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Abstract

This paper is prepared for the forthcoming publication of Frisch's 1930 Yale lecture notes, *A Dynamic Approach to Economic Theory: The Yale Lectures of Ragnar Frisch* (details at: http://www.routledgeeconomics.com/books/A-Dynamic-Approach-to-Economic-Theory-isbn9780415564090). As the lecture series was given just as the Econometric Society was founded in 1930. We provide as background, a blow-by-blow story of how the Econometric Society got founded with emphasis on Frisch's role. We then outline how the Yale lecture notes came into being, closely connected to Frisch's econometric work at the time. We comment upon the lectures, relating them to Frisch's later works and, more important, to subsequent developments in economics and econometrics.

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Why should eighty-year-old lecture notes in economics by Ragnar Frisch have any interest today? We shall try to answer the question in this introduction and also prepare the reader for what he/she will find in them.

The lecture series for which these notes were prepared was given by Ragnar Frisch as Visiting Professor at Yale University in the autumn of 1930. This was the second of three successive terms Frisch spent in the USA. Practically all the ideas he worked on in the 1930s and even after the war can be traced back to this period. Frisch set out in the lectures his conception of a more scientific economics. He focused particularly on the most important issue throughout the interwar period, the explanation of business cycles, to which his approach, best known from his 1933b article, may well be said to provide the basis for macroeconomics. While he revered a number of the great names who had contributed to the development of economic theory he generally found much of the empirical work done in economics lacking in stringency with regard to proper methods. In the lectures he set out his own ideas and, as we argue below, anticipated methodological approaches to be further developed by others in the ensuing decades.

The timing of this lecture series can also be directly related to the foundation of the Econometric Society. Frisch's lecture series lasted well into December 1930. As we shall see Frisch was essentially involved in every step of the way towards the foundation of the Econometric Society, which took place on 29 December 1930 in Statler Hotel, Cleveland Ohio, in between other events at the annual joint meetings of the American Economic Association, the American Statistical Association, and other professional associations.

The Econometric Society was founded as a programmatic association, with the primary goal to scientify economics. Physics was set as the scientific ideal, as referred

to in the Constitution. The Econometric Society was the first international organization in economics. At the time, it was of less importance in the USA than in Europe, where scholars from different countries were brought together at the annual Econometric Society meetings and speeded up the exchange of methodological and theoretical ideas considerably. The membership remained small until long after World War II. But many young budding talents were attracted to the Society almost from the beginning, as reflected in Nobel economics laureates over the first 20 years of the Prize, many of whom had acquired the membership in the 1930s or 1940s. The success of the Society is perhaps far beyond what its founders had expected.

A key to this success was the financial support of Alfred Cowles III, which allowed the Society to publish its own journal, *Econometrica*, from 1933.[1] In the very first issue Joseph Schumpeter wrote programmatically on behalf of the Econometric Society:

> 'We do not impose any credo — scientific or otherwise — and we *have* no common credo beyond holding: first, that economics is a science, and, secondly that this science has one very important quantitative aspect. We are no sect. Nor are we a "school".' (Schumpeter 1933)

But what did Schumpeter, and those he spoke on behalf of, mean by economics being a 'science' and this science having 'one very important quantitative aspect'? What, indeed, were the ideas about economics and science of those 16 people who took part in the "organization meeting" which declared the Econometric Society as established? Ragnar Frisch, the second youngest of the 16 founders, was, in fact, the

[1] Alfred Cowles also funded the Cowles Commission at Colorado Springs in 1932, a research facility devoted to promote the scientific aims of the Econometric Society (see Christ, 1952). He originally wanted to call his new enterprise *The Econometric Foundation*, which, indeed, would have been a suitable name, but had to find another solution as European Council members reacted negatively. The Commission moved to Chicago in 1939 and was affiliated to the University of Chicago. In 1955, it moved to Yale University and changed its name to the Cowles Foundation.

kingpin everything revolved around with regard to the the founding of the Society and has cast lasting impact on its future developments that few of the other founders have.

It is therefore important to recapitulate the evolvement of Frisch's idea about econometrics in association with the founding of the Econometric Society and its journal, which went back as early as 1926. Of the large amount of Ragnar Frisch's archival remains, his Yale lectures stand out as the single most important work representing the spirit of the founders of the Econometric Society. At first sight, these lecture notes may read loose and tedious. In fact, 'econometrics' is nowhere used or mentioned in the notes. But these notes were not prepared for a showpiece marking the start of econometrics. They were simply for the lectures Frisch chose to give as visiting professor at Yale. However, as they were prepared at a time when his mind was very much concerned with bringing to fruition the idea of an econometrics society, these notes provide us with invaluable information of his frame of mind on the matter.

In order to fully extend the historical importance of Frisch's Yale lecture notes, we shall present a blow-by-blow story of how the Economic Society finally got founded in 1930 with emphasis on Frisch's role (see the next section). Many of documents drawn upon here have never been published and never drawn upon in the accounts of these events. We then describe briefly how the Yale lecture notes came into being in connection with Frisch's contribution to econometrics of the time. In the final section we comment in some detail on the text of the lectures, relating them to Frisch's later works and subsequent developments in economics and econometrics.

1. The quest for the scientization of economics

Studies on the history of econometrics have given plenty of coverage on the role of the Cowles Commission in the formation of econometrics. Surprisingly, far less has

been written on the history of the Econometric Society and the background of its establishment. The summary facts about the foundation of the Econometric Society, as stated by various authors, are often drawn from incidental remarks in Christ's (1952) history of the first twenty years of the Cowles Commission. Christ's account is, however, incomplete and somewhat incorrect.[2] Furthermore, it conveyed the impression that the Econometric Society was rooted in the USA. But as we shall show here, it was an idea conceived and nurtured in Europe, and replanted on to the more fertile ground in the USA when the occasion arose. The man at the centre of all this was Ragnar Frisch. The history would probably have been very different without Frisch's active involvement and participation.

Let us trace back to Frisch's essay Sur un problème d'économie pure (1926a). The essay has been cited more for its opening lines where the term '*économétrie*' (econometrics) was coined, than for its substantive content. It was undoubtedly a lucky strike as a coinage of terms but it was more than that.[3] While Frisch coined 'econometrics' in the first sentence of Frisch (1926a), the second sentence defined the aim of 'econometrics' as that of turning economics 'into a science in the strict sense of the word'. There is a surprisingly, if not straight, intimate link from this coinage to the establishment of the Econometric Society in 1930 and the journal *Econometrica* in 1933. Exactly how and when Frisch got inspired to coining this term with its programmatic connotation is not known. It was stated in the paper that the work was done in Paris in 1923 but the opening paragraphs may have been added later. Frisch sent François Divisia a reprint of the essay soon after the work was done. An

[2] Christ may have relied upon Roos (1948), whose accounts of these early events are less reliable than the documentary sources quoted here, as is his unsubstantiated assertion that Irving Fisher had tried to establish an econometric society in 1912.

[3] See Bjerkholt and Dupont (2010) for more on Frisch (1926a). Frisch was extremely fond of coining terms – in several languages – but only 'macroeconomic/microeconomic' can match 'econometric' with regard to international success of acceptance.

exchange of letters followed between the two men in 1926. In one letter, Divisia proposed that the inherent aim of 'econometrics' be supported by a suitable organization of some sort for scholars sharing an interest in a more scientific economics, including a journal of some sort. Frisch responded by proposing to name the journal 'Econometrica'.

Both Divisia and Frisch took meticulously care of this early correspondence and gave related documentary accounts on later occasions (see Divisia 1953; and Frisch 1970: 222-6, which is his Nobel Prize speech). After the exchange with Divisia, Frisch passed the ideas to Ladislaus von Bortkiewicz, Eugen Slutsky, and couple of other European scholars.[4] These letter exchanges lasted into January 1927, when Frisch was preparing himself for the USA in early February on a Rockefeller Fellowship.[5]

Once in the USA, Frisch set to turning the ideas into concrete plans. In the spring of 1927 he wrote a three-page memorandum which was mainly about 'the establishment of an international periodical devoted to the advancement of the quantitative study of economic phenomena, and especially to the development of a closer relation between pure economics and economic statistics'.[6] To support the journal, an organization was proposed, which was temporarily designed as a 'self-

[4] Ragnar Frisch Archive has a handwritten list by Frisch of 14 letters, written shortly before his departure for the USA, entitled *Letters relating to "Oeconometrika"*, with columns for 'date', 'from', 'to', and 'brief summary of the content', as if Frisch already felt convinced of the historical importance of his initiative and these early letters. The first letter was dated 4 September 1926 and the 14th letter 17 January 1927. Note that Frisch in the first letter to Divisia had spelt the name of the journal, which still was a pie in the sky, as 'Econometrica'. In the ensuing years Frisch used about five more names, accommodating persuasive advice from different quarters.

[5] Frisch had a three-year fellowship from the Laura Spelman Rockefeller Memorial but intended to spend the last two years in Europe (possibly because the terms did not allow Frisch's wife, Marie, to accompany him in the USA). Soon after Frisch returned to Europe Frisch's father fell ill and died. Frisch felt obliged to surrender the fellowship for family reasons and the bottom fell out of his scientific career (see Bjerkholt and Dupont-Kieffer 2009, Editors' introduction: xviii-xix).

[6] Deposited at Ragnar Frisch Archive, the National Library of Norway. The document was just entitled *Memorandum*, with no date or author given. Divisia received the memorandum in a letter of 22 May 1927, and quoted it at length in Divisia (1953: 24-25). The archive also has a draft version of Divisia's memorandum.

perpetuating executive committee' with representatives from five major countries, who had the power to appoint two editors, one American and the other one European, and an editorial secretary. The financial support was to come from an 'American endowment'.

The design was revised later. In July 1928 Frisch wrote again to Divisia from his summer recess at Glacier National Park, telling him that he had come to the conclusion that it would be preferable to have only one responsible editor. Furthermore, Frisch proposed Camille Colson or Divisia to be the editor!

In the autumn of 1927 Frisch rewrote and extended the memorandum to a five page version dated October 1927.[7] Frisch had surely promoted these ideas as often as he could during travels to different universities in the USA. Schumpeter visited Harvard in 1927/28 arriving in September 1927; Frisch wrote to him from Berkeley and said that he would come to Harvard in November or December to discuss the plan with Schumpeter there. He was most likely the European economist Frisch considered as his best ally. Their meeting certainly must have comprised a discussion of the extension of the memorandum. The revised memorandum is interesting because it mainly concerned the scientific aspect and is worth quoting at length:

> 'An important object of the Journal should be the publication of papers dealing with attempts at statistical verification of the laws of economic theory, and further the publication of papers dealing with the purely abstract problems of quantitative economics, such as problems in the quantitative definition of the fundamental concepts of economics and problems in the theory of economic equilibrium.

[7] Deposited at Ragnar Frisch Archive, the National Library of Norway. The new version bypassed the question of one or two editors and mentioned only 'editorial staff'. The name of the journal had become *Oekonommetrika*!

> The term equilibrium theory is here interpreted as including both the classical equilibrium theory proceeding on the lines of Walras, Pareto, and Marshall, and the more general equilibrium theory which is now beginning to grow out of the classical equilibrium theory, partly through the influence of the modern study of economic statistics. Taken in this broad sense the equilibrium problems include virtually all those fundamental problems of production, circulation, distribution and consumption, which can be made the object of a quantitative study. More precisely: The equilibrium theory in the sense here used is a body of doctrines that treats all these problems from a certain point of view, which is contrasted on one side with the verbal treatment of economic problems and on the other side with the purely empirical-statistical approach to economic problems.'

In other words, it was a bold attempt to draw a line demarcating what was 'in' and what was 'out' with regard to publication in the journal. More importantly, the emphasis on 'equilibrium theory' placed the concept as the corner stone to build on for scientization of economics. Frisch was at the time highly concerned with the understanding and identification of business cycles, the emphasis may be related to his conviction that cycles were part of an equilibrium phenomenon rather than an independent state as opposed to an equilibrium state.

The rather strictly defined editorial policy was modified by an additional statement that the journal should accept papers 'of a more purely statistical character provided they deal with such statistical technique or such statistical data that they have a definite bearing on problems in the equilibrium theory'; and an even finer delimitation restricted the journal from publishing 'papers dealing with statistical technique in general', as such papers had other more appropriate publication channels.

Towards the end of his fellowship year in the USA, Frisch reported to Divisia in January 1928 that the prospect of getting financial supports for a journal was bleak there (Divisia 1953: 26). It seems overwhelmingly likely that Frisch had raised the issue to a number of potential donors including the Rockefeller Foundation, which financed not only a large number of fellowships in economics, but also empirically oriented economic research institutions in many countries. The effort seemed to have come to nothing.

There is one more event of interest before Frisch left the USA in early March 1928. On 29 February 1928, Frisch met Joseph Schumpeter and Gottfried Haberler in the Colonial Club at Harvard. Their conversation was minuted down by Frisch.[8] According to the minutes, the conversation was more or less as a constitutional convention on econometrics (and very much a European one at that!). Paragraph 1 of the minutes put down the fundamentals: 'The terms *econommetric* and *econommetrics* are interpreted as including both pure economics and the statistical verification of the laws of pure economics, in essential distinction to the purely empirical manipulation of statistical data on economic phenomena' (our italics).[9] Then the minutes dealt with the journal, which was downgraded to a 'systematic, annotated bibliography of econommetric litteratur[!]', possibly issued as a supplement 'to some existing economic journal'. There were some details about the selection principles. Bibliography covered by the journal should comprise engineering, natural science,

[8] The typewritten minutes were entitled 'Abstract of Conversation between Professor Schumpeter, Dr. Haberler and Dr. Frisch on Possible Measures to Promote the Study and Teaching of Econommetrics', Ragnar Frisch Archive, the National Library of Norway. The key points occupied two and half pages with additional two pages of a list of people from 18 European countries, who were 'suggested as susceptible to support the work, or as being able to give information about such persons'. With the minutes in the archive were also 17 page of handwritten notes on the stationary of Colonial Club, Cambridge MA, which on closer scrutiny can be identified as ten pages by Frisch, four by Schumpeter, and three by Haberler, mostly of names and comments related to proposed names. Schumpeter's pages also had a nicely drawn map of Northern Italy.

[9] Schumpeter, who had strong credentials in classical languages, particularly Greek, argued consistently on the double 'mm' in the coined new term on logical and etymological grounds.

and mathematical journals as these frequently would have articles which 'for methodological or other reasons are highly significant from the econommetric point of view'. The bibliography should naturally also allow proper review papers of 'more important contributions'.

The issue of an organization was also brought up. Some interesting points were made though in a modest way. The minutes described it as 'an informal circle of scientists the world over interested in problems of econommetrics'.[10] The circle should be open to not only economists but also 'scientists in other fields, who by the nature of the problems have an affinity to econommetrics'. Cooperation with mathematicians was especially mentioned. And, not least important, the circle was given a name, undoubtedly put forward by Schumpeter: *ERANOS OEKONOMMETRIKOS – an International Circle for the Promotion of Econommetric Studies*.

So what would the circle do apart from reading the journal of *Oekonommetrika*? A low-key realism seemed to have prevailed at the Colonial Club that day. Until more experience was gained the activity would have to be restricted to circulating a list of names and addresses to facilitate correspondence. The topic of 'possibilities of establishing regular courses of econometrics in colleges and universities' was the only one explicitly specified for the correspondence.

The meeting with Schumpeter and Haberler on the eve of his return to Europe was an important boost to Frisch in pursuing his bold dream, after failing to get financial support during his American visit. Frisch travelled widely during his fellowship year and tried energetically to find and contact people who shared his interests and could become potential allies in his econometric cause. He didn't find many. Now, he prepared himself for taking the cause back to Europe. Frisch had already established

[10] This mirrored Divisia's reaction to Frisch's proposal in 1926 of an international association, namely that the first aim ought to be a 'cercle restreint' (see Divisia, 1953: 22-23).

contact with many Europeans who might be interested in econometrics and had got additional names from Schumpeter and Haberler.[11]

During his American visit, Frisch naturally visited Irving Fisher at Yale. Frisch was in a sense a pupil of Fisher, as he had been greatly inspired by Fisher's 1892 dissertation, which Frisch had studied in Paris (see Bjerkholt and Dupont-Kieffer, 2010). The two men shared an interest in measuring marginal utility, as well as in promoting mathematical economics. Frisch also sought out Charles Roos at Princeton; Roos had been a student of Griffith C. Evans and was involved in promoting economics with the American Association for the Advancement of Science. The latter got him in contact with the Harvard polymath E. B. Wilson. Nothing much seems to have come out of Frisch's contact with Fisher or Roos in this period, however.

The theoretician that Frisch really had looked forward to meeting and enlisting in the support for econometrics in the USA was Allyn A. Young, who for some years had been at Harvard. Allyn Young gave Frisch, at his consult for leading figures in the USA in mathematical economics, a list of eight names - Irving Fisher, H. L. Moore, Warren M. Persons, Holbrook Working, Frank H. Knight, Frederick C. Mills, Mordecai Ezekiel and E. H. Chamberlin. These, wrote Young, 'includes practically everyone in this country who has a serious interest in the field of mathematical economics'. Frisch also asked Allyn Young about leading figures in statistical economics. Young passed Frisch's request to Mordecai Ezekiel of the USA Department of Agriculture. Ezekiel came up with a list of another eight names, but apart from Henry Schultz, the names were lesser well known in the history of economics or econometrics.[12]

[11] Among the many names of potential interest floated on the table in the Colonial Club, several were distinguished pure mathematicians, such as Felix Hausdorff and Constantin Caratheodory.
[12] The other seven named by Ezekiel were Bradford B. Smith, Hugh B. Killough, Elmer Rauchenstein, Clyde Chambers, Edward M. Daggitt, C. F. Sarle and G. C. Haas.

After returning to Oslo in early March 1928, Frisch drafted a memorandum in French.[13] Its structure and content was very similar to his Memorandum of October 1927, with some additional ideas from the Frisch-Haberler-Schumpeter conversation. *Eranos Oekonommetrikos* was not mentioned explicitly. The importance of building a network along with the bibliography was set out. The memorandum ended with a suggestion that the chief editor and editorial staff had to be on the alert for the econometric cause and that, if the occasion arose, they should constitute themselves as a committee and take all possible measures to promote the econometric program, by initiatives such as convening an international congress or founding an international association.

There is a gap in Frisch's meticulously kept records after this document. The gap occurred at a time when Frisch's career came to a critical point, due to his father's death and the responsibility he felt for the family firm (see Bjerkholt and Dupont-Kieffer 2009, Editors' introduction). Frisch may have considered giving up his scientific career; at least he told Irving Fisher so in the spring of 1929. Fisher responded by arranging him an invitation from Yale University for one year as Visiting Professor on fairly generous financial terms. Frisch accepted the invitation and came to Yale in February 1930.

This American trip reignited Frisch's dream to promote econometrics. In the spring of 1930, Frisch lectured at Yale on a number of topics that interested him and worked intensively on several research ideas. He had ample contact with Fisher; they planned to write a book together and Frisch assisted Fisher in a number of odd jobs. Charles Roos dropped by, perhaps on more than one occasion. Interaction of the three men led them to the conclusion that the time was ripe for a renewed attempt to launch

[13] Deposited at Ragnar Frisch Archive, the National Library of Norway, in the form of a five-page carbon copy of a typewritten document dated April 1928; there was also a handwritten draft dated April 1928 as well.

an econometrics association. In a letter to Schumpeter written on 11 June,[14] Frisch informed him that he would very soon receive important news about 'the plans of establishing an international association of mathematical economics'.

The plan was drawn up on 17 June 1930, when Fisher, Roos and Frisch met again in Fisher's spacious property on Prospect Avenue in New Haven. The three men drafted a five-page letter about 'the organization of an international association for the advancement of economic theory' and distributed it to a carefully selected list of leading scholars in a number of countries. In addition to the three signees, the list consisted of 28 names from 11 countries.[15]

The story thereon up to the organization meeting in December is well known in the literature. The 17 June letter is quoted at length in Divisia (1953).[16] It stated that the chief purpose of the association was 'to help in gradually converting economics into a genuine and recognized science' and that the term 'theory' used in connection with the organization should 'not be interpreted as synonymous with abstract reasoning only, but as including also the analysis of empirical evidence suggesting or verifying theoretical laws'. Interestingly, the term 'econometrics' was not introduced, neither for the name of the association, nor for the stated purpose of it. The letter went on to suggest four requirements for membership eligibility and asked the recipients whether they agreed to these requirements and whether each one considered himself eligible and interested in helping to form the association as one of its charter members. The requirements were stated as limiting membership to those who

[14] Schumpeter was expected to be visiting Harvard again in late September that year.
[15] By country of affiliation the numbers and names were: USA 10 (T. N. Carver, J. B. Clark, J. M. Clark, G. C. Evans, M. Ezekiel, I. Fisher, H. L. Moore, W. M. Persons, C. F. Roos, and H. Schultz), Italy 5 (L. Amoroso, C. Gini, U. Ricci, T. de Pietri Tonelli, and G. del Vecchio), France 4 (C. Colson, François Divisia, J. Moret, and J. Rueff), United Kingdom 3 (A. L. Bowley, J. M. Keynes, and A. C. Pigou), Germany 2 (L. von Bortkiewicz and J. A. Schumpeter), Sweden 2 (G. Cassel and B. Ohlin), Austria 1 (H. Mayer), USSR 1 (E. Slutsky), Poland 1 (Wl. Zawadski), Denmark 1 (H. Westergaard) and Norway 1 (R. Frisch).
[16] Quotes from the letter are from the copy in the Ragnar Frisch Archive at the National Library of Norway.

(a) are thoroughly familiar with general economic theory,

(b) have a working knowledge of mathematics as applied to economic theory and statistics,

(c) have some knowledge of accounting,

(d) have published an original contribution to economic theory or to the analysis of such economic statistics or accounting as have a definite bearing on problems in economic theory.

(Letter to multiple recipients by Irving Fisher, Charles F. Roos and Ragnar Frisch, 17 June 1930, page 1, Ragnar Frisch Archive, the National Library of Norway)

One may wonder where the 'accounting' requirement came from. It seems rather unlikely to have come from Frisch, some of the recipients found it irrelevant. Why was it not 'some knowledge of statistics' instead? Did the term 'statistics' carry too much ambivalence as to its connotation as a requirement? An additional question raised in the letter provides some indication of this unresolved difference among the triumvirate. It asked the recipients whether they think 'have some experience in handling statistical data' should be added to the four requirements.

The letter did not specify the exact meaning of the 'science' that economics should be converted into, apart from it being 'genuine and recognized'. Physics or other sciences were not mentioned. Nor was there any trace of the idea of jumpstarting economics from scratch.[17] On the contrary the letter emphasized conversion rather than reconstruction of economics, as implied in the following quote:

[17] After the Econometric Society was established there were apparently occasional calls in the USA for a break with the past and a complete reconstruction of economics.

> ‘You will notice that in the requirements for eligibility we have put a good deal of emphasis on the quantitative character of economic theory. As you know, the quantitative movement in economics started on an a priori basis through the introduction of mathematics into economic theory. Afterwards and independently, empirical studies in mathematical statistics were made. Still more recently, a beginning has been made to bridge the gap between these two approaches. These attempts at putting economics on a scientific and quantitative basis by introducing numerical and statistical observations into the theoretical structure we consider as one of the most promising developments in modern economics, and one to which the association should give considerable attention, In our opinion, it will be largely through a constant and close connection between the abstract-rational and concrete-empirical points of views that the modern quantitative movement in economics will produce significant and lasting results.’
>
> (Letter to multiple recipients by Irving Fisher, Charles F. Roos and Ragnar Frisch, 17 June 17 1930, page 3, Ragnar Frisch Archive, the National Library of Norway)

The quote conveyed the understanding, shared perhaps both by the triumvirate and most of the recipients, that the task at hand was to build upon existing theory, enhanced through greater emphasis on mathematics in the formulation of theory, and particularly through ‘bridging the gap’ through theoretical and empirical work. In particular, the last sentence in the quote can be recognized as an early and rough version of the key sentence in the Constitution of the Econometric Society (see below). The pre-eminence of theory in this bridge building was even related to the practical criteria for future membership, as the letter explicitly proposed to prevent

those who thought they could do empirical analysis of economic problems 'without reference to fundamental theoretical principles' from joining the association:

> 'We believe that the association should not include those who have merely treated economic problems empirically, without reference to fundamental theoretical principles. If you and the others consulted are in sympathy with us in laying down this policy, mathematical statisticians as such will not be included. They will only be included if they satisfy all the requirements (a) to (d). In practice, the line may be difficult to draw. On this we would like to have your judgment.'
>
> (Letter to multiple recipients by Irving Fisher, Charles F. Roos and Ragnar Frisch, 17 June 1930, page 3, Ragnar Frisch Archive, the National Library of Norway)

In addition to consulting the recipients on this potentially sensitive barrier for membership, the letter also asked for responses on a number of other points, such as people to be invited for membership, the proposed association's activities, its envisaged journal and its content as well as editorial policy, etc. The letter tentatively named the journal as *Oekonommetrika* for the recipients' opinion.

26 out of the 28 recipients responded to the 17 June letter, some at great length. Frisch summarized their responses into a 13-page document with a digest of answers.[18] The triumvirate subsequently decided to go ahead to the next stage of organizing a meeting during the joint meetings of the American Economic Association, the American Statistical Association, the American Mathematical

[18] Only G. Cassel and A. C. Pigou did not respond. The typewritten document put together by Frisch was entitled 'International Association of Economists. Digest of Answers to Letter of Invitation to First List', dated 12 October 1930, deposited at Ragnar Frisch Archive, the National Library of Norway. It is a highly interesting 13-page document to assess the international interest and, indeed, enthusiasm over the idea of an econometric association but we leave it aside here.

Society, and the American Association for the Advancement of Science. A draft 'Constitution of the Econometric Society' was agreed by the triumvirate in November and enclosed in an invitation letter for the 'organization meeting', which was sent to a list of 84 names on 29 November 1930, exactly a month prior to the due date of the meeting. The list covered the majority of European countries as well as Japan, China, Brazil, and Algeria. One month in advance was of course a relatively short notice for overseas recipients. Few, if any, might at all have considered the possibility of attending.

It was in the draft Constitution that the name 'Econometric Society' appeared for the first time. The draft Constitution also set out key phrases that would serve as signposts for the association and its journal in years to come. As an explanatory subtitle, the Society was called 'An International Society for the Advancement of Economic Theory in its Relation to Statistics and Mathematics'. In paragraph 1 on the 'Scope of the Society' its main object was set out as 'to promote studies that aim at a unification between the theoretical-quantitative and the empirical-quantitative approach to economic problems and which are penetrated by a constructive and rigorous way of thinking similar to that which has come to dominate in the natural sciences'.[19]

Interestingly, the triumvirate appeared to be quite uncertain of the name, as could be seen from the following almost apologetic statement in the invitation letter:

> 'As to the name of the society, we consider it essential that the name should indicate quite clearly the specific object which the society has in view. If the society is formed with the scope we have suggested, it seems advisable to coin

[19] Although it is not known how these key formulations of the Constitution came about the formulations, it is worth noting that they were strikingly similar to formulations used in Frisch (1926a, 1926b).

a word, since no current single word will connote exactly the correct idea. So far, we have been unable to find a better word than "econometrics".

… …

We are aware of the fact that in the beginning somebody might misinterpret this word to mean economic statistics only. But if the complete subtitle of the society is always given in the official publication and in the letterheads of the society, and if the members and fellows of the society persist in using the word "econometric" and "econometrics" in their proper sense, we believe that it will soon become clear to everybody that the society is interested in economic theory just as much as in anything else.'

(Letter to multiple recipients by Irving Fisher, Charles F. Roos and Ragnar Frisch, 29 November 1930, p.3, Ragnar Frisch Archive, the National Library of Norway)

Their hesitancy turned out to be unwarranted as the name was very quickly accepted. The letter further stated that at the organization meeting 'a President of the society and a Council will probably be elected'. At least probability was there from the very beginning!

The organization meeting finally took place on the night of the first day of the annual meetings of the professional associations in Cleveland. The attendance was however, not impressive. Only eight from the list of 83 turned up in addition to Roos and Frisch. Irving Fisher was absent due to conflicting commitments. But the attendance comprised heavyweights – Harold Hotelling, Henry Schultz, Joseph Schumpeter and E. B. Wilson. The other four from the list were Wiliam Ogburn, J. Harvey Rogers, W. A. Shewhart and J. Wedervang, a professor of economics at the University of Oslo, whom Frisch had helped to arrange a visit to study the American

education system. Schultz may have fitted Frisch's ideal description of an econometrician better that anyone else. Hotelling was considered the best statistician in the USA by Frisch. Sadly, Allyn A. Young had died from influenza in 1929, after returning to England in 1928.

But among the attendance there were six people who were not on the list. Three were eminent mathematicians: Karl Menger, visiting from Vienna, Norbert Wiener, and Oystein Ore, a Norwegian Professor at Yale. Frisch was a member of American Mathematical Society (and not of AEA or ASA). The mathematicians were probably brought over by him from the AMS sessions. The other three were Frederick C. Mills, M. C. Rorty and Carl Snyder, the latter two were stalwarts of the ASA (and together with Wilson and Ogburn successive Presidents of ASA 1928-1931).

It is interesting to observe that theoretical economists were almost absent from the Cleveland meeting. This reflected perhaps the state of American economics of the time, which was strongly influenced by the German historical school, noticeably different from the situation in Europe, where economics was dominated by theoretical economists. It seems odd, however, that none of the econometrically oriented American agricultural economists invited showed up.

2. Emergence of econometrics and the Yale lecture notes

The historic Cleveland meeting and Frisch's intimate involvement set a unique background for the Yale lecture notes. It is remarkable and almost surprising that Frisch made himself a pivotal figure during the foundation of the Econometric Society and was widely recognized and respected as so soon afterwards. We must note that Frisch was not at all well established in 1930, especially in comparison with Fisher. He was just a visiting professor at Yale, not yet having a professorship in his home country. He had not published in any international economic journals. Moreover, his

education background was unimpressive – only a two-year economics program at the University of Oslo.

Frisch was almost a self-taught man. He became proficient in many branches of mathematics through studying in Paris in the early 1920s and he might have felt much more at home among mathematicians than among economists.[20] But Frisch's interest in economics had never left him since his university days. The drive for scientization of economics was certainly inspired by advances in natural sciences, but the underlying motivation was the need for social improvement as much as purely intellectual interests. His Paris treatise (1926a), published in a Norwegian mathematical journal, was an attempt at introducing an axiomatic approach in economics and, at the same time, a bold heuristic attempt at confronting theory and data.

Subsequently, Frisch's attention moved from mathematics to statistics. He defended a doctoral dissertation in statistics in Oslo in 1926. He was in his statistical period while visiting the USA in 1927-28. There, he completed two major works. One was about the analysis of time series, with particular regard to the identification of business cycles. The work was widely distributed as a mimeograph in 1927. It occupies a prominent place in the history of econometrics (see Morgan, 1990; Bjerkholt, 2007). The other work was the essay Correlation and Scatter, completed in 1928 and published in 1929 (Frisch 1929a). It puts forward a very comprehensive approach to the analysis of statistical data. However, the work did not attract wide interest due partly to its use of matrix methods and partly to its publication in a journal which had rather limited distribution outside Scandinavia. In the same year, he also published a paper on the meaning of 'static' and 'dynamic' in Norwegian in a

[20] For more on Frisch's background and early career, see Bjerkholt and Dupont-Kieffer (2009, 2010).

Danish journal (see Frisch, 1929b). The discussion laid the foundation for the Yale lecture notes.

In fact, the Yale notes were not just a summary of his previous studies,[21] they also presented us a comprehensive picture of Frisch's thinking of what economics should be as a scientific discipline. They offer us a vital piece of evidence showing the coming into being of a systematic framework, upon which modern econometrics was to be established into a formal sub-discipline of economics over a decade later by his student, Haavelmo and a group of young researchers at the Cowles Commission (see Qin, 1993). The notes could be seen as a timely herald of the founding of the Econometric Society and also a forceful justification for the rapid rise of Frisch into the leadership position of the Society.

The Yale lecture notes were also taken at a time when Frisch was approaching the prime of his innovative research. He accumulated an inventory of theoretical ideas and developed them substantially during his fellowship year in the USA. The result was a flow of papers appearing in major journals in the USA, UK, France, and Germany during the early 1930s. The best known of the papers are Pitfalls in the Statistical Construction of Demand and Supply Curves (Frisch 1933a), Propagation Problems and Impulse Problems in Dynamic Economics (Frisch 1933b) and the *Poincaré lectures* (see Bjerkholt and Dupont-Kieffer, 2009). His output in the early 1930s also comprised two monographs: *New Methods for Measuring Marginal Utility* (1932) and *Confluence Analysis* (1934). The Yale lecture notes offer valuable insight to these forthcoming works, and also to other ideas that he was working on but never

[21] In the Archive, Frisch kept his translation of parts of his 1926a work from French into English, and also his 1926b work from Norwegian into English. The translation was dated May 1930. Much of the translated materials were used in Part I of the Yale lecture notes.

got published properly. The present publication of the lectures has finally made those ideas openly available.[22]

Readers who have tried Frisch's research papers and monographs before may be surprised at how plain and verbose the Yale lecture notes read, which contrast sharply to his abstruse and sometimes taciturn style of writing known for his published works. We should feel lucky to learn Frisch's general thinking in such a comprehensive and easily comprehensible way. Evidence from Ragnar Frisch Archive tells us that Frisch had in mind of producing a textbook out of his Yale lecture series when these notes were prepared. In a report form to Yale University submitted in the first term of 1930-1 (ie the autumn term), Frisch wrote, as item 3 in the section 'Research and Publication', the following:

> '"A Dynamic Approach to Economic Theory." This is an attempt at developing some parts of economic theory from an angle admitting of statistical verification. The manuscript is now being dictated. Will be a book of about 200 pages.'

The lecture notes have a subtitle: 'Lectures at Yale University beginning September, 1930'. According to the report quoted above, the lectures were for a small course of 13 students (6 registered students and 7 visiting students) at the Graduate School. But Frisch must have thought of the plan for a book during his first teaching term at Yale, which started in February. The very first lecture was delivered on the 13 February 1930, entitled 'What is meant by economic theory?' Frisch prepared a five-page note for that lecture (Ragnar Frisch Archive). The note was extended into a 24-page manuscript, dated in April 1930, with the title 'What is Economic Theory?', which later became the first section of Part I of the Yale lecture notes.

[22] Notice that section 5 of Part II in the planned content list is clearly suggestive of his macrodynamic modelling work (1933b) and section 5 of Part III from the list foreshadows Frisch's (1933a) essay. Unfortunately, both sections were left unwritten in the manuscript.

During the spring term, Frisch also lectured on the topics of time-series analysis and of productivity theory. The latter topic was planned to come out as a separate book,[23] while materials from the former topic were to become part of the Yale lecture notes, as shown from the planned content list in Table 1 (see section 3, 'the separation of short-time and long-time components in an empirical time series' of Part III). The actual existing manuscript of Part III consists of only two sections, one as an introduction and the other section 1 according to the planned list. Some basic features of economic time series were discussed in the introduction section (see the relevant discussion in the next part), paving the way to the planned section 3. In the Archive, there is a pile of lecture notes taken by students who attended Frisch's autumn course. Table 2 presents a list of the lectures covered by these notes. It is clear from the table that the topic of time-series analysis was taught in a number of the lectures in November and December 1930.

Table 2 also tells us that the first few lectures in October were on the topic 'steered vs. free fluctuations', which matches with section 3 of Part II in the planned content list shown in Table 1, another unwritten section of the manuscript.[24] Actually, section 2 of Part II is well represented in the students' notes if we scan through them. The notes recorded a microdynamic system of a market with demand and supply/production side and with a stock of (unsold) goods. It was represented by dynamic equations with numerical parameters displaying price and stock dynamics. In fact, the same model was used in his Poincaré lectures in 1933 (see Bjerkholt and Dupont-Kieffer, 2009: lecture 4).

[23] In the same report quoted above, Frisch wrote as item 4: '"Marginal and Limitational Productivity." This is an attempt at clarification in the theory of productivity. Will be a book of about 150 pages. Most likely to be published by the Yale University Press'.

[24] Frisch later moved from the concept of 'steered' to 'bound' in respect of business cycle analysis; his view was that the cycles were generated as 'free', ie inherent in the economic system, rather than 'bound', ie induced through exogenous shocks.

Frisch must have intended, when he collected and kept these students' notes, to use them as reference when he had time to carry on writing those unfinished sections of the planned contents. Scanned images sampled from the students' notes are given in Illustrations 1 and 2. It is historically interesting to note from the first illustration that the concepts of 'propagation' and 'impulse' were discussed in the fifth lecture on 17 October. That serves as strong evidence that Frisch's had by then more or less worked out the overall ideas for his macrodynamic model paper (1933b), as mentioned previously.

Interestingly, Frisch's plan for the book could be grander than the planned content list, as indicated from the endnote of the list in Table 1, 'New Sections may be added during the course. At the end of the course a corrected table and contents will be inserted instead of the present one'. Unfortunately, Frisch never found the time to finish even the planned contents after delivering the Yale lectures. He was appointed Professor in Oslo from 1 July 1931 and secured funding from Rockefeller Foundation to set up a research institute in Oslo, which was establish at the beginning of 1932. Meanwhile, he became more and more involved in the affairs of the Econometric Society as Council member, long-time editor, President, organizer, contributor and also a key figure in every way in the pre-WWII European meetings of the Econometric Society. He got himself involved with the Cowles Commission as well. With mounting commitments from various activities, the idea of a textbook became a shelved and forgotten dream.

3. The Yale lecture notes from a historical perspective

The lecture notes were incomplete and not ready for publication, as described above. The notes were distributed in very limited numbers as carbon copies bound in

folders.[25] The lecture notes are rendered here exactly as they were written down and distributed in 1930, apart from correction of misprints and minor infelicities, and replacement of the handwritten graphs by computer based reconstructions.

In addition to the editing duty, we shall also make an attempt to provide some commentary discussion on the three parts of the existing lecture notes from the perspective of the history of economics and econometrics.

Part I, General Consideration on Statistics and Dynamics in Economics

1) What is economic theory?

This section is arguably the most philosophical writing by Frisch. His primary attention is to bridge statistical modeling with macrodynamics in economics. His general motivation is to mode economics after science disciplines such as 'astronomy, physics and biology' (p.1).[26] From a scientific perspective, he argues that economics is in need of 'a new fusion between theory and observations' (p.2) and maintains that 'the true theorist in economics has to become at the same time a statistician' (p.2). It sets the ground for his later definition of econometrics as the unification of 'statistics, economic theory, and mathematics' supported by the aim of Econometric Society – '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' (Constitution of the Econometric Society adopted 1930, published *Econometrica*, 1, 106-8). In his subsequent lectures and writings, Frisch only gave summary statements on the matter.

[25] It was recorded on a sheet, dated 2 December 1930 and glued to the inside back cover of the Yale lecture notes kept in the Archive, that eleven copies of the lecture notes had been distributed. The recipients included Schumpeter, Furniss (i.e. Edgar Stephensen Furniss, at the time Dean of Graduate School, Yale University), and most of the students who attended the course.

[26] The page numbers given in this section refers to the transcribed manuscript of Yale lectures.

The essential medium of the unification is model building. Frisch describes models as creation of the intellectual mind of investigators, who are 'sovereigns in the model world' so long as 'the rules of formal logic' is maintained (p.4). Crucially, the creation should serve the primary purpose of 'transobservational invention' (p.5). The invention is embodied mainly in two key components: new concepts/objects and new relations. Once models are created, the investigators would engage in exploring them. Here, Frisch carefully distinguishes two types of regularities: empirical laws, ie 'something which exists in the real world', and rational laws, ie 'something which exists in the model world'. Frisch maintains that 'between them there is a gap, which can never be bridged' (p.7). The continuous attempt to narrow the gap by the investigators reflects the second purpose of model building: to 'understand' and 'explain' empirical regularities. Further elaboration of that purpose leads to discussion of two concepts: causality and probability.

Frisch takes an instrumental viewpoint about 'causality'. He is opposed to the 'animistic' connotation of causes, ie the belief of causes as 'something imperative which exists in the exterior world' (p.10). Instead, he advocates a scientist's expedient attitude of utilizing causal description as a means to digest and simplify the giant mass of observations. Frisch refers to that as 'scientific causality' and points out that its main attribute is 'the direction of causality' (p.10). Such directional relations form the essence of models representing the modelers' description and explanation of the exterior observations. It is interesting to note that Frisch's instrumental viewpoint has maintained its dominance not only in the Cowles Commission work on identification issues with respect to simultaneous-equation models, eg see Koopmans (1949), up to the subsequent invention of Granger causality (1969) in econometrics, but also in mainstream economic methodology, eg Friedman (1953).

As for the concept of 'probability', Frisch carefully distinguishes three notions: 'frequency, probability and belief', or 'empirical, abstract and personal probability'. In the context of model building, Frisch relates frequency or empirical probability with the empirical world, abstract probability with the artificial model world and belief or personal probability with irrational behavior, and cautions against the confusion of personal belief with abstract probability. The discussion again shows that Frisch is not anti-probability as commonly believed, as forcefully argued by Bjerkholt (2005).

2) A discussion of the fundamental distinction between a static and a dynamic economic theory

Here, Frisch criticizes explicitly the Austrian static equilibrium approach as being inadequate in charactering dynamic movements in economics. His dynamic analysis bears discernible influence of the Scandinavian school of thought, eg Wicksell. But he moved significantly further than Wicksell on the subject and his innovative conceptualization was to overarch the Cowles Commission paradigm of structural econometrics.

Frisch starts his introductory exposition by emphasizing that 'the distinction between statics and dynamics is a distinction between two different ways of thinking, not a distinction between two different kinds of phenomena' (p.20). The exposition relies closely on his (1929b) work. Here, Frisch describes static laws as 'variations with regard to certain alternatives' whereas dynamic laws explaining 'how one situation grows out of the preceding' (p.21); or in other words, 'dynamic theory is sequential while the static theory explains things without taking account of the time order of the events' (p. 26). In view of the time dimension underlying all phenomena, Frisch further classifies static analysis into two types: *instantaneous* type and

asymptotic type (p. 24). The latter, referred to as 'asymptotic-static' type, is essentially a 'long run' feature (p. 25). The concept is further extended in the subsequent section when he comments on the notion of equilibrium in section 4.

In the present section, Frisch also discusses several pairs of concepts, eg stationary versus shifting phenomena, one-dimensional versus several dimensional analysis, short-time versus long-time components. Here, what is particularly interesting in terms of econometric methodology is his discussion on the 'ceteris paribus' clause. Frisch remarks that 'the meaning of ceteris paribus is one of the points in economics which has been the subject of the greatest amount of loose and cheap reasoning'. He maintains that 'ceteris paribus has no meaning unless we have already constructed a model world with a several dimensional relationship' as that would enable us 'to specify which these "other things" are that shall be assumed constant'. Therefore, 'ceteris paribus regards the nature of a model world' rather than the 'exterior world' as it is impossible to specify which these other things are (p. 33). Moreover, he uses ceteris paribus as a condition for the validity of individual explanatory variables in a several dimensional analysis, ie a multiple relation setting. He points out that these explanatory variables must possess the property of 'free variability' with respect to each other in order to allow for the ceteris paribus clause. This attributes effectively rules out the problem of multicollinearity, a concept which was yet to be developed.

3) Analytical and historical dynamics

Frisch defines 'historical dynamics' as the method of applying dynamic principles to specific phenomena which have happened, and 'analytical dynamics' as the method of generalizing the phenomena into abstract formulation. It is discernible from the discussion that Frisch's position was to promote econometric research in analytical dynamics, and that he thus regarded it vitally important to define dynamic, as opposed

to static, in a manner as precise as possible. Subsequently, he extended his definition of dynamics in association with the concepts of equilibrium versus disequilibrium (Frisch 1936); and he also augmented his position into 'an ideal programme for macrodynamic studies' (see Phelps Brown 1937: 365-6), as well as his eloquence to convert Tinbergen from the 'historical' method to the 'structural' method (Tinbergen, 1935; 1938).

4) The notion of an equilibrium. Assumption-equilibrium and situational equilibrium

Here, Frisch draws an interesting distinction between 'assumption' equilibrium as against 'situation' equilibrium. The distinction was first discussed in his 1926b paper. Here, the distinction is discussed in the context of a parametric model of a set of interdependent variables. An 'assumption' equilibrium amounts to a normative statement, a certain possible solution of the model, which bears no relevance to the issue whether the solution could be fulfilled in reality. The issue of relevance is pertinent to 'situation' equilibrium, which describes a positive state, a real situation where the characterization of certain assumption equilibrium happens to be fulfilled. Frisch further categorizes situation-equilibrium into two types: stationary equilibrium where values of the variables concerned are asymptotically static/constant, and moving equilibrium where the 'long time components' (long-run values) of the variables are moving/trending together with time. Remarkably, the latter type conceptualizes the essence of cointegration analysis which is developed over 50 years later (see Engle and Granger 1987). But more remarkably, Frisch's conceptualization goes further to the dynamic situation of what is known nowadays as the error-correction mechanism (ECM), eg see Hendry (1995, Section 7.10). In Frisch's description, ECM is termed as a 'stable' equilibrium in which the equilibrium state

would attract small deviations (errors) back to itself (ie a negative feedback system), whereas the equilibrium would be 'unstable' if it stimulates small deviations further away from the equilibrium (ie a positive feedback system). As mentioned above, Frisch's present discussion foreshadows his 1936 paper.

5) Structural, confluent and artificial relations in economic theory

The discussion here is intimately related to the correspondence of empirical analysis with theoretical analysis, although the mathematical relations that Frisch uses to define the three concepts are extremely simple, too simple perhaps for any useful econometric models. Nevertheless, his definition exposes the very essence of structural econometric modeling. Frisch spells out two conditions for a structural relation under the implicit assumption that the relation is set within a constant parameter model. His first condition the relation 'holds good identically in the variables involved', amounts to the ideal situation when there exists a perfect fit of the explained variable given the values of the explanatory variables. His second condition the relation 'holds good even though we do not assume any particular side relation to be fulfilled', effectively rules out the possibility of a so-called 'structural break'. In other words, it requests that the model should remain valid even under the situation when the way by which a certain explanatory variable is generated has been altered. The condition precedes the formal definition of 'super exogeneity' for over half a century (see Engle *et al* 1983). It effectively removes the ground of Lucas' critique (1976). In Frisch's terminology, what the Lucas critique attacks has nothing to do with structural relations but only 'confluent' relations, ie those relations that meet the first condition but not the second.

Frisch defines artificial relations as those which fulfill neither of the two conditions. Interestingly, this definition is close to Yule's (1926) description of nonsense regression.

Part II, Dynamic formulation of some parts of economic theory

In the planned content list, Part II comprises the following sections:

1) A dynamic analysis of marginal utility
2) A dynamic formulation of the law of demand
3) A simple case of steered oscillations. The reaction problem.
4) A simple case of initiated oscillations
5) Dynamic analysis of a closed economic system

As it turned out only the first of these five sections was included in the lecture notes and, furthermore, the manuscript itself strongly suggests that even this first section is incomplete. Part II was devoted to a presentation of Frisch's prime areas of methodological innovation in theorizing. The key concepts here are *dynamics*, *axiomatization* and *marginal utility*. The plan was to display how *dynamization of theory* should be conducted, but the actual lecture notes were little more than preliminaries towards such a dynamization. The second key concept is *axiomatization*, which again is closely connected to the third, *marginal utility*. Frisch viewed axiomatization as a crucial element in the theoretical quantification programme. He had pioneered axiomatization in economics by demonstrating how the postulation of a utility function could be replaced with an axiomatic structure from which the existence of a utility function could be proved (see Frisch, 1926a). His 1926a work had made a strong impression on many of those who had had an opportunity to study it. The work was reviewed and commented in various journals but mainly with emphasis on the empirical part of it. Here, Frisch's discussion on

axiomatization is set in a much more developed conceptual framework. But just as he comes to promising a statement and discussion of the axiomatic system the notes are cut off abruptly. We don't know for sure what was actually covered in the lectures.

The quantification or measurability of marginal utility was a common interest between Frisch and Irving Fisher. By Frisch's own testimony it was Fisher's 1892 dissertation that had inspired and convinced him that it was possible to fulfil Jevons' dream of achieving empirical measurement of marginal utility. In 1925 Fisher made, without being aware of the forthcoming Frisch (1926a), an effort at empirical estimation of marginal utility, only to recognize that Frisch had chosen a better approach. After Frisch's arrival at Yale in 1930, they set out to write jointly a monograph on utility measurement. Their plan somehow aborted; instead, Frisch completed the manuscript of a monograph entitled *New Methods for Measuring Marginal Utility* while at Yale in 1931 and had it published in 1932. The monograph shows how utility reasoning and measurement methods could be very helpful in various policy areas. It does not pursue axiomatization or dynamization of the utility concept. Frisch wanted to deal with those topics in the lecture series.

Frisch (1929b) had, as discussed under Part I above, introduced his ideas about *static* and *dynamic*, which soon became adopted among mathematically oriented economists. But Frisch (1929b) had also made a first attempt at dynamizing the marginal utility concept.[27] It seems likely that Frisch had intended to use that material in this part as well.

1) A dynamic analysis of marginal utility

[27] The discussion of marginal utility and dynamization in Frisch (1929b) is in sections 4-7, which were not included in the 1992 translation.

There are three subsections in this single section of Part II. In the first two, Frisch discusses current theoretical controversies on the use and understanding of the utility concept. In both cases Frisch holds the mainstream position and presents the arguments in a lucid and persuasive manner.

Subsection (a) is on the controversy between psychological vs. the behaviorist approach to utility measurement. The two key proponents Frisch holds up for are F. Y. Edgeworth and Irving Fisher, respectively. The psychological approach is very closely associated with Edgeworth. It had more adherents in UK than elsewhere but was on a losing edge. Fisher represents the opposite view, which discards the relevance of unobservable psychological processes and argues for an approach where acts of individuals' choices constitute the basis for a quantitative definition of utility. It should be noted that Vilfredo Pareto was a better known protagonist for the anti-psychological approach that Irving Fisher, although Fisher's 1892 work was earlier than Pareto's. Frisch seemed to prefer Fisher's dissertation for its quantitative approach, as shown from his discussion here.

An interesting detail in Frisch's non-sectarian presentation of the controversy is his embrace of an argument by Slutsky in his 1927 critique of Böhm-Bawerk's us eof psychological argumenst, an article Frisch received as reprint from Slutsky. Slutsky, incidentally, had also sent Frisch his 1915 article on demand theory. Somehow Frisch has not mentioned it. Actually few were aware of the existence of Slutsky's 1915 work at the time.

In subsection (b) Frisch presents the controversy between adherents of a utility concept as something that is needed for economic analysis and those who argue for an economic theory based only on observable market phenomena free of any utility reasoning. The latter view was represented foremost by Gustav Cassel in his famous

book *Theoretical Social Economics*. Frisch argues for the utility reasoning with respect to 'internal price analysis' as opposed to 'external price analysis'. Frisch describes the latter 'as a dogma that the pricing process should be studied exclusively by external notions, strictly avoiding the notion of utility' (p.76).

Frisch is a strong proponent of letting theory guide the empirical and statistical analysis. Only on the basis of theoretical assumptions and empirical studies could 'be raised from a blind and busy collection of data into the dignity and significance of a scientific investigation' (p.76). This argument is elaborated by two illuminating examples set in simple mathematical terms. The first example illustrates how the demand curve is derived from a utility structure rather than just being postulated; the second is about the derivation of properties of a labor supply curve. Years later the 'external analysis' has a resurged in a more sophisticated form of the revealed preference approach in demand theory.

The axiomatic foundation of utility definition in subsection (c) is the only part where Frisch breaks new ground. He sets his axiomatic approach on the basis of 'fictitious interrogation experiments' and distinguishes the key concepts of 'choice object' versus 'choice situation'. Frisch elaborates on the conceptual framework, including some dynamic elements but his presentation is hampered by the fact that the axiomatic system and the relevant discussion is not included in the lecture notes as necessary prerequisite. It is unlikely that Frisch presented his lectures without touching upon the axiomatic system itself.

A strong proof of Frisch's awareness of the necessary prerequisite is his Poincaré lectures, given at the Henri Poincaré Institute in Paris 2-3 years later. The first topic of the lectures is the axiomatic approach, which is given as a system much enhanced than the original one set in Frisch (1926a). Unfortunately, his Poincaré lectures

remained unknown to the public until about 75 years later (see Bjerkholt and Dupont-Kieffer, 2009).

Part III, Statistical verification of the laws of dynamic economic theory

1) A short statement of some of the classical formulae in correlation and linear regression analysis

Most of this long section is devoted to simple description of correlation and regression theories. Nevertheless, Frisch has not forgotten relating the theories to economics. In particular, he is concerned with the special features of time-series data. That is best seen from his application of Pearson's principal component theory,[28] as an alternative to Yule's (1926) nonsense regression analysis, to the case of two time-series variables, each is assumed of being made of a strong time trend component and a cyclical component. The application demonstrates how misleading it would be to look at the correlation coefficient of the variables if the aim of the investigator is to study the relationship between the cyclical parts (short-time components) of the variables, since that coefficient is dominated by the correlation of the trend (long-time) component. Frisch uses the case to show 'the necessity of decomposing our time series before analyzing their interrelationships' (p. 127). Note that the above conclusion is at odds with the view from many present-day business cycle studies that Frisch is the founder of the Frisch-Slutsky impulse-propagation structural modeling approach (see Frisch 1933b) as opposed to the time-series decomposition and measurement approach led by the NBER (National Bureau of Economic Research). Frisch worked in fact on a wide range of methodological approaches.

[28] Note that Frisch discussed principal components earlier in his (1929a) paper.

Another interesting part of Frisch's discussion of economic time series is in his 'preliminary remarks on the phase diagram' (pp. 132-40). The discussion is focused on how to relate variables whose cyclical components are similar in length but different in timing. He shows how lagged correlation coefficients could be used for the purpose, the essential intuition underlying the modern day Granger causality test (1969).

2) Types of clustering in scatter diagrams and the non-significance of partial correlation

This section demonstrates Frisch's main concern over the exploratory use of correlation and regression analyses for economic inference. He is preoccupied with two problems – the problem of variable choice (ie the missing-variable problem) and the lack of variable variations in data samples (similar to the multicollinearity problem due to inadequate sample data information). From the standpoint of a theoretical/economic model being linear and deterministic, Frisch sees the two problems closely entangled. Nevertheless, his focus is on the situation where the correct and complete model is unknown. He attempts to tackle it first by decomposing the variations of a modeled variable into three parts: 'systematic variations', 'disturbances' and 'accidental variations'. The classification is defined by what he views as the nature of explanatory variables. The variations are 'systematic' when all the relevant explanatory variables are correctly included; 'disturbances' occur when one or a few highly significant variables are missing whereas 'accidental variations' are the result of omitting variables which are unimportant and negligible. When the complete set of the relevant variables is undecided, or in his words the set is 'not closed', 'partial correlation coefficients will be undefined'. Therefore, the modeler should realize that 'partial and multiple correlation coefficients are not primarily

descriptive of the character of the systematic variations, but are essentially indicators of the presence of accidental variations and disturbances' (p 163). In other words, modelers should view them as indicators of incomplete and possibly mis-specified models. Unfortunately, this interpretation of his has left almost no trace in mainstream econometrics to be established during the 1940s.

Frisch then discusses ways to determine if certain variables should be exclude from the variable set and when the variable set comes 'near to being closed' (complete) by means of studying the scatter diagrams. The discussion lays the foundation of his bunch map method in *Confluence Analysis* (1934), as well as the notion of 'irreducibility' versus that of 'redundancy' (Frisch 1938). Although his technical pursuit has not turned out to be as fruitful as he expected, much of his conceptual insight remains thought-provoking, especially his cautious interpretation of the classical correlation parameters.

Noticeably, Frisch's cautious attitude towards the role of statistical analysis in the verification of economic theories preceded his taking the firm position of the structural model approach (1938), an approach which was to dominate econometrics, via the collective work of the Cowles Commission, for years to come. In that respect, the present lecture notes provide us with a wider and clearer historical background upon which the setting to that powerful modeling approach was laid.

Table 1. Planned Contents List of the Yale Lecture Notes

II. General Consideration on Statistics and Dynamics in Economics

1) What is economic theory?
2) A discussion of the fundamental distinction between a static and a dynamic economic theory
3) The static and the dynamic conception of an equilibrium
4) Structural, confluent and fictitious relations in economic theory

III. Dynamic Formulation of Some Parts of Economic Theory

1) A dynamic analysis of marginal utility
2) A dynamic formulation of the law of demand
3) A simple case of steered oscillations. The reaction problem.
4) A simple case of initiated oscillations
5) Dynamic analysis of a closed economic system

IV. Statistical Verification of the Laws of Dynamic Economic Theory

1) Types of clustering in scatter diagrams and the non-significance of partial correlations
2) General principles regarding the possibility of determining structural relations from empirical observations
3) The separation of short-time and long-time components in an empirical time series
4) The phase diagram. Phase elasticities and structural elasticities. The comparison problem in time series components
5) Critical remarks on some of the recent attempts at statistical determination of demand and supply curves
6) A new theory of linear regression. The diagonal and the arithmetic mean regression. The invariance problem
7) A statistical analysis of selected groups of data by the methods developed in the present course

This table of contents is only tentative. New Sections may be added during the course. At the end of the course a corrected table of contents will be inserted instead of the present one.

Table 2. Yale students' notes

Oct. 17, 1939 Lecture V
II.3 Steered oscillations and free oscillations.

Oct. 21, 1930 Lecture VI
II.3 Steered vs. Free fluctuations (Impulse vs. Propagation) (cont.)

Oct. 24, 1930 Lecture VII
II.2 A Dynamic Formulation of the Law of Demand. Illustrations of Steered, Semi-Steered and Free Oscillations

Oct. 28, 1930 Lecture VIII
III.1 Types of Clustering in Scatter Diagrams and the Non-Significance of Partial Correlations.

Oct. 31, 1930 Lecture IX
Correlation Study in n Variables.

Nov. 4, 1930 Lecture XI
Collective Scatter and the Correlation Coefficient.

Nov. 7, 1930 Lecture XII
Regression. Time correlation in Two Variables.

Nov. 11, 1930 Lecture XII
III.4 The Phase Diagram.

Nov. 18, 1930 Lecture XIV
Linear dependency in cyclical curves. III.3 The separation of short-time and long-time components in empirical time series.

Nov. 25, 1930 Lecture XV
Second Difference Operation: K. Simple Addition Operation: H. Weighted Moving Average Operations: M, G.

Dec. 2, 1930 Lecture XVI & XVII
Effect of K and H on sin function (concluded). More General Operation: Ω.

Dec. 5, 1930 Lecture XVIII
Preparatory Smoothing of Time Series. Graphical short hand method of determining any number of components providing all differences in wave order are large

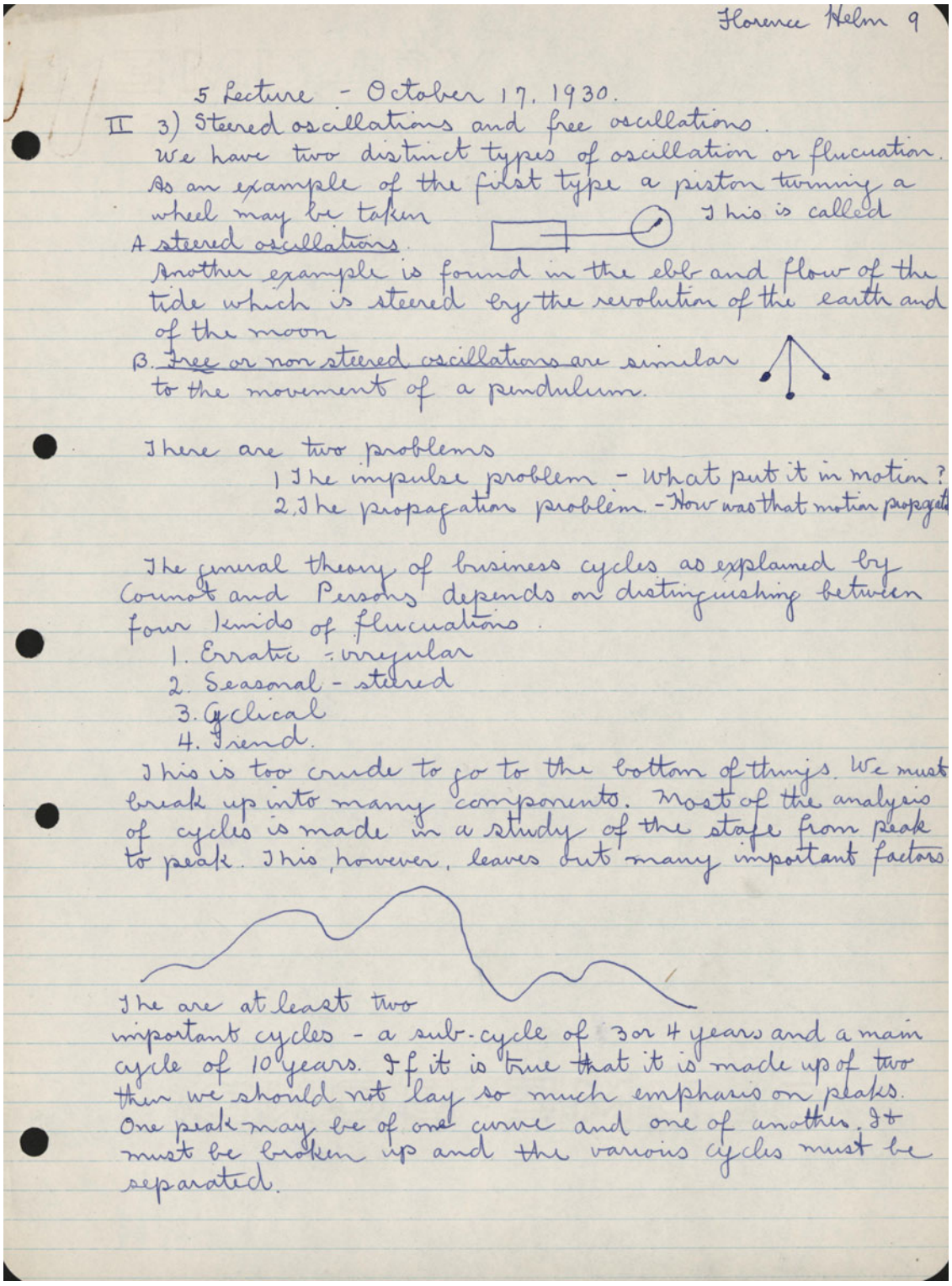
Dec. 9, 1930 Lecture XIX
Treatment of Empirical Time Series. Case I: One cyclical component and no trend. Case II: One cyclical component and a trend.

Dec. 12, 1930 Lecture XX
An initial digression demand-supply (related to the Pitfalls paper). Case II (cont.); Case III: Two cycles and No Trend.

Dec. 16, 1930 Lecture XXI
Some examples of key equations.

Source: Frisch Archive.

Illustration 1. Scanned lecture notes by students: Lecture 5



Florence Helm 9

5 Lecture - October 17, 1930.

II 3) Steered oscillations and free oscillations.

We have two distinct types of oscillation or fluctuation. As an example of the first type a piston turning a wheel may be taken This is called

A steered oscillations.

Another example is found in the ebb and flow of the tide which is steered by the revolution of the earth and of the moon

B. Free or non steered oscillations are similar to the movement of a pendulum.

There are two problems

1 The impulse problem - What put it in motion?

2 The propagation problem. - How was that motion propgat

The general theory of business cycles as explained by Cournot and Persons depends on distinguishing between four kinds of fluctuations.

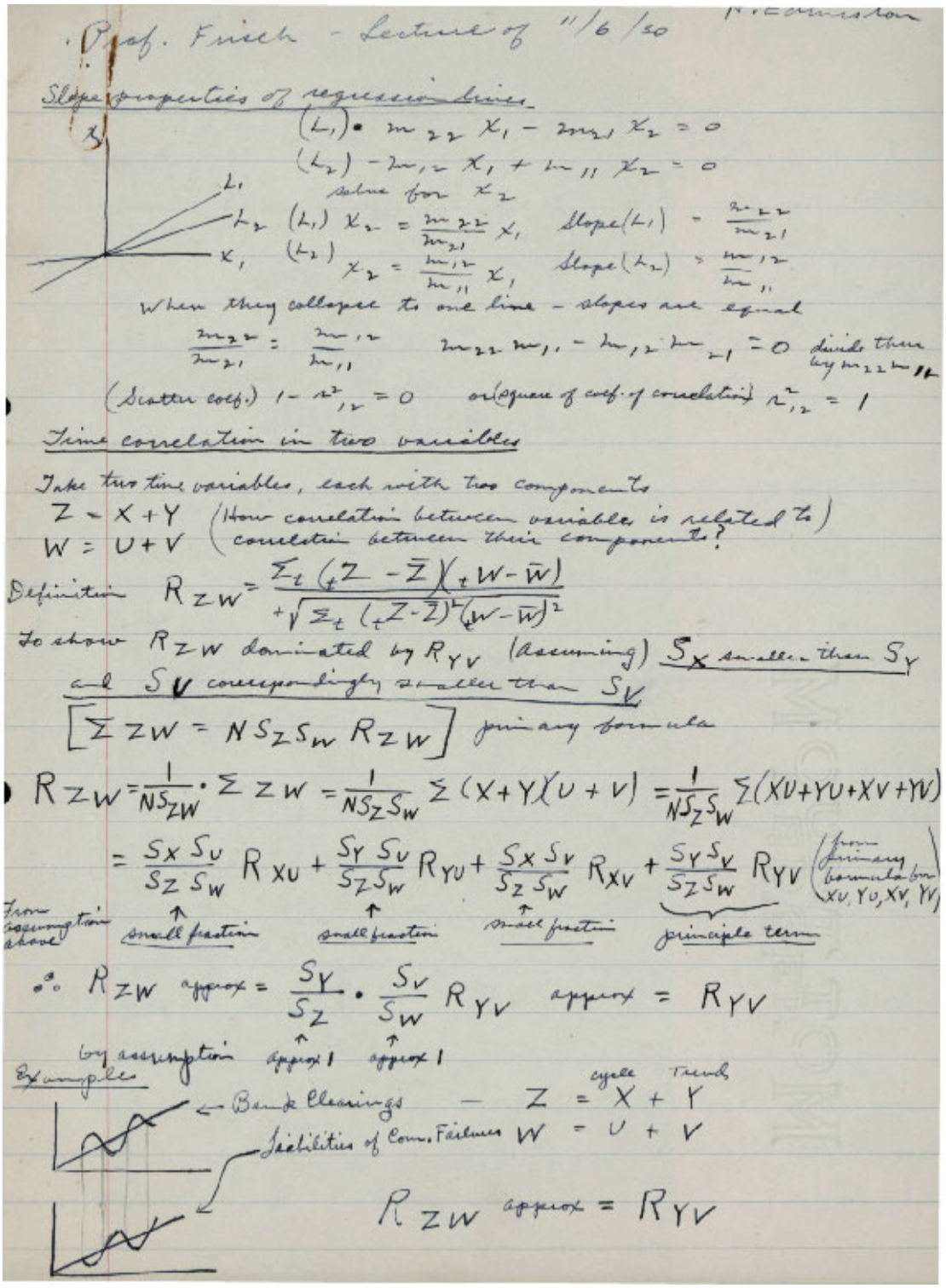
1. Erratic = irregular
2. Seasonal - stired
3. Cyclical
4. Trend.

This is too crude to go to the bottom of things. We must break up into many components. Most of the analysis of cycles is made in a study of the staple from peak to peak. This, however, leaves out many important factors.

The are at least two important cycles - a sub-cycle of 3 or 4 years and a main cycle of 10 years. If it is true that it is made up of two then we should not lay so much emphasis on peaks. One peak may be of one curve and one of another. It must be broken up and the various cycles must be separated.

Source: Frisch Archive.

Illustration 2. Scanned lecture notes by students: Lecture 12



Source: Frisch Archive.

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