assumptions.' (Letter from Keynes 28th November 1935). Frisch was undeterred and explained that his purpose was '... to indicate how a certain measurement can be made, namely a price index computation between different countries, or any other two situations where tastes, etc. are not the same. This problem, as you have so well explained in your "Treatise on Money", is the same as to develop observable criteria for the "equivalence" of income. To me this seems a perfectly definite problem and one of considerable consequence for a number of other problems in economic theory.' (Letter to Keynes 14th December 1935). Keynes retracted his sharp remarks and welcomed a revised version of the paper, but still maintained that it was '... vitally important that econometrists[!] should avoid using an elaborate symbolic language and pretentious mathematical formulae unless they do really bring something out at the other end. It has to be admitted, I think, that at the present time these methods are proving disappointing and in risk of falling in general discredit.' (Letter from Keynes 30th December 1935). Frisch had, however, already withdrawn the manuscript to make the results 'more general and more usable' (letter to Keynes 16th October 1935).
38. The article is marred by an unusual (for Frisch) high number of misprints and typographical errors, which must have been much of an annoyance for the Editor-in-Chief who was very particular about proof-reading.
39. A.A. Chuprov (1873-1926) lived outside Russia/USSR after 1914 and visited Kristiania (Oslo) several times. He was with Markov and Chebychev among the most influential Russian statisticians, and Frisch had personal acquaintance with him. L. von Bortkiewicz, of course, was Polish, but educated in St. Petersburg and, hence, within the Russian tradition. He was professor in Berlin from 1901 until his death in 1931 and was at the time one of the eleven Council members of Econometric Society.
40. Hotelling and Frisch could not agree on the terminology! Frisch had changed Thiele's "half-invariant" to "semi-invariants". Hotelling found this "hard to pronounce" and proposed "cumulants" which Frisch found not a 'desirable word to use in France, Germany and Scandinavia', but it won forth nevertheless (letters from Hotelling 12th February 1931, and to Hotelling 21st February 1931).
41. This cannot be quite true, however, as while Frisch lectured on time series at Yale in 1930 he recruited an eager group of applicants of his methods. Frisch did not see his time series approach as a method limited to economic time series, hence, the applications comprised Uranus longitude residuals (Dr. Brouwer, Yale Observatory) and rainfall at Boston, Massachusetts (J.R. Wolf), as well as economic applications, such as freight car loadings in the U.S. (Professor C.W. Cobb, Amherst College), wheat prices in Europe (Professor Thompson, University of Allahabad, India), pig iron production in the U.S. (H.M. Cleland), and the BLS index of wholesale prices (H. Edmiston). Schumpeter also showed great interest in Frisch's method at this time and relied on it in his *Business Cycles* where he characterized it as 'a stroke of genius' (Schumpeter, 1939, p.208n). Schumpeter's reliance upon Frisch's method later drew severe criticism from S. Kuznets, see "Schumpeter's Business Cycles", *American Economic Review* 30, (1940), pp. 257-271.
42. I.e. at the event which also included a late night meeting to found the Econometric Society.
43. Letter to C. Gini 1st October 1930.
44. His work on time series problems in 1939-40 was accompanied by intensive numerical calculations (communication from Petter Jakob Bjerve).
45. Frisch wrote to Hotelling and asked for assistance in making these points known to American statisticians. Hotelling must have had a bleak view of his own profession as he commented scathingly: 'I do not believe that you need to worry about most American statisticians not knowing it, since most of these gentlemen know nothing whatever of a theoretical nature' (Letter to Frisch 26th February 1931).

46. See Frisch (1934b). Leontief added "A Final Word" (*QJE* 48, Aug. 1934, pp. 755-759). Jacob Marschak tried to smooth out the differences in "Some comments" (*QJE* 48, Aug. 1934, pp. 759-766), and Schumpeter interfered as well.

47. In April he wrote the editor, Thor Andersson, saying it might come to more than 25 pages (the usual limit). In May he offered to refrain from honorarium because of the length of the treatise. In June he estimated it at 125 pp. In the end it came to 192 pp., but was not published in the journal at all, probably because the journal folded (the editor and owner, T. Andersson, died in 1935). The 1000 reprints which had been ordered by Frisch, were delivered, though.

48. Zvi Griliches dedicated his Henry Schultz memorial lecture at the Econometric Society meeting in Oslo, 1973 on "Errors in variables" to Ragnar Frisch, and suggested that the lack of success of Frisch's confluence analysis was due not only to the difficult presentation, but to the fact that Frisch '... tried to solve *simultaneously* the errors-in-variables problem, the *simultaneity* (confluence) problem, and the model *choice* problem' (*Econometrica* 42, November 1974, pp. 971-998).

49. Neither of these sources goes much below the surface. A more complete evaluation of the confluence analysis is still missing. R.E. Kalman, who is one of the few who has paid much attention to the confluence analysis in recent years, seems to agree to this when he says: 'It is impossible to avoid the conclusion that the lack of progress on and the present unpopularity of Frisch's ideas are due to mathematical rather than conceptual difficulties.' (p.190) in R.E. Kalman: "Identifiability and problems of model selection in econometrics" in *Advances in Econometrics*, W. Hildenbrand, ed., Cambridge: Cambridge University Press, 1982, Chapter 6, Part V, pp. 169-207.

50. Koopmans' studies resulted in Koopmans (1937) and must have contributed to modify Frisch's views. A letter from Frisch to one of his American students during Koopmans' visit bear witness that the discussion about the relationship between confluence analysis and probability theory was a vivid issue at the time and Frisch had not locked himself into a dogmatic position:

'The difference between these two points of view is this. In sampling theory, in order to test the significance of a statistical observation, one puts up the fiction of a "universe", that is some big collection from which the actual observations are "drawn" in a more or less "accidental" manner. Whatever assumptions one makes are made in the form of assumptions about this universe. This point of view is fruitful, it seems to me, in problems concerning experiments that can be controlled. For instance, agricultural or biological experiments. But this theory is very inadequate when it comes to applications in economics, or in social sciences in general, where we most of the time have to accept observations that are presented to us without our being able to influence the results to any considerable extent. In these cases all the problems of confluence analysis crop up, and these can, it seems to me, be better treated by another type of analysis, namely an analysis where the assumptions being produced are assumptions about the sample itself. For instance, one may assume that each observation is a sum of a systematic part and a "disturbance", and then introduce assumptions concerning what has been the connections, or lack of connections, between the disturbances in the sample. In this way one arrives at identities, exact upper and lower limits, etc., not results which are formulated in probability terms. One does have a means of investigating how a particular constellation of assumptions entails a particular consequence for the result obtained. This analysis of the effects of alternative assumptions is very important for applications to economics. This is of course only a very rough outline of the difference between the two approaches. If I should give a fuller statement I would have to explain that in some sense, the notion of probability comes in in my approach and that, after all, there may be some points of contact between the two approaches. But it would lead too far to go into this in a short letter.' (Letter to Paul G. Hoel 15th October 1935).

51. When L. Klein reviewed Richard Stone's *The Role of Measurement in Economics* (Cambridge, England: University Press, 1951), he praised the book but noted that 'Stone is one of the few modern workers clinging to the methods of confluence analysis' (*Econometrica* 20, 1952, p.105). On the other hand E. Malinvaud gave a positive evaluation of the bunch maps of confluence analysis as a systematic, yet laborious, way of analyzing data as late as 1964 in his *Méthodes Statistiques de l'Économétrie* (Paris: Dunod).

52. Frisch evaluated the *Probability Approach* as an opponent at Haavelmo's doctoral dissertation in 1946 and assessed Haavelmo's overall qualifications for the professorship conferred on him in 1948. Frisch considered Haavelmo's "The Statistical Implications of a System of Simultaneous Equations" (*Econometrica* 11, January, 1943, pp.1-12) as more important and influential than the *Probability Approach*. For the latter he had some reservations, but not of a kind that could be construed as antagonistic.

53. The memorandum was received two days after the conference finished by Dennis H. Robertson who found it 'far above my head ... but I felt dimly that in the concluding pages you were expressing in scientific language the same kind of criticisms or warnings as some of us have felt impelled to lisp in crude and ignorant terms' (letter from D.H. Robertson 23rd July 1938).

54. Tinbergen responded to Frisch's criticism. His reply was published together with Frisch's memorandum, an extract from Haavelmo's *Probability Approach*, another note by Frisch, and a Koopmans' Cowles Commission Discussion Paper on "Identification Problem" as a Memorandum from the Institute of Economics, November 6, 1948.
55. A joint project with Haavelmo after the war was a textbook in statistics and econometric methods for students of economics. It was distributed in instalments as Memoranda from the Institute of Economics, but never completed according to Frisch's ambitious outline of its contents.
56. Frisch noted that a phenomenon which is 'evolutionary' at the 'microcosmic' level might be 'stationary' at the 'macrocosmic' level (Frisch, 1992, p.392, note 5), and, hence, introduced the first of his micro-/macro- pair of concepts.
57. Frisch, (1992, p.394). See also J. Tinbergen: Annual Survey of Significant Developments in General Economic Theory, *Econometrica* 2 (January 1934), p.27 and the Editor's note on the same page.
58. Samuelson (1947) embraces Frisch's definitions of 'statics' and 'dynamics', but with reference to Frisch (1936b) rather than Frisch (1929a/1992), which is not referred to. Samuelson may have had Frisch's ideas mediated through other channels, see Andvig (1992, p.388).
59. 'Sometimes it happened that I thought I had finally caught him in an inconsistency or in unclear thinking. Every time this happened, it turned out, however, that the error was mine. After a number of such experiences, I reached the conclusion that whenever a person thinks that he has found an inconsistency or a piece of unclear thinking in Wicksell's works, and wants to "correct it", that is only a sure criterion that the person in question has not yet penetrated to the bottom of Wicksell's ideas. The discovery of the fact that Wicksell is, after all, right, will always be a matter only of patience and intelligence on the part of the reader.' (Frisch, 1952a, p.654f).
60. After Frisch (1931f) followed a little interchange. J.M. Clark replied in "Capital Production and Consumer-Taking - A Reply" (*JPE* 39, December 1931, pp.814-816), with a later rejoinder "Capital production and Consumer-Taking: A Further Word" (*JPE* 40, October 1932, pp.691-693). Frisch followed up with "Capital Production and Consumer-Taking - A Rejoinder" (*JPE* 40, April 1932, pp.253-255) and "A Final Word" (*JPE* 40, October 1932, p.694).
61. Samuelson (1974, p.7f) reports his attempt at tracing the origin of "macroeconomic", presuming it originated with Frisch, but finding it first used by Lawrence Klein in 1946. Frisch used the term prior to this date, and is most likely the one who first used it in any language, as it was used in his lectures on macrodynamics in Norwegian in 1933/34, see Frisch (1934c, par 8506). In Frisch (1946) from 1945 Frisch used the term "general macroeconomics" as covering the problem of 'how big economic issues, employment, investment, labor, living costs, fiscal policies, etc., gear into each other' (p.71), adding 'Men who can handle general macroeconomic problems cannot be produced at short notice'(!). The term must have been in use in English in the 1930s, however, because there is a trace of it in Tinbergen (1938a, p.10), and this might even be the first occurrence in print in English? Samuelson also overlooked that E. Lindahl had introduced the term in his *Studies in the Theory of Money and Capital* (London: Allen & Unwin, 1939), p.52.
62. Frisch's reference to Wicksell in *Propagation and Impulse* was not correct, however, see Velupillai (1992, note 4) and Thalberg (1990, note 1).
63. J. Åkerman: *Om det ekonomiska livets rytmik*. Stockholm, Nordiska Bokhandeln, 1928.
64. References in Frisch (1933f, p.28note). Few outside the Soviet Union may have become earlier aware of Slutsky's article. Frisch later arranged to have Slutsky's article translated and printed in *Econometrica*, see note 84.
65. There may have been some flaws in the reasoning relating to the cycle generating mechanisms, as Zambelli (1992) shows that the '*propagation mechanism* is not intrinsically cyclical' (p.52) and poses the question why 'such a paradoxical result ... went unnoticed for almost sixty years' (p.53).
66. Samuelson (1974) reflects in a personal memory on the dogmatic effects such persuasiveness may have (p.10)!
67. P.J. Bjerve (1986) gives a detailed and lucid (and to a great extent also an *eyewitness*) account of the development of Frisch's ideas on national accounting from his first and very pioneering lectures on this topic in 1928-29. He lectured on national accounting concepts again in the mid-1930s and worked with a group of assistants on this in 1941-42, i.a. with an elaborate and very original graphic *eco-circ* representation accompanying the algebraic formulae, see Frisch (1943). His attempts at compiling national accounts data continued until the University was closed in 1943. The numerical work continued, however, in the Central Bureau of Statistics (by P.J. Bjerve) building on the system of concepts and the numerical efforts undertaken by Frisch at the Institute. After the liberation Frisch had another go at the system of concepts, resulting in Frisch (1948a). Bjerve (1986) suggests that Frisch's pioneering work on the principles of national

accounting - much ahead of R. Stone - might well have been worthy of a Nobel Prize recognition if it had become better known, especially because the motivation for Frisch's work in this field was very much oriented towards the needs of macroeconomic modelling and analysis. One of Frisch's consistently held principles was to distinguish real flows from financial flows. A softer point was the treatment of money which was dealt with as a claim without debtor, implying a definitional difference between saving and investment. The latter idea was hard to surrender for Frisch, stemming as it did from Wicksell, comp. note 59.

68. The discussion at the meeting was reported in much detail in *Econometrica* (2, April 1934, pp.187-203). At the meeting Frisch doubted a uniqueness result which Kalecki asserted, but added an Editor's note in printing to the effect that Kalecki had been perfectly right.

69. For Frisch's claim with regard to input-output analysis, see e.g. Frisch (1954, p.1). But although Frisch was in *Circulation Planning* using a formal framework similar to the closed input-output model, his claim on originality seems to be unfounded. Johansen (1973,1974) relates Frisch's optimization approach to economic planning to the ideas pursued in *Circulation Planning*.

70. The manuscript *Changing Harmonics Studied from the Point of View of Linear Operators and Erratic Shocks* was originally intended as an article in *Econometrica*, duly announced as a forthcoming paper in the journal (*Econometrica* 1, October 1933, p.448). Frisch attempted to rewrite the paper to make it easier accessible for economists, and publish it in two instalments in *Econometrica* in 1934. 3pp.). E.B. Wilson, Council member of Econometric Society, refereed it and found it to be mathematics of little interest for economists and ought to be presented in a more compact way, adding 'it looks as though Frisch were developing theory on the assumption that his time series was infinitely long' (letter from Managing Editor W.F.C. Nelson 17th October 1933 quoting a letter from Wilson). Frisch decided to publish the manuscript instead as a Cowles Commission Monograph, but it never appeared, perhaps a result of Frisch's reluctance to publish anything he was not fully satisfied with.

71. The paper was an outgrowth of early postwar lectures by Frisch on O. Lange's *Price Flexibility and Employment* (1944). The lectures never got beyond the introductory chapters on equilibrium as Frisch took issue with Lange's presentation (communicated by Arne Amundsen, October 1994.) Lange had been Acting Editor of *Econometrica* in 1943-45 and completed his book while Frisch was *incommunicado* in Norway.

72. Arrow (1960, p.181) makes the observation that 'Frisch's formulation is in many respects very game-theoretical' and with a solution concept closely related to *Nash equilibrium*.

73. The idea was first put forward in a letter to F. Divisia 4th September 1926. In the letter Frisch noted that only *Economic Journal* and *Revue d'Economie politique* accepted mathematical papers and even these journals were quite reticent about it.

74. In the letter to F. Divisia of 4th September 1926 Frisch suggested that the association he hoped would be founded should be called "Association internationale d'economie pure" and the journal "Econometrica". Divisia was enthusiastic about the name (letter to Frisch 16th December 1926), but pointed out the oddness that the Greek roots 'οικος', 'νομος', and 'μετρον' had been cast into a name following Latin rather than Greek transcription. Frisch reflected on this and tried out several spellings until he settled for "Oekonometrika" which also hinted at the parallelism with "Biometrika" and avoided confusion with 'Economica'. Frisch stuck to this spelling until the name was finally settled by a vote in the Council shortly before the first issue was due to appear. Divisia voted for "Econometrica"!

75. Letter to Fisher 20th May 1931. The idea of a bibliography reemerged when *Econometrica* was established, but was put on ice as the Social Science Abstracts seemed to fill this function well enough. The Social Science Abstracts closed at the end of 1932 and Frisch took up the idea of short bibliographic notices about both members' work published elsewhere and other econometric articles. Frisch worked out with usual energy a classification of articles to be used, notes for authors of entries and an announcement of the idea to be published in January 1934. It never appeared, apparently because there was still a backlog from the initial push of articles and the issues were already running larger than Cowles' original financial commitment allowed.

76. Fisher wrote to Frisch: 'It is exceedingly wonderful to have an "angel" suddenly fall down from the sky to supply us with the one thing needful to make our Society a huge success. Without financing we can never amount to a great deal but with financing we can leap years ahead of what we otherwise would' (18th October 1931).

77. Fisher decided to consult (by letter) only three persons, F. Divisia, who was Vice President, and two Council members, Schumpeter and Frisch. He discriminated between them with regard to the extent that he found that their approvals were needed: '.. if I do not hear from them, I shall assume that they at least have no objection. In your case, however, I should like to be sure that you approve...' (letter to Frisch, 18th October 1931).
78. More followed in the ensuing volumes, but Amoroso on Pareto (*Econometrica* 6, January 1938, pp. 1-21) became a slight scandal because of the homage to the Italian fascist state that came with it. Jerzy Neyman who was then at University College, London, wrote to Frisch that several members of the Econometric Society regretted that Amoroso's article contained so much political propaganda (letter from J. Neyman 4th March 1938). Frisch stood by his acceptance of the manuscript, arguing that 'on going through the MS carefully, I was unable to pick out any definite sentence that could not in a sense been taken as a description of Pareto's work. Everything is "d'une façon très adroit" presented as a contribution towards the understanding of Pareto. And, this being the case, I did not feel that I could rightfully ask Amoroso to change the presentation although I very much wanted to do so' (letter to J. Neyman 7th March 1938).
79. E.g. the correspondence of L. Walras with A. Cournot and S. Jevons (*Econometrica* 3, 1935, pp.119-127) and a letter from Marshall to C.Colson in 1907 (*Econometrica* 1, 1933, pp.221-222).
80. See C.F. Roos: A Future Role for the Econometric Society in *International Statistics* (*Econometrica* 16, April 1948, pp.127-134), which was more about the past than the future, and F. Divisia: La Société d'Économetrie a Atteint sa Majorité (*Econometrica* 21, January 1953, pp.1-30)
81. Except for the first lot of 29 Fellows elected by the Council in 1933. By the constitution the Fellows would vote on nominees for fellowship. The first regular election took place in 1935; but of 39 nominees only four were elected Fellows (*Econometrica* 3, 1935, pp.477-479). Among those passed over were two future Nobel Laureates. Frisch was, however, explicit about keeping the number of Fellows low, say always less than forty, and managed to gather support also from some other Council members. The report on the election observed that: 'A surprising result of the vote was the discovery that works of several well-known nominees had been read by only a few Fellows. Indeed, the ballots showed that some of the nominees failed of election primarily because their work was to a large extent unknown to the Fellows. This indicates how difficult it was to keep abreast of new developments in econometric research throughout the world when publication was as widely scattered as it was before the founding of *Econometrica*.' (ibid., pp.479).
82. E.g. a committee appointed on Frisch's initiative to survey the availability of data for production studies (see *Econometrica* 1, pp.218-219).
83. In response to a proposal to reduce the length of articles to cope with the backlog John Hicks would rather have 'stricter standard of selection, [and] prune drastically that part .. which does not consist of papers' (letter from J. Hicks 11th March 1949).
84. These included A.A. Kontüs: The Problem of the True Index of the Cost of Living (*Econometrica* 7, January 1939, pp.10-29, translated by H. Schultz), and the famous paper by E. Slutsky: The Summation of Random Causes as the Source of Cyclic Processes (*Econometrica*, 5, April 1937, pp.105-146), both originally published in the *Economic Bulletin of the Institute of Economic Conjecture*, Moscow in 1924 and 1927, respectively.
85. There are numerous instances of this, e.g. an endnote to A.L. Bowley: Note on Professor Frisch's "The Problem of Index Numbers" (*Econometrica* 6, January 1938, pp.83-84), a similar endnote to H. Mendelshausen: The Definition of "Equal Well-Being" in Frisch's Double Expenditure Method (*Econometrica* 6, 1938, pp.285-286), a note between footnote 8 and 9 in J. Tinbergen: On the Theory of Business-Cycle Control (*Econometrica* 6, January 1938, pp.29-33), and note 10 in A. Wald: A New Formula for the Index of the Cost of Living (*Econometrica* 7, 1939, pp. 319-331).
86. Frisch's criticism was formulated in a prefatory note to Frisch (1935), while Kalecki's article: A Macrodynamical Theory of Business Cycles, appeared in the July issue, pp.327-344.
87. Letter from Hotelling 26th May 1938. Frisch's comment was Frisch (1939c), published in April 1939 together with an additional exchange, see Hotelling (1939a, 1939b) and Frisch (1939a).
88. This was particularly the case for D.H. Leavens who was Managing Editor from 1937 until 1948. But not until he had decided to leave the journal did he put it to Frisch in straight words: 'In the early years of my work on *Econometrica*, many MSS came back from you in about 30 days, representing probably about 3 weeks in transit and a week in your hands. In recent years, however, papers have remained in your hands much longer. This combined with slowness of referees and of the printer, has meant that we have been publishing consistently a month or two months late, and that the authors have had to wait unduly long for a decision on their MSS. Delays by referees are excusable in that they do a thankless job with no remuneration or public credit. The printer's slowness has been inevitable in the postwar situation. It does seem to me, however, that you as editor, honored by the Society with that position, and given a

financial honorarium for your services, have some obligation to give reasonably prompt attention to MSS after they reach you accompanied by opinions of referees in this country. I have refrained from protesting your slowness in recent years, but now that I am about to turn over my work to Mr. Simpson, I feel that it is only proper for me to put this up to you rather strongly, in the interest of our authors, our readers, and of Mr. Simpson, who will be considerably handicapped in taking up his new work if these long delays continue.' (letter from D.H. Leavens 24th August 1948).

89. Quoted by Frisch from the Constitution in his editorial in the very first issue (*Econometrica* 1, p.1).

90. Letter from I. Fisher 23rd September 1935.

91. The committee consisted of P.A. Samuelson, T.J. Koopmans, and J.R.N. Stone. The report of the committee was published in *Econometrica* 22, (April 1954), pp.141-146.

92. Letter to Koopmans March 1956.

93. In 1967 the Editorial Board was replaced by an Editorial Advisory Committee with five year terms and rotating chairmanship for which Frisch accepted to become the first chairman.

94. *Econometrica* 28 (April 1960).

95. See note 67.

96. See O. Bjerkholt & S. Longva (1980, pp.15-17) and Odd Aukrust: "The Scandinavian Contribution to National Accounting" in *The Account of Nations*, Amsterdam: IOS Press, 1994, pp.16-65.

97. This was observed by Jean Waelbroeck who in a survey of national models conducted in 1975 noted: 'A completely different tradition in model building exists in Norway. ... These models ... are completely different from the other models surveyed; no comparable work exists elsewhere: in other countries input-output has found applications in long-term rather than in short-term planning.' (J. Waelbroeck: "A Survey of Short-Run Research Outside the United States" in *The Brookings Model: Perspective and Recent Developments*, G. Fromm & L.R. Klein, eds., Amsterdam: North-Holland, p.425, quoted from Bjerkholt & Longva, 1980).

98. It was this model presentation J. Tinbergen had in mind when he in the preface to his *On the Theory of Economic Policy* (Amsterdam: North-Holland, 1952) credited Frisch by stating that the theory presented was 'nothing but an application of the notion of "decision models" as introduced by Ragnar Frisch'. Tinbergen even borrowed denotation from Frisch (Tinbergen, 1952, p.9f).

99. Frisch later recounted his experience when trying to explain the use of the model to the Sub-Commission:

'I tried, of course, to present the idea as simply and briefly as possible, but even so it was quite obvious that the members of the Commission got more and more into a state of panic in the face of such terrible waste of the Commission's precious time. And they felt great relief when I had finished my exposé' (Frisch, 1962b, p.259).

100. Frisch expounded his model philosophy at a seminar in the Vatican (Frisch, 1965b) at which he met with opposition to some of his central tenets about modelling, particularly about his way of dealing with preferences, vigorously from M. Allais, but also from J. Dorfman, T. Koopmans, P.C. Mahalanobis et al.

101. A fact which Frisch emphasized: 'From my experience in interviews with leading politicians ... I have reached the very definite conclusion that a workable formulation can be found if the policy maker is approached with wisdom and comprehension and with full knowledge of the role which the preference function has to play in the programming analysis. It will not do to let the policy maker be approached by some youngster in the opinion-poll trade who has been instructed how to put checkmarks "yes" or "no" or "don't know" in some standard questionnaire' (Frisch, 1963a, p.44).

102. It is well known that he interviewed at length an influential Labour Party politician (Trygve Bratteli) who would become Prime Minister 20 years later (see Frisch, 1962b, p.260n).

103. The interview technique and the smoothing process was chosen as topic by Frisch when invited to give a lecture in Sweden in 1970 in connection with his Nobel award, see Frisch (1971a). See also Rustem & Velupillai (1984).

104. The books are Alfred Marshall: *Principles of Economics*. Seventh Edition, London: Macmillan, 1916, and Knut Wicksell: *Über Wert, Kapital und Rente and Geldzins und Güterpreise*, Jena: Gustav Fischer, 1893 and 1898.

105. Frisch's notes on Marshall written as a series of *excursuses* became a most valuable compendium to his students and were in use with shifting contents over time (and increasingly losing touch with Marshall) from the early 1930s until the late 1960s. It was known as *Notatene [The Notes]*. In the 1930s Frisch occasionally sent the compendium to acquaintances abroad, although it was all in Norwegian. J. Hicks received it in 1939 and paid particular interest. He suggested to Frisch to publish it as a textbook, in English. Hicks even established contact with the Clarendon Press on Frisch's behalf and suggested *The Principles of Economics* as a suitable title. Frisch was delighted, especially as Hicks offered to supervise the translated manuscript, but could not get around to it until the war disturbed everything. (Letters from Hicks 13th March, 25th March, and 3rd April 1939).

106. To his students he would refer to Knut Wicksell honourably as 'den store gutten [the big boy]' (communicated by P.J. Bjerve). See also note 59.

107. Letter from A. Marshall to J.M. Keynes, 27th July 1922.

108. Letter to Erik Lundberg 15th August 1970.

109. Frisch, 1970a, p.212.

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