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Ragnar Frisch on scientific economics

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Abstract

Ragnar Frisch promoted rather than specific theories a more *scientific economics* and gave many methodological contributions toward this aim. He is known for having coined the term *econometrics* as well a co-founder of the Econometric Society and the first editor of *Econometrica*. The original connotation he gave to the term *econometrics* can be rendered as *scientific economics*.

Despite Frisch's many publications it may be argued that his fundamental ideas of economics as a science are not widely known. The paper aims at illuminating some key features of Frisch's scientific conception, drawing partly on unpublished works from his archival remains. Among these are a series of lectures on the theory and methods of econometrics given by Frisch at the Poincaré Institute at the University of Paris in 1933, as well as extensive lecture notes from Yale University in 1930.

These documents elaborate on Frisch's scientific views, how he aimed at modelling economics on physics by transferring methodological principles, and on the methods he proposed for economics, such as an axiomatic approach to microeconomic behaviour, his structural modelling approach which became a cornerstone for macroeconomics, his refined explication of concepts such as static/dynamic, micro/macro, et al., and his concern with the probabilistic nature of the real world of economics. Frisch's concept of scientific economics was a broad one, comprising not only the scientific and methodological concerns but also the instrumental role of scientific economics in policy preparation for welfare and improvement of human conditions.

1. Introduction[*]

Ragnar Frisch was the first Norwegian economist of international renown and became the dominant influence in Norwegian economics over a long period in the twentieth century. In Norway he had to fill multiple roles including that of a self-appointed advisor on economic policy as in the '*Saving and Circulation Regulation*' pamphlet (Frisch 2007). But that was not really his field of comparative advantage. We may ask what Frisch would have had to say on the current failure of financial markets. We may find clues in his works as to what he would have had to say but neither would that be his home ground.[1]

Soon after having been introduced to economics just before 1920 Frisch recognized that it was not really a science, neither with regards to how economic theory was formulated nor in empirical practice. He was of course not the only one who reached this conclusion at that time. Furthermore, the social importance of economics as the key to betterment of the society made a deep impression on Frisch. From his own observations of the post-WWI chaos, the painful 1920 (practically ruining the Frisch family business), and the onset of the depression both in Norway and the USA he noted the lack of rationality in the social and economic order. On a scientific level Frisch was as much attracted to statistics as to economics and he saw in statistics a key element in the scientification of economics.

When Ragnar Frisch in 1926 coined the term *econometrics* he gave meaning to the term was by stating that '*econometrics has as its aim…to turn pure economics, as far as possible, into a science in the strict sense of the word*' (Frisch 1971:386). The purpose of this presentation is to illuminate what Frisch meant by '*a science in the strict sense of the word*'. I shall refrain from going more than superficially into any of his recognized contributions to economics but still hope the presentation will amount to more than just '*generalities on Ragnar Frisch*' (to borrow a title from one of his articles in an Italian journal[2]).

Obviously, when Frisch introduced the term *econometrics* it meant nothing to anyone except to himself, his statement was a programmatic declaration of what he wanted to strive towards

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[*] I have benefited much from the cooperation with my co-editor in the preparation of the Poincaré lectures for publication, Ariane Dupont-Kieffer, and I am most grateful to Duo Qin for a very substantial effort in the discovery and reconstruction of the Yale lectures. I furthermore owe sincere thanks to John Aldrich, Marcel Boumans, Kevin Hoover, Roy Weintraub and other participants at the Econometric Society summer meeting, Duke University, June 2007 for comments on a related

[1] Frisch might have pointed to what he had written about 'encapsulation' (in the sense of 'contraction') and market failure during the Great Depression but also in immediate post-WWII period, see Bjerkholt (2006) for references and a brief discussion. In the late 1950s he coined the term '*unenlightened financialism*' for economic institutional arrangements he regarded as vulnerable to disastrous collapse sooner or later.

[2] 'Generalities on planning' (Frisch, 1960).

towards. The meaning of 'econometrics' as introduced by Frisch in 1926 was different and more comprehensive than the narrower current meaning of 'statistical methods for the determination of economic relations'.[3] The original meaning Frisch gave to the term may be rendered, as suggested by the quoted statement, simply as '*scientific economics*'. Notwithstanding the fact that economic thinking already had a long history and great pioneers, anyone could to be an economist and offer opinions on economic issues and even the most crackpot ideas could have a day in the sun. Scientific procedures of verifying theoretical statements and testing economics laws were largely missing.

Frisch was born in 1895 and lived in his adolescent and formative years through a period of the most spectacular advances in physics.[4] Frisch held the view that for economics to become a science it should adapt and adhere to the principles and procedures of modern physics, incl. the theory of relativity. His scattered brief remarks on this issue reflect the influence from advances in physics at the time.

Frisch's concern about the lack of scientific rigour in economics comprised the need for better econometric methods, in the current usage of the term. He was even more concerned about how theory should be formulated in economics to fulfil positivistic scientific requirements. That meant concepts to be given empirical meaning through operational definitions allowing them to be 'measured', and statements of economic relationships to be formulated such that they could be 'tested against observations', at least under ideal conditions. Frisch elaborated his view only to a limited extent as a philosophy of science, it was as much expressed by his innovative research ideas in the following years.

It is generally recognized that economics underwent a major shift in the direction of a more scientifically based discipline, particularly in the decades between 1930 and 1950. This period was marked by the emergence of macroeconomics and advances in econometric methods. Frisch was one among many who participated in this development, but his role was pivotal in several respects.

After coining *econometrics* in 1926 Frisch propagated the idea of a more scientific economics to fellow economists, mathematicians and statisticians in several countries; using his new term as a banner. This effort resulted in the foundation of the Econometric Society in 1930

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[3] The shift was not least due to Frisch's student, Trygve Haavelmo. At the Cowles Commission Haavelmo's *The Probability Approach in Econometrics* constituted from 1944 the core of econometrics, from then on a sub-discipline of an increasingly scientific economics.

[4] This is oddly reflected in Frisch's Nobel Address which comprises in section 1 a table of nuclear particles and a brief discussion of the difference between the neutron and the anti-neutron! See Frisch (1970a).

(Bjerkholt 1998). Frisch drafted the key formulation in the Econometric Society's constitution stating its aim as to '*promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences*.'[5] The constitution thus pointed to the natural sciences as an ideal for economics to emulate with regard to '*rigorous thinking*'.

Ragnar Frisch's role in the foundation of econometrics and the Econometric Society in the 1930s is well known, his scholarly contributions in this period are dealt with at length in the history literature.[6] His contribution to economics was recognized by the award of the first Nobel Prize in economics in 1969 (jointly with Jan Tinbergen). Among the early econometricians Frisch became known for his attempt at 'measuring marginal utility', for his 'propagation and impulse' explanation of business cycles, and for his 'confluence analysis' approach to analysing economic relationships. But it may still be argued that his overall scientific conception of economics did not become very widely known. Some of his key papers were published only in Norwegian or in little accessible publications.

Some documents shedding more light on Frisch's scientific ideas have relatively recently been retrieved from Frisch's archival remains. Among these are extensive notes for a lecture series given at Yale University in 1930 titled *A Dynamic Approach to Economic Theory* (Frisch, 1930) and a series of eight lectures on *The Theory and Methods of Econometrics*, given by Frisch at the Poincaré Institute at the University of Paris in 1933 (Frisch 2009).

I shall not go much into biographical details, just mention that Frisch was born into a family of jewellers and as the only child destined to take over the family business and also trained as a silversmith. In the 1920s he was silent partner with his father in the family business while he pursued his scientific interest by delving deeply into mathematics, statistics and economics. He chose mathematical statistics as the topic of his doctoral dissertation (Frisch 1926a) and became the first modern statistician in Norway.

Frisch was concerned from an early stage with profound methodological issues but dealt with them often by just throwing around catchphrases like '… theoretical economics is about to enter the phase of development at which natural sciences, particularly theoretical physics long have been, the phase in which the <u>theory gets its concepts from the observational technique'</u>

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[5] The key phrases of the constitution were stated on the inside back cover of *Econometrica* for several decades.
[6] See e.g. Morgan (1990), Hendry and Morgan (1995), cf. the first assessment of Frisch's lifetime work in Arrow (1960).

(Frisch 1926b, transl. by ob, Frisch's emphasis). He added that experiments or observations that could serve as a foundation for the definition of concepts did not have to be workable at present, for the logical definition it would suffice that they were workable in principle, existing as a logical construction, similar to the light signal experiments in the theory of relativity.[7]

Inspired by Pareto and Irving Fisher Frisch attempted in the early 1920s to measure empirically the marginal utility of income.[8] This early attempt reflected another of Frisch's scientific maxims, that empirical analysis should be guided by theory. The primacy of theory was one of his main tenets; analysis of economic data was futile unless inspired and led by theory.

Frisch's two visits to the USA came to be of great importance for the development of his scientific ideas. Frisch benefited as many other young European scholars from the generosity and policy of the Rockefeller Foundation by getting a three year fellowship in 1926. As the fellowship terms did not allow Frisch's wife to accompany him to the USA he chose to spend only one year of his fellowship across the Atlantic there and moved to Italy early in 1928 for a continuation of the fellowship. Shortly afterwards Frisch's father fell ill and died. Frisch surrendered the fellowship to take care of the family business which was in dire straits. He confided to Irving Fisher in the spring of 1929 that he was considering giving up his scientific endeavour to take care of the family business and his economic responsibilities. Fisher responded by arranging for a generous invitation from Yale University as Visiting Professor.[9] Frisch arrived at Yale University in February 1930, a visit that would last until June 1931, with decisive importance for the development of Frisch's scientific ideas and for the emergence of econometrics, including the foundation of the Econometric Society half way through Frisch's stay in the USA.

In the spring of 1931 Frisch was by a special act of the Norwegian Storting (Parliament) called to a new chair in economics and statistics at the University of Oslo to prevent him from

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[7] After this reference to physics Frisch hastened to add that there were differences in the research method: 'The methods of natural science cannot without reflection be copied for use in economics.' Frisch (1926b: 302-303. transl. by ob).

[8] Frisch studied in the early 1920s in Paris Irving Fisher's 1891 dissertation in a French edition (Fisher 1917), sharing with Fisher the inspiration from natural science. Fisher posed as a challenge in the book that (marginal) utility could be measured and Frisch underlined it heavily in his copy!

[9] It is hard to believe that Frisch in 1929 would even consider giving up a scientific career. Fisher was wealthy he gave Yale the money for inviting Frisch. Fisher gave his commitment before the stock market crash which eventually wiped out Fisher's entire wealth but stood by it.

accepting an offer from Yale University. From 1932 Frisch was director of his own research institute at the University of Oslo, thanks to support from Rockefeller Foundation.[10]

In the following I shall let Frisch speak for himself through extensive quotes from the two abovementioned documents. The Yale lectures 1930, comprising Frisch's broad scope for scientific economics and his introduction of structural models is dealt with in section 2. An overview of the Poincaré lectures is given in section 3 together with an introduction to Frisch's axiomatic approach. Some other scientific innovations are dealt with in section 4. Frisch's structural approach to explaining business cycles or more generally bridging economic theories and randomly influenced observations are dealt with in section 5. Section 6 looks at the issue of empirical determination of economic relationships of structural models and the meaning of 'autonomy' and 'confluence'. Finally, we try to convey the 'philosophy of chaos' in Frisch's enigmatic and epistemological remarks at the end of the Poincaré lectures in section 7.

2. A broad conception of the role of the scientific economist

Frisch's view of the need for a more scientific economics was driven by more than an intellectual interest in science as a human activity. When lecturing to students at Yale in 1930 he impressed upon the students that the issue was a matter of life and death:

> 'Man has proved sufficiently intelligent to create a huge economic machine capable of producing a great variety of useful things. But he has not been sufficiently intelligent to understand how to <u>handle</u> this big machine.…We may only think, for instance, of the situation, which occur again and again in the production cycle; huge productive forces, machinery, and labour being idle at the same time as there are millions of people who want very badly a great variety of things which could be produced by the idle machinery and labour. Not only has man been able to create a big economic machine which he cannot handle, but he is making it bigger and bigger and more complicated all the time. … It is a race of life and death, and man is certain to lose if he does not succeed in developing economics into the state of a true science, that is, a study based not only on fact collection, but also on constructive theoretical thinking'. (Frisch 1930, ch.I.1).

Frisch was not the only one to have such concerns. The disastrous deterioration of the economic situation in 1930 in the USA as well as in Europe provided the background for emphasizing to the students the social importance of a better understanding of what was

[10] For more on Frisch's background, training etc., see Dupont-Kieffer (2003).

happening in the economy and how to do something about it. Frisch conveyed in simple language the range of mental activities for which a more scientific approach was needed:

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

1. The descriptive procedure.

   One sort of question which the scientist has to answer is: What happened? What is the situation? What course did the events follow? In order to answer these questions he has to engage in descriptive, historical and experimental work. In some sciences, like economics, direct experiment is more or less impossible and the scientist must rely largely on the descriptive and historical answers to the questions here considered.

2. The understanding procedure.

   Another sort of questions which the scientist has to answer is: Why did it happen? Why did this situation exist? Why did the events follow the course they did? The answers to these questions constitute the rational part of the investigation. By the power of his mind the scientist tries to bring some reasonable order into the happening and the things he observed.

3. The prediction procedure.

   The questions here are: What will happen? What will the course of events be in the future? In order that this sort of questions shall have a meaning, the phenomenon must be such that it cannot easily be controlled by man. If it can be fairly completely controlled, no forecasting problem really exists.

4. The human purpose decision.

   Here the questions are: What do we wish shall happen? What do we wish the situation to be? The three first sorts of questions are exclusively of an intellectual character. On the contrary the sort of questions here considered is of an ethical or moral sort. It cannot be answered unless we adopt some sort of standard of social values. If the answer to such a question shall be socially significant, it must, of course, in some way or another weigh the opinions of the different individuals. It is not a question of what you or I personally think in this matter, but of what is a socially fair position.

5. Social engineering.

   The question here is: What can we do to produce such happenings or such situations? This last sort of question is the most complicated we can ask. In order to give a significant answer to this sort of question, we have to build on an analysis of all the first four sorts of questions.

(Frisch, 1930,ch.I.1).

We may note in passing here that Frisch spread his life-time work over all five activities. Frisch was not a really a theory builder his concern was the nature of theory. He was not a compiler of data but had insights on the construction of national accounting systems. He was not a forecaster but was concerned with methods for forecasting. He was not a formulator of social goals but developed methods for eliciting preferences from those who did. He was not a policy maker, but designed comprehensive modelling systems for social planning purposes. Thus we find Frisch covering all five activities, primarily concerned with the methodologies.

Although each of categories had its theoretical components economic theory as such belonged under the 'understanding procedure'. The point of theory was to understand the real world: '*By the power of his mind the scientist tries to bring some reasonable order into… things observed*.' The essential feature of theory was to 'bring a rational order into things', by which Frisch meant to make a model of the phenomenon under consideration. This is commonplace today but was not a common conception around 1930. Notwithstanding Walras' elegant equilibrium system it was not a common procedure to study an economic problem by modelling it as a system of equations. Frisch explained it to his student audience as follows:

> 'The observational world itself, taken as a whole in its infinite complexity and with its infinite mass of detail, is impossible to grasp. Taken in its entirety, in its immediate form of sense impressions, it resembles, so to speak, a jelly-like mass on which the mind cannot get a grip. In order to create the points where the mind can get a grip, we make an intellectual trick: In our mind we create a little <u>model world</u> of our own, a model world which is not too complicated to be overlooked, and which is equipped with points where the mind can get a grip, so that we can find our way through without getting confused. And then we analyse this little model world instead of the real world.' (Frisch 1930, ch.I.1).

Frisch then explained the art of modelling. '*The model world builder is a sovereign in the model world. He can decide which features and characteristics the model world shall have and the relations between various phenomena in the model world, as long as we do not break the rules of formal logic.*' But are the decisions regarding the constitution of the model world then '*ruled completely by free fantasy or caprice?*' No, because the model world shall serve a purpose, it shall help to adopt a way of thinking that will ultimately be useful in our fight for control over nature and social institutions. The model world shall picture '*those indefinable things in the real world which we might call "essentials" … with regard to our own ends*'.

Are there criteria to judge if the model world conforms to this ideal? No, said Frisch, there is no criterion to be formulated as a definite logical rule:

> 'We have nothing except a mysterious, inborn 'sense of smell' which as a rule will guide us so that we finally get on the right track. This is precisely the reason why the scientist is to be considered as a logical sovereign in his model world. He is just like a wise, absolute monarch. He knows that this is the only way of ultimately obtaining his ends. He listens to the suggestions of facts but takes care to consider them as non-obligatory.' (Frisch 1930, ch.I.1).

The model world's '*sovereign*' may be guided, naturally, by observed empirical regularities, like a downward sloping demand curve but empirical generalizations will not suffice:

> 'often the investigator will equip his model world with something more than this. By a heroic guess, he will add something which is entirely outside the body of observation at his disposal. It is exactly in this kind of heroic guesses, transgressing the observational facts, that the great constructive minds distinguished themselves from the average scientific worker.' (Frisch 1930, ch.I.1).

What was the nature of such transobservational notions? As an example from physics of a transobservational object Frisch pointed to the classic example of the atom. As a transobservational relation he suggested the relation between the diminishing return of land and the fact that rent exists. Both facts were known long before Ricardo, but 'the relation between them was not seen until revealed by the abstract speculations of Ricardo', contradicting the explanations given by the physiocrats and Adam Smith.

Frisch thus conveyed his theory of scientific investigation, and the idea of economic modelling, in an intuitive non-formalized way. He elaborated upon the need for exploring the model world. Despite the fact that we have ourselves created it we cannot overlook all consequences, systematic investigations of the model world are needed.

The real world had empirical laws, the model world had rational laws. Frisch touched upon induction and deduction, and elaborated upon what was meant by an 'explanation' within a model. He also discussed probability, sorting out various concepts of probability and hinted at how the model itself could be formulated in a probabilistic way, leading to probabilistic rather than deterministic rational laws. Finally, he discussed at some length the concept of 'cause', arguing that it was 'perfectly possible to do without altogether':

> 'If we strip the word 'cause' of its animistic mystery, and leave only that part which science can accept, nothing is left except a certain way of thinking, an intellectual trick, a shorthand symbol, which has proved itself to be a useful weapon, legitimate or illegitimate, in our fight with nature and social institutions. As I see it the scientific (as distinguished from the

> scholastic), problem of causality is essentially a problem regarding our way of thinking, not a problem regarding the nature of the exterior world.' (Frisch 1930, ch.I.1).

Then he came to one of his maxims we have already mentioned, the indispensability of theory for a comprehensive and accurate understanding of the phenomena under consideration. The phenomena often make noise and attract attention to things that are inessential for a real understanding, and thus

> '…are only apt to capture the pure empiricist and keep him at some laborious and sterile tasks of fact getting. The key to the phenomena is very often furnished by some feature which seems utterly unimportant from the empirical point of view. This is why the purely empirical and so-called institutional approach to economics is so dangerous. If we go to our economic or social investigations under the motto that we shall "let the facts speak for themselves," what we will hear will very often be childish talk. … [In] the deeper problems of science the crucial contribution towards a real understanding of the phenomenon is always furnished by one of these heroic guesses transgressing observational facts.' (Frisch 1930, ch.I.1, my emphasis).

He mentioned Albert Einstein's theory of relativity as a 'grandiose example' of a heroic guess.[11] So was also Isaac Newton's explanation of the orbit of the moon:

> "In his imaginative mind he constructed a model world where bodies attracted each other with a force proportional to the masses of the bodies and inversely proportional to the square of their distances. He started exploring this model world and found that certain bodies would move in certain orbits, and one of these orbits that could be computed from the law of his model world, was the orbit of the moon. The real discovery was brought about by a brain, not by a staff of patient observers. All the observational material would have been a dead mass if not animated by a theorist of genius." (Frisch 1930, ch.I.1, my emphasis).

Business cycles had been the most important economic issue for all the years Frisch had taken an interest in economics. Large amounts of data had been compiled by business cycle institutes but in Frisch's the effort was mostly wasted 'because the investigations have not been animated and directed by constructive theoretical thinking'. The data compilation needed to be guided by theoretical insights. Knowing Frisch's explanation of business cycles as it would become known in 1933 the thought is close at hand that when he lectured to the

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[11] Frisch attended at the University of Oslo Albert Einstein's first lecture series outside Germany about the general theory of relativity, shortly after observations of the solar eclipse in 1919 had confirmed in a spectacular way the predictions of the general theory of relativity. A source of inspiration for the young Ragnar Frisch!

Yale students of economics about ‘heroic guesses’ and ‘transgressing the observational facts’ he had in mind his own forthcoming contribution.[12]

The moral responsibility for social improvement that Frisch had imposed upon the scientific worker came to the surface several times in his future production. It was conveyed in an optimistic mood in *The responsibility of the econometrician*, which marked his return as editor after the war time isolation (Frisch 1946). The article reflected Frisch’s almost euphoric enthusiasm of being at the centre of events for the application of econometrics after war time clandestine involvement with preparation of post war reconstruction. It conveyed the impression that econometrics was indeed the tool for solving social and economic problems. In a more sombre and pessimistic tone was the warning to the community of econometricians close to the end of his career of letting the important responsibility of econometrics degenerate to ‘*playometrics’*.[13]

3. Axiomatics

Not long after the foundation of the Econometric Society Frisch was invited by the Poincaré Institute to give a series of lectures for which he suggested the title *Problèmes et Méthodes de l’Econométrie* [The problems and methods of econometrics]. The lecture series was given in the spring of 1933.[14] The list of contents reflects Frisch’s main scientific concern at the time, the eight lecture titles were the following:

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1. The philosophical foundations of econometrics. The axiomatic method. Utility as quantity.
2. Examples of static and semi-static econometric theories. Monopoly, polypoly. The concept of force.
3. What is a “dynamic” theory? Properties of determined and undetermined systems.
4. Examples of dynamic econometric theories. Oscillations in closed systems. The theory of crises.
5. The creation of cycles by random shocks. Synthesis between a probabilistic point of view and the point of view of deterministic dynamic laws.

[12] These excerpts from the Yale lectures are all from a short introductory section entitles ‘What is Economic Theory’, a really splendid introduction! The lecture notes comprised altogether about 250 pp.
[13] Frisch (1970b: 163), based upon the presentation at First World Congress, Econometric Society, Rome, 1965.
[14] Frisch was invited to the lecture series in the spring of 1932 but he was too busy setting up his Institute and it was postponed for one year. The lecture was meant to be published by the Poincaré Institute but Frisch never submitted a final ms. The lectures series has been reconstructed from archival remains and will be published as Frisch (2009).

6. The statistical construction of econometric functions. Autonomous and confluent equations. The danger of analysis of many variables.
7. Time series techniques. Decomposition of series. Linear operations and their inversion problem.
8. Conclusion: The significance of social and mechanical laws. Invariance and rigidity. Remarks on a philosophy of chaos.

Frisch (2009).

The key word in Frisch's scientific program was, as already noted, *quantification*. He had stated to use this term in Norwegian in 1926 (Frisch 1926b), and later wrote it into the constitution of the Econometric Society. Most important was *theoretical quantification*. In his introductory words in Paris he almost identified quantification with axiomatics:

> 'The attempt at quantification in econometrics comprises two aspects of equal importance. First, we have the axiomatic aspect, i.e. an abstract approach which consists in establishing as far as possible logical and quantitative definitions and to construct from the definitions a quantitative theory of economic relations. Then we have the statistical aspect, here we use empirical data. We try to fill the boxes of abstract quantitative relationships with real numerical data. We try hard to show how the theoretical laws manifest themselves at present in this or that industry or for this or that consumption category, etc. The true unification of these quantitative elements is the foundation for econometrics.' (Frisch 2009, Lecture1).

Axiomatics was a rather general methodological idea that David Hilbert had promoted in mathematics and Frisch referred explicitly to Hilbert (Frisch 1995). But as the title for the first lecture, *The philosophical foundations of econometrics, the axiomatic method, utility as quantity*, made clear Frisch regarded it as a foundation for scientific economics and the prime application was in the measurement of utility. Frisch had made an earlier attempt at an axiomatic foundation for utility in 1926 (Frisch 1971). At the Poincaré Institute he presented a set of axioms which encompassed and extended the 1926 axioms.

It is interesting to note here also that during his first visit to the USA in 1927 Frisch had got a chance opportunity to argue in favour of an axiomatic approach. He had happened to attend a panel discussion on the "present status and future prospects of quantitative economics" at the meeting of the American Economic Association in December 1927:[15]

> 'We speak of one statistical procedure as giving a better result than another. … we engage in this kind of approximation work without knowing exactly what we are trying to approximate. We engage seriously in target shooting without having any target to shoot at. The target has to be furnished by axiomatic economics. The axiomatic process of target making must necessarily be rather abstract, a fact which accounts, perhaps, for its lack of popularity in these days when it is considered quite a virtue to disregard abstract thinking in economics….Axiomatic economics will construct its quantitative notions in the same way as theoretical physics has constructed its quantitative notions'. (Frisch/Mills, 21 February, 1928, National Library of Norway).

The axiomatization of marginal utility probably became so important to Frisch because 'utility' was widely used in so imprecise and unscientific ways that it served to discredit the whole idea.[16] Frisch drew inspiration from Jevons, Pareto and Fisher in his quantification of utility through axiomatization. The ultimate aim was clearly empirical measurement.

In the lecture Frisch tried to dispose of the widespread view that psychological factors were essential in the understanding of utility. He chose the example of a banking crisis, created by the distrust of depositors. Certainly there would be psychological factors at play in a run on the banks but the whole phenomenon was driven by the fact that the public had observed over a period of time the development in objective and measurable circumstances of the economic situation:

> 'The essential condition which must be fulfilled for the econometrician to be able to formulate his quantitative laws, is not that the psychological element is present, but that it manifests itself with a certain regularity in the empirical phenomena he observes, whether psychological or not. Regularity – or lack of regularity – is not necessary linked to the absence or presence of a psychological factor.' (Frisch 2009, Lecture 1).

In Frisch's axiomatization of the behaviour of *homo œconomicus* as a theory of choice he distinguished between a *choice position* given as vector of economic goods and a displacement of the initial situation, called *choice objects*. The *homo œconomicus* was supposed to be able to compare combinations of choice objects and choice positions.

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[15] The panel was chaired by F.C. Mills who invited Frisch to submit his statement. In the end the editor of AER decided to publish only the statements of the invited panelists, i.e. W.C. Mitchell, E.B. Wilson and others; see Mills et al. (1928).
[16] To some extent this is still true. Utility reasoning is widespread but without agreement on how to measure it.

The axioms were implicitly based on the assumption that the utility for a given individual could be determined by a series of questions 'which we suppose have been posed' to the given individual. Such questions about the individual's preferences were called *choice questions*. The nature of the questions was defined by the choice axioms.[17] From the answers to the choice questions Frisch aimed to show how marginal utilities could be determined. It was then only a short step to define utility as function over vectors of consumer goods.

The axiom system presented in the first Poincaré lecture was as follows:

The axiom system for quantifying utility.

Notation:

P, Q, R, … - denote vectors of choice situations.

a, b, c, d, … - denote vectors of finite changes.

α, β, γ, δ, … - denote vectors of infinitesimal changes.

ω - denote a vector of marginal utilities.

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1. Local axioms

1.1 Determination

(P,a) f (P,b) **or** (P,b) f (P,a) **or** (P,a) ; (P,b)

1.2 Transitivity

If (P,a) f (P,b) **and** (P,b) f (P,c), **then** (P,a) f (P,c)

1.3 Additivity

If (P,α) f (P,β) **and** (P,γ) f (P,δ), **then** $(P,\alpha+\gamma)$ f $(P,\beta+\delta)$

2. Interlocal axioms

2.1 Determination

(P,a) f (Q,b) **or** (Q,b) f (P,a) **or** (P,a) ; (Q,b)

2.2 Transitivity

If (P,a) f (Q,b) **and** (Q,b) f (R,c), **then** (P,a) f (R,c)

2.3 Additivity

If (P,α) f (Q,β) **and** (P,γ) f (Q,δ), **then** $(P,\alpha+\gamma)$ f $(Q,\beta+\delta)$

3. Affinity axioms

$P + \mathbf{a} = Q$

3a. Contact axiom

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

[17] Frisch's approach may also be considered as foreshadowing the methodology of 'stated preferences'.

3b. Potential axiom

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

4. Connectivity axiom

If (*P*,*a*) f **(*Q*,*b*) & (*P*+*a*,*α*)** f **(*Q*+*b*, *β*), implies (*P*,*a*+*α*)** f **(*Q*,*b*+*β*)**

5. Reversibility axiom

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

6. Roundabout indifference axiom

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

7. Point determination axiom (or Position axiom)

If (*P*→*R*) f **(*P*→*S*), then (*Q*→*R*)** f **(*Q*→*S*)**

8. Combination axiom

If (*P*,*a*) f **(*Q*,*b*) and (*R*,*c*)** f **(*S*,*d*), then (*P*,*a*) + (*R*,*c*)** f **(*Q*,*b*) + (*S*,*d*)**

Frisch (2009, Lecture 1).

The justification for the list of axioms, Frisch told the audience, should not be judged purely by a priori considerations. The scientific attitude behind the axiomatic structure was to investigate which consequences we could deduce by adopting this or that axiom and then see whether the consequences agreed with the observations: 'It is by the subsequent agreement of the consequences of the axioms with reality that we can judge the plausibility of them'.

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

Frisch's first axiom system from 1926 corresponded to the first two sets of axioms in 1933. The additional axioms introduced in 1933 related mostly to the question of the existence of total utility indicator. It was an issue rooted in Pareto's work, which certainly had inspired Frisch. We shall have to refrain from a discussion of the axiom system.[18]

[18] A further discussion is given in Bjerkholt, O. and A. Dupont: 'Ragnar Frisch's axiomatic approach in econometrics', Paper presented at *Axiomatics in Economics: the Rise and Fall*, European Conference on the History of Economics, Siena, 4-6 October, 2007.

Frisch's <u>potential axiom</u> $\boldsymbol{\omega_{ij} = \omega_{ji}}$ the symmetry of the cross marginal utilities is known in the literature as the integrability condition. The integrability discussion originated in Pareto and Frisch had studied Pareto's discussion very thoroughly.

When the potential axiom was satisfied then it existed a *potential,* i.e. a function whose partial derivatives were exactly the components of the vector. But Frisch emphasized that the concept of a potential function was not the same thing as an indicator of the total choice of the individual. The analysis had so far not introduced any axiom permitting to establish the consequences of total choice. It was admittedly true that if the potential axiom was satisfied one could integrate the vector, but that did not imply an indicator for the total choice. Frisch warned about false prophets:

> 'We are here at a crucial point where it seems to me that most authors who have dealt with the ophelimity problem have committed inexactness or even a fundamental error. They have assumed more or less implicitly that if the integrability criterion is satisfied in such a way that a potential exists, then that potential can be taken as total ophelimity.' (Frisch 2009, Lecture 1).

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

If (P,a) f (Q,b) **&** $(P+a,\alpha)$ f $(Q+b,\beta)$**, implies** $(P,a+\alpha)$ f $(Q,b+\beta)$

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

Let this be enough about the axiomatization effort.[19]

4. Other approaches to quantification: production theory, conjectural action, static/dynamic, and micro/macro

There were other ways of 'quantifying theory' than axiomatization. Frisch demonstrated one in his production theory. He had lectured on it at Yale in 1930 in a further development of

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[19] Frisch pursued the measurement of utility also in Frisch (1932). His theoretical assumptions were more restrictive than he realized, as they implied homotheticity. The full story is told in Chipman (1998:59-67).The interest in the measurement of utility waned after the rediscovery of Slutsky's 1915 article and the demand revolution in the mid-1930s.

lectures he first had given in Oslo in 1926. The point of the production theory was to provide a structured mathematical framework for the analysis of how the combination of a set of 'factors' result in a 'product'. It was naturally particularly applicable to economic production processes but Frisch used it inventively in studies e.g. of optimum population size and the diet problem (after 70 years still available only in Norwegian). Frisch failed however completely in making his theory known, nothing was published in international languages until the 1960s, including an Italian edition (Frisch 1966).[20]

In the second Poincaré lecture, *Examples of static and semi-static econometric theories - Monopoly, polypoly - the concept of force*, Frisch embarked on the design of a theoretical framework able to account for various market forms. The concepts of 'force' was of course borrowed from physics, as indeed was 'equilibrium'. Both were key concepts in Frisch's scientification endeavour.

Frisch's attempt to formalize market structures is another interesting attempt at quantifying theory involving equilibrium concepts. He introduced *conjectural* action, which anticipated a game-theoretic approach.[21] He introduced and discussed the following list of strategic types in the market:

Strategic types

I. Elementary adjustment
   A. Quantity adjuster
   B. Stochastic price adjuster
   C. Option receiver
   D. Option issuer
II. Parametric action
   A. Autonomous action
   B. Conjectural action
   C. Superior action
III. General negotiation

The *quantity adjuster* is the well known agent who takes the prices as given and adjusts his (demanded/supplied) quantity. If quantity is given instead of the price, e.g. a buyer asks a

[20] Frisch had negotiated with Yale University Press already in 1931 about a monograph. His Norwegian editions were, however, studied by various economists able to read Norwegian, e.g. Erich Schneider, Jan Tinbergen and also (by help from a Norwegian student) Joseph Schumpeter.
[21] See Dimand and Dimand (1996). Frisch drew to on ideas by A.L. Bowley and the Dane F. Zeuthen.

producer at which price he can deliver a certain quantity of goods, it is tender situation. The individual has one parameter, i.e. the price, at his disposal but cannot be sure to finalize the transaction. He is then a *stochastic price adjuster*. The *option receiver* is in a take-it-or-leave-it situation, being offered both price and quantity. An *option issuer* on the other hand is in a position to force other agents to act as option receivers.

In the parametric action cases each agent controls a number of parameters. The benefit of each agent, *polist* in Frischian terminology, depends upon the parameters set by all agents. The distinction between the three kinds of parametric action is the attitude of the polists in this generalized competitive or game-theoretic situation. *Autonomous action* rules when each polist acts as if a (small) change in own parameters will not induce any change in the parameters of others, hence, a generalization of a Cournot market. *Conjectural action* rules when each polist takes into account the possibility that a change in own parameters will induce a change in the parameters of others, hence, a game-theoretic situation.

> ‘Each polist acts as if the possible changes in the parameters of others will be (differentiable) functions of the changes in his own parameters. We introduce the elasticities:
>
> $$z_{ij}^{hk} = \frac{\partial z_i^h}{\partial z_j^k} \cdot \frac{z_j^k}{z_i^h} \qquad (1)$$
>
> expressing the change in the parameter i of polist no. h which polist no. k believes will be induced when he changes his own parameter j. These coefficients do not necessarily express what will actually happen, when polist no. k change a little his parameter j, but rather what polist no. k believes will happen. For this reason I call these coefficients conjectural coefficients or conjectural elasticities, as different from elasticities expressing what will actually happen. (Frisch 2009, Lecture 2).

Finally, *superior action* means that one group of polists act autonomously, while another group exert conjectural action between themselves, while playing, so to say, upon the behaviour of the first group. Frisch elaborated by defining various derived concepts, particularly the *attraction force* of the various parameters, to analyze equilibria, friction and cyclical oscillations in autonomous and conjectural regimes.[22]

Frisch’s *static/dynamic* pair of concepts was motivated by imprecise and confused usage of ‘dynamic’, often reflecting unclear notions about equilibrium, e.g. that a situation was ‘dynamic’ if it was out of equilibrium. He formulated his notion that *statics* and *dynamics*

[22] An applications Frisch had in mind and he developed further in Norwegian university lectures was the labour market with the interaction of trade unions and employers.

should be used to characterize the methods of analysis, not the phenomena themselves, in 1928, published (in Norwegian) the following year (Frisch 1992). Recognizing that his innovation was not widely known he chose it as a topic in the Poincaré lectures. In Frisch's terminology a phenomenon could be *stationary* or *changing*, while the model for analyzing such a system could be either *static* or *dynamic*:

> 'What will then be the difference between a static theory and a dynamic theory? I propose the following distinction: a relation, with all variables entering into it referring to the same point in time is a static relation, while a relation comprising variables referring to different points in time is a dynamic relation. A static theory will be an analysis with only static relations, according to this definition. Likewise, a dynamic theory will be an analysis comprising at least one relation which is dynamic in the sense I have indicated.' (Frisch 2009, Lecture 3).

Was static analysis then to be used for stationary phenomena and dynamic analysis for changing phenomena? Not really, Frisch counterpoised the two dichotomies by attempting to show that all four combinations of them might be perfectly meaningful, i.e. (1) static analysis of stationary phenomena, (2) static analysis of changing phenomena, (3) dynamic analysis of changing phenomena, and (4) dynamic analysis of stationary phenomena. In the Poincaré lecture he illuminated by examples.

He used the opportunity to give reasons for his insistence on a dynamic analysis for the analysis of business cycles:

> 'The justification of a simple analysis by a static theory is often the fact that the phenomenon under investigation adjusts very rapidly to the changing factors it depend upon. …But if the adjustment of the phenomenon under various conditions is not rapid, because there are frictions or inertia and if, even more, the conditions the phenomenon depend upon, change frequently, then a static analysis will not have a *raison d'être*. That is precisely the situation where it is absolutely necessary to develop a dynamic theory if we want a true analysis of the phenomena under consideration.' (Frisch 2009, Lecture 3).

'Dynamic' did not mean out-of-equilibrium. Frisch had elaborated at Yale about equilibrium concepts, distinguishing *assumption-equilibrium* from *situation-equilibrium*. The *assumption-equilibrium* referred to the model world, while the *situation-equilibrium* referred to the real world, as a characterization of a situation:

> 'A stationary equilibrium is not the same as a static equilibrium, any more than a rainstorm is the same as that part of meteorology which is concerned with rainfall. The stationary equilibrium is something characterized by a particular kind of situation which might arise under certain circumstances, the emergence of which it is the object of theory to explain, and

> this explanation may be attempted either by a static theory (involving the idea of static assumption equilibria) or by a dynamic theory (involving the idea of dynamic assumption equilibria).' (Frisch 1930, ch.I.4).

An equally important pair of concepts was the distinction between *micro* and *macro* analysis.[23] The seminal distinction between micro and macro led to (or was perhaps led by) the idea of modelling the entire economy at a macro level, ignoring details of the functioning of the economic system, but facilitating the understanding of the nature and causes of the fluctuations of the economic activity:

> 'I shall instead now deal with the study of global systems. In such systems it is impossible to introduce as much detail as we do in the partial systems. Of course, we could always introduce all sorts of details in a formal way, … keep count of the equations and variables and assure ourselves that the problem was determined. However, that would be a totally formal procedure which could serve as a general overview of the nature of the interdependence. To study the temporal pattern of a global phenomenon in a profound way it will be necessary to limit the study. We cannot envisage so much detail as we can do in the partial system case. The most important variables we can envisage in a global system are indicated in the … *tableau economique* of the same kind as developed by the physiocrats …adapted to our special purpose of studying the movement of a determined system of dynamic equations.' (Frisch 2009, Lecture 4).

It foreshadowed the methodology of Keynesian macroeconomics.

5. Probabilistic shocks and deterministic laws: the explanation of business cycles

Frisch is known in business cycle analysis for the model he presented in the festschrift for Gustav Cassel (Frisch, 1933), reflecting a suggestion by one of the Frisch's great heroes, the Swedish economist, Knut Wicksell, who had compared economic fluctuations with the movement of a rocking horse, which when hit by club will move rhythmically in a pattern largely independent of the club. Frisch's development of this idea, for which he introduced the terms *propagation* and *impulse*, was influenced also by penetrating insight of Slutsky (1927). Slutsky's assertion that manipulation, such as smoothing of statistical data by moving

[23] The Poincaré lecture antedated Frisch's famous Cassel festschrift by six months. The overall presentation is more balanced and attractive in the Poincaré lectures. The micro/macro distinction can be traced to the Yale lectures and to Frisch (1992).

averages, could generate artificial waves, appealed immediately to Frisch, thinking of an economy as represented by a (linear) dynamic structural model exposed to stochastic shocks.

Due to Frisch's incomplete publication of his work his Cassel festschrift contribution has been interpreted with too much emphasis on the content and properties of the specific macroeconomic model he presented.[24] His real message was 1) to promote macro analysis in economics and 2) to convey his quite sophisticated paradigm for a scientifically appropriate explanation of more or less regular fluctuations, as given in the title of the Poincaré lecture 5, *The creation of cycles by random shocks, synthesis between a probabilistic point of view and the point of view of deterministic dynamic laws*.

The Wicksell-Slutsky-Frisch paradigm was that a stream of 'erratic shocks' energized the dampened cycles of the economic system, the famous 'rocking horse' model. Once understood the propagation-impulse mechanism had strong intuitive appeal. Random shocks energized the damped cycles of the 'structural-economic theory' and prevented them from dying out. The outcome could be observed as 'changing harmonics', defined as

> '…a curve that is moving more or less regularly in cycles, the length of the period and also the amplitude being to some extent variable, these variations taking place, however, within such limits that it is reasonable to speak of an <u>average</u> period and an <u>average</u> amplitude."(Frisch 1933).

The characteristics of the maintained cycles could be explained partly by the weight system and partly by the distribution characteristics of the shocks. Thus the dynamic structural-economic theory only furnished one part of the explanation; the other and equally important part was the superstructure of the general theory of changing harmonics.

Or shorter and more succinctly: Economic theory furnished the weight system, statistical theory did the rest! But Frisch's full presentation of his model with all ramifications never materialized.[25]

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[24] On Frisch's model, see Bjerkholt (2005,2007) and Morgan (1990), Ch.3.2 'Frisch's rocking-horse model of the business cycle'. Tinbergen (1935) stated that Frisch's model was only 'illustrative', which was probably Frisch's opinion too. Ironically, Frisch erred in his model, which has been studied more than any other model of business cycles. It did not generate cycles, cf. Zambelli (2007)!

[25] The focus on business cycles as the key topic in economics soon after took new directions. An indirect outcome of Frisch's effort is that it inspired his student and assistant, Trygve Haavelmo, to turn the problem around in his *Probability Approach* (Haavelmo 1944).

6. The empirical determination of economic relations

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

> 'In astronomy the fusion between theory and observation has been realised more perfectly than in the other fields of study. … the astronomical observations are filled into the theoretical structure. It is this unification that raises astronomy to the dignity and significance of a true science. Also in economics we have had theoretical speculations, but most of the time it has not been that kind of theory which is built with a view to being verified by observations. Economic theory has not as yet reached the stage where its fundamental notions are derived from the technique of observations.' (Frisch 1930, ch.I.1).

This could be misinterpreted, as another attempted revival of Newtonian science as an ideal for economics, but that was not what Frisch was after. He contrasted the 'unification' he praised in astronomy with the situation in economics. Since the 19th century an overwhelming statistical and historical material of economic facts had been compiled, but economics would not really take off as a science:

> 'These observations have not been guided and animated by constructive theoretical thinking in the same way as the astronomical observations. Theory and observations in economics have gone along in a more or less disconnected way. There have been cycles of empiricism and rationalism. At times when it became too obvious that economics did not progress so rapidly as, for instance, astronomy, physics and biology, even though theoretical thinking had been applied to it some economists would lose confidence altogether in theoretical thinking in this field and plunge themselves into a pure empirical fact collection in the hope that such a blind grappling with facts should reveal something of the nature of the complicated phenomena with which the economist is faced. Then again it became obvious that such a pure empiricism did not lead anywhere, theoretical speculations in economics had a revival and the abstract-minded type of people ruled the ground for a while.' (Frisch 1930, ch.I.1).

Rather than such cycles what was needed economics was a 'new fusion between theory and observation' in economics. To achieve that it was required with a 'theoretical structure which is such that it is capable of being connected directly or indirectly with actual numerical observations':

> 'The true theorist in economics has to become at the same time a statistician. He has to formulate his notions in such a way that he gets a possibility of ultimately connecting his theory with actual observations. I know of no better check on foggish thought in economic

> theory than to have the theorist specify his notions in such a way as if he were to apply the notions immediately to some actual or hypothetical statistical material.' (Frisch 1930, ch.I.1).

Frischian econometrics can be viewed as a new methodology built around structural modelling as a new methodology for connecting theoretical quantification and empirical quantification. The model appears in Frisch's works as the core of the scientific investigation on the one hand, and as the tool for connecting theoretical and empirical quantitative economics on the other. Frisch's modelling concept was new at the time but is ruling economics now (cf. Morgan and Morrison, 1999: 10-11).

Frisch's 1929 essay *Correlation and scatter in statistical variables* (Frisch 1929) was a profound and inventive introduction to data analysis that provided the basis for Frisch's later work on simultaneity and confluent relations.[26] It is in Frisch (1929) that we find the origin of the ideas that led to the focus on *simultaneity* and *autonomy* in Haavelmo's probability approach.[27] Frisch studied how observations scattered and clustered in observation space when relations were represented by geometric planes of different order, noting the mathematically trivial fact that when two relations were assumed to be fulfilled at the same time the observations would be scattered on or close around the interface of the two planes representing the relations.

This led to the tentative conclusion that when analysing a part of the real world, as represented by structural equations, we would not in general be able to determine from data the individual relationships, as the observations would be clustered along the interfaces of the jointly fulfilled relations.

Within Frisch's attempt to explain business cycles this meant that there was a limit set by simultaneity to what extent it would be possible to identify (not Frisch's term!) the model from data. Hence, the problem of simultaneity was closely connected to his business cycle analysis, although in the history of econometrics these topics are literally dealt with in separate chapters.

Poincaré lecture no. 6, *The statistical construction of econometric functions, autonomous and confluent equations, the danger of analysis of many variables*, dealt with issues he would later

[26] The essay, published in poorly distributed journal, remained almost neglected in the literature, notwithstanding the fact that it foreshadowed both principal component analysis, credited to H. Hotelling (1933), and Generalized Least Squares, credited to A.C. Aitken (1934). I owe this observation to Michael Leznik. Frisch also introduced new concepts and methods of presentation, including the first use of linear algebra for econometric analysis (often credited to Aitken).

[27] The terms autonomous/autonomy were not used in print before Haavelmo (1944) but Frisch had coined the terms already in 1931.

take up in the confluence analysis book (Frisch 1934). The book was found difficult to read, the unpublished lecture offered a more attractive introduction. The distinction between structureal (autonomous) and confluent relations was explained by an example.[28]

Three kinds of relations: (1) structural relations, (2) inflated confluent relations, and (3) deflated confluent relations
Let y be the log of the area of a rectangle, x_1 log of the base and x_2 log of the height. (1) $y = x_1 + x_2$. Consider rectangles with base equal to height, i.e. (2) $x_1 = x_2$ For this class of rectangles we shall have (3) $y = 2x_1$. another formula for the same case, not very much used but nevertheless absolutely correct, is (4) $y = \frac{3}{7}x_1 + \frac{11}{7}x_2$. When $x_1 = x_2$, then (4) expresses correctly the area of the square.
The structural relation (1) can be verified with no regard for whether other relations are satisfied; it is an autonomous relation, holding identically in the right-hand side variables. The deflated confluent relation (3) distinguished from the structural relation by being satisfied only when relation (2) is true. Also (3) is true identically in the right-hand variables. The inflated confluent relation (4), contrariwise, is satisfied only if (2) is true, and it is not identically true in the variables.

Frisch (2009, Lecture 6).

Thus for the structural relation and also for deflated confluent relation, the coefficients have a well defined meaning, while in the case of the inflated confluent relation this not the case, only the sum of the two constants has any significance. Frisch commented:

> 'Suppose someone attacks the problem by means of theoretical scheme which implies at the same time the hypothesis that the area can be written as $y = b_1x_1 + b_2x_2$, and the hypothesis that x_1 is equal to x_2, then the matter becomes serious. The theoretical position of the problem itself is then such that if the statistical data really fulfilled the theoretical conditions which

[28] The example originated in the Yale lectures.

> have been assumed, then this will prevent that the available data material can give any information about the parameters of the theory adopted.' (Frisch 2009, Lecture 6).

Recognizing the problem of simultaneity for the identification of economic relationship, Frisch tried to establish a battery of methods to mitigate the problem. Simultaneity implied that the coefficients in structural relation could only be determined as confluent coefficients differing from the true structural coefficients. If we knew upper limits on the absolute values of (any of) the structural coefficients we might indicate an upper limit to the error committed by setting the confluent coefficients equal to the structural coefficients.

In Frisch's methodology the econometric investigation can be pictured as a process of trials and errors. The initial stage consists in determining at a theoretical level the relationships that are assumed to structure the economic system (in modern terminology: *model choice*). Then the econometrician had to estimate as many structural coefficients and as closely as he could from the statistical data, recognizing the limits set by simultaneity. Frisch's generally assumed that measurement errors were present which did not make the task any easier.[29]

The Poincaré lecture series presented a paramount statement about Frischian econometrics. It explained his motivation for a more scientific approach and demonstrated that econometrics could not be reduced to applications of statistical methods. Econometrics was not only a sophisticated technical matter but also had to do with scientific requirements and social issues. The whole message of the Poincaré lectures reverberated in an odd way in Frisch's final statement, the Nobel address (Frisch 1970a).

7. Conclusion: the philosophy of chaos

In the last Poincaré lecture titled *The meaning of social and mechanical laws, invariance and rigidity, remarks on a philosophy of chaos*, Frisch surprisingly touched upon fundamental cognitive issues related to the probabilistic nature of the outer world. He started out by the notion of a scientific law as a mathematical relationship in one coordinate system, which by a non-singular transformation can be transformed with all its complexity preserved into another coordinate system. Trivial as this may seem the situation becomes much less trivial if there are small irregularities in the observations:

[29] As observed many years later by Zvi Griliches: 'He tried to do too much; he tried to solve *simultaneously* the errors-in-variables problem, the *simultaneity* (confluence) problem and the model choice problem.' (Griliches 1974:972).

> 'In that case the nature of the transformation itself begins to exert an independent influence upon the complexity of the outcome of observations. … In fact, if the systematic law that we try to identify among irregular fluctuations, depends upon the system within which it is observed, what is then left of the concept of a law itself?' (Frisch, 2009, Lecture 8).

Frisch then outlined the topic Poincaré lecture:

> 'I will speak about three aspects of this question. First, I shall make a somewhat technical remark on the invariance of statistical laws, then say something on the reversibility of the phenomena of the external world, and, finally, speak on the most important aspect, namely, the rigidity of observable laws and the question is then to know to what extent we have constructed the observed laws ourselves. These three aspects of the question will lead us towards the same general conclusion, namely the absolute relativity of all our observations and all our conclusions concerning the external world. We conclude with the conception of an external world as being ultimately essentially chaotic.' (Frisch, 2009, Lecture 8).

His technical remark about the invariance of statistical laws was as follows. Let a scatter of observations be given in one coordinate space and a regression plane determined. Then let a non-singular linear transformation transform the scatter to another coordinate space of equal dimension. Frisch then posed the question of the relation between the regression plane transformed to the second coordinate space versus the regression plane determined by the same method in the second space. It is mathematically trivial but epistemologically uncomfortable that there is no invariance whatsoever of most regression methods under a simple linear transformation. Thus Frisch shed doubt on the invariance of economic laws by pointing out the lack of invariance of regression methods.[30]

On the rigidity of laws Frisch formulated his assertion as the following mathematical theorem:

> 'For any given scatter of points in (x, y) which is not perfectly correlated, there exists a non-singular transformation into (u,v), such that the correlation can be chosen arbitrarily close to 1 and furthermore the regression coefficient of the transformed mass of points can also be chosen freely.' (Frisch, 2009, Lecture 8).

The proof was mathematically trivial but Frisch elaborated persuasively upon the epistemological implications:

> "In a large number, if not the majority, of the problems we meet with in social, in biological and above all in physical series, it is nothing which imposes the choice of one coordinate

[30] This was in fact an old topic for him as Frisch (1929) had explored invariant regression methods.

system rather than another. But where are we then? By a change of variables we can make the appearance of a law disappear, or we can create a law and give it the appearance we like.

But what is then the object of science? The incessant preoccupation of science is to find theoretical schemas, new coordinate systems that fit better and better to the so-called facts. If science finds a discrepancy, it modifies its theoretical scheme, it introduces other variables, and in short it makes a transformation. Having done that, it declares triumphantly that now it has succeeded in finding a scheme fitting even better with experience. What does that mean? It means that science has made transformations closer to singular transformations than before. You probably find such a view of science disgusting, you will like better to regard scientific activity as disinterested research for objective truths which are perpetually outside us.

We have here arrived at the point where it is necessary to draw the final consequences of the perspective I have presented for you here. It is necessary to translate this perspective to the biological plane. Let us suppose that we have a biological being of some sort which was first equipped with sense organs which could register the influences (x,y). It lives in a chaotic world and it will have neither the means of looking ahead nor the means of serving itself from the forces of nature. Thus it will most likely be eliminated. But other biological beings will develop, perhaps some supplied with sense organs influenced by (u,v). These beings live in a very beautiful world; they will develop natural sciences for discovering the laws of the world. Their science will research the same genre of singular transformations according to which the biological transformation has taken place. But there is more. Science, encouraged by its success, will probably engage in speculations and in research of new singular transformations which go beyond by far the biological transformations, which have defined the sense organs, and in that new domain, at the same time both abstract and empirical, there are infinite possibilities of discovering new so-called laws of nature.

Most likely these new adventures of science will have a repercussion on life and perhaps even on the biological development of the species. There will be interdependence between the biological evolution and the scientific development, very similar to the relation between a demagogue and the people. Under the influence of this mutual interdependence the biological and the scientific life will continue their evolution. During this evolution science will certainly from time to time register new fundamental discoveries. But the world which science in that way will discover, will be very, very distant from being an objective world.

Why then do science? Because we can perhaps by that hope to soften at least a little the pain of the species which develops, for evolution will always be accompanied by pain, that is only universal and eternal principle which we never will have to question the existence of.' (Frisch 2009, Lecture 8).

After being awarded the Nobel prize in Economics in 1969 Frisch recapitulated in his Nobel address the message of this ominous concluding part of the unpublished Poincaré lectures, suggesting that it was a topic close to his heart, even 36 years later:

> ‘It is quite clear that the chances of survival of man will be all the greater the more man finds regularities in what seems to him to be the “outer world”. The survival of the fittest will simply eliminate that kind of man that does not live in a world of regularities. … Science considers it a triumph whenever it has been able by some partial transformation here or there, to discover new and stronger regularities. …If “the ultimate reality” is chaotic, the sum total of the evolution over time – biological and scientific – would tend in the direction of producing a mammoth singular transformation which would in the end place man in a world of regularities. How can we possibly on a scientific basis exclude the possibility that this is really what has happened? This is a crucial question that confronts us when we speak about an “ultimate reality”. Have we created the laws of nature, instead of discovering them?’ (Frisch, 1970, 218-219)

By focusing on the technical and innovative aspects of the various works of Frisch, one may miss the coherence of his research program. The Poincaré lectures help us to understand that Frisch is defining a new research field based on the intertwining of empirical and theoretical quantification but also attempting to articulate economic rationality and political rationality. Indeed, Frischian econometrics has to be read in a broader perspective. According to the Poincaré lecture 8 (but also the Nobel address) science aims at discovery and the formulation of laws in order to avoid chaos. This works well through social order and economic growth. Financial collapse may remind us that chaos is not so far away as he had presumed.

The work of Frisch is clearly two-fold: on one hand structural modelling and on the other economic policies. But to what extent these two aspects are separated? The interrelation of heuristic aims and political ones of Frisch’s work was already noted by Andvig (1985), who related the Frischian stake in the post war development of planning tools and models by the social consequences of the Great Depression. The Poincaré lecture 8 is a peculiar way of reminding us about the social responsibility of the scientific worker but does little to remove the ambivalence of Frischian scientific economics in its crossing of normative and positive ambitions.

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