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Promoting econometrics through Econometrica 1933 - 37

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Promoting Econometrics through Econometrica 1933-37

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Promoting econometrics through Econometrica 1933-37

By

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Memo 28/2013-v1

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Abstract

The journal of the Econometric Society, *Econometrica*, was established in 1933 and edited by Ragnar Frisch for the first 22 years. As a new journal *Econometrica* had three key characteristics. First, it was devoted to a research program stated in few but significant words in the constitution of the Econometric Society and for many years printed in every issue of the journal. Second, it was the first international journal in economics. Third, it was the journal of association (Econometric Society) with members committed to a serious interest in econometrics. The paper gives a brief account of the circumstances around the establishment of the journal and of the relationship between Frisch and Alfred Cowles 3rd who in various capacities played a major role in launching the journal and keeping it going. It furthermore conveys observations and comments related to the editing of the first seven volumes of *Econometrica*, i.e. 1933-39. The main aim of the paper is to shed light on how the editor and a small core group of econometrician attempted to promote econometrics via *Econometrica*. The paper is overwhelmingly based on unpublished material from Frisch's editorial files. Editorial principles, controversies, and style are illuminated through excerpts from the editorial correspondence. The paper was presented at ESEM-67, University of Gothenburg, 26-30 August, 2013.

JEL: B23, B31

Keywords: Econometrica, Ragnar Frisch, editorial style

1. Introduction

Econometrica, was established in 1933 and edited by Ragnar Frisch for the first 22 years. As the journal of the Econometric Society *Econometrica* was founded to promote a research program - *econometrics* - set out in a compressed but pointed formulation as the goal of the Society in the constitution of the Econometric Society, for many years printed on the inner back cover of every issue of the journal:

> *Its main object shall be to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences. Any activity which promises ultimately to further such unification of theoretical and factual studies in economics shall be within the sphere of interest of the Society.*

In rankings of international journals in economics based on more or less sophisticated criteria the journal of the Econometric Society, *Econometrica*, seems almost invariably to be among the three top ones. Many academic economists would without thinking twice about it point to *Econometrica* when asked which journal is the most prestigious in the profession.

Econometrica is younger than most of the other top journals. It was not a university or institute based journal like e.g. *Quarterly Journal of Economics*, *Journal of Political Economy* and *Weltwirtschaftliches Archiv* but published by a membership association. Other journals published by membership associations such as *American Economic Review* and *Nationaløkonomisk Tidsskrift* (Denmark) were mostly national journals. In the early years the *Econometrica* had marked characteristics of being a membership magazine for Econometric Society, not least by the number of pages devoted to reports from meetings in the Econometric Society and to other membership information. But *Econometrica* differed by being an international journal. The Econometric Society was the first international association in economics and *Econometrica* the first international journal. It was thus a journal with an international constituency in the emerging international community of econometricians which in 1933 comprised 362 members from more than 30 countries. When Alfred Cowles 3rd traded his way into the University of Chicago in 1939 to establish a new site for the Cowles Commission he could boast of having is main asset (apart from his wealth) was the

At the centenary of *American Economic Review* in 2011 a long and impressive paper was published by Torgler and Piatti (2011) reviewing a century of AER. While Torgler and Piatti (2011) can be described as an external retrospective review of the journal it describes, based on compilation of an impressive range of information and observation about AER as seen from the outside. This is in distinction to the account about *Econometrica* given in this paper, which is an inside story in the sense that it overwhelmingly draws on unpublished material, mainly Ragnar Frisch's editorial papers and correspondence with no attempt at evaluating the journal from other points of view.

The paper gives a brief account of the circumstances around the establishment of the journal and of the relationship between Frisch and Alfred Cowles 3rd who in various capacities played a major role in launching and journal and keeping it going. The journal was different from other journals in various ways. The aim of the paper is to shed light on the efforts of the editor towards promoting econometrics via *Econometrica*. Editorial controversies, refereeing practices, internal criticisms et al. is illuminated through excerpts from the editorial correspondence, mainly the correspondence between the editor and a series of assistant/managing editors who in succession were William F.C. Nelson (1932-36), Dickson H. Leavens (1937-1948), William B. Simpson (1949-53)[1] and Robert H. Strotz who took over after Simpson from April 1953 and continued for the remaining part of Frisch's time as Editor.

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[1] From June 1951 Simpson's title changed to Co-Editor.

The main purpose of this essay is to convey an impression of how econometrics was promoted from the editorial chair of *Econometrica*. Then it elaborates on the starting up situation of *Econometrica*, editorial plans, practices and problems. The journal was from the start committed to cover by extensive reports the Econometric Society meetings. One of the editor's main devices for raising the level of econometric insight was by soliciting survey articles on theory, on statistical methods and on data. The aim was one survey article in every issue. It was not a wholly successful effort but perhaps overambitious. The structure of the paper is largely chronological with Frisch's long period of editor subdivided in the early years (1933-39), the war years (1940-1945), and the post-war years (1946-1954).

A more subtle issue, but not discussed in the paper, is how the original conception of econometrics changed over time. It is well known that Frisch severely criticized some aspects of the development within the econometric community in his address to the first World Congress of the Econometric Society in 1965 (Frisch 1969).

2. The dream of a journal

The first issue of Econometrica appeared immediately after New Year in 1933, almost to the day three years after the organization meeting which founded the Econometric Society on December 29, 1930. The establishment of an association had been a coveted goal for a small group of economists and statisticians on both sides of the Atlantic. On the US side the effort in this direction is mainly associated with Irving Fisher (Christ 1952). His interest in promoting the goals that were later taken up by the Econometric Society is well known while the early efforts he shall have undertaken to create an association to promote them are under a veil of myths and anecdotes.

In Europe Ragnar Frisch published in 1926 in a Norwegian mathematical journal a paper titled *Sur un problème d'une économie pure*, which in the opening paragraphs coined and defined the term *econometrics* as the name for new discipline. Frisch sent a reprint of the article to his friend from his study years in Paris, François Divisia. This set off a seminal letter exchange between Divisia and Frisch from September 1926, from which emerged the idea of both an association and a journal to promote econometrics. Frisch elicited support for the idea from savants in several European countries and also took it across the Atlantic to elicit American. It eventually led to the low-key event founding the Econometric Society in Cleveland on December 29, 1930.

The idea of a journal devoted to econometrics was first raised in a letter exchange between Ragnar Frisch and François Divisia in 1926. Divisia suggested the journal. Frisch, who had an irresistible desire and an unsurpassed capability to coin terms in economics, put forth a proposal for a name for such a journal – *Econometrica*. The proposed name derived rather directly from the name of the proposed discipline (with an explicitly recognized inspiration from Karl Pearson's journal *Biometrika*). But it was at that stage a dream with no realistic prospects. Frisch can be credited more than anyone else for having carefully nurtured these ideas toward fruition.[2] Frisch recounted in his Nobel address in 1970 "the years when the plans for an econometric society took shape:

> [François Divisia's letter of 1 September 1926 from his home in Issy les Moulineaux (Seine) was handwritten in his fine characters, 8 pages to the brim with every corner of the paper used. Most of the letter contained discussions on specific scientific questions, but there were also some remarks of an organizational sort. … Answering Divisia in a letter of 4 September 1926 I said inter alia: "Je saisis avec enthousiasme l'idée d'une liste ou d'un autre moyen de communication entre les économistes mathématiciens du monde entier. J'ai eu moi-même l'idée de tâcher de réaliser une association avec un périodique consacré à ces questions. Il

[2] A detailed account of the developments from the first chimera of events set out below until the foundation of the Econometric Society in December 1930 can be found in Bjerkholt and Qin (2010).

> est vrai que les périodiques ordinaires tels que la Revue d'économie politique ou l'Economic Journal, etc. acceptent occasionnellement des mémoires mathématiques, mais toujours est-il que l'auteur d'un tel mémoire se trouve dans l'obligation de restreindre autant que possible l'emploi de symboles mathématiques et le raisonnement par démonstration mathématique. Je connais déjà plusieurs économistes-mathématiciens dans différents pays, et j'ai pensé écrire un jour ou l'autre une lettre à chacun d'eux pour avoir leur opinion sur la possibilité d'un périodique, (que dites-vous d'une « Econometrica »?, la sœur du « Biometrika ».) (Frisch 1970, 18-19)

Divisia had recounted the same events in an article in Econometrica written on the occasion that Econometrica Society turned 21 in 1952. In an annex to the article Divisia quoted from a number of letters exchanged by Frisch and Divisia. In his response to the Frisch's letter quoted above Divisia said:

> Je suis, vous le savez, tout à fait d'accord avec vous sur l'utilité d'une Association International d'Economie pure et j'aime beaucoup le titre d'Econometrica auquel vous avez songé pour un périodique. Toutefois, avant de passe aux réalisations, je pense qu'il indispensable de réunir tout d'abord un certain nombre d'adhésions et je me demande si une simple circulaire suffirait pour cet objet. Les adhésions ainsi obtenues sont parfois un peu superficielles et risquent de manquer de la vigueur nécessaire pour la continuation se l'œuvre. Et tout d'abord il faut un programme précis : le titre d'Econometrica en est un à lui seul, mais peut-être aura-t-il besoin, pour certains esprits, d'être un peu commenté. (Divisia 1953, 23)

Divisia drew a somewhat broader picture painted of early events that pointed the way towards the establishment of a journal and an association as Frisch and Divisia in 1926 exchanged views on. In the article Divisia named Frisch as both father and godfather of *Econometrica*. For these early events see Bjerkholt and Qin (2010).

In the end it was European erudition and American generosity which brought the dream of a journal to fruition.

Alfred Cowles, 3rd read about the Econometric Society, took an immediate interest in the idea and after consulting with associates he decided to get in touch with Fisher, see Christ (1953). Cowles came to Fisher in New Haven on October 18, 1931. Charles Roos was also present. After getting more information from Fisher and Roos about the Econometric Society, still in its first year, Cowles committed himself on the spot to support the Society. In Fisher's office in 460 Prospect Avenue he put the commitment in writing as a letter addressed to Fisher as President of the Econometric Society. The letter is arguably a document of paramount importance in the history of the Econometric Society and Econometrica. The commitment was stated in two short paragraphs and deserves being quoted in full:

> 'In accordance with our conversation I am putting in writing the offer which I am making to the Econometric Society. I am ready to make up any deficit on the proposed journal, "Econometrica," including all the expenses of editing, printing, etc. As part return, I am, of course, to receive the proceeds from subscriptions.' (Cowles to Fisher, Oct. 18, 1931.)

Cowles then proceeded to enhance his commitment:

> 'I am endeavouring to establish a foundation which can be called, if the Econometric Society wishes, "The Econometric Foundation," and which will, when formed, stand in my place in this matter. The plan is to have five trustees, two of whom I am to appoint, two to be appointed by the Econometric Society, and the fifth to be selected by the above-named four.' (Cowles to Fisher, Oct. 18, 1931.)

Alfred Cowles stood by his commitments in a most generous way. The cumulated deficit of Econometrica turned out to become rather large, probably larger than Cowles had imagined on that day in 1931, before the increase in membership and other subscriptions made it profitable. The

Cowles Commission was established in 1932, after the European Council members had ruled out the name proposed by Cowles, indeed, a rather fitting name.

But also the remainder of Cowles' three-page letter is of interest. The commitment was of immediate value for the Econometric Society. But Cowles, after sketching briefly how he had been led to redirect his activity as expressed by the commitments, also took the long view towards a possible separation of the Cowles Commission and the Econometric Society in the future.

He referred to his business as an 'investment supervisory service' and not a big one, mainly for the benefit of his own family and with less than forty subscribers. One of the recent studies undertaken by Cowles' company had been a survey of the success of other services in the same line of business. The conclusion of the study was that these other services had in general 'failed to be of benefit to the investor, either in predicting the general movement of the stock market or in predicting the movement of individual stocks in reference to the general average'. Cowles claimed for his own business that it had been more successful than the average but still 'the record is not one of which to be especially proud'.

The bottom line and the reason for including these passages in the letter was that 'I have come to the conclusion that instead of conducting such a service on the basis of inadequate knowledge, it would be better to convert it into a non-profit research foundation'. Thus it appeared that Cowles had reached this conclusion just at the same time as he read and heard about the foundation of the Econometric Society. He had been in touch with Fisher since some months earlier and had clearly been well informed beforehand about the plans and hopes for a journal of the Econometric Society named Econometrics. Cowles had actually brought to the meeting a "dummy" entitled Econometrica, 'which I have gotten out to show you and which I would suggest for a format'.

Cowles may have anticipated that the leading European members might be sceptical about the involvement of an American business man. In the letter he gave his assurances that his 'primary interest in the new project is not that of enriching myself personally'. Cowles was economically in comfortable circumstances, further money-making is a 'minor matter'. The only advantage he looked forward to from the connection with the Econometric Society was 'prestige which will help me in raising funds to be devoted to this research'.

He then went on to set out what he wanted to achieve with his 'Econometric Foundation':

> 'I expect to be guided very largely in the research policy by the advice which would naturally come to me from the Econometric Society through the trustees that would be appointed and with the set-up as above suggested. The Society would have the power to advise as to the research policies pursued and to prescribe for their change if it disapproves in any way. Furthermore, in case of any disagreement on this or any other score, the arrangement between us could be terminated at reasonable notice by either party.
>
> Naturally in a new enterprise of this sort we must feel our way along, rather than make a long-time contract. Ultimately, I hope that the foundation can be established in a permanent way with endowment funds, but at the present time, the arrangement will be more on a year to year basis.' (Cowles to Fisher, Oct. 18, 1931.)

But Cowles' letter stands out as a uniquely remarkable document which in a couple of sentence provided in unconditional terms the generous guarantees needed to make the econometric movement a success. Half a year later the editorial work of Econometrica started up exactly as set out by Cowles with Frisch as editor and Nelson as assistant editor.

Fisher wrote the following day almost identical letters to Frisch, Divisia and Schumpeter about Cowles' offer enclosing a copy of Cowles' letter. Fisher conveyed that he and Roos found the proposed financing advisable and mutually advantageous, adding 'I myself am enthusiastically in favor of it'. Fisher gave Cowles top marks:

> ‘I know [Cowles] by reputation and knew his father in college and also his mother’s family and other friends of his. I feel positive that Mr. Cowles is of the highest character and has the highest sense of honor. ... His motive is, I am satisfied, exactly as he represents it and, in my opinion, we will never have any embarrassment from the connection. It is exceedingly wonderful to have and “angel” suddenly fall down from the sky to supply us with the one thing needful to make our Society a huge success. Without financing we can never amount to a great deal but with financing we can leap years ahead of what we otherwise would.’ (Fisher to Frisch, Oct. 18, 1931.)

Then Fisher added a passage, which was not in the letters to Divisia and Schumpeter, and expressed Fisher’s respect and confidence in Frisch:

> ‘I think the matter is so important that I would not like to take the responsibility, even if I were sure that as president I had the power, to decide this without consulting you as the original founder of the Society. ... I shall be glad to have [Divisia and Schumpeter] cable me their approval or disapproval if they feel like doing so, but if I do not hear from them, I shall assume that they at least have no objection. In your case, however, I should like to be sure that you approve and I suggest that you cable me the single word “approve” if you do. I shall understand then that no objection will be offered and that I may feel free to work out the details.’ (Fisher to Frisch, Oct. 18, 1931.)

In a handwritten postscript Fisher added some further personal remarks about Cowles:

> ‘Young Cowles like myself, is a recovered case of tuberculosis. He decided to stay in Colorado Springs where he recovered. He practically told me that he makes this offer largely in order to have satisfactory occupation – in supervising or initiating economic research – especially in regard to stock market price movements. Last night I had a talk with Wm. Cowles, uncle of young Cowles. W.C. is editor of a newspaper in Spokane, Wash. – a very fine man. An old college friend, a man of wealth. He had young C. in his office until he broke down some ten years ago with tuberculosis. He (the uncle) spoke of young C. in very high terms. This confirms my conviction that this is a very fortunate connection for us. As you see, there are no “strings” at all altho’ we would scarcely be fair to young C. if we did not at least give a chance to Nelson to see what he could do to relieve you or whoever is editor of routine work and so give a chance to young C. to busy himself over our magazine and keep in touch with the best econometric thought that is what he wants.’

The postscript was marked NY City, Oct. 20, 1931, which presumably means that Fisher had the Oct. 18 letter written in his office in New Haven and then took it with him to New York City to meet with William Cowles before he sent it off to Frisch.

Fisher didn’t really seem to have understood that although Frisch and Schumpeter were highly positive about accepting Cowles’ offer it had to be approved by the Council. The European Council members as elected at the foundation of the Society comprised, in addition to Frisch and Schumpeter, Luigi Amoroso, L. von Bortkiewicz, A. L. Bowley, François Divisia and Wl. Zawadski. Von Bortkiewicz had died in August 1931. The Society had an awkward decision structure as there were few opportunities to get the Council members together. Some of the European Council members had doubts of yielding so much influence over their venture to an American businessman. But no other alternative of financing a journal was anywhere within reach. The European Council members had wanted Frisch to go to USA at the end of 1931 to negotiate on their behalf with Cowles before any decision was taken. This was not possible for Frisch and such a meeting did not take place. In January

1932 the Council authorized the President and the American Council members to accept the offer of Alfred Cowles 3rd for financing a journal for the Society.[3]

The name "The Econometric Foundation" for the foundation and research institute Cowles had offered to establish was ruled out by the European Council members. The term 'econometric' was not a dictionary word yet and the Council members may well have considered it as a proprietary term belonging to the Society. Underlying the scepticism may well have been a feeling that "econometrics" was after all a European idea and a fear that the Cowles' offer implied an American take-over.

Cowles also had to establish his foundation for which he chose the name *The Cowles Commission for Research in Economics*. The formalities took a bit of time and the Commission was not formally chartered as not-for-profit corporation in Colorado until September 1932. Already early in 1932 Cowles had asked Fisher for Econometric Society to appoint five members for an Advisory Council for the Cowles Commission (instead of trustees as mentioned in letter written in Fisher's office). Fisher arranged for the Council to appoint A. L. Bowley, Irving Fisher, Ragnar Frisch, Carl Snyder, and W. C. Mitchell.

In the letter Cowles wrote in Fisher's office there was also a section on the editorship of *Econometrica*, reflecting the discussion that had taken place between Fisher, Roos and Cowles:

> '…the picture which has seemed to come out of the discussion is something like this. Professor Ragnar Frisch, if so disposed, could be editor-in-chief of the journal without salary and my present chief research-worker, William F. C. Nelson, would be assistant editor. There would be an editorial advisory board appointed by the Society which could help in reading manuscripts or assigning them to be read by experts in various lines, all under any plan prescribed by the editor-in-chief, who would have full power.' (Cowles to Fisher, Oct. 18, 1931.)

The statement is remarkable as Cowles had by then never met Frisch or been in touch with him. Cowles' assessment of Frisch as the first choice for editor-in-chief must entirely have been based upon what Fisher had told him. Fisher's appreciation of Frisch was certainly very high, particularly as the driving force without whom the Econometric Society could not have been founded. Frisch's reaction when he was informed about Cowles' letter was to propose Roos as editor, and later on Hotelling instead of himself. Hotelling would very likely have been a very good choice, Roos perhaps a more doubtful one.[4]

In February 1932, one month after accepting the Cowles offer, the Council of the Econometric Society elected Frisch as 'Editor-in-Chief with power to select his own editorial board and assistant editor'. At the same time the Council elected Alfred Cowles Treasurer of the Society and Circulation Manager of the journal and, furthermore, decided that the name of the journal of the Econometric Society should be *Econometrica*.[5]

Frisch was told by a letter from Fisher that he had been elected editor. By this decision Frisch and Cowles were tied together in a close relationship around the editing and publishing of the journal, even before they had met. As it turned out they got on quite well with each other in personal terms. But it was not only about the journal. Cowles and Frisch dealt with a number of matter related to the Econometric Society.

As Editor and Treasurer respectively, they were both members of the Council and involved in all activities of the Econometrics Society. And they would, as we shall see, become even more involved

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[3] See Report of the Work of the Council, Econometrica 1 (1), p.109. The assertion in Christ (1953) that Frisch visited USA prior to the January 1932 decision seems not be unfounded.
[4] Letter references to be added.
[5] See Report of the Work of the Council, Econometrica 1 (1), p.109.

with each other as Cowles also became Secretary of the Society while Frisch became research consultant to the Cowles Commission.

Cowles wanted, however, to get going as soon as possible. He wrote to Frisch in April, 1932 to check how soon they could meet in USA for discussions about the journal. Cowles had already made preliminary arrangements with a printing company in Wisconsin and wanted an issue published already in October, 1932. This issue would be distributed free of charge and to be used for soliciting subscriptions for forthcoming issues. Cowles hoped that Frisch could bring manuscripts to be printed in the first issue with him. They agreed that Frisch's visit could be combined with the Econometric Society meeting in Syracuse in June 1932. Frisch had already exerted considerably activity to put the first issue together, foremost by soliciting papers presented at the first European meeting.

The early contact by correspondence between Cowles and Frisch they tried to get to know each other. In April 1932 Cowles set out his view on the situation in economics:

> 'As I have gained familiarity with the ins and outs of Econometrics, it has become clear that there are several schools of thought represented therein. One classification which I have heard given is somewhat as follows:
>
> (1) Utility School (value economics); - Irving Fisher, Ragnar Frisch, A.L. Bowley, J.M. Keynes
>
> (2) Institutional Economics (theory of little if any value); - T.N. Carver, Holbrook Working, F. A. Pearson, G. F. Warren.
>
> (3) Theory may be of value if checked by statistics; - Wesley C. Mitchell, Mordecai Ezekiel, Frederick C. Mills.
>
> (4) Those who believe that value theory should be replaced with price economics, using hysteresis and other functional relations; - C. F. Roos, G. C. Evans, J. B. Canning, Joseph Mayer.
>
> (5) Those attempting to rework static theory of Pareto and Walras into dynamical one by means of trend ratios, link relatives and other statistical devices designed to introduce time implicitly into the equations of equilibrium; - H. L. Moore, Erich Schneider, Josef Schumpeter.
>
> There seems to be some doubt as to just where to place Harold Hotelling, Henry Schultz, and Wassily Leontief. I hope that I am not presumptuous in suggesting that you make a particular effort in the early issues of the journal, to see that the various schools of econometric thought receive representation. Thus it will be possible to keep everyone happy.'
> (Cowles to Frisch, April 28, 1932.)

Frisch may not have been too enthusiastic about the idea of 'keeping everyone happy', he had higher ambitions than that. In fact he was not very impressed with the conceptions of various schools of econometric thought that Cowles conveyed: 'Any mechanical division of a field of thought that is in a period of intense development like econometrics must of course be more or less superficial. And I think a critic might say this also of your classification. In particular I do not think it is quite possible to classify the men under these headings. For my on part, for instance, I feel that I would belong in all your classes except No. (2).' And he added a further reaction. 'It goes without saying that in the columns of "Econometrica" there will reign freedom of thought and any valuable contribution will be accepted regardless of which school it represents, provided it can contribute to the attainment of the goal put up by the Econometric Society, namely to advance economic theory in its relation to statistics and mathematics.'[6] This catchphrase, as lifted straight out of the Society's constitution, did not make it abundantly clear what the editorial policy should actually be. What was the *econometrics* to be promoted?

[6] Both quotes from Frisch to Cowles, May 19, 1932, Frisch's emphasis.

As we shall see this became a repeated issue of discussion and a recurrent theme in the editorial priorities. Also papers that hardly fulfilled anyone's definition of econometrics were published, not least in periods of drought in submitted papers. Cowles would sometime pull in the direction of a business viewpoint, naturally as he would cover the deficit while Frisch fought for econometrics.

Frisch left Oslo towards the end of May, 1932, arrived in New York on June 6, and then went to New Haven to visit Irving Fisher. He moved on to Colorado Springs where he spent about a week or so familiarizing himself with conditions in Cowles' laboratory and the staff there. Frisch had discussions with Cowles, Roos and William F. C. Nelson, he also met with Harold T. Davis. The topics of discussion in Colorado Springs were in addition to editorial matters, all sorts of practical matters related to Econometrica, and, not least, the research activity of the Cowles Commission.

The Council decision about editor stated clearly that Frisch as editor was free to choose assistant editor. This was perhaps also at the insistence of European Council members. Earlier in the spring Cowles had communicated with Frisch and made sure that he had no wish to find his own assistant editor. Frisch was very happy with the Cowles' proposal of letting Nelson serve as assistant editor.

Frisch had gone along with the idea of an early promotional issue. But later on the plan was revised for the first issue to be published in January 1933. Cowles instead produced a booklet to promote the journal. In the summer of 1932 Cowles had run out of copies and wanted to reprint but feel obliged to ask permission.

> 'This summer I have received over 200 letters asking for copies of the booklet and for information about the work of the Cowles Commission. I have had nothing to mail out in answer to these inquiries since discontinuing distribution of the original booklet. It seems to me very important to have a statement, couched in popular language, which can be sent out to those interested without risk of giving offense to any member of the Advisory Council. I should therefore very much appreciate hearing what your reaction is to the inclosed copy.' (Cowles to Frisch, July 27, 1932.)

The logistics for the new journal were demanding. Cowles had got solicited bids for printing Econometrica for several publishers and was inclined to contract the Banta Publishing Company located in Menasha, Wisconsin.

Frisch travelled to Syracuse in the company of Cowles and Roos to attend the meeting in the Econometric Society. They stopped in Chicago for a meeting with a representative for the Banta publishing company.

The Syracuse meeting was one of the very first Econometric Society meetings in USA and the first summer meeting. At Syracuse Frisch gave a paper titled "On the invariance of linear regressions" and was an active participant in the discussions. Among those presenting there were L. L. Thurstone who had joined the Econometric Society but resigned as a member shortly afterwards giving as reason that the membership fee was too high.[7]

In Colorado Springs the discussions about the research program for Cowles Commission had not lead to very definite conclusions. Frisch naturally put forward some of his ideas. William Nelson, who like Frisch had no direct experience in editorial work, felt the responsibility not only of being appointed assistant editor but as Cowles' 'chief research-worker also for the research program. In a letter he sent Frisch immediately after he had left Colorado Springs (and addressed to Frisch's hotel in Syracuse) he vented his uncertainty about Cowles Commission selecting and conducting a research program. The research at the Cowles Commission ought to result in something of real value and 'merit the approval of the leaders in our particular line of investigation'. Nelson clearly saw the need for outside assistance:

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[7] The advisory council for Cowles Commission apart from Bowley met for the first time in Syracuse and incidentally approved Cowles stock prices project.

> 'Two factors would seem to fortify the conclusion that such consultation would be wise. The Commission has an economist [i.e. Nelson] who is not a mathematician, and a mathematician [i.e. Forest Danson] who is not an economist. The union of the two does not necessarily make an econometrist. With the handicap imposed by such a rigid division of function, it would seem obvious that from time to time recourse should be had for advice to those who are both mathematicians and economists.
>
> The Commission is isolated from the centres of activity in our line. As you remarked, sustained personal contacts bring much of value in the way of assimilating subtle attitudes and opinions which never reach print. The most diligent reading of periodicals is an inadequate substitute. In these circumstances, again, some effective communication with one who makes these contacts would appear valuable, if not imperative.
>
> The simple prudence that invites anyone with a problem to consult an expert, the peculiar constitution of our Commission, and its rather isolated position, all seem to point to the necessity of frequent consultations with, and a measure of guidance from, one who himself is experienced in our problems, is at once economist and mathematician, and makes constant contact with the leadership of our field.
>
> Logically our [Advisory] Council should so function. Some community of interest in certain problems, personal acquaintance, and may I say, a demonstration of interest on your part in our work, would seem to indicate that, of the Council, your are uniquely fitted to meet our needs. This, you understand, is a purely personal opinion.'
> (Nelson to Frisch, June 18, 1932)

The outcome of this first contact between Frisch and the Cowles Commission was that Frisch in the absence of a research director was offered and accepted to become non-resident Research Consultant of the Cowles Commission, the only one in such position. The arrangement lasted until Roos became Research Director in the autumn of 1934 but was renewed when Roos left in 1937.

During Frisch's visit the question of providing textbooks for students of econometrics was discussed. The idea is likely to have come from Frisch. Cowles embraced it and it quickly developed into a project of two volumes to be written with joint authorship of the gifted but also somewhat eccentric Professor at Colorado College H.T. Davis and Cowles' right-hand man W.F.C. Nelson, and with the common title *Introduction to the Statistical Theory of Econometrics*. Cowles got cold feet about the ambitions relative to ability and wrote to Frisch a few weeks after he had left:

> I have been thinking a good deal about our projected manuscript for the book reviewing the field of econometrics up to date. It seems to me that it would be presumptuous for us to undertake a very ambitious critical appraisal which might be interpreted as telling men of much longer experience in this field what was the true significance of their contributions. On the other hand, I believe we are ideally qualified to undertake something in the nature of popularizing econometrics. Davis, Nelson and I have all had considerable experience in this kind of thing. I believe that when our manuscript is prepared it will probably be an attempt to tell the man who is new in the field of econometrics what it is all about, what has been accomplished heretofore, and in general what useful tools he will find available in modern mathematics for the analysis of economic problems. (Cowles to Frisch, July 1, 1932)

The first volume was thus retitled to be *Elements of Statistics with application to economic data*. It was initiated immediately and eventually published in 1935 (Davis and Nelson 1935). The second volume was to be titled *Introduction to the Statistical Theory of Econometrics*. But some disagreement over the planned content of the second volume arose between Davis on the one side and Cowles and Nelson on the other. The latter two wrote a memo to Davis in 1933 questioning Davis' plan for the second volume, referring to a comment by Frisch 'that Volume I contained no

econometrics, Volume II a little, but not much more.'[8] Key formulations of the memo were incorporated in the introductory chapter of volume I when it appeared in 1935:

> In the econometric program, statistics has already played, and must continue to play, a large part. It is easily possible to envisage a "Statistical Theory of Econometrics." Such a study would be an exposition of economic theory in the measure that this theory has been subjected to statistical treatment. … Such a "Statistical Theory of Econometrics" would not be merely economic statistics, and certainly not statistical methodology, but an exposition of economic theory as that theory had been developed, confirmed, challenged, or stultified, by statistical evidence or processes. … It does not require a violent effort of the imagination to see how statistical methods are indispensable tools in the more precise measurement and adequate interpretation of economic facts, and thus vitally forward the development of economic truth. Statistical technique bears something of the relation to econometrics that a machine tool does to an automobile. The second cannot be produced without the first. But the first derives its whole meaning from the second. It is possible to conceive of a mechanic becoming so entranced with the beautiful and involved precision of his tool that he loved to manipulate it for its own sake, rather than to make automobiles. Statisticians are not immune from such temptation. It is important, therefore, while acquiring a mastery of statistical methodology, that one should always have his mind on his ultimate goal. Thus he can see the progress and direction of his efforts and probably arrive at his destination more easily and more quickly , because he knows where and why he is traveling. (Davis and Nelson 1935, 6-9).

Exactly what happened in the continuation is unknown. Nelson died in 1936 and a second volume was never published by the Cowles Commission. Instead Davis published on his own some years later a monograph titled *Theory of Econometrics* (Davis 1941).

We pause to reflect on the multifaceted relationship between Frisch and Cowles. They were both members of the Council, the decisive body of the Econometric Society. Cowles had been elected Treasurer and thus ex officio member of the Council while Frisch had been elected to the Council at the Organization meeting. Cowles' commitment to support Econometrica financially was not in the form of an annual ex ante grant but rather that Cowles managed the Society's accounts and covered whatever deficit became the outcome. The editor thus did not have a given budget but had to negotiate all proposals for increased expenses on a current basis with Cowles. Frisch had as editor-in-chief an honorarium of $500 a year. He had an additional honorarium, as research consultant.

In the start-up and the first few years of Econometrica the connection was very close between Frisch and Cowles both about the journal and about Econometrica Society matters such as members. But gradually the contact with Cowles faded out while it grew closer between Frisch and his editorial assistant, to begin with it was assistant editor W.F.C Nelson, but from the beginning of 1937 it was managing editor Dickson H. Leavens who lasted 10+ years in that position.

Frisch also took a great interest in most of the technical issues related to *Econometrica*. the design, format and typeface, layout, etc. He even tried out his own ideas in Oslo:

> 'I have just experience with the title of "Econometrica" and I think I have just found a solution which is entirely satisfactory. It gives a very refined and intellectual appearance and I am sure you will like it. This was obtained by limiting the material of the title page to just the name of the journal, the editorial staff and the advisory editorial board, reserving the table of contents for the back page of the front cover. The information regarding the Society will then be on the first and second page of the back cover.' (Frisch to Cowles, July 12, 1932).

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[8] "Memorandum for Davis from Cowles and Nelson", March 31, 1933, typewritten, 2 pp.

The Council decision of electing Frisch Editor left it to him to decide on who would be assistant editor. Frisch was indeed most happy to accepted Cowles' suggestion of letting W.F.C. Nelson be assistant editor in Colorado Springs. Frisch had liked Nelson from the very beginning and vice versa. It was a generous offer as Nelson was employed by Cowles and his salary as assistant editor was not charged to the Econometric Society.

3. Starting up

Econometrica thus started out with the Editor in Oslo and the business office with an assistant editor in Colorado Springs. This was by itself a logistic challenge exacerbated by the Banta printing office being located in Menasha, Wisconsin. The time schedule would be demanding as the MSS and proofs had to pass across the Atlantic. Frisch took a great interest in all the technical details of the journal he had been put in charge of, format, type face, the editing of the front and back inside and outside cover, etc. He even had special types prepared in Oslo for the printing of the journal title on the front cover.

As soon as he had been elected Editor in February 1932 and thus before he went to USA to meet Cowles and attend the Syracuse meeting Frisch began to plan the journal and the editorial work. He had even before that gathered manuscripts from the Lausanne meeting. In April 1932 he considered the selection of three Associate Editors. He thought of them as covering economics, statistics and mathematics, respectively. With the Editor in Europe practical reasons suggested that at least two of the Associate Editors ought to reside in Europe.

Frisch first asked his compatriot Oystein Ore, professor of mathematics at Yale, to become Associate Editor for mathematics. He outlined to Ore that the main duty was to write an annual survey on the development in mathematical statistics. This was in fact a bit slightly odd as Ore's mathematical specialization went in other directions than the applications called for in econometrics. Ore declined, however, but accepted to be on the Advisory Editorial Board (AEB).[9]

Frisch then checked the willingness of Henry Schultz of the University of Chicago to become Associate Editor: 'Would you be interested, in case you are asked, to accept the position of Associate Editor? It will be necessary to make an arrangement with two or three associate editors residing in the United States, who could read the final proofs, cooperate with certain sections of the bibliography and on other ways share the duties of the Editor.' It was perhaps not the most attractive description Frisch could have given of what the job involved. Schultz declined: 'It is kind of you to think of me in connection with the assistant editorship of *Oekonometrika*. Unfortunately, I cannot accept the honor even if it is offered to me, for the reason that I am already an assistant editor of two journals, and for the further reason that I expect to be out of residence during 1933-34.' Schultz was indeed in Europe for a full year. He added, however, that 'when three or four years have passed, and I have had a chance to complete several projects on which I am at present engaged, I may be able to join you in this work.'[10]

Frisch's third choice was Frederick C. Mills. He was less confident about Mills than about Ore and Schultz and decided to consult with Irving Fisher and others before he asked Mills. Mills passed the scrutiny and Frisch found replacements for Ore and Schultz. Hence the Associate Editors eventually approved by the Council on Frisch's recommendations were Alvin H. Hansen (economics), Frederick C. Mills (statistics) and Harold T. Davis (mathematics).[11]

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[9] Frisch to Ore, April 4, and May 25, 1932. Frisch's remark to Ore about surveys indicates that Frisch's plan for surveys had already been worked out, see below.

[10] Frisch to Schultz, May, 3, 1932; Schultz to Frisch, May 24, 1932.

[11] Alvin Hansen served until 1938 and was succeeded by Joseph Schumpeter, who in turn was replaced by John Hicks in 1950. Frederick C. Mills was replaced by Tinbergen already from June 1934. Hicks, Tinbergen and Davis continued as associate editors when the number of associate editors in 1951 was increased to eight.

The editor was authorized to appoint members of an Advisory Editorial Board (AEB). Frisch chose for the first AEB to include all the Council members apart from Cowles in the AEB and in addition appointed additional members among people he knew and presumed to be supportive both as contributors, referees and advisors. The AEB thus comprised at the outset Eugen Altschul (Frankfurt), Luigi Amoroso (Rome), A. L. Bowley (London), G. Darmois (Nancy), Gustavo Del Vecchio (Bologna), François Divisia (Paris), Irving Fisher (New Haven), Maurice Fréchet (Paris), Harold Hotelling (New York), J. M. Keynes (Cambridge), Wesley C. Mitchell (New York), Oystein Ore (New Haven), Charles Rist (Versailles), Charles F. Roos (Washington), Henry Schultz (Chicago), Joseph Schumpeter (Cambridge, Mass.), E. B. Wilson (Boston), WI. Zawadzki (Warsaw), and Professor F. Zeuthen (Copenhagen).

Authors interested in submitting papers for publication in Econometrica were instructed to send manuscripts to the business office of Econometrica in Colorado Springs if they resided in the United States or Canada, and directly to the editor in Oslo from all other countries. The US office would the typically conduct the refereeing of papers submitted to it and then send the papers and referee reports, the latter often just summarized in a letter from the assistant/managing editor, to Frisch in Oslo. For manuscripts submitted directly to Frisch he would conduct whatever referee process he would find warranted. There were exceptions to this procedure, e.g. manuscripts passed on to Frisch without being refereed in the US for one reason or the other.

In these first years many of the papers had been presented at meetings and the authors invited by a representative of Econometrica, such as one of the editors or members of the advisory editorial board to submit a paper. It could even be submitted by one of these persons with a recommendation. What might happen was that the recommendation was considered as a referee report and the paper was sent on to Oslo. The treatment might also depend upon who the author. Gradually, the refereeing process became more streamlined, and as we shall see more and more papers passed through the business office first.

The editor-in-chief was supposed to approve every single article. This did, however, not always happen. There were also a number of cases when time just did not allow the sending of final version of the manuscript across the Atlantic. Usually, this was directly or indirectly due to Frisch's inability to adhere to the very strict time schedules drawn up. Frisch often overbooked his time and teaching, travel and, not least, his own research prevented him for doing editorial work on a daily basis. It was quite typical that he cleared time for himself in days on end to go through stacks of manuscripts and taking editorial decisions. At times it could bring Frisch's counterpart at the other end of the editorial channel almost to desperation and the problem occurred already before the first issue had come out.

> 'Distasteful as it is, I must plead that you adhere to the schedule I sent you. That schedule means that 22 weeks is the minimum time that must elapse from the moment you send copy to me to the moment the article appear in Econometrica. It means that Europeans must dispatch MSS to you 23 weeks, American 25 weeks (almost six months) before they expect to see their articles in the magazine. Only in this way can the material go through its proper routine. The July material should be leaving author's hands in two weeks. At the beginning this may be only an ideal but only by attaining this ideal can the things go smoothly and properly.' (Nelson to Frisch, Dec. 13, 1932).

There were a small group of devoted econometricians who the Editor could rely on to shoulder the responsibility when there e.g. was need to step in to write take over one of the surveys, write a meeting reports, review a submitted paper, and still contribute good papers on top of that. Tinbergen, Marschak and Staehle were in this group, and also others but Frisch may have been disappointed how difficult it sometimes seemed to mobilize the necessary effort to keep things going in the early years.

The first paper submitted to Econometrica was by Henry Schultz, just as Frisch was formally elected Editor. Schultz wrote to Frisch already in February 1932 that he would like to submit a paper titled *A*

Comparison of the Elasticities of Demand for Selected Commodities Obtained by Different Methods, which had been presented at the a joint session of the Econometric Society and ASA in Washington DC, December 1931. Irving Fisher had suggested to Schultz to submit the paper.

When Schultz submitted the paper in March 1932, the name of the journal had not yet been decided and Schultz just called it the *Journal of the Econometric Society*. Frisch on the other hand accepted the paper preliminary on behalf of *Oekonometrika*. Schultz added in the letter that he would appreciate an early report, but Frisch did not respond, however, until October 1932. Frisch's letter may well be regarded as the first referee report in *Econometrica*. It went as follows:

> 'I am writing you today in regard to your MS on "A Comparison of the Elasticities of Demand for Selected Commodities obtained by Different Methods". … I have now read your MS thoroughly and I have taken the liberty to condense it on some points. Such condensation is necessary because we find ourselves very cramped for room in "Econometrica" in these first issues. I believe, however, that the abbreviations that I have made do not modify your arguments on any essential points. I have amongst others, omitted your discussion of the three dimensional diagrams. The discussion of these is, I believe, contained in your German book on the subject. All the essential points in your results are contained in the equation which I have, of course, left in the MS. I have also suggested one single heading for the last three sections.
>
> With regard to the Table of Results, I would suggest a modified form which you will find enclosed. … I would very strongly suggest that all correlation coefficients be omitted from this table. The computation of correlation coefficients in a case like this does not throw much new light on the subject, it seems to me. They are rather dangerous because they may convey to the reader the idea of an exactness which is not contained in the material. The standard errors of the elasticities should, of course, be included.
>
> In the beginning, where you give references to other works on elasticities, I would suggest that you include also a reference to Lehfeldt, in the *Economic Journal* of 1914. I would also suggest that you include a table giving the raw data you have used. I suggest that this table would not be too excessive in volume. I think the publishing of the raw data is very important in order to stimulate criticism and control.
>
> I would suggest that your appendix regarding the discussion with Amoroso be published in some other Journal, for instance, the *Zeitschrift für Nationalökonomie*. While in Paris I talked over with the editor of the *Zeitschrift*, Mr. Rosenstein-Rodan, the possibility of the *Zeitschrift* and *Econometrica* co-operating on the publication of papers that are of interest to both journals. According to my agreement with Rosenstein-Rodan I am, therefore, sending this appendix to him. Of course, this does not involve any decision whatsoever. In case you do not care to have this topic published in the *Zeitschrift* you need not do so. You will probably hear from Mr. Rosenstein-Rodan directly. The other appendix, namely the one on "Standard Errors", I would suggest you present to the *Journal of the American Statistical Association*. (…)
>
> Will you please go through the suggestions I have made in the MS and make up your mind if you want to accept these modifications? In case you do, please forward the MS to the Assistant Editor, Mr. William F.C. Nelson, Mining Exchange Building , Colorado Springs. Possible there are parts of the MS that need to be re-typed: could you take care of this? Please also go through the MS carefully and check the numbers of the formulae. I believe that the formulae should be numbered either consecutively or, if you want to use a formula number with a comma, please let the first figure of the formula refer to the section number.' (Frisch to Schultz, October 29, 1932.)

Frisch's referee statements had several characteristics features, like his dislike of correlation coefficients and his suggestion that the 'raw data' ought to be included. Furthermore, the specific suggestions about which other journals ought to be approached about publishing Schultz' two

appendices and the action Frisch had already taken vis-à-vis Rosenstein-Rodan, may surprise but this was not unusual.

Schultz responded enthusiastically but not without reservations:

> 'With one or two exceptions, to which I shall refer later I cheerfully accept all of the modifications which you recommend. In fact, I would have made similar modification myself, had I known that the paper would be submitted to *Econometrika*. …
>
> The suggestion which, after due deliberation, I find impossible to accept, is that I omit all coefficients of correlation and all references to them. You will please recall that I used these correlations simply as measures of the goodness of fit of the various formulas. From this point of view, I see no objection to them. In fact, I find them quite instructive. But that is not the main reason for my seeming obstinacy. Some of my coefficients of elasticity have been criticized as being of little value for the reason that they have been derived from equations which, as judged by the coefficient of correlation (simple or multiple) do not give a very good fit to the data. The implication was that had I taken sufficient pains to develop a formula which describes the data with the highest degree of probability—whatever that may mean—my elasticities of demand would have been entirely different. This paper is in part a reply to the criticism. My assistants and I have devoted lots of time and energy to the presentation of it. To omit the coefficient is, therefore, to destroy one of the main objectives of this paper. I trust, therefore, that you will see the justice of my position. It would be a comparatively easy matter to add a table of coefficients of correlation comparable to the table of elasticities which you were kind enough to suggest.
>
> I also think that the omission of the three-dimensional graph which compares the intersection of the logarithmic with the arithmetic plane, as well as the three-dimensional graphs which illustrate the shifting obtained by the link relative method and the trend ratio method, respectively, would destroy much of the utility of the paper (I am not questioning the advisability of omitting all the other graphs). There is nothing in the formulas, for example, which would give any idea of the way in which the demand surfaces intersect. Furthermore, this graph does not appear in any of my published works. With respect to the link relative and trend ratio surfaces, I wish to say that they summarize something which cannot conveniently be grasped from the formulas. I shall be glad to include a table giving the raw data that I used, for I agree with you that the "publishing of raw data is very important in order to stimulate criticism and control." You may be interested to know, however, that I intend ultimately to publish this study, together with other studies of mine, in book form, in which I will, of course, include all the raw data.
>
> The appendix of Amoroso which you sent to the *Zeitschrift für Nationalökonomie* will have to be somewhat rewritten, in view of the fact that it is to be divorced from the tables in which the standard errors of the elasticities are given. But this, I suppose, will not be a serious task.
>
> Thank you heartily for calling my attention to the failure to refer to Lehfeldt. This was wholly unintentional. As you know, I have a discussion of Lehfeldt's method in my book on sugar. Now that I think of it, I have forgotten to refer to one or two other economists who have done pioneering work in the field of statistical demand curves. (Schultz to Frisch, November 26, 1932.)

Schultz' paper was eventually published in *Econometrica* 1(3). As it was the first paper submitted it ought perhaps to have been published earlier but Frisch in fact forgot about it.

The Econometric Society was the first international association in economics and Econometrica the first international journal in economics. Although the foundation of the Society had taken place in USA and the journal was published in USA as well both the Society and the journal had been conceived in Europe. A major task for the Society was to bring together those who joined the

Econometric Society to become an international community of people who shared an interest in the econometric program. The prime means for promoting this task was the meetings. The pattern of meetings was established already from 1931. A European meeting was held every year in September. The American varied a little and continued also after 1939 in a less regular pattern than in Europe.

The meetings were announced in Econometrica (except for 1931 and 1932 meetings) usually with the name of the person organizing the meeting. The later system with submitted proposals and a program committee supervising submissions came much later.

European Meetings of the Econometric Society, 1931-39

ESEM-1 Lausanne, Sept. 22-24, 1931 (Hans Staehle, Jan. 1931)

ESEM-2 Paris, Oct. 1-4, 1932 (Georges Lutfalla, Revue d'Economie Politique, April 1933)

ESEM-3 Leyden, Sept.30–Oct.2 (Jakob Marschak, April 1934)

ESEM-4 Stresa, Sept. 25, 1934 (No report)

ESEM-5 Namur, Sept. 23-26, 1935 (Hans Staehle, Jan. 1937)

ESEM-6 Oxford , Sept. 25-29, 1936 (E.H. Phelps Brown, Oct. 1937)

ESEM-7 Annecy, Sept. 12-15, 1937 (Georges Lutfalla, Jan. 1938)

ESEM-8 Cracow, Sept. 18, 1938 (B. Debiński & J. Wiśniewski, April 1939)

ESEM-9 Elsinore, August 25-26, 1939 (Pieter de Wolff, July 1940)

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Place, name, author of report and Econometrica issue published. Dates for each meeting are according to the report and sometimes differ from dates announced. ESEM-8 was announced to last for four days but contracted to only one day. The meeting took place during the Sudeten crisis. ESEM-9, planned to take place only days before the outbreak of WWII was announced to last for three days but attracted few participants and was shortened to two days. No information about ESEM-4 appeared in Econometrica after the announcement of the meeting. A report by G. Lutfalla was, however published in Revue d'Economie Politique in 1935.

At ESEM-2 in Paris "The Econometrica Committee on Source Material for Quantitative Production Studies" was formed on RF's initiative. E.H Phelps Brown was elected chairman, the members comprised J.Tinbergen, Hans Staehle, J. Marschak, Erich Schneider, Hans Bolza, G. Lutfalla, Frederik Zeuthen, and Louis Bean. A note on scope for the committee was written by Lutfalla.

American meetings of the Econometric Society, 1931-39

1931 Washington, Dec. 28-29, 1931 (Irving Fisher, Jan. 1933)
New Orleans, Jan. 1, 1931 (Irving Fisher, Jan. 1933)

1932 Syracuse, June 20-22, 1932 (Joseph Mayer, Jan. 1933)
Atlantic City, Dec. 26-28, 1932 (Charles F. Roos, April 1933)
Cincinnati, Dec. 29-31, 1932 (Alfred Cowles 3rd, April 1933)

1933 Chicago, June 28-30, 1933 (Alfred Cowles 3rd, Oct. 1933)
Philadelphia, Dec. 27-29, 1933 (Alfred Cowles 3rd, April 1934)
Boston, Dec. 30, 1933 (Alfred Cowles 3rd, April 1934)

1934 Berkeley, June 20-22, 1934 (John B. Canning, Oct. 1934)
Chicago, Dec. 27-28, 1934 (Charles F. Roos, July 1935)
Pittsburgh, Dec. 28-29, 1934 (Charles F. Roos, July 1935)

1935 Colorado Springs, June 22-24, 1935 (Harold T. Davis, Oct. 1935)
New York, Dec. 30-31, 1935 New York (Herbert E. Jones, April 1936)
St. Louis, Jan. 2-3, 1936 (Herbert E. Jones, April 1936)

1936 No summer meeting
Chicago, Dec. 28-30, 1936 (Dickson H. Leavens, April 1937)

1937 Denver, June 24-26, 1937 (Dickson H. Leavens, Oct. 1937)
Atlantic City, Dec. 27-29, 1937 (Dickson H. Leavens, April 1938)
Indianapolis, Dec. 30, 1937 (Dickson H. Leavens, April 1938)

1938 No summer meeting
Detroit, Dec. 27-30, 1937 (Dickson H. Leavens, April 1938)

1939 No summer meeting
Philadelphia, Dec. 27-29, 1939 (Dickson H. Leavens, April 1940)

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Place, name, author of report and Econometrica issue published. Dates for each meeting are according to the report and sometimes differ from dates announced.

In addition to the report Econometrica would for each meeting have an announcement, sometimes twice. For some of the European meetings there were also separate entries with attendance at the meeting and the program. Frisch was much in favour of publishing these items. The announcement of the program was of interest also when published after the meeting.

The European and the American meetings were held in very different settings. The European meetings were singular events with participants from several European countries and often with some American participants, usually scholars who visited in Europe for a period. The American meetings of the Econometric Society were held as sessions at major scientific gatherings of several associations, particularly the American Economic Association, the American Statistical Association, the American Mathematical Society, and the A.A.A.S. There were usually three American meetings each year, twin meetings in two different cities at the end of December or in December/January and a summer meeting in June.

The annual European meetings were different from the American meetings, as Frisch explained to Nelson:

> '[F]or the European meetings … those attending came – many of them from long distances – with the only object of taking part in the Econometric Society meeting. The European meetings, as you know, are organised exclusively as an Econometric Society undertaking. The audience is not "inflated" by arranging joint meetings with other Societies and thereby obtaining a big audience of people who are only to a small extent, interested in econometrics proper.' (Frisch to Nelson, Oct. 11, 1933.)

The American meetings were more formal in every way, also meticulously prepared from a formal point of view while the European meetings had a more improvised character:

> 'In the United States the meetings are generally organized very rigorously at least sometime before the meeting is being held. Changes in the programme when this once has been settled occur very seldom. In Europe, on the contrary, everything is more or less floating up until the last moment, and therefore the programme appears to be more of a report of what actually took place than an indication useful for those who plan to attend the meeting. I therefore think it is worthwhile to have the programme appear as an official statement of what has happened. Maybe we should not call it "programme" if it is printed after the event has taken place. Perhaps we might use such a word as "schedule" or "account" or some other better word that you could find.' (Frisch to Nelson, Jan. 4, 1934.)

There were entries in Econometrica announcing the meetings. Sometime after the meeting followed usually the report. Frisch insisted on having this programme published in Econometrica although the meeting had already taken place:

> 'A special reason why I think it is desirable to have this schedule appear is that the indication of the list of chairmen gives a very imposing impression of the international character of the Society. If you will look through the list you will see that there are chairmen from six or eight countries and from three continents and the statistics of the speakers as indicated by the schedule is quite interesting. Such information as this will of course not appear at all in [the] report, or if it does, not in this striking manner. I therefore think it would be definitely worthwhile to print the schedule. If it is desirable to save space, it could be done in small type, but even if set up in ordinary type it should not take up more than a couple of pages.' (Frisch to Nelson, Jan. 4, 1934.)

Frisch also wanted to display the complete list of attendants at the meeting but here the difference between the meetings on the two side of the Atlantic created a problem:

> 'I wonder whether it would not be a good idea to print at the end of the survey the names of those attending. At least I think this would be a good plan for the European meetings. … On the other hand, it would seem desirable to use the same manner of presentation both for the European and the American reports, which would mean that the list of those present should be published also for the American meetings. Would this be practicable? And could something be done to obtain a list restricted to those who really came only to attend the Econometric Society meetings, and not only dropped in more or less incidentally as belonging to other Societies. One solution would perhaps be to split the list in two: members and visitors at that particular meeting, the latter part of the list consisting of those who attended the meetings without, however, being as yet inscribed as regular members of the Econometric Society. The practice might then be followed of including for Europe all those who came to the meeting without as yet being members. They would only be very few in number. At the Leyden meeting, for instance, perhaps four or five would come in this category, and the total number of names would be between 30 and 35. On the contrary in the States one may follow the practice of listing only the members and of adding visitors, only in the exceptional cases when somebody had showed particular enthusiasm in the matters of the Society. What is your reaction to this?' (Frisch to Nelson, Oct. 11, 1933.

Econometrica as the journal of the Econometric Society put much emphasis and space on information about these meetings. Frisch certainly found this as very important. Ideally he would like to have not only the announcement of the meeting and the report after the meeting, but also the program as soon as it was prepared, usually just before the meeting. The reports were highly important as a means of communication of what was going on in econometrics. The reports differed in content, style and length.

The reports varied in form and quality. For some meetings reports were never published. The best reports from meetings provided much information of historical interest. Some of the reports from the European meetings convey a live and vivid impression of the atmosphere and discussion at the meetings.

Meeting reports was not the only way of distributing information about on-going work. A major project was the Econometric Catalogue, providing abstracts of econometric work, or, more precisely, work by members of the Econometric Society, which was not quite the same thing.

There was also other membership information in Econometrica, such as an updated membership list with complete addresses. On some occasions it was printed as part of one issue, while more often it was printed separately and enclosed with the latest issue. The Editor was strongly in favour of distributing the membership list with *Econometrica*. As he explained to Nelson:

> 'I think the membership list should be published in Econometrica once in a while. The first one may go into the April or July number. The list we have now is quite imposing and ought to be known by all members. It may also be an idea to preserve this list for historical purposes' (Frisch to Nelson, February 12, 1934).

One of the first editorial decisions Frisch made was to publish Eugen Slutsky's paper *The Summation of Random Causes as the Source of Cyclic Processes* (Slutsky 1927). It was a paper studied thoroughly by many outside Russia shortly after it was published as it had with a 5-page English summary. It had meant much for Frisch personally as it inspired his propagation-impulse explanation of business cycles. Henry Schultz had in fact commissioned and received a translation of Slutsky's work. At Frisch's request Schultz sent him the translation in March 1932. The Slutsky paper was listed already in the first issue of Econometrica among 15-20 'papers to appear in Econometrica,' but it did not appear until 1937. The 1937 paper was however a revision and enhanced version of the original paper. For the circumstances surrounding the publication of Slutsky's paper, see section 5 below.

Where would the articles submitted to Econometrica come from before it had become sufficiently widely known and built a reputation to ensure a steady flow of manuscripts? Naturally, from econometricians, i.e. members of the Econometric Society, and particularly from papers presented at meetings. At each meeting several of the editors and members of the Advisory Editorial Board would be present and keep on outlook for good material. Sometimes the presenter would be approached at the meeting and invited to submit, in other cases it promising presentations could be mentioned to the editor or assistant editor if they were not present, sometimes in a brief report.

Well established econometricians often continued to publish in the journals they had used earlier rather than submitting to *Econometrica*. What kind of articles should particularly be encouraged and promoted in Econometrica? What were the distinguishing features relative to other journals to be pursued. The topic came up again and again in discussion within the editorial circle.

In the preparation of the first issue of Econometrica Frisch had tried to get hold of all the papers presented at the Lausanne meeting. During the preparation Frisch took part in the Syracuse meeting in June 1932 and three months later the Paris meeting in September 1932. The associate editors and members of the AEB had scrutinized the presentations at the winter meetings in Washington and New Orleans. Frisch considered also publication of papers by himself.

For the Jan. 1933 issue Frisch wrote his often quoted *Editorial*. It was drafted shortly after Frisch had arrived back to Oslo in July 1932. Frisch seems very much aware that the editorial would be read and scrutinized by many. Thus he drafted it in ample time and asked for and comments and corrections from Irving Fisher, François Divisia and Joseph Schumpeter and also from the Cowles Commission. He asked Nelson explicit to discuss the matter with both Cowles and Davis. The comments that came back from Colorado Springs were mainly in the way of idiomatic English.

He had got Schumpeter to write the also often quoted article *The Common Sense of Econometrics*. Much to Frisch's disappointment this became the only Schumpeter article in Econometrica (apart from a memorial paper on Irving Fisher in 1948).

A very visible feature of the early volumes of *Econometrica* was the attention given to selected great names in the history of economics. Frisch called them econometric pioneers. There were over the first 5-6 years articles about A.A. Cournot, Knut Wicksell, Johann von Thünen, F.Y. Edgeworth, W.S. Jevons, Léon Walras and Vilfredo Pareto, plus additional articles, letters, photographs and other historical material shedding light on the life and works of the econometric pioneers.

Frisch sent a big folder with material for the first issue and another folder with additional mss to Nelson on August 22, 1932. At this time he was ahead in his editorial work, while at other times most behind.

During the deliberations in Colorado Springs in 1932 the idea of survey articles in Econometrica had been discussed. As noted above Frisch had started to think about these surveys already in March

1932. On the eve of his departure from USA Frisch decided on a plan for surveys and set it out in a letter to Cowles. Frisch wanted four surveys a year, i.e. one survey in every issue. The surveys over one year would cover (1) General Economic Theory, (2) Business Cycle Theory, (3) Statistical Techniques and (4) Statistical Information, and Frisch wanted them to appear in this order every year. This was in fact stated in the Editorial.

Frisch had at the outset intended to have one responsible author over several years for each of these surveys. The responsible author could seek the cooperation of specialists as he sees fit. As Frisch expressed it the 'the surveys should endeavor too hit the high spots, being selective and not all-inclusive' (Frisch to Cowles, June 25, 1932). Irving Fisher with whom Frisch has discussed the idea had, however, advised not to have the same author for successive surveys. In fact Fisher's advice was most appropriate. None of those solicited for and accepting surveys seemed to like the idea of writing successive surveys.

The survey idea was obvious an important one for Frisch with regard to what Econometrica should achieve. The survey should provide high-level education in econometrics, comprising updated theoretical insights, understanding of new statistical methods and knowledge about data sources.

Frisch decided to go ahead with surveys in the middle of 1932. The very first issue, Jan. 1933, was already filled up by then. Hence, the first survey appeared in the April 1933. The survey articles were usually placed prominently in the issues, often as the front page article. In the entry "papers to appear in Econometrica" forthcoming survey articles were placed in a section for themselves. The survey articles were solicited solely for publication in Econometrica and must therefore be considered as an important part of the promotion of econometrics through Econometrica.

Econometrica Surveys

1933	Jan.	No survey
	April	Alvin H. Hansen & Herbert Tout: Annual Survey of Business Cycle Theory: Investment and Saving in Business Cycle Theory
	July	W. E. Shewhart: Annual Survey of Statistical Technique: Developments in Sampling Theory
	Oct.	Jakob Marschak: Annual Survey of Statistical Information: The Branches of National Spending
1934	Jan.	J. Tinbergen: Annual Survey of Significant Developments in General Economic Theory
	April	Felice Vinci: Significant Developments in Business Cycle Theory
	July	G. Darmois: Développements récents de la technique statistique
	Oct.	Hans Staehle: Annual Survey of Statistical Information: Family Budgets
1935	Jan.	J. R. Hicks: Annual Survey of Economic Theory: The Theory of Monopoly
	April	No survey
	July	J. Tinbergen: Annual Survey: Suggestions on Quantitative Business Cycle Theory
	Oct.	Charles F. Roos: Annual Survey of Statistical Information: Capital Formation and the Flow of National Income in the United States Paul Lorenz: Annual Survey of Statistical Technique: Trends and Seasonal Variations

1936	Jan.	Ragnar Frisch: Annual Survey of General Economic Theory: The Problem of Index Numbers
	April	Johan Åkerman: Annual Survey of Economic Theory: The Setting of the Central Problem
	July	Paul R. Rider: Annual Survey of Statistical Technique: Developments in the Analysis of Multivariate Data I
	Oct.	Charles F. Roos: Annual Survey of Statistical Technique: Developments in the Analysis of Multivariate Data II
1937	Jan.	No survey
	April	No survey
	July	Nicholas Kaldor: Annual Survey of Economic Theory: The Recent Controversy on the Theory of Capital Horst Mendershausen: Annual Survey of Statistical Technique: Methods of Computing and Eliminating Changing Seasonal Fluctuations
	Oct.	No survey
1938	Jan.	No survey
	April	No survey
	July	Gabriel A. D. Preinreich: Annual Survey of Economic Theory: The Theory of Depreciation
	Oct.	No survey
1939	Jan.	No survey
	April	Costatino Bresciani-Turroni: Annual Survey of Statistical Data: Pareto's Law and the Index of Inequality of Incomes
	July	No survey
	Oct.	No survey

But as the table list reveals only the first two volumes had surveys strictly according to Frisch's plan. In 1935-36 the schedule became a little itrregular and in 1937-39 the survey plan sadly fell apart. Frisch cheated a little on one or two occasions by publishing as survey paper a submitted as regular articles. This was the case e.g. for his index number article in 1936. He had in fact worked hard to solicit surveys but it too often happened that his econometric colleagues rescinded their promises. The editorial correspondence has the sordid details of how and why the surveys dwindled.

A further idea that Frisch pursued was to have articles in the early volumes related to the 'econometric pioneers': portrayals, discussion of their work and various kinds of additional historical material. It was of course not only Frisch who held this view. Several key members shared the opinion that although 'econometrics' was a new term and concept, econometric research had much to learn from predecessors. Frisch got most of his favourite pioneers covered in this series of articles through the 1930s with one exception, Irving Fisher, whose 1892 dissertation had influenced Frisch's very much. Fisher was present in person as President of the Econometric Society in the first four years 1931-34 and a very active participant in the meetings and as a contributor to *Econometrica*. Fisher had lost prestige in America after 1929 but that did not affect the high esteem Frisch had him.

Schumpeter wrote of him in his obituary in *Econometrica* as 'the most important of the pioneers of econometrics since William Petty.'[12]

4. Vol. 1 – 7, 1933-39

On December 27, 1932 Nelson could write to Frisch that *Econometrica* had gone to press. The second and final) page proof had arrived on Saturday December 24 at one p.m. Cowles and Nelson had worked all Saturday afternoon and Nelson worked also six hours on Sunday (Christmas Day) and got it mailed the same night. Nelson hoped it would be in the mail by January 4th but Banta the publisher) did even better and had the issue in the mails December 31. Nelson received two copies January 2. 'Technically at any rate, Econometrica appeared absolutely on schedule. Three cheers!'[13]

4.1 1933

Vol. 1(1) - January 1933

The first issue was fronted by Frisch's Editorial and Joseph Schumpeter's programmatic *The Common Sense of Econometrics*. Both have been much cited and quoted.

Frisch selected two of the papers that had been presented at ESEM-1 in Lausanne. René Roy (Ecole Nationale des Ponts et Chaussées) on Cournot opened the series on econometric pioneers.[14] Then Tinbergen, perhaps Frisch's closest and most reliable ally in the Econometrica project, who in Lausanne had presented a hardcore mathematical paper on the application of functionals and complex numbers in economics.

Two papers came from the first US meetings, by John B. Canning (Stanford) on accounting and James Hervey Rogers (Yale) on the absorption of bank credit. Finally, there was paper by W.A. Shewhart (Bell Laboratories) on 'economic standardization', based on a lecture in London in 1932.[15]

The Jan. 1933 issue of Econometrica had to cope with an overload of reports from meetings. Frisch refrained for space reasons from soliciting a survey for the first issue. It comprised reports from the first four meetings of the Society, Lausanne 1931, Washington DC Dec. 1931, New Orleans Jan. 1932, and Syracuse June 1932. Together with the report from the Organization Meeting in 1930 the total number of pages ran to slightly above half of that of the six articles in the issue. But even without a backlog the reports continued to amount to a considerable number of pages every year. Hence, the question of the importance of giving the meetings so much attention came to the forefront already at this stage. Nelson conveyed to Frisch how they looked at it in Colorado Springs:

> 'Regarding Reports of Meetings, the January issue had too many (which couldn't be avoided) and, apart from Mayer's they were not well prepared. Details as to luncheons etc. are irrelevant. The Lausanne report was too long, diffuse, and insufferably detailed. Printing the program is a waste of space, after the event. If programs could be printed <u>before</u> the meetings, they would have great news value and justify themselves thoroughly. The report ought to be a digest and an interpretation, not a schedule which, with intolerable fidelity, reproduces every trivial remark. Such, at least, is Mr. Cowles' feeling and mine. A good

---

[12] Schumpeter in July 1948, 219-31. Schumpeter also mentioned that Frisch surprised the audience at the American Statistical Association's testimonial dinner for Irving Fisher at 80 in 1947 by speaking of Fisher (1892) as a work of 'monumental importance' (p220).

[13] Nelson to Frisch, Dec. 27, 1932 and Jan. 4, 1933.

[14] There would be more about Cournot later in connection with the centenary of the 1838 volume, also by Roy, see 6(3).

[15] Five of the seven above-mentioned authors of the first issue had been among the 16 present at the organization meeting in Cleveland 1930.

> report is worth any space it takes, a poor one isn't worth any space at all.' (Nelson to Frisch, Jan. 31, 1933)

Frisch responded:

> 'I agree with you that the account of the Lausanne meeting was not exactly of the kind we want to be published in Econometrica. I am perhaps myself, at least partly, to be blamed for this particular form because I asked Staehle to prepare the report in the form of minutes. I think we should rather stick to the kind of report prepared by Meyer.'
>
> 'On the other hand I do not agree that programmes should not be printed unless it can be done ahead of time. Even if it cannot be printed until after the meeting, these programmes should be printed I think as historical documents of the activities displayed by the Econometric Society'. (Frisch to Nelson, Feb. 20, 1931)

The first issue introduced a list of 'Papers to appear in Econometrica.' The list became controversial over time between the editor and assistant editor because it comprised more than the backlog of submitted papers. Frisch could proudly announce the four forthcoming surveys. The Schultz and Slutsky papers were on the list, which was perhaps not unreasonable, although the Slutsky paper did not appear until 1937. Frisch had an inclination to put papers by himself on the list at a too early stage. The first list had one Frisch paper and it was never published. Already from 1934 the list occurred only occasionally.

Vol. 1(2) - April 1933

For the April 1933 issue Frisch had asked Johan Åkerman to translate his paper about Knut Wicksell, as presented in French at Lausanne into English. To translate the paper was not an obvious thing to do as *Econometrica* was bilingual. Frisch may done this to further the knowledge about Wicksell in the English speaking world; or perhaps, only to balance out the homage to Cournot in French in the preceding issue. Frisch admired Wicksell perhaps more than any other of the econometric pioneers and let the article front the issue, although he would usually let the surveys on business cycle theory or general economic get that slot.

The annual business cycle survey came next, Alvin Hansen and H. Tout (Univ. of Minnesota) on investment and saving in business cycle theory. Their critical remarks on Hayek's theory did not go unnoticed, Hayek responded in April 1934.

Three papers from Lausanne were included. A statistical paper by G. Darmois (Univ. of Nancy), a theoretical paper by Marco Fanno (Univ. of Padua) on supply and demand curves, and by G.H. Bousquet (Univ. of Alger) on equilibrium concepts.

The remaining three papers had all been presented at the American meetings: J. Shohat (Univ. of Pennsylvania) on interpolation, and M. Ezekiel (Federal Farm Board) on the analysis of prices of competing and substituting commodities had both been presented in Syracuse, where Frisch had been present. A paper by Umberto Ricci (Egypt) was also on demand and had been presented in New Orleans. On the author-editor exchange over Ezekiel's paper see section 5.1.

After the second issue had been sent to the printer but well before it appeared Nelson sent off to Frisch a memo with points on editorial policy and process (perhaps with some input from Cowles):

> 'I believe that there should be a definite attempt to give the magazine a broad range of interest, so that it may appeal to various groups, where this can be done without any sacrifice of quality. Each issue should attempt to have articles, for instance, on (1) history and biography, like Roy's and Akerman's, (2) mathematical technique, e.g. Shohat's, (3) some phase of business cycle theory, e.g. Hansen's, (4) demand studies, e.g. Ezekiel's and Schultz's, (5) frequency distributions, e.g. Bowley's, (6) banking, e.g. Rogers' (7) industrial problems, e.g. Shewhart's, (8) investments, on which we have had nothing, (9) accounting, e.g.

> Canning's, and so forth. Such an issue would carry a large and immediate appeal to a great many groups.' [Frisch's commented in margin: "Yes, but there must be a face."]
>
> The April issue, on the contrary, concentrates heavily on demand studies, a subject of unlimited interest to a limited number of people. Those accountants or bankers who had their interest aroused by Canning and Rogers in January will find nothing of interest in April. You may favour a symposium covering a single field exhaustively and disposing of it entirely for a time. That is not, in my judgment, the best way to sustain interest in the journal. Business cycle theorists, say, would buy the issue on business cycles, and ignore Econometrica for perhaps two years, so with accountants, etc. If you give each group something in each issue, interest is perpetual. As I frame this program, I realize it is an ideal, but I should think we could at least approximate it. [Frisch's comment in margin translated: 'It is impossible in every issue to everything, a certain specialization is an advantage.']
>
> This can best be done by laying down a program for the next four issues, and trying to fulfill it. You probably have some such program in mind, and I only write about it because your letters have not covered this point. My suggestions are that such a program be definite, well-rounded, and aimed to appeal to all potential sources of interest. For example, in your place, I should write Rogers asking if he had anything further on banking analysis, or if he knew anyone who had, or write to other likely sources. So with Canning, etc. By soliciting the cooperation of those most familiar with different phases of econometrics you get better results than merely by waiting for these things to turn up. In my ideal program we should even now be trying to secure an article, for example on pure economic theory for January 1934, an article on mathematical economics for July 1934. A program lends comprehensiveness and direction to a journal. I raise the question merely to give my ideas on a subject that you doubtless have been wrestling with. [RF comment: 'Agree. There is something in that. As soon I am back from Paris I shall . . .'][16] (Nelson to Frisch, Feb. 24, 1931).

Thus Nelson was an assistant editor with initiative and a number of talents. Even if Frisch didn't go along with all his ideas he certainly appreciated his initiative. To some extent Nelson may have conveyed view that came from Cowles. Nelson was furthermore a perfectionist, also that appreciated by Frisch who also often paid the utmost attention to details. Nelson had told Frisch immediate on receiving issue no. 1 that he was quite annoyed after finding 4-5 small errors, all of them negligible. Shortly afterwards he also discovered that the Jan. 1933 issue did not have standardization of procedure regarding quotes, italics, roman letters, etc. in quoting journals, books, and articles, etc. in the footnotes. As a consequence there were inconsistencies. Nelson did his utmost to keep such infelicities out of Econometrica and largely succeeded.

Frisch responded right away (from Paris!) to Nelson's points on editorial policy:

> 'I think there is much to be said for your suggestion that there should be in every number a little bit of the various topics to which *Econometrica* is devoted, that is to say:-
>
> History and biography of economists.
> Mathematical technique.
> Some phase of business cycle, etc.
>
> But on the other hand there is some danger in this policy. We must by all means strive to give to *Econometrica* a certain distinctive physiognomy of its own. Giving in each number something to suit every taste is just what all the other journals are doing. We should I believe in some way or another do something which is different from the others. That is the reason why I had on purpose accumulated various papers on demand studies in the April

[16] Frisch was at the time in Paris at the Institut Henri Poincaré delivering a series of eight lectures on the *Problems and Methods of Econometrics*.

> issue. I had even planned to have my own paper on demand in that issue but did not get the manuscript in shape for it. Perhaps that was only to the good, since it would still more have accentuated the specialized character of the April issue.'[17]
>
> 'After all the best general policy would perhaps be most frequently to bring out diversified issues, that is to say issues containing a little bit of each one of the various fields in which we are interested, but from time to time, say once a year, or at more or less irregular intervals, bring out a number specially devoted to some particular and important aspect of econometrics, i.e. the construction of demand curves, or the like.'
>
> 'I agree entirely with your suggestion that we should lay down more or less definitely a programme for say the next four issues. As you will see from the carbons I have transmitted to you I have already done something of that sort by communicating with various people on the writing of surveys or of historical and biographical notes. In this connection I may mention that Bowley in a recent letter tells me that he will be glad to write the paper on Edgeworth. I think your suggestion is good that I write to men like Rogers, Canning, etc. who have recently made interesting contributions to *Econometrica* asking them if they know of somebody else. … As soon as I get back from Paris I shall pay more attention to this aspect of the editorial work.' (Frisch to Nelson, March 13, 1933).

Thus Frisch went along with Nelson's suggestions but they were hardly completely aligned. Nelson and Cowles were, naturally, highly concerned with the commercial success of the journal and leaning in the direction of catering to many tastes. Frisch had a different agenda.

But there was more that needed clearing up. Nelson had already noted that Frisch could cause severe and uncomfortable delays. Frisch overbooked his time between research, teaching, editing Econometrica and a load of other commitments. He was inclined to fall into a patters where he put Econometrica aside while he was doing other things and then worked for days and nights reading manuscripts and making editorial decisions. Nelson told him in no uncertain words:

> 'One of the principal concerns of my life is to get Econometrica materials on time. I speak out of a clear conscience. Every letter I have had bearing on Econometrica has been answered the day it was received or the day following. Every task has been dispatched immediately, all other work being suspended till Econometrica work was brought up to date. Banta has been entirely satisfactory, getting the material back with great promptness. The delays occur with the Editor and the contributors. I am perfectly willing to assume the function of perpetual irritant to the Editor and to the contributors, but I am handicapped in not knowing whom to irritate. Right now, apart from Bowley and Roy, whose stuff is in type already, and Shewhart, whom I have already written, I don't know whom to attack for July materials. The inevitable result is tardiness all along the line.
>
> I think we should not get a reputation for such tardiness. … Further, your notification of acceptance or rejections should be prompt, because, in the case of rejections, there may be good possibilities of publication elsewhere, and long delays give authors a legitimate sense of grievance against Econometrica.' (Nelson to Frisch, Feb. 24, 1931).

Then on top of that Frisch had made a most unfortunate blunder, causing a strain in his relation with Cowles. At the Cincinnati meeting in December 1932 Cowles had presented the paper 'Can Stock market Forecasters Forecast?' and then submitted it to Frisch for consideration in *Econometrica* but no reaction came from Frisch:

> 'You received it about January 5. Allowing a week for review, a cabled rejection (as was suggested to you by Cowles) would have reached us about six weeks ago, or a letter three

[17] Frisch's paper was titled 'The Notion of Elasticity with Regard to a Given Definition System ', announced among 'Papers to Appear' in the January 1933 issue. It was never published.

> weeks ago. This protracted uncertainty places Cowles in an awkward position due to his relationship to Econometrica. … in his position he is highly reluctant even to raise the question of its status, lest his actions be misinterpreted. For his purposes the Journal of the American Statistical Association, which would have printed it, would have been a satisfactory medium of publicity. The circulation is much larger than *Econometrica* 's and the paper would have been published earlier. As bearing on my point that Econometrica should try to minister to the interest of various groups, the reception of Cowles' papers is pertinent. It had a fine reception at Cincinnati. It made headlines in leading newspapers throughout the country. Within a month he had hundred requests for copies so that we have exhausted our entire supply of 100 mimeographs. More than half the inquiries came from influential sources, professors, researchers, bankers (one from France) etc. Several teachers have since written that they are using the paper in their classes. Theodore Dreiser, the distinguished writer, asked for further articles by Cowles. Professor H. C. Carver of Michigan, editor of the Annals of Mathematical Statistics, said at Cincinnati that some of the statistical tests in the paper interested him more than anything else at the symposium, and he took a copy because he wished to comment editorially on leading publishers, asked Cowles to write a book for them. In short, there was an impressive paper response. … I have not written of Cowles' paper to try to induce you to accept it, but simply in illustration of the two major theses of my letter.' (Nelson to Frisch, Feb. 24, 1933).

Frisch could only apologize meekly for having been too slow in handling some of the manuscripts. 'I think I have been a little fatigued over all the trouble caused by bringing out the first issue.' With regard to the submission from Cowles he stated that

> 'in some unexplainable way there has occurred an oversight in regard to his letter. By a misunderstanding it had been, without being read to the end, slipped into my file of "answered letters". I suppose I must have read the first half of it and then been interrupted and finally have mixed this letter with others.' (Frisch to Nelson, March 13, 1931).

Vol. 1(3) - July 1933

The July 1933 issue was fronted by the first annual survey on statistical technique; it was by W. A. Shewhart on recent work in sampling theory. Shewhart reviewed briefly six recent books (including new editions of R.A. Fisher and Yule).[18]

Then there were two more papers from ESEM-1, René Roy on demand curves and elasticities and Ph. Le Corbeiller (Paris) on oscillation theory and introducing the work of B. van der Pol. In connection with Le Corbeiller's submission Roos, as secretary of the Society, objected on the grounds that Corbeiller was not a member of the Society. But the objection was overruled and the question was not raised again.[19]

In the issue were both of the articles that had slipped from Frisch's mind as mentioned above, by Alfred Cowles on stock market forecasts and by Henry Schultz on demand elasticities. Then there was Tinbergen's second article which offered evidenced that price expectations influenced observed behaviour. Tinbergen's paper was number three on the list of contents, preceded by Lewis S.

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[18] Shewhart advised the student of statistical technique to read them in a stated order to 'begin to appreciate that the rational interpretation of data depends not only upon a knowledge of the statistical techniques involved in testing statistical hypotheses, but also that the techniques of collecting and presenting data inherently depend upon the techniques and the hypotheses that are going to be used in the interpretation thereof (p237).

[19] But the question of the exclusivity of the Society came up in other forms. The Society had adopted strict membership rules requiring not only recommendations from two members to be considered but also approval by vote from the other members. It happened that someone objected to new, proposed members, to preserve the exclusivity, causing embarrassment in the Council. Fisher abolished this rule.

Maverick (UCLA) on time series, discussing one of the main tenets of Frisch (1927). There was also a brief paper by Vice-President F. Divisia (Ecole Nationale des Ponts et Chaussées) presented in New Orleans on the theory of aggregates.

Vol. 1(4) - October 1933

The October issue had up front Irving Fisher's article on the debt-deflation theory of depressions. Fisher had worked it out in 1931 wrote in out as book (Fisher 1932), and then submitted a short version of book to Econometrica.[20] The annual survey on statistical information by Jakob Marschak (Oxford) on financial national income flows.[21]

Marschak, whom Frisch came to appreciate very much and always could rely on to step in if necessary if a problem arose, was given third place in the issue and Frisch gave second place to the English member of the Council, Arthur L. Bowley, with whom Frisch had been in touch already in 1926 about the idea of an econometric journal. Bowley, whose paper had been presented at ESEM-2 in Paris, noted the divergence of purpose between mathematical statisticians and mathematical economists (= econometricians) and posed the questions of how far the forces which produce variations in the frequency distribution of common phenomena are produced by economic forces, illuminated by empirical examples.

The issue comprised Frisch and Waugh paper with the result nowadays often cited as the Frisch-Waugh-Lovell theorem. Nelson played it safe on Frisch's behalf: 'I purposively avoided putting your article closer to the front than fourth, lest the captious misinterpret it prominence (Nelson to Frisch, Sept. 27, 1933).

The issue had two exchanges, one of them mild and constructive the other one less attractive to put in print. Raymond Garver (UCLA) submitted a brief paper on Edgeworth's taxation phenomenon, apparently without being aware of Hotelling (1932). Frisch sent it to Hotelling for refereeing. The outcome was that Garver's article was followed by a 2-page comment on Edgeworth and Garver.

The other exchange was caused by an article by Joseph Mayer (Library of Congress) on pseud-scientific methods in economics, naming Frank Knight as one of the culprits. Frisch gave Knight opportunity to respond in the same issue with a rejoinder by Mayer, see section 5.2.

Finally, there was a rather marginal article by D.I. Vinogradoff (Westinghouse) on employment effects of technological improvement. Vinogradoff doesn't deserve further mention except as one of a number of fellow travellers who often with no or poor academic affiliation joined the Society, retained the membership for a number of years, and occasionally submitted a manuscript of mediocre quality, which sometimes even got accepted as in Vinogradoff's case. There were times in the early years with a vanishing backlog.

At the end of the year a manuscript by Frisch was under review. It resulted in rejection or perhaps withdrawal, see section 5.3. Also at the end of the year Wassily Leontief submitted a manuscript which met with so much criticism by Frisch, serving as referee, that Leontief in the end refused to resubmit it, see section 5.4.

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[20] Fisher who had been written off by most American economist after his soothing statements about the stock market in October 1929 was retrieved from obscurity to star status in *The Economist* February 12, 2009 for the his understanding of financial and economic crises as expressed in his article in Econometrica in 1933. In a long article titled *Out of Keynes's shadow* The Economist did what it could to rehabilitate the soundness of Fisher's insight.

[21] Marschak's survey was written or completed while he fled Germany at Hitler's accession to power. Marschak stood by all his commitments in that uprooting situation and wrote the report from the ESEM in Leyden in 1933 at which he also was a very active participant.

Frisch and Nelson seemed both reasonably satisfied with the first volume. Technically, it had gone well, the international character of the journal was obvious, the surveys were launched and got positive responses. Both Frisch and Nelson learned a lot from the experience of the first year, Nelson learned a lot about Frisch. An unsettled issue was which readership the journal should cater too. This was not so much an editorial issue as a financial one. At the end of 1933 Frisch decided to poll the opinion of leading members about proportions of articles of different kinds, etc. and prepared a questionnaire, see section 5.5.

4.2 1934

Vol. 2(1) - January 1934

The series of articles on econometric pioneers continued with Erich Schneider's article on Johann von Thünen, which fronted the issue ahead of Tinbergen's annual survey of general economic theory. Tinbergen made some apologetic remarks about the virtual impossibility of writing a survey of general economic theory but did impressively well, subdividing his paper in sections on static theory and dynamic theory, citing original works in at least six different languages, and recapitulating arguments made at ESEM meetings. The issue also brought Griffith Evans only article in *Econometrica*, it was on maximum production in a simplified system. Evans' way of theorizing seemed a bit outdated by this time. A similar impression gave the article by Roos, Evans' student, on theoretical studies of demand. Then there was a study by Hans Staehle comparing the consumer behavior of Scandinavian immigrants with American families and Swedish families. There were not too many empirical papers in *Econometrica*. Finally, the issue also had articles based on the ESEM-2 (Paris) presentations of Phelps Brown and Jakob Marschak.

Vol. 2(2) - April 1934

The issue was fronted by Bowley on Edgeworth, followed by Felice Vinci's survey of business cycle theory. It further had articles on demand theory by E.J. Working and by R.G.D. Allen, and a paper by Wiśniewski presented at ESEM-3 (Leyden) on interdependence between cyclical and seasonal factors

Vol. 2(3) - July 1934

The series of pioneer articles continued with Jevons. In the list of contents it was listed as a article jointly authored by Jevons's two children, H. Winifred and H. Stanley, but it was an fact an article by each of them.

The survey article by Darmois (in French) was on recent developments in statistical techniques. It gave a brief survey on recent advances of interest for econometricians mostly 1933 and 1934 publications. This was much in the spirit that Frisch had wanted. The survey comprised i.a. Hotelling's 1933 paper on principal components and some works by Frisch including his lecture series at the Institut Henri Poincaré.[22]

A paper by Allen was a sharp criticism of the methods used by Pigou (1930) in deriving demand elasticities. It may well be view as inspired or as a follow-up of the sharp criticism of Leontief (1929)'s method in Frisch (1933a), the *Pitfalls* essay, to which it made due reference. In fact it criticized also

[22] Darmois had helped to arrange this series of lectures on *Problems and methods in Econometrics*.The survey mentioned that the lectures would soon appear in *Annales de l'Institut Henri Poincaré* but this didn't happen. The lectures were not published until an English translation appeared as Bjerkholt and Dupont (2009).

Leontief (1929) and in stronger terms than used against Pigou.[23] The paper thus belongs among the early contributions on simultaneous equations.

The fourth and last paper in the issue was Frisch's Circulation Planning article. It is the longest article ever published in *Econometrica* (except for Haavelmo's *Econometrica* Supplement in 1944). In the issue it took 79 pages but a Mathematical Appendix of 15 pages had to be left for the following issue. The article resulted in heavy criticism from Charles Roos, see section 5.6, both for its length and for misuse of editorial prerogative. The articles had not been refereed. The article was about how to build an alternative exchange mechanism when the market has collapse. Not everyone was enthusiastic about it. Arrow (1960), the first survey of Ragnar Frisch's scientific publications, did not even mention it. But there are several interesting features of that article, including the earliest statement of the linear-quadratic programming problem, foreshadowing Frisch deep interest in programming.

Vol. 2(4) - October 1934

The issue was fronted by the Econometric Society's one-page address to the University of Lausanne on behalf of 317 members from 29 countries on the occasion of the centenary of the birth of Léon Walras, followed by Hicks' excellent article on Walras.

The survey of statistical information was by Hans Staehle on family budgets . Staehle discussed difficulties inherent in family budget data and the use of econometric methods in analyzing them. Family budget data were still very scarce, as Staehle pointed out.

The other papers in the issue were (1) a paper by F. Creedy on the mathematics of business cycles by F. Creedy, citing no one apart from the author himself on this highly focused issue in the journal; (3) with no references apart from to other: (2) an applied paper by O. Bakker (The Netherlands) on the problem of cutting wages in the depression; by Louise O. Bercaw (Bureau of Agricultural Economics) a bibliography with selected international references on the theoretical aspects of supply and demand curves and related issues; and the continuation of the long Frisch paper.

4.3 1935

Vol. 3(1) - January 1935

The issue was fronted by the annual survey of economic theory which was by J. Hicks on the theory of monopoly.

In the issue appeared the only article in German ever appearing in Econometrica. It was by Erich Schneider, who was professor at the University of Aarhus, Denmark and titled 'Bemerkungen zu einer Theorie der Raumwirtschaft.' To publish in German ran counter to the rules that had been adopted and Frisch explained to Nelson as follows:

> 'I have discussed at length with Schneider the question of language. There are various reasons I think in this particular case to make an exception to the rule that papers should appear only in English and French. There are amongst other personal reasons connected with Schneiders position in Germany during the present regime there. As a matter of fact if I had not promised that the paper should appear in German, Schneider could not have given it to *Econometrica*. As I think the paper is a very valuable one you will understand that I felt the only right thing to do was to accept it in German. It must therefore appear in that language, but there is a summary (written by Schneider in German but to be translated into English and appear at the end of the

[23] There was a harsh tone in some of these early battles in *Econometrica*. Allen criticized e.g. Leontief for a having a complicated apparatus serving 'strongly to disguise the appearance of fictitious or meaningless cases' while in Pigou was not dependent upon complications and 'the fictitious or meaningless cases are more obvious' (pp255-6).

> paper). Could you see to it that this summary is translated. I don't think that our accepting this particular paper in German will cause any difficulties with regard to the consequences for the future.' (Frisch to Nelson June 3, 1934).

It is a bit mysterious what Schneider actually told Frisch about his 'position in Germany during the present regime.'[24]

Nelson was most happy about the Jan. issue: 'My personal opinion is that this is a very good issue with wide appeal since the contents are English, French, German, mathematical, statistical, and historical, practical and theoretical, and the writers English, American, German, Swiss and French. I trust you will agree with this judgment' (Nelson to Frisch, Jan. 11, 1935). Frisch didn't say what he thought but he might have resorted to other criteria than Nelson.

Vol. 3(2) - April 1935

In the April issue there was no annual survey. It had been arranged a long time in advance that Tinbergen should write it the survey on Business Cycle Theory, which he duly did. Frisch read it and dispatched it to Colorado Springs on Jan. 11, 1935 and there it arrived one week later. Frisch was exuberant: 'excellent survey. Full of details both theoretical and statistical – in fact just the kind of thing we want for a survey.' According to Nelson the MS was in poor condition and needed a substantial amount of language improvement. In the middle of February Nelson stated that the survey could not make the April issue unless it was held up. It was decided to move the survey to the July issue and thus Frisch's survey plan started to unravel.[25] Nelson got upset and is likely to have blamed Frisch for sitting too long on the MS before passing it on. But instead of stating that he merely commented: 'My ideal is that *Econometrica* should come out on time. I can see no valid reason why a scientific journal cannot be published with the same business-like punctuality as a secular journal.'[26]

Vol. 3(3) - July 1935

Tinbergen's 1935 annual survey of business cycle theory, postponed from the April issue was also a very long one, it ran when published 68 pp. Charles Roos who was Research Director of Cowles Commission since the beginning of the year followed the editorial work at close range and had no inhibitions about interfering. After having Tinbergen's MS he cabled to Frisch: 'I THINK TINBERGEN MORE EFFECTIVELY REDUCED TWO THIRDS LONG PAPERS WIDELY CRITICIZED URGE HE REDUCE AND MAIL REVISION WITHIN FORTNIGHT NO TIME AUTHORS PROOF CABLE DECISION.' Roos felt strongly that Tinbergen had written a very wordy paper which should not under any consideration be allowed to appear in Econometrica in the form in which Frisch sent it to Nelson. Frisch cabled back: 'NELSON MAKE VERBAL ABBREVIATIONS OMIT NO IDEA MAKE APRIL NINE OR TEN FORMS SPECIAL DONATION OBTAINED FOR THIS INCREASE TINBERGEN SEE GALLEY.' In an accompanying letter Frisch argued that 'it would be a mistake to reduce Tinbergen's paper. Perhaps our difference of opinion may be explained by your putting an extreme emphasis on those aspects of economic dynamics which are in vogue in the United States at the present moment, while I try to maintain a sort of balance between

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[24] After the German unification and the opening of the Nazi archives it was found that Erich Schneider was a member of NSDAP since 1933 (oral information form Harald Hagemann). In decades after WWII Schneider was the leading figure in German economics, not least due to a number of well written textbooks.

[25] Frisch inquired why there was no note in the April issue to say that the "Annual Survey of Business Cycle Theory" was postponed until July. Nelson humbly answered that he felt that a great many readers, perhaps almost all, would not notice the omission unless their attendance was specifically directed to it: 'I thought it best, therefore, to "let sleeping dogs lie". However, since you think it would have been be wise to include such a note, I am sorry I did not do so.' (Nelson to Frisch, April 5, 1935).

[26] Frisch to Nelson, Jan. 11, 1935; Roos to Frisch, Jan. 30, 1935; Nelson to Frisch, Feb. 11, 1935; Nelson to Frisch, Feb. 14, 1935.

the American and the European ideas. I think there is a difference between the two worlds in this matter, and I think the European side was so admirably represented by Tinbergen's contribution that it would have been a pity to cripple his paper.' Roos responded that Frisch's cable failed to correct the situation, unless interpreted to mean 'no essential idea', but that was not how Nelson had interpreted it and Nelson would send the manuscript to the printer essentially as it was, which is also what happened.[27]

TO BE COMPLETED

Vol 3(4) - October 1935

TO BE COMPLETED

4.4 1936

Vol. 4(1) - January 1936

The issue was fronted by Frisch's general economic theory survey on the problem of index number. Frisch had not intended to write a survey but used it to fill the gap created by failed efforts to solicit surveys. It was a long paper (38 pp.) and advanced paper that became much cited. It made reference to an impressive list of 16 predecessors weho had contributed towards the same problem from Jevons and Edgeworth to von Bortkiewicz, Konüs, Allen and Staehle. It was completed in a rush as often happened with Frisch, hence a full page of errata followed in the April issue (and even that didn't recognize that he misspelled Laspeyres' name throughout he paper).

Then followed Leontief's – also much cited - article on composite commodities and index numbers. That caused editorial deliberations over priority as Hans Staehle had just published an article with related results in the *Review of Economic Studies*, see section 5.7.

The three other papers were (1) Gerhard Tintner's first paper (of many) in *Econometrica* on income distribution over time; (2) John M. Thompson, a student of Griffith Evans at Berkeley, on mathematical theory of production stages in economics, discussing inter alia the point of disagreement between Hansen & Tout and Hayek, mentioned above; and (3) Hans Bolza (Würzburg) on dynamic economics based on a paper presented at ESEM-4.

The assistant editor, W.F.C.Nelson, was taken ill towards the end of 1935 and died in May 1936. Cowles stepped in at first and then hired as Acting Assistant Editor, Charles H. Sisam, Professor of mathematics at Colorado College to help out with issue 3 and 4 1936.

Vol. 4(2) - April 1936

The issue was again fronted by a survey paper on economic theory by Johan Åkerman (Sweden) on 'the setting of the central problem', dealing with a number of different problems through the history of economics, naturally including in his discussion a number of Swedish authors and Frisch who had been the official opponent at the doctoral defence in 1928. Frisch added an initial editorial note stating that the survey had been requested by the author and as Åkerman had laid 'special emphasis on the time element and the equilibrium and disequilibrium problems, it has been found appropriate to let this paper replace…one of the surveys on business cycles.' (4(2), 97).

Then followed an article E.H. Phelps Brown (Oxford) titled 'The marginal efficiency of a productive factor', which was not regular contribution but a report from an Econometrica committee established as an impromptu decision at ESEM-2 on source material for quantitative studies. The other members who had contributed material for the report were F. Zeuthen, G. Lutfalla (France), O. Lange (Poland) and Louis Bean (USDA). This was another way of using the *Econometrica* to help raise

[27] Roos to Frisch, Jan. 30, 1935; Frisch to Roos, Feb. 5, 1935; Roos to Frisch, Feb. 13, 1935;

the level of econometric research of the members. It was denoted the first report and the second and third reports from the same committee but covering different topics followed in the July and October issue, also by Phelps Brown.

The four other papers were (1) an applied paper by R.H. Whitman (New York) on the demand for a producers good exemplified by the demand for steel; (2) O. Pankraz (Chechoslovakia) on the law of demand with particular reference to the work of by Charles Roos[28]; (3) R.W James and M.H. Belz (Univ. of Melbourne) on a generalization of the solution method of a mixed difference and differential equation by Frisch and Holme in 3(3) for the solution of the Kalecki model in 3(4); and (4) Elmer Bratt (Lehigh Univ.) on institutional factors in the determination of long-term trends, in fact a rare case of an *Econometrica* paper firmly rooted in American institutionalism.

Vol. 4(3) - July 1936

The issue was fronted by F. Zeuthen (Univ. of Copenhagen), the leading Danish economist, on monopolistic competition and the homogeneity of the market, adding to the wave of monopolistic literature in the preceding years with E. H. Chamberlin's and J. Robinson's books, both in 1933, and others, including Hicks' survey in 3(1). Zeuthen who was a pioneer in this field with his monograph Zeuthen (1930), cited also Frisch (1933) and even Frisch's lecture notes on the topic in Norwegian. In fact, Frisch must have found that Zeuthen's article would make a great survey paper if it hadn't been for the fact that the previous year's economic theory survey was on monopoly.

The issue comprised also a survey on statistical technique by P.R. Rider (Galton Laboratory, London) on developments in the analysis of multivariate data. This was announced as part I of the annual survey on statistical technique with part II following in the October issue on a different topic and with different author. Rider's survey did not get good marks by readers. The quality of the surveys was of course immensely more important than strict regularity that Frisch originally had announced. But the surveys were in fact soon to practically disappear from the pages of *Econometrica*.

The other papers in the issue were (1) 'an econometric model of production and distribution' by V. Edelberg (LSE), in fact the model was more than a Wicksell inspired production function in land, labour and capital distribution fitted to UK and US data, with results that could be compared with those of Cobb and Douglas[29]; (2) an empirical study by W.A. Tweddle and Richard Stone (Cambridge) on costs, particularly labour costs in British industry (Stone was 22 at the time); (3) a demographic paper by Silvio Vianelli (Bologna) exemplified by data for Italy and USA; (4) the second report from the Econometrica committee on source material by Phelps Brown on cost categories and the total cost function.

Vol. 4(4) - October 1936

The issue was fronted by Frederick C. Mills (NBER) on 'price data and problems of price research.' Mills, who had been the first associate editor for statistics in *Econometrica*, was NBER's renowned expert on prices. Mills article showed his familiarity with price data from many countries and reflected his deep insight in analysis of price problem. It was not declared as an *Econometrica* survey but it certainly served that purpose.

The survey in the issue was part II of the survey on statistical technique by C.F. Roos on correlation and analysis of time series which surveyed much contemporary work by statisticians and econometricians and became a frequently cited survey paper.

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[28] Roos (1934) and Roos 2(1), 73-90.

[29] Wicksell's lectures, which Edelberg, cited specified the Cobb-Douglas function at the very beginning of the 20th century but there are also other claims for having originated the Cobb-Douglas function, see Lloyd (2001).

Another notable article in the issue was Max Millikan (Yale) on Pareto's sociology, a review of *The Mind and Society*. Pareto's sociology was thus presented in Econometrica before Frisch had succeeded in soliciting a paper on Pareto as on of the pioneers of econometrics, see 6(1). Millikan's article was a very critical review of Pareto, not so much on his sociology as on his conception of scientific method, which according to Millikan was based on fundamental misunderstandings. Millikan found however that the book's characteristic which made it bad science at the same time made it excellent reading. He praised the 'stinging irony and the fine invective splattered through its pages' (p337) while warning that 'his exposition of scientific method is so faulty that it should be kept from tender young minds' (p336)!

An article by M. Kalecki commented upon Tinbergen's survey in 3(3) which had asserted with about Kalecki's theory (in 3(4)) 'prices do not appear at all in the theory', and attempted to prices played a role although not explicitly visible in the formulation. He also commented upon the Frisch and Holme in 3(3) who had objected to Kalecki's assumption of a constant amplitude for the cycles. On this point Kalecki conceded that they were right, 'for it is by no means sufficient to say that an assumption is correct just because it is confirmed by the conditions of real life' (p350).

The other three papers in the issue comprised (1) an empirical study of the demand of boots and shoes by V.S. von Szeliski and L.J. Paradiso (Washington); (2) Geoffrey Shepherd (Iowa State College, Ames) on vertical and horizontal shifts in demand curves; and (3) the third report from the Econometrica committee on source material by Phelps Brown on the profit-experience of producers and their response to price.

4.5 1937

Vol. 5(1) - January 1937

After Nelson's death The Colorado Springs end of the editorial channel was from 1937 upgraded to Managing Editor with Dickson H. Leavens filling that position.

The issue was fronted by a 55-page long article by Irving Fisher on 'income theory and income taxation in practice', based on a series of four lectures given at the second Cowles Commission conference in 1936. It is hard to understand how and why the article was accepted for Econometrica, except out of misconceived respect for the first president of the Society.

The long Fisher article left room for only two more articles. One of them was Roy F. Harrod's paper on Keynes presented at ESEM-6. The other was by C. Gini, written during a visiting stay at Harvard, on 'methods of eliminating the influence of several groups of factors.'

Vol. 5(2) - April 1937

The issue was fronted by Eugen Slutsky's article on the summation of random causes as the source of cyclic processes, a revised version of his famous 1927 article in Russian, which Frisch had singled out already in 1932 as a paper he wanted to publish, see section 5.8.

The issue also comprised the celebrated ISLM-paper by John Hicks (Cambridge) (although this acronym does not appear in the paper). Hicks had presented the paper at ESEM-6 the previous and could have been published earlier. Frisch may have contributed to holding it back for more discussion. An initial note by Hicks stated that the article had been modified partly in the light of the discussion at the meeting and 'partly as a result of further discussion in Cambridge' (p147).

The other two articles in the issue were (1) an article by Tintner (Cowles Commission) on monopoly over time; and (2) R. Bretherton (Oxford) on the sensitivities of taxes to fluctuations of trade.

Vol. 5(3) - July 1937

The issue was fronted by two annual surveys. One on economic theory by Nicholas Kaldor (LSE) on controversies of the theory of capital and the other on statistical technique by Horst Mendershausen (Geneva) on methods of computing and eliminating changing seasonal fluctuations.

Alfred Cowles 3rd and Herbert E. Jones (Cowles Commission) on some posteriori probabilities in stock market analysis, a further outcome (after Cowles in 1(3)) on the only research project of importance of the Cowles s Commission, Cowles 'project on stock prices which resulted in Cowles (1938).

The other articles in the issue were: (1) J.H. Cover (Univ. of Chicago) on the sampling and distributions of retail prices; (2) an empirical study of the demand for bicycles in the Netherlands by J.B.D. Derksen and A. Rombouts (The Hague); and V. Travaglini (Catania) who expressed his disagreement with Millikan's article in 4(4) in the interpretation and presentation of Pareto's view.

Vol. 5(4) - October 1937

The issue was fronted by an article by Herbert E. Jones, one of Alfred Cowles' staff at the Cowles Commission since it was established. The article was titled 'The nature of regression functions in the correlation analysis of time series.' The article was conceived more or less within Frisch's framework for analysing time series, cited several works by Frisch, and reflected perhaps the involvement of Jones in the work done when Frisch was research consultant for the Cowles Commission 2-3 years earlier. Jones's manuscript seems to have inspired Frisch to a brief paper in the same issue as a 'Note on the phase diagram of two variates.'

The other two articles in the issue were (1) by Horst Mendershausen (Geneva) on 'meaningful curvilinear regressions in economic time series', presented at ESEM-6; and (2) an applied paper by L.R. Nienstaedt (Denmark) on economic consequences of technical development.

4.6 1938

Vol. 6(1) - January 1938

The issue was fronted by Amoroso's paper on Pareto. Frisch had taken up with Amoroso on a number of occasions since 1933 that he wanted an article on Pareto, suggesting that Amoroso should write it. When it was not forthcoming Frisch asked if Amoroso whether he could find someone else to write it. In the end Amoroso submitted. But it was not an original paper. Amoroso had translated a paper he had written in an Italian encyclopaedia. Frisch was hardly happy about that but decided to accept the article. Others reacted strongly to the fascist overtones in the paper. Jerzy Neyman told Frisch in no uncertain terms: 'You may be interested to know that many members of the Econometric Society, both in America and in this country, regret that Mr. Amoroso's article on Pareto contains so much political propaganda which has nothing to do with the objects of the society.' Frisch can hardly have been very happy about Amoroso's article and he responded somewhat meekly, defending his decision: 'I understand the feeling about Amoroso's paper. I had the feeling myself but on going through the MS carefully, I was unable to pick out any definite sentence that could not in a sense been taken as a description of Pareto's work. Everything is "d'une façon trés adroit" presented as a contribution towards the understanding of Pareto. And, this being the case, I did not feel that I could rightfully ask Amoroso to change the presentation although I very much wanted to do so.'[30]

Tinbergen had yet another business cycle article with an early version of the US model in his forthcoming League of Nations volume. An interesting aspect of the article is the emphasis on policy analysis, what Tinbergen called 'business cycle control.' The article also shows the close links

[30] Neyman to Frisch, March 3, 1938; Frisch to Neyman, March 7, 1938.

between Frisch, Tinbergen and Haavelmo. Tinbergen discusses the practical needs for simplifications in business cycle models, mentioning Frisch's "macro-analysis" as one and Haavelmo's discussion at the ESEM-6 of how a model can be closed as another model. The Editor (Frisch) has inserted a note at one point in Tinbergen's article about how he and Haavelmo at the Institute in Oslo after experimenting with American data had come to suspect that cycles could be generated by erratic shocks also from an exponential (non-cyclical) component in a model. Haavelmo made this the topic of a paper published in Econometrica in 1940.

The issue also had Frank Knight's reply to remarks in Kaldor's survey on the theory of capital the previous year. The rejoinder by Kaldor came in the April issue. Knight's and Kaldor's positions were not that different but Knight took the opportunity to reiterate his views and hit at his opponents. The famous Stockholm School economist did not show much interest in Econometrica, none of them published anything there. But Frisch had found a Swedish econometric colleague whose work he liked very much in K.-G. Hagstroem, who in the January issue wrote on 'Pure economics as a stochastical theory.' Bowley had a one-page note on Frisch price index survey the previous year, which again inspired Frisch to develop in the issue his "double-expenditure" method as anew attempt to get closer to the 'exact indifference-defined index.' Finally, there was a paper by H.W. Robinson of the London School of Economics on equilibrium in a perfect intertemporal market.

Vol. 6(3) - July 1938

The Econometric Society commemorated the centenary of A.-A. Cournot's *Recherches sur les principes mathématiques de la théorie des richesses*, published 1838, at its Atlantic City meeting in 1937. The issue was fronted by two papers from the memorial session, one by A.J. Nichol (UC Berkeley) on tragedies in the life of Cournot, and one by Irving Fisher titled 'Cournot forty years ago', which was recirculated version of an article Fished had published in *QJE* in 1998 in connection with the translation of Cournot into English.

The issue comprised Trygve Haavelmo's first *Econometrica* paper, presented at ESEM-6 and completed while Haavelmo took part in Tinbergen's Geneva group in the spring of 1938. Frisch had made sure that it came as close to the front of the issue as possible before the annual survey but naturally after the Cournot pieces.

Then followed annual survey of economic theory was by G.A.D. Preinreich (New York) on the theory of depreciation. Preinreich would soon be enmeshed in a conflict with Alfred Lotka, see section 5.

The issue comprised Harold Hotelling's celebrated and much cited article *The general welfare in relation to problems of taxation and of railway and utility rates*.[31] Much of what Frisch wrote in economic theory arose from reading someone else's work and noticing as errors or weaknesses. Reading Hotelling's manuscript after submission thus led Frisch to inform Hotelling that he would append a note to his article when it got published. Appending notes was a somewhat dubious habit Frisch practiced but Hotelling didn't want anything of that kind. He wrote back to Frisch and told him in no unclear words that he had worked on the problem for 6-7 years and 'under these circumstances, I do not think it likely that any criticism conceived within a few days and published immediately is likely to have much force' (Hotelling to Frisch, May 26, 1938). Frisch yielded and published his remarks three issues later in the April 1939 issue, organized as an exchange with Hotelling, see 7(2).

One of Hotelling's students, Irma Hilfer, became the second woman to publish a paper in *Econometrica*. In the paper titled *Differential effect in the butter market* Hilfer identified a serious error in a Danish article about the UK butter market.

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[31] Econometric Society and *Econometrica* did not introduce a Presidential Address until long after Frisch's time as editor but there was an inkling of it in the title footnote of Hotelling's article, stating that it was presented at the Econometric Society meeting by the retiring president.

Finally, the issue had a two-page note by Mendershausen on the interpretation of Frisch's criterion of 'equal well-being' in his double expenditure method in 6(1), which again was an off-shoot of Frisch's price index survey. Frisch could not resist adding an explanatory note to the brief article.

Vol. 6(2) - April 1938

The issue had Kalecki's third *Econometrica* paper; it was on the determinants of the labour share of national income. There was also another Kalecki connection as R.W. James & M. H. Belz, (University of Melbourne) in a short paper tried to add realism by adding distributed lags to Kalecki's macro model (October 1935).[32] Tintner continued his long series of papers in *Econometrica*, this time on maximization of utility over time. Tintner had just moved to the Iowa State College at Ames after two years with CC in Colorado Springs. Pieter de Wolff, an associate of Tinbergen, wrote on the demand for cars in USA, which drew a reply from Robert Solo (and also a rejoinder) in July 1939.

Horst Mendershausen's critical review of the Cobb-Douglas production function as discussed in Paul Douglas's *Theory of Wages* (1934) was perhaps a criticism by proxy. Frisch had been present at the Cobb-Douglas paper in 1927 and his critical attitude was probably well known. Frisch had befriended Cobb in 1930 and encouraged his further research on production functions. Mendershausen's article was written during a visiting stay in Oslo, employed the Frisch tools (bunch maps) and acknowledged suggestions and assistance from Frisch, Haavelmo and Reiersøl.

Vol. 6(4) - October 1938

Frisch gave the front place to René Roy (Univ. of Paris) for an introductory note to an article by E. Morice (Institut de statistique) on the demand for postal services in France 1873-1936, conducted under guidance of Roy.

The issue comprised Marschak's article on 'Money and the theory of assets'. The first *Econometrica* article by Paul Samuelson was in the issue, titled *The empirical implications of utility analysis*. For a brief note on how it was refereed, see section 5.9.

James and Belz followed up their 3(3) article by more on the solution of difference-differential equations. An article by Acheson J. Duncan on Marshall's paradox can serve to give an example of the regular refereeing process, see section 5.10.

The prolific Tintner wrote on the generalization of Hicks and Allen for the dynamic case, it was already his 4th Econometrica since 1936 and one more would be published less than a year later.

Finally, there was a brief 3-page mathematical note on democracy by K.G. Hagstroem.

4.7 1939

Vol. 7(1) - January 1939

The front article was by Henry Schultz on 'the true Konüs condition of cost-of-living.' Schultz had tragically died in a car accident in California in November 1938. The article was one of several on index number theory in the journal. The Konüs condition had been mentioned, referring to an article by Konüs on the true index of the cost of living, published 1924 (in Russian). Schultz had however, done as he did for the Slutsky 1927 paper, namely arranged to have it translated and it accompanied Schultz's article. Schultz himself did not read Russian.[33]

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[32] Belz had visited Frisch's Institute in 1934 and published with James (April 1936) an article enhancing Frisch & Holme's (July 1935) analysis of Kalecki's model (October 1935).

[33] Konüs article had been translated by Jacques Bronfenbrenner, the father of Martin Bronfenbrenner. Schultz died before the proofs for the two papers reached him. They were instead proofread by Abraham Wald, who soon after submitted his own paper on the true cost of living, published in 7(4).

Another brief article in the issue was by C.W. Cobb titled *Note on Frisch's Diagonal Regression*, see section 5.11.

The issue comprised articles of limited econometric relevance by E.A. Richardson on controlling the economy; by E.A. Radice an empirical paper on business cycles in England; by Henry Smith on employment; and by Edward Dodd on periodogram analysis.

Some submitted papers by General Motors staff aided Charles Roos who's company had General Motors as client were rejected by the Society, see section 5.13.

Vol. 7(2) - April 1939

The issue was devoted to commemoration of Henry Schultz by the two front articles: Hotelling wrote on the work of Henry Schultz and Paul Douglas on Henry Schultz as a colleague. See section 5.11.

The survey article on statistical data was by C. Bresciani-Turroni (Egypt) on Pareto's Law and the index of inequality of income. Bresciani-Turroni was in the Council of the Society, supposed elected as an Italian member but lived at the time in Giza, Egypt because of the fascist regime in Italy.

In the issue was also the exchange between Frisch and Hotelling after Hotelling's article in July 1938. Frisch had written a 6-page article, titled *The Dupuit Taxation Theorem*, and explained his comment as meant to 'be useful in order to prevent that more to be read into the fundamental theorem of [Hotelling's article] and its conclusions than they really contains' (p145). Then followed Hotelling's response, Frisch's *Further Note* and Hotelling's *Final Note*. This was the kind of exchange Frisch wanted, cordial and constructive.

The issue had an article on Cournot's works by René Roy, given at the Cowles Commission Research Conference in 1938 to commemorate the centenary of Cournot's 1838 volume.

Vol. 7(3) - July 1939

After receiving the July 1939 issue Frisch sent the following comment to Leavens:

> 'I think the July *Econometrica* was very good. Full of life. And with material that was truly <u>econometric</u> using theory and statistics as well. Probably one of the best issues we have had. I should like to have your opinion as well as that of Mr. Cowles and other friends in the U.S. on this. It would be valuable as a guidance in the further editorial policy. Please find out something on this at your leisure, and of course without going at it in any too sensitive way.' (Frisch to Leavens, August 19, 1939.)

The formulation suggests that this was more than just casual enthusiasm over yet another issue published. The issue was 'truly econometric'! Which were the articles that evoked so much enthusiasm? Or was it the overall composition and impression that caused Frisch's exhilaration.

J. Tinbergen and P. de Wolff: A Simplified Model of the Causation of Technological Unemployment.
H. Gregg Lewis and Paul H. Douglas: Some Problems in the Measurement of Income Elasticities.
P. D. Bradley, Jr. and W. L. Crum: Periodicity as an Explanation of Variation in Hog Production.
Gabriel A. D. Preinreich: The Practice of Depreciation.
Gerhard Tintner: Elasticities of Expenditure in the Dynamic Theory of Demand.
Robert Solo: The Demand for Passenger Cars in the United States: A Reply.
P. de Wolff: The Demand for Passenger Cars in the United States: A Rejoinder.

Vol. 7(4) - October 1939

The issue was fronted by an article by K.G. Hagstroem (Stockholm) on the theory of depreciation. Frisch was interested in depreciation and replacement in connection with business cycles. The better known Swedish economists (the Stockholm School) had largely shied away from the activities of the Society. But the actuarian Hagstroem, who had published two papers in Econometrica the previous year, had become a devoted econometrician.

Irving Fisher presented at the Detroit meeting in 1938 paper titled *A three-dimensional representation of the factors of production and their remuneration, marginally and residually*, which he subsequently submitted. Hotelling hadn't attended the presentation but Schumpeter had been there and wrote to Leavens: 'This, of course, is difficult; if submitted we cannot refuse.' Leavens conveyed to Frisch what he took to be Schumpeter's opinion, namely that it would have been an excellent paper a generation ago, but was not of great importance now in view of modern progress in econometrics' (Leavens to Frisch, Jan. 16, 1939). Frisch accepted the paper and sent Fisher a note suggesting some deletions. In the issue was also an exchange Fisher had with Clyde H. Graves over Fisher article on the concept of income in 5(1).

The issue had some notable papers, especially a paper by Abraham Wald on the index of cost-of-living, with reference to his own earlier work in Vienna and to Frisch's double expenditure method.[34] J. Marschak wrote on combining market and budget data in demand studies which was an original idea at that time.[35] A. Smithies (Univ. of Michigan) wrote on maximization of profits over time with changing cost and demand functions applying calculus of variation with reference to Hotelling's use it in his celebrated exhaustible resources article in *JPR* 1931. A final paper was by Francis McIntyre on the undistributed profit tax.[36]

5. Selected editorial matters 1933-39

5.1 Mordecai Ezekiel snubbed!

Mordecai Ezekiel (1899-1974) had presented a paper at the Syracuse meeting in June 1932 and Frisch invited to submit it to Econometrica. The paper submitted was titled *Some Considerations on the Analysis of the Prices of Competing or Substitute Commodities* (the original 1932 title was slightly different) and Frisch sent it to Nelson in the first batch of papers in August 1932 with the following comment:

> 'I have checked the mathematics in Ezekiel, and have also changed the notation somewhat in order to bring it in accordance with the notation of another paper which is to appear on the subject (you will receive ms. of this later).[37] I think I mentioned to Ezekiel in Syracuse my intention of changing the notation and he agreed, if I remember correctly. At any rate I am sure he will agree. Will you be kind enough writing Ezekiel saying that his paper is scheduled to appear in an early issue. This means of course that he must guarantee that the paper will not be published in any other place before it has appeared in Econometrica . This must be made plain to him.' (Frisch to Nelson, Aug. 22, 1932.)

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[34] Frisch had, as he sometimes did, inserted an explanatory note signed 'The Editor' in the article (p329).

[35] Marschak's affiliation was given as Oxford albeit he had decided not to return to Oxford by the time the paper was published. Wald'd affiliation was given as Cowles Commission but he had moved to Columbia.

[36] McIntyre was a former research assistant of Henry Schultz who had joined the Cowles Commission after its move to Chicago in September 1939. Cowles Commission.

[37] Frisch was here referring to a paper by himself titled *The Notion of Elasticity with Regard to a Given Definition System*. Frisch's paper had been presented at ESEM-2 and listed among *Papers to Appear* in Ecm 1-1. But the paper was never submitted despite what he told Nelson.

Frisch's idea and his rationale for changing the notation in Ezekiel was to publish his paper in the April issue together with papers by Ezekiel, Marco Fanno and Umberto Ricci, all dealing with demand issues, describing his own paper as 'a discussion of the fundamental problem approached in Ezekiel's and Fanno's papers'.

Nelson sent the Ezekiel's paper as revised by Frisch back to Ezekiel. But Ezekiel did not agree in Frisch's changes in his notation and conveyed his view to Nelson:

> 'It does not seem to me that anything has been gained by the changes in notation which the editor has made in my paper. Price analysis methods have been developing some 15 to 20 years; something of a definite notation has been evolved in that period. In general, I have followed that notation in writing this paper. In substituting a purely arbitrary notation, I do not see that the clarity has been improved, while those who are accustomed to the familiar notation will find the paper less easy to follow. The matter of notation is, however, relatively unimportant. I can see no reason, however, for excluding an important element in my chain of evidence, as was done on page 9 of my typed copy. My statement is precisely correct, according to R. A. Fisher, the outstanding authority on the significance of results drawn from small samples and on the significance of correlation results of all types. In view of that fact, I see no justification for its exclusion, and must insist that it be inserted.' (Ezekiel to Nelson, Dec. 12, 1932.)

Nelson responded by explaining that all changes in notation or in content in the paper had been made by Frisch and that his own prerogatives in such direction were strictly limited. He quoted to Ezekiel from Frisch's letter of Aug 22, 1932 (as above). Nelson had also sent the paper to Associate Editor Fredrick C. Mills who without hesitation sided with Ezekiel:

> 'The notation should, doubtless, be allowed to stand as it is since it has already been set up. I think that the insertion requested by Ezekiel should be permitted. He has followed standard procedure here. In the absence of any definite statement from Frisch concerning his reasons for deleting this passage I think the author's wishes should be respected.' (Mills to Nelson to Ezekiel, Dec. 27, 1932.)

Frisch yielded but not without marking his dissatisfaction:

> 'With regard to the notation in Ezekiel's paper: the notation as originally used by Ezekiel contained several direct mistakes that could not be left in. In particular he had used the same symbol in different meanings. The notation as modified by me is at least mathematically correct I think, and I believe it is chosen in a way that will appeal to the mathematician. But of course if Ezekiel feels very strongly that he wants to use other letters as function signs instead of those I have used, I see no object provided he carries through a system that is logically consistent and that the change will not involve too much expense. However, I must admit that I do not see the need for making a change. No regard need be taken to the notation in my own paper on the subject. I shall probably have to publish this paper in another journal since there is so much material pressing for publication in Econometrica.' (Frisch to Nelson, Jan. 11, 1933.)

The passage that Frisch had deleted remained in the manuscript. Ezekiel's paper was published in the April 1933 issue.[38] It was a defeat for Frisch, perhaps not a very great one, but Frisch had strong belief in standardization of notation and continued to work in that direction.

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[38] Ezekiel who published much, especially in *Journal of Farm Economics*, did not submit any other article to Econometrica while Frisch was editor, apart from a book review (of Mitchell) and a brief reply to Lawrence Klein who criticized in *Econometrica* a paper Ezekiel had published in AER.

5.2 The Mayer-Knight controversy 1931

Joseph Mayer had presented a paper at the New Orleans meeting in Jan. 1932 and submitted an abbreviated version of it titled 'On pseudo-scientific method in economics.' As Mayer's paper had explicit criticism of Frank Knight Frisch sent the paper Knight in Chicago, inviting him to write a brief comment if he so desired. As Frisch set it out to Nelson:

I plan to offer Mayer an opportunity of making a rejoinder or possibly, as a consequence of Knight's answer, of modifying his statements. The latter alternative would probably be the best. Dr. Ross editor of the [JASA] told me once that he found it very expedient to let opponents thrash out the issue in a more or less private correspondence first, and then publish only the salient points that become crystallized through the discussion. He found that it was much easier to get people to make concessions and thus reach an agreement when they had not made commitments in print. I trust that something of this sort will happen in the Mayer-Knight case. I have asked Knight to forward the material to Hansen, and I have asked [Alvin]Hansen to offer Mayer an opportunity as above explained, returning the material to Hansen who will then form his opinion in the matter and forward to me. Of course, this is not material for the first issue. The earliest it will appear is April. (Frisch to Nelson, Aug. 22, 1932.)

Frisch got the paper back from Hansen with his comments and was not very comfortable. He conveyed to Nelson. 'The matter has been sent back to me from Prof. Alwin[!] H. Hansen. It is a little embarrassing. I have promised Mayer to publish something by him on this subject and I think there are certain points in his paper that are quite good. But other points are rather weak. Hansen is definitely of the opinion that the paper contains weak parts. I shall probably have to go through the matter and return it to Mayer, suggesting further modifications. (Frisch to Nelson, Oct. 10, 1932)

The direct contact between Frisch and Mayer must have taken place at the Syracuse meeting for which Mayer wrote the report. Frisch probably learnt a lesson from having given Mayer too much of a promise to publish the paper. In the end it was published in the Oct. 1933 issue with a brief reply from Knight and an even briefer rejoinder from Mayer. Nelson, who had praised Mayer's Syracuse report (despite its lack of dates) didn't hide his dislike of Mayer's paper: 'I liked his paper less and less each time I saw it. There seemed a faint suggestion of a dog-fight about the whole thing. I hope we avoid such wrangles in the future as much as possible.' (Nelson to Frisch, Sept. 27, 1933). Frisch often gave directions of which paper ought to be up front in the issue but left (and generally liked to see the theoretical surveys prominently placed) but otherwise left the ordering to Nelson, who made a point of placing the Mayer-Knight exchange at the end.

5.3 Frisch rejected 1933!

The Editor's own publications in the journal he is editing may become controversial, especially if it has not gone through normal refereeing procedure. As we shall see Frisch met with severe criticisms on occasion. In 1933 Frisch submitted a paper titled *Changing Harmonics Studied from the Point of View of Linear Operators and Erratic Shocks*. The idea of submitting the paper may seem to have been conceived in the first quarter of 1933 as it was included in *Papers to Appear* in the April issue but not in January. The topic was absolutely central in Frisch's research program. The understanding and explanation of business cycles had been an obsessive interest with Frisch since the middle of the 1920s. At least since before 1930 Frisch had nurtured the idea of business cycles being generated by random shocks on a macroeconomic system that works as linear operators on the random shock. The paper wrote for publication in Econometrica was the general mathematical theory for this kind of phenomenon.

Frisch went ahead with the paper at high speed. In mid-April he wrote to Nelson that he still had hopes of making the July even if it mean that he had to give up page-proofs. Frisch dispatched the MS to Nelson in the first half of August 1933 on the presumption that Nelson should have it refereed and then published in the October issue. Nelson immediately cabled Frisch that the paper would

delay the October issue with a month and a half and advised postponing publication. He sent the MS to Edwin B. Wilson who undertook to referee it. Frisch had chosen with some hesitation to write two of the six chapters with the less mathematically reader in mind while the other chapters were quite demanding. Frisch got Wilson's assessment in a letter from Nelson in mid-October 1933:

> 'Wilson makes the following comments, which I present in an ordered digest, because his letter is long, and, in places, repetitious.
>
> > "I. If the paper is meant for the mathematically trained, Chapter II (or most of it) could be omitted. Even Chapter I could be condensed. Chapter VI could, and should be omitted, since the formulae are available in text books. However, if the paper is designed for readers of little mathematical training, and their working through of Chapter II will really enable them to follow more advanced theories dealt with in Chapters III, IV and V, then Chapter VI should be interspersed properly throughout the text.
> >
> > II. The paper will not appeal to economists. It is really a mathematical paper, whereas the economist is interested in the application of mathematics to his economic problem; and the paper gives no indication of any such application.
> >
> > III. Since the paper is mathematical, and will be read by mathematicians and not by economists, it can be condensed, as indicated, for that specialized type of reader. A journal is not an "educational" project, and "educational" material is generally not read by those for whom it is intended. The inclusion of "educational" material, therefore is a waste and a mistake.
> >
> > IV. On the technical side, "It looks as though Frisch were developing theory on the assumption that his time series was infinitely long. Now it is, from a practical point of view, very doubtful whether a time series is infinitely long. Economic time series might last on more or less comparable bases for a few hundred years, but that would not make very many 10 year cycles, or very many 20 year cycles, and only a few 50 year cycles."
>
> My own resume of Wilson's attitude is: No economist who is not a mathematician will attempt to work through your paper; since it will be read only by mathematicians, the paper can be much condensed; and the attempt to make it "educational" by reproducing elementary explanations is a mistake in policy. I have given you Wilson's reaction this bluntly, because you will want to know how he felt. I return the MS, temporarily at any rate, because if his criticisms appeal to you, you may wish to re-cast the whole article, and possibly you may decide to publish it in a purely mathematical journal rather than in Econometrica.' (Nelson to Frisch, Oct. 19, 1933.)

The paper got a sad fate. It was not re-submitted to Econometrica. Frisch instead decided he would prepare it as a volume for the Cowles Commission Monograph series but it never appeared.[39] Wilson's referee report was fairly superficial with regard to whatever scientific merits the paper had. He was mostly concerned with dilemmas Frisch had had when he wrote, primarily how to cater to the needs also of the less mathematically skilled. But Frisch accepted the conclusion.

5.4 The Leontief incident 1933-34

In November 1933 Leontief sent to the Colorado Springs office a manuscript entitled *Economic Changes and General Equilibrium*, stating in his cover letter that Schumpeter had read it and advised

[39] It would have been Monograph 1. The monograph series was established on Frisch's initiative in 1934, he proposed it as the 'Cowles Commission Monographs in Econometrics.' The name was changed before the first monograph appeared. (Cowles to Frisch, June 28, 1934).

him to submit it for publication in Econometrica. Nelson passed it on to Frisch with the remark that he took 'Professor Schumpeter's suggesting as equivalent to refereeing, and therefore forwarding the paper directly to you.'

Leontief and Frisch were at the time in the beginning of a debacle over Leontief's Kiel dissertation from 1929 (Leontief 1929), which presented a method asserted to allow the estimation of both demand and supply curves from a single set of data. After studying Leontief's paper since it was published and discussing it with several others Frisch wrote the Pitfall's essay (Frisch 1933) to discuss the problem addressed and show that Leontief's method was flawed.[40]

Frisch wrote to Leontief in mid-December acknowledging receipt of the manuscript studied and promised to respond as soon as he had read it. Frisch wrote back already on January 4, 1934 with three pages of comments. The paper has been lost, the only information about is what can be deduced from Frisch's referee statement, which is rendered below in full. The most intriguing aspect of this incident is that from the evidence available the manuscript must have been on input-output economics, and thus would have been Leontief's first paper on this topic in the USA. It appears to been an approach in a direction that was abandoned. There is no indication in Frisch's letter that there was any empirical analysis in the paper but this cannot be entirely ruled out. The title indicated clearly enough a general equilibrium framework. But as we shall see below the paper was revised with the title changes to *Vertical repercussions in a chain of production processes*. Hence it may have been an attempt at formulating the input-output relations in a general equilibrium framework.

Frisch's referee statements on the original and revised manuscript displayed characteristics of Frisch's style in refereeing. Clearly the topic of the paper and Leontief's approach appealed much to Frisch. Seen with Frisch's eye Leontief had both mathematical and notational problems with the paper and Leontief's response seems to have acknowledged that. The first Frisch letter is rendered below in its entirety:

> Dear Professor Leontief,
>
> I have just read the manuscript of your paper submitted to Econometrica. I am very much interested in your general idea of investigating the repercussions from one system to another which may be caused by changes in any one system in a closed circuit. So far as I understand, this is the essence of your approach. This idea, I think, is very promising, and I believe definitely worthwhile to follow up.
>
> The general way in which you have set up the problem is very good, but I am under a strong impression that the whole exposition could in places be very much improved upon, if it were condensed and perhaps accentuated a little more by indicating sharply the object followed within each of the sections. I should therefore recommend that you go over your manuscript once more with this in view.
>
> While reading the manuscript I have taken the liberty to suggest – with pencil marks – certain pages, which I think could be omitted without weakening your whole chain of argument. As a matter of fact, I think your main idea would stand out more clearly if this were done. In places I have also suggested the re-phrasing of certain sentences. Of course you will understand that these are only suggestions - it will be up to you to work out the modifications in detail. In this connection I also want to say that it is primarily in the first part

[40] An 'acrimonious' exchange in the *Quarterly Journal of Economics*, with a response by Leontief responded, followed by a rejoinder by Frisch wrote a rejoinder at the end of 1934 rebuking Leontief, who added 'A Final Word' and Marschak then concluded the exchange, see Morgan (1990, 183-187), Hendry and Morgan (1995, 257-270), also Qin (1989) CHECK. In his rejoinder Frisch had also insulted Harvard University by scathing words about QJE. Schumpeter warned Frisch that he had come close to destroying a great talent and informed him that he was unlikely to be invited to Harvard's tercentenary celebration.

of the manuscript that I have made definite suggestions for modifications. For the last part of the manuscript I have only made very few and very general suggestions.

In addition to the indications I have made with pencil in your manuscript I should like to make the following remarks.

On Page 17 you say that if the particular transformation process is governed by the law of diminishing return, then the derivatives $\dot{f}_i$ will be between 0 and 1. This is true only if the variables involved represent the logarithms of the actual quantities. Of course it is always possible to define the variables in such a way that this will be the case. But if such a definition is adopted then notion of price cannot be defined by the ratio indicated on top of Page 19 in your manuscript. It is possible that this conflict between the meaning of the variables and your assumption about $\dot{f}_i$ can be solved, and that your main thesis can still be maintained, but at least this needs to be looked into very carefully.

There is also another point I would like to mention. On Page 17 you say that the transformation processes can be lumped together in such a way that all the $\dot{f}_i$ become equal. Of course from the purely logical viewpoint this true, but if such a lumping together is done, we may get results that have very little economic meaning. If the $\dot{f}_i$'s shall have economic significance they must be interpreted as marginal costs or something of that sort referring to concrete processes of the type we know from actual economic life, and if they are defined in this way, they may of course turn out to have definite magnitudes.

But this is not all. Even from a purely logical viewpoint, there is something which makes it perfectly artificial to assume all the derivative equal; these derivatives depend of course on the magnitudes of the various variables involved so that for one set of values of these variables one would have to adopt one system of sub-division for the process, and for another set of variables another kind of sub-division would have to be adopted. The whole system of division would simply change continually as the general system of general parameter (prices, quantities, etc.) changes in the market. This leads to so much arbitrariness that I do not see how the idea of equality of derivatives can help to shed light on the normal happenings.

There is, however, no need to assume these various derivatives equal. It seems that your whole argument could be developed without making such an assumption. For instance a large number of these derivatives will be small and possibly negligible by the mere fact that each of the derivatives is a proper fraction. In order to make the product a negligible quantity there is no need to assume the factors so that the product reduces to a power.

This has a consequence for your graphical exposition in the diagrams containing the arrows. The distances between the points of application of each arrow are of course quite arbitrary, so that the smoothness of the curve described by the end points of the arrows is only a conventional phenomenon. I wonder if you could not arrange it in such a way that the distances between the applications of the arrows represented directly or indirectly the way in which the division between the individual processes was determined. This would make your graphical exposition still more striking. Maybe it would also be an advantage to introduce at least some of this graphical analysis at an earlier stage in the development. Personally I find that this graphical exposition helped very materially towards a better understanding of what you were driving at. I think it would also help if you would give concrete examples, for instance from actual production processes where the product of one process enters as factor of production into another process. Some actual numerical examples of such a case and the translation of this into your symbols in a very simple case described in the beginning of your article would help the reader greatly to follow the meaning of your symbols in the later development. May I also make some suggestions of a more typographical sort. If you write a string of letters with a sequence of sub-scripts it

appears in many cases more elegant to start with the smallest sub-scripts and increasing up to the largest, for instance the first term in brackets at the bottom of Page 18 would look nicer, I think, if it were written this way

$$\dot{f_1}\ \dot{f_2}\ \dots\ \dot{f_{k-1}}$$

It will in general be enough to indicate the two first and the last of the items, and there is no need of complicating the sub-script by writing it in parenthesis as you do whenever there is a question of a subscript like *k*-1. In the last term in brackets there occurs another string of derivatives which you have indicated by nine letters and nine dots. This whole string can be indicated in an absolutely clear way as

$$\dot{f_1}\ \dot{f_2}\ \dots\ \dot{f_n}$$

In this where actually <u>all</u> the derivatives in the circuit are involved, you could even use the simple notation Πf.

In the notation of differentials please adopt some more precise symbolism – for instance in certain cases you use d as the total increment d as distinct from partial ∂. In your development there is of course a difference between the operation you denote as d and the one you denote as ∂, the first of the two being "more total" than the latter. But even the first of the operations you consider is in fact a partial derivation since it is a derivation with respect to one single magnitude keeping a series of other magnitudes constant. For instance if you differentiate with respect to x_i you keep, I understand, all the b's constant. You ought to use a notation so that this becomes quite clear – you could, for instance in one case use ∂ provided with an asterisk.

Finally I would suggest that all your formulae are written in ink and be made a little larger so that the type-setting can be easily done.

If you would like to work through your paper once more with these various things in your mind, I should be very much interested if you would submit a new version of it for consideration with a view to its being published in Econometrica. But some such revision appears to me to be essential as I do not think we could accept the paper in its present shape. Your manuscript is returned herewith.

With best regards,

Sincerely yours, Ragnar Frisch

Three weeks later Leontief sent a revised version to Frisch with title as given above. He had adhered to Frisch's advice by dropping the assumption $\dot{f_1} = \dot{f_2} = \cdots = \dot{f_n}$ and changing the mathematical treatment somewhat. Leontief added, however, that it would be rather difficult for him to make new computations and to redraft the graphs, but he offered an explanatory sentence in the text to preclude any misunderstanding.

This was not quite enough for Frisch who responded a few days later

> 'I am sorry to say that I find that your corrections do not adequately take account of the objections I formulated in my last letter. I found therefore that if the material should be utilized at all I had to work through the details myself. This I have done. On doing so I find that your formula regarding the effect on the output x_i is actually correct, but it is developed in an all too cumbersome way, furthermore you have not stuck to your notation consistently but have occasionally for instance let the subscript on *f* denote the number of the factor and on other occasion the number of the product. … In reality the whole thing can be developed rather simply by using the classical rules for the derivation of implicit functions. This I have done and changed the manner of presentation accordingly.

> ‘In the latter part of your paper, more precisely in the part discussing prices, there is a fundamental mathematical error which makes it necessary to scrap this whole part. The error is this: You assume that all the processes work under the application of the marginal remuneration principle. But this assumption is inconsistent with the very basis of all the previous formula, indeed if the marginal remuneration principle is generally applied the system of equations by which the derivatives are determined becomes singular.’
>
> ‘Instead of the last part of the paper I have therefore written a short Section pointing out the true connections between the assumptions. I have also formulated the conclusions in terms of elasticities (not only in terms of marginal productivities) in order to get a statement that is invariant for a change in the units of measurement. In the new formulation there is no need for the graphs that accompanied the first version.’
>
> ‘If you care to have the ms. appear in Econometrica in this new form I shall be glad to accept it. In this case I think there should be added a footnote where you make an acknowledgement to me for instance something like this:
>
>> ”The author is indebted to the editor of Econometrica, professor Ragnar Frisch, for certain corrections and simplifications in the mathematical derivations as well as for many suggestions for improvements in the manner of presentation in general.”’

Two weeks later Leontief wrote back:

> ‘I appreciate very much the trouble you have taken with my Ms. and I am glad to know that you have found that the formula describing the quantity changes is correct. Your remark about the impossibility – under the given assumption – of applying the marginal pricing principle universally is very pertinent, but I do not think this can invalidate the second part of my analysis devoted to the changes of the marginal productivities. My conclusions still hold in relation to the majority of prices which are proportional to the marginal productivities. Indeed, I would be very interested to have your further opinion on this point. I think it would be interesting to drop the assumption of a purely circular arrangement of the elements and investigate the more general cases. I feel unable to accept your kind proposal to publish your version of my article.’

That finished it. No paper was published and Leontief’s careful formulation suggesting an interest in a further dialogue with Frisch was not heeded.[41]

5.5 The 1934 Questionnaire

After the end of 1933 Frisch decided to have a poll about the editing of *Econometrica* among leading members of the Society about how the first four issues. The questionnaire went straight to the key issue:

> Kindly fill in this blank and return it to the Editor of “Econometrica”,
> Professor Ragnar Frisch, Slemdalsveien 98, Oslo, Norway
>
> 1. Have any papers appeared in the first four issues (Jan.-Oct. 1933) which, in your opinion, are not up to the high scientific standard that ought to be maintained in “Econometrica”? If so, name these papers.
>
> 2. Are there any papers which for other reasons should not have been accepted?
>
> 3. Please indicate how you think the space of *Econometrica* should, roughly speaking, be allocated between the following categories:

[41] Nelson to Frisch (Nov. 25, 1933), Frisch to Leontief (Dec. 12, 1933; Jan. 4, 1934; Feb. 8, 1934), and Leontief to Frisch (Jan. 26, 1934; Feb. 27, 1934), Ragnar Frisch collection, National Library of Norway.

i. Historical (The history of econometrics)
ii. General Economics (price and value, static and dynamic analysis of general equilibrium etc.)
iii. Consumption (demand curves, marketing)
iv. Production (cost curves, engineering problems)
v. Banking and money
vi. Business cycle analysis
vii. National and collective planning
viii. Statistical technique
ix. Mathematics and mechanics
x. Bibliography
xi. Other topics

4. Within these various fields, do you think that the main emphasis should be put on theoretical investigations or on numerical and statistical verification?

5. Have you any other suggestions or points which you want to criticize concerning the editorial policy?
(Questionnaire, January 1934)

The questions could have been better formulated to elicit constructive criticism. Most respondents just bypassed the first two questions. The percentage approach to the contents was not very sophisticated either. One who did answer question 2 was Alfred Cowles who mentioned as papers that should not have been accepted both Shohat ('Interpolation, no application to economics suggested') and Mayer-Knight exchange ('too contentious and inconclusive').

Frederick C. Mills returned the questionnaire on Jan. 8, 1934 with the following additional remarks:

I think the first year of *Econometrica* has been a thoroughly successful one - more successful than might have been anticipated when the venture was launched. There were many who thought that the general field in which this publication fell was already well cultivated, and that a new quarterly would inevitably duplicate work already being done. The first year has clearly demonstrated the error of this view. *Econometrica* cultivates a distinctive field in a distinctive fashion. In addition to providing an opportunity heretofore largely lacking for the effective international cross fertilization of ideas, *Econometrica* has been encouragingly original in respect to the general subject matter of the articles included, and the modes of treatment. I congratulate you heartily on the year's work. I have ... hesitation about trying to define the relative amount of space to be allocated to articles in different categories. Within the general field covered by *Econometrica*, excellence of treatment, rather than subject treated should, I suppose, be the main consideration in passing upon articles submitted.

5.6 Roos's criticism of Frisch

Roos was annoyed over Frisch letting Tinbergen publish such a long survey in 1935, but the underlying discontent was with Frisch's even longer article in 1934. Roos must have thought of it as misuse of editorial prerogative and he let out on Frisch in the wake of the skirmish over Tinbergen's paper. He reacted by writing a long letter to Frisch:

'Dear Frisch:
There are times when one can best be of service by leaving the easy path of flattery and taking the more difficult road of complete frankness. Thus, it is often possible to save a friend from embarrassment and to me this is essential to true friendship. Because I feel deeply about you, I am going to speak frankly. I could tell you these things without giving offense if you were here. If anything I have to say offends you, please know that it is because I am unable to say it in the way I desire. [After a brief reference to the Tinbergen issue Roos

added that the problem raised in his cable had nothing to do with the financing of Econometrica.] There was a very much more fundamental issue. Briefly here is the situation.

I. Criticisms of the Editor

1. No less than half dozen members of the Society have questioned the propriety of the journal printing a 96 page article on “Circulation Planning,” written by the editor. One questioning member has sarcastically written the following: “If you were running the country, would you rather have Frisch's *Circulation Planning* or Tugwell's *Industrial Discipline* at you elbow?” My reply, if entirely truthful, would have been that I did not think either represented a very vital contribution to economic knowledge. It seems to me especially unfortunate that by far the longest paper so far to appear in *Econometrica*, of a length greater than is customary in any scientific journal, should come from the editor, particularly since, in my opinion, it does not read like a very important piece of research. I do not mean that is should not have been published, but I do mean that a pair of scissors could have been used to advantage. I assume, however, that for your own protection, you had it refereed and, in that case, evidently the referee did not agree with my judgment.

2. There has been some criticism to the effect that you have failed to recognize the difference between mathematical exercises and contributions to economic knowledge by means of mathematics, and that in your paper on “Circulation Planning” and in papers such as “Marschak's "Econometric Parameters in a Stationary Society” [published in Econometrica 2(1)] certain elementary mathematical exercises which have no real mathematical interest and are far from economic reality are published under the guise of economic theory.

3. Several people have commented on the frequency with which authors' papers have been documented with reference to your own work especially when reasons for these references have not been particularly obvious. In particular, references [in 2(1), 27] to your contribution on dynamics seem rather farfetched, especially since similar remarks had previously been made by Pareto, Evans and others. (…) In this country an editor elected by a scientific society invariably tends to deemphasize his own work. It is generally held that he should be careful to avoid all suspicion of using his position to promote his own scientific reputation. The need for this is apparent when it is considered that many authors, especially the younger ones, are likely to insert references to an editor's work at every slight opportunity in the hope that, by flattering the editor, the paper will be more likely to be accepted.

II. Tinbergen's Paper

I shall now write you concerning some specific criticisms of Tinbergen's paper and in this way illustrate some further editorial problems. Please understand, however, that there are good ideas in his paper and that I am not saying it ought not to be published. I am merely insisting that a pair of scissors would greatly help it. The author has attempted to spread himself all over the field. What he should do is enlarge his manuscript two-fold and publish it as a monograph or else select his material much more carefully.

1. The entire first 35 pages of Tinbergen's paper could be omitted without loss of an important idea. Page 1, for example, lists as "the facts" current production, replacement of factors of production, labor market, credit, bonds and stocks, markets for consumables, raw material markets, etc. Now, may I ask you for whom is this written? Almost certainly 1,000,000 Americans know this and we can scarcely expect to publish econometric papers which will be understood by every man on the street. On page 2 we find “The aim of business cycle theory is to explain certain movements of economic variables.” This profound idea is followed by a lot of talk about endogen and exogen movement. Why not go on and on and split up further each of these types of movement. At least fifty pages could be added by doing this. Most amazing of all Tinbergen lists as subjects: wage earners, bankers, capitalists,

government, consumers and savers, etc., and you instruct the printer to tabulate such elementary stuff.

2. I would think that an annual survey of business cycle theory would treat only material which was one, two or three years old at the most. It theories as old as 1920 are to be considered, why not discuss ancient Chinese theories of depressions?

3. Why should Tinbergen review Hayek's theory unless he feels that Hansen did a poor job? He does not say that Hansen did so. The same criticisms applies to his consideration of Keynes' theory.

4. Tinbergen's discussion of the “labor market” is entirely incorrect and superficial. No mention, whatever, is made of the units of labor (30, 40, 50 or 60 hours per week) and yet Tinbergen says: "Wages themselves will then move inversely proportional to the integral curve of excess of unemployment.

To recapitulate, omission of considerable material would not detract from the interest of the paper and would considerably help the reputation of the author, the journal, and, indirectly, the editor.
I could make many other criticisms of this paper citing examples from my book, *Dynamic Economics*, Bloomington, 1934,but Tinbergen did not have a chance to see this book and consequently references to it should not appear in his paper. This invalidates his criticism of my work, but all that is necessary to protect him is a statement to the effect that he had not read *Dynamic Economics*.

III. Observations on Editorial Problems
1. Once Tinbergen's excessively long paper is published, other members of the Econometric Society will feel justified in submitting lengthy (and in place elementary) articles, since the editor published a 96 page article in July 1934 and Tinbergen a 60 page article in April 1935. I can see no end of embarrassing situations for you, as editor. Furthermore, these situations will be developing at a time when Cowles has urged that the journal be placed on a self-supporting basis in the next few years.
2. The fact that a donation has been received covering part of the cost of printing Tinbergen's long paper does not suffice. Do you intend to state in the April number that it is the policy of the journal not to publish articles in excess of 25 or 30 pages unless contributors or their friends pay for pages in excess of that amount? I can hardly imagine successful operation of a first class journal on such a basis. If you do not make a statement of this nature, you will certainly invite additional long papers.
3. My belief is that most of the space of the journal should be devoted to papers with new ideas reconciling economic reality with theory through recourse to statistics and mathematics rather than to lengthy surveys. If it is to be devoted principally to mathematical “theories”, without regard to their relationship to economic reality, there is no limit to its possible size.

On several other occasions I have written parts of these criticisms, but rather than risk the possible wounding of your feelings have destroyed my letters. Please understand that I am trying only to be helpful in the belief that our mutual regard is sufficiently strong to survive a critical letter inspired by friendship.

Mary and I are both looking forward to an opportunity to see you this summer.

Always, Your sincere friend,

Charles F. Roos
(Roos to Frisch, February 13, 1935**).**

Frisch answered soon after:

‘Dear Friend Roos,

Thank you for yours of February 13th. Let me tell you first that I appreciate very much your letter and the motives behind it. This is the kind of way a friend writes to a friend. Before entering into details let me say also that there is much in your criticism which I think I accept and by which I hope to be able to profit. On certain points, however, I am not able to agree with you, at least not entirely.

1) I agree that I should probably not have let my “Circulation Planning” appear in *Econometrica*. I do not think I would consider it as a single paper: it was rather two papers, but even so it was of course very long. So much for its length! With regard to its scientific value, I should not judge this myself. I have noticed your opinion but have also received others of a different sort. If the paper has any merit it is of a technical sort. Whether or not somebody speaks sarcastically about “running the country” and having Frisch or Tugwell “at his elbow” has very little to do with these technicalities. When it comes to questions of politics opinions pro or con may take many forms. But I do not think I would take them as reliable judgments on scientific merit.

2) With regard to distinguishing between mathematical exercises and contributions to economic knowledge by means of mathematics, it may perhaps be difficult in all cases to establish a criterion which everybody would agree to. You speak of Marschak's “Econometric Parameters” as being mathematical exercises. Allow me to say that I disagree. It is true that he does not use his mathematics to deduce any very striking conclusions. But he uses them for a purpose which I think is also rather important, namely that of defining precisely the notions involved and the fundamental relations between them. If you take most of the current work on monetary theory (say Hawtrey and Robinson in recent numbers of the Economic Journal) you will find that three-quarters of the papers are taken up by perfectly elementary exercises on simple algebra. This is indeed “mathematical exercises”, but still I believe they are valuable. I happen to have worked a good deal myself recently on monetary theory, so I think I can appreciate <u>why</u> these people do all these elementary mathematical exercises. They feel that the urgent need of the moment is to work towards a clear system of conceptions. Just as a comparison I may tell you that some years ago a very prominent member of our Society expressed to me the opinion that your paper in the Chicago Journal was only “Mathematical Exercises”. I explained to him that I thought otherwise, and I think that he somewhat changed his mind after our conversation. This is just to indicate that opinions may differ. If the objections of “Mathematical Exercises” should be directed against a paper in E, I would personally rather pick out the paper by Vinogradoff on technological improvement [in 1(4), 410-7].

3) I am sorry if somebody feels that authors in Econometrica have given too frequent references to own works. With regard to Tinbergen's reference to my definition of dynamics, I should like to point out that the essence here is of course not the occurrences of time derivatives, time lags, etc. in the equations. The essence is that my distinction between statics and dynamics is not a definition between two sorts of phenomena, but a distinction between two sorts of theories. Consequently, stationary and static become two very different things, also changing and dynamic become two different things. In my opinion the confusion between these two sets of ideas explains why there has been so much discussion of what dynamics are. The only way to get order and system into this field is, I believe, to carry these ideas through strictly. This I did in the paper in *Nationaløkonomisk Tidsskrift* 1929, to which Tinbergen refers. I use some 20 pages discussing the distinction. I have not seen that this point has been developed by Pareto, Evans and others. I wish you would give the exact references. On the contrary, I have found that there is nearly always a confusion between the notion of changing and "dynamic". Speaking about the nature of dynamic economics in the introduction to your "Dynamic Economics" you, too, adopt this point of

view, it seems to me. If you will allow me ta say so, I got the impression when I read this that it was a rather superficial description of what dynamics are. I am sorry that in Tinbergen's report too much emphasis had been put on the relatively unimportant question of whether time derivatives or time lags, etc. should occur in the equations. It would have been much better if he had stressed the former point of view: the distinction between two sorts of phenomena on the one hand and two sorts of theories on the other.

These questions of what dynamics are of course not of avail if we take them just as a question of terminology. The various parts of the theory may of course be called by any names we choose, but they have some importance I think from the new point of classification. If dynamics are defined the way I prefer, "dynamic laws" will now have certain properties very distinct from the "static laws". Amongst others, if all the laws included in a theory are static, we cannot make a system determinate except by locking it, while this does not hold good if at least one of the relations is dynamic.

Although I cannot accept all particulars of your criticism of specific instances regarding references to my own works, I am very glad that you have mentioned this. It has made me understand certain things of which I was not aware. In the first place I shall be careful to delete references to my own work in cases where it is not absolutely unavoidable, and in the second place I think it will be wise to find an outlet for my own papers in other journals than *Econometrica*.

4) With regard to Tinbergen's big paper, I think the difference of opinion in this case is to a considerable extent a difference between what I would term the American and the European viewpoints. Our views on Tinbergen's enumeration of the main variables or groups involved are typical of this. For instance, when Tinbergen lists wage earners, bankers, capitalists, governments, consumers and savers, and I instruct the printer to tabulate these, it is of course not because it is something sensationally new to tell the people that in economic life such a thing as a banker exists or that a capitalist exists, etc. If looked upon from this point of view it appears of course only as “elementary stuff” as you say. The point is that, by basing a theory on a certain set of variables picked in this list, you will get one type of theory, by picking other variables you will get another type , and enlarging the list to all those tabulated you will get a fairly complete macro-dynamic theory. (Be kind enough to interpret my expression “fairly complete” with a grain of salt). I can explain this attitude towards the theoretical set-up in no better way than by referring to Ricardo's work on rent. He considered the landowner the farmer and the worker. In any survey of his theory it would certainly be justified to list these three, even to tabulate them. The connoisseurs of classical theory would I think admit this and know why. The selection of this list defines a particular type of theory which we call Ricardo's theory of rent. If the list is taken as a description of the economic life in England in those days, it becomes certainly “elementary stuff”, which probably, not only one million, but the entire adult English population knew. I do admit that in places Tinbergen's developments are too wordy. That is also why I asked Nelson to try to reduce it.

5) Regarding the editorial policy, I think I agree with you that in future we should in general try to keep the size of the papers down. If a paper is very long we should rather split it, giving the various parts different titles. I also agree entirely to the view-point that the type of material that comes most definitely within the scope of the journal are papers that try to reconcile theory with economic reality with recourse to statistics and mathematics. This type of paper, however, is by far the most difficult to obtain. At least it is difficult to obtain good papers of this type. That was just why I was so eager to get some of the econometric material that had been brought together in the department in Washington. I wish you would now and in the future keep an eye open for material of this sort and suggest its submission to *Econometrica* whenever you think you have found something really worthwhile.

> In concluding I want to repeat that I am really thankful to you for writing your letter of February 13th. I fully appreciate the friendly and constructive motive behind it, and I can assure you that there is no question of your letter having wounded my feelings, or anything of that sort.
>
> With cordial regards to Mrs. Roos and yourself, always

Your sincere friend, Ragnar Frisch
(Frisch to Roos, on March 3, 1935).

5.7 The Leontief-Staehle incident 1935

Wassily Leontief presented to the Colorado Springs meeting in June 1935 a paper titled *Composite commodities and the problem of index numbers*. Roos and Hotelling were both favourably impressed by the presentation and suggested it be submitted to Econometrica. Leontief's manuscript arrived on Nelson's desk on July 12, 1935 and on the same day also the latest issue of the *Review of Economic Studies* containing an article by Hans Staehle titled *A Development of the Economic Theory of Price Index*. The two articles seemed to Nelson to overlap in content. He vented his quandary to Frisch:

> 'In running through Staehle's article, I find no reference to Leontief's work. 'A rather delicate situation thus arises. In the circumstances, I am sure you will wish to go through both articles carefully yourself in order to arrive at some judgment as to the independent value of Leontief's paper. If Staehle and Leontief both have reached new conclusions, then, of course, publication of the report of the Colorado Springs meeting will give Leontief some priority. If Staehle seems to have appropriated Leontief's material without acknowledgement, an awkward situation emerges. I am, therefore, referring the whole matter to you.' (Nelson to Frisch, July 7, 1935).

And he added. 'I should say that all of us here, after meeting and talking with Leontief several days, formed a very high opinion of his capacity and personality.'

Frisch responded after receiving and studying Leontief's paper:

> 'I find Leontief's paper very interesting. It is accepted for publication in Econometrica. In view of the appearance of Staehle's paper, I have as Editor appended a footnote to the MS, giving, amongst others, an excerpt from Leontief's letter to Cowles and information about the fact that Leontief's paper was presented on June 24th before the American meeting. I think it is fair to give this information in order to protect Leontief's priority. … In view of the delicate situation regarding this MS I have preferred not to do any editing at all. If there are verbal or other slight changes or condensation which you as Assistant Editor would suggest, please communicate direct with Leontief. Please also try to get this in an early issue, preferably the January issue. I have preferred not to write either to the *Review of Economic Statistics* or to Staehle.' (Frisch to Nelson, Sept. 9, 1935).[42]

This was a rare case of priority conflict in *Econometrica* and just a touch of it. Staehle was on his way to become a renowned expert on index numbers.

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[42] The footnote Frisch added in Leontief's article was as follows: 'This article was completed more than a year ago. In the fall of 1933 it was communicated to professor Schumpeter's Discussion Group at Harvard and on June 24, 1935 it was presented at the meeting of the Econometric Society in Colorado Springs. The June 1935 issue of the Review of Economic Studies contains a paper in Index Numbers by Dr. Hans Staehle in which the treatment of several topics is very similar to that given in the present article.' (Econometrica 2(1), 39).

5.8 The re-issue of Eugen Slutsky's 1927 paper in 1937

Eugen Slutsky's article 'The Summation of Random Causes as the Source of Cyclic Processes' which appeared in Econometrica 5(2) is one of few cases of a published paper being translated and republished in Econometrica. It was originally published as Slutsky, E.E. (1927), The Summation of Random Causes as the Source of Cyclic Processes [in Russian]. *Problems of Economic Conditions*, 3: 34-64. But it was more than a translation. Slutsky reworked the paper and made many extensions and enhancements. It was however published with the same title as in the original paper.[43]

Slutsky's article was published in the journal of the Conjuncture Institute in Moscow, where Slutsky worked at the time. The Institute was headed by N. D. Kondratiev. Kondratiev's work had attracted great interest and the business cycle institutes at Harvard, in Berlin and many other places subscribed to the journal of the Moscow Institute, which equipped the journals with extensive summaries in English. Slutsky article thus had a five-page summary in English which gave a comprehensive impression of the content of the article even without a translation. A number of scholars, including Ragnar Frisch and Henry Schultz, immediately acknowledged the importance of Slutsky's analysis and results.

Slutsky is known in economics mainly for two papers. Even better known than his 1927/37 paper is his famous 1915 paper on demand theory, 'Sulla teoria del bilancio del consumatore', which appeared in a Italian journal but remained virtually unknown outside Italy until the 1930s. According to Slutsky's biographer, Vincent Barnett, "a good case can be made for the notion that Slutsky is the most famous of all Russian economists, even more well-known [than] N. D. Kondratiev, L. V. Kantorovich, or Mikhail Tugan-Baranovsky. There are eponymous concepts such as the Slutsky equation, the Slutsky diamond, the Slutsky matrix, and the Slutsky-Yule effect … Slutsky's work is thus an integral part of contemporary mainstream economics and econometrics, a claim that cannot really be made by any other Soviet economist, perhaps even by any other Russian economist." Barnett (2007).

But how did the article come from the Moscow journal to Econometrica? It was mainly due to Ragnar Frisch and Henry Schultz (in addition to the translator E.V. Prostov). It has sometimes been asserted that Henry Schultz was fluent in Russian but that is incorrect. He had roots in Poland and no acquaintance with Russian. The interval between the two editions of the Slutsky article comprised the organization of the Econometric Society and the establishment of Econometrica but also the excesses of Stalin's terror regime in the USSR. Both Slutsky and Kondratiev were invited to join the Econometric Society. Kondratiev accepted, Slutsky never responded. When the election by the Council of the first batch of Fellows of the Society was prepared in 1933 Frisch had nominated Slutsky unaware that he was not a member and this ineligible for fellowship. Kondratiev was elected as Fellow; the Council of the Econometric Society was unaware that he was imprisoned and incommunicado at that time and was executed in 1938. Also other colleagues of Slutsky became victims of the terror. We can shed no further light on why and how Slutsky escaped theory fate. His avoidance of accepting membership in a foreign organization may be viewed as a protective measure. He did, however, not refrain from corresponding with Frisch and Schultz about his article, which he finally submitted in October 1934.

Frisch and Slutsky had been in touch since around 1925, exchanging papers and ideas. Frisch was one of very few who had received from Slutsky (in 1926) the Italian journal issue with the 1915 article. Slutsky was also in a small select international group of scholars who Frisch consulted in 1926-27

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[43] An oddity is that in *Econometrica* the author's name was rendered as 'Slutzky'. It seems that this may have been a mistake by Henry Schultz which Frisch as editor accepted without further checking. A correct transcription would have been Slutskii. Also the use of Eugen in English may seem odd, as the standard rendering in English would have been Eugene. But this latter oddity can be traced to the fact that Slutsky's 1915 article was sent as manuscript in German with the author's name given as Eugen E. Slutsky to the Italian journal which translated the article but did not adapt the spelling of the author's name.

about the idea of establishing an econometric society and a journal. Slutsky sent Frisch a reprint of the 1927 article in a letter dated May 5, 1927.[44] He indicated in the letter that the article constituted the first part of a larger study. Frisch who had just arrived in the United States on Rockefeller Fellowship responded immediately: "I am sorry that I am not able to read "The Summation ..." in extenso. Anyhow the summary is sufficient to show the extreme importance of your problem. I have found your treatment is very suggestive indeed. It can be no doubt about the fact that you have here a very fruitful idea, the following up of which seems highly promising. I am anxious to know of the publication of the next part of your study and hope that you will provide for an English, French or German edition of the complete study when it is finished." (Frisch to Slutsky, undated but likely June 1927). At Slutsky's request Frisch supplied in the same letter a long list of scholars in several who might be interested in the paper.

In Slutsky's following letter which for the first time was written in English he made a remark at the end about Frisch plan and vision of journal which Frisch at the time denoted Econometrika: "I allow me to utter some doubts about the terminus Econometrika. I believe that it would be better to change it for Economometrika for there are two distinct Greek words: the first ending and the second beginning with the same letter 'm'. I fear that every philologist will find the omission of one 'm' impossible and the word itself barbarous, —if you kindly permit me to use the right word in its technical meaning." (Slutsky to Frisch, July 4, 1927).

As this correspondence evolved near the peak of Frisch's interest in time series analysis he gave Slutsky's work immediate attention, cited the article and drew on it on a number of occasions in the ensuing years, including in his Nobel laureated 1933 "Propagation and Impulse" essay. Frisch and Slutsky continued to exchange reprints and occasional letters.

Henry Schultz whose fascination with Slutsky (1927) may have been no less than Frisch's initiated a translation of Slutsky's paper in 1930 or early 1931. It has not been possible to ascertain to what extent he was in touch with Slutsky about it. Schultz may have wanted a translation for his own interest and for teaching purposes without plans for publishing it. Schultz mentioned the initiative he had taken to Frisch sometime in 1931. But as soon as Frisch had been elected editor of *Econometrica* at the beginning of 1932 he put the Slutsky article on the top of his list of articles he wanted to publish. At Frisch's request Schultz sent him in March 1932 the translation and mentioned to Frisch that he was about to write to Slutsky to ask him to review the translation, and also to ask his permission to use it in class. The translation Schultz may well have been a carbon copy and Schultz asked to get the translation back "at your earliest convenience".

Frisch moved swiftly. He quickly read through the translation and within days he wrote to Slutsky, told him about what Schultz had done, adding: "I have read the copy with great pleasure and find that it contains valuable material of which no adequate account is given in the short English summary. I therefore write you today to ask if you will let us publish this paper of yours in English in "Oekonometrika", the newly founded journal of the Econometric Society, of which I have been elected the Editor in Chief." (Frisch to Slutsky, March 31, 1932.)

Slutsky responded without delay: 'Certainly I should be very glad to see my paper "The Summation of Random Causes etc." published in *Oekonometrika*. I trust that you would have no objection to let me supplement it with some short comments and additions as may suggest themselves to me going over the MS of the translation. Will you agree with me that one can be practically sure that Prof. Schultz

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[44] Slutsky apologized for having neglected for a while the otherwise frequent correspondence with Frisch and offered the following explanation: "Ich bitte Sie doch dieses Versäumnis nicht als eine zu grosse Sünde zu betrachten, da ich während aller dieser Zeit so mit einer Arbeit eingenommen wurde, so in der Arbeit lebte, dass ich für Alles in der Welt fast vollständig blind und stumpf wurde. Krank war ich — fast könnte man sagen — mit dieser Arbeit: meine Freunde scherzten, dass ein Wurm „Gordius stochasticus" [here Slutsky referred for illustration to Figure 10 and 12 in Slutsky (1927)] eines Ihnen mit derselben Post zugesandten Aufsatzes) mir an der Mark frass und mein Blut saugte" (Slutsky to Frisch, May 5, 1927).

will have no objection to this, you will perhaps find it convenient to send me the MS, as it will come to me from Oslo at a much earlier date than from Chicago." (Slutsky to Frisch, April 9, 1932). Frisch followed up and indicated clearly he wanted the paper published as soon as possible: 'I send you enclosed a copy of the English translation which I received from Professor Henry Schultz. Will you please make the desired modifications or additions and then return the material to me at the address Storgaten 9, Oslo. Would it be possible for you to prepare the tables exactly by clipping out from reprints of your original paper and pasting the columns into the tables so that they could be sent directly to the printer without my having to go through this part of the paper thoroughly?' (Frisch to Slutsky, April 19, 1932).

It took time however for Slutsky to revise the paper. The next message from him was not until a year and a half later: According to your friendly suggestion to publish my paper „The Summation of Random Causes etc." in *Econometrika* I send you now enclosed the MS revised and enlarged with Tables and Figures ready for rep*ro*duction. I am very sorry indeed not having been able to accomplish my task earlier and I shall be very obliged to you for your kindly communicating to me concerning your disposition about the MS. (Slutsky to Frisch, October 12, 1934). Slutsky wrote later to Schultz and stated that the correction of the translation, the revision of the text and the writing of supplements had cost him more time than the writing of the original paper.

Frisch responded quickly but with less enthusiasm and drive: 'I am afraid that considerable time may elapse before your paper can appear in our columns. At the moment we are literally swamped with manuscripts, most of which are of a very high calibre, and unfortunately our financial resources are very limited, so that we are not in a position to extend the volume of the journal as fast as we should wish.' (Frisch to Slutsky, October 24, 1934). Frisch may have been a bit under pressure at this time both from his own commitments and from Cowles' worries about the financial costs of *Econometrica* and expressed requests for financial support from other sources. He added, not very wisely perhaps, in his response to Slutsky: 'Do you know of any scientific funds in your community that may be sufficiently interested in the matters of the Society to lend some financial support for our publication. If such support were given we would of course be glad to publish in our columns a notice regarding this fact.' (op.cit.).

In a missing letter Slutsky seems to have worried about whether it would take years to see it published. Actually, it did take years but at the moment Frisch tried to smooth out the issue and at the same time let Slutsky in on why the paper was so important for Frisch's own research: 'Of course there will be no question of years before your paper will appear, but it may be several months. I should say that it will probably be incorporated in one of the last issues in 1935. … As you know, I consider it one of the outstanding contributions in this field which has been received made for the last years. … I believe I have solved in a fairly complete manner the problem which was still left in suspense after your paper, namely what sort of cycles will be created by an accumulation of an arbitrarily given weight system. And, furthermore, I have tried to build a synthesis between this mathematical statistical view-point and the view-point of macro-dynamic economic theory. It appears indeed that what dynamic economic theory gives us is not the time shape of standard curves with which the empirically observed time series are to be compared, but it gives us the weight system by which to perform the accumulation. The fundamental problem therefore rests on what is the harmonic nature of the time series produced by accumulation according to such a pre-assigned weight system.' (Frisch to Slutsky, December 14, 1934).

The pace slowed considerably at this stage. At the beginning of 1935 Slutsky sent a brief correction (suggested by an article by R. von Mises in an Italian journal). That was quickly but another year passed before Frisch could tell Slutsky in June 1936 that the paper was being set up in type and again apologize for the long delay due to lack of space. When it eventually appeared in April 1937 the article had been equipped with the following introductory note: '[Slutsky (1927)] has in a sense become classic in the field of time-series analysis. While it does not give a complete theory of the time shape that is to be expected when a given linear operator is applied to a random (auto-non-

correlated) series, it has given us a number of penetrating and suggestive ideas on this question. It has been, and will no doubt continue to be, highly stimulating for further research on this vast and—not least for business-cycle analysis—most important problem. … At the request of the Editor Professor Slutzky has prepared for our Journal a revised English version with which he has incorporated also a number of important results obtained after 1927.— EDITOR.' The note oddly claims that it was Schultz' initiative to have the paper published in Econometrica, although the record clearly shows that this was Frisch's idea.

5.9 Wilson on Samuelson 1937

Paul Samuelson presented his first paper at an Econometric Society meeting in Atlantic City, Dec. 1937, see Econometrica 6(2), 180, and submitted it to Econometrica shortly afterwards. The paper was entitled *The Empirical Implications of Utility Analysis*. Leavens gave it to E. B. Wilson who submitted the following statement:

> 'I know of Samuelson's work. He is a very good man and careful. I asked him to do two lectures in my course on mathematical economics last year and he did a beautiful job both with respect to the selection of his material which was mostly his own and with respect to his presentation of it.
> I have already looked over his Empirical Implications of Utility Analysis which he wishes you to print and I don't see anything the matter with it. I don't believe that any further examination by me of this paper which you hold and which I have in carbon copy would give me any more light on the desirability of your printing it. Of course anybody can make a mistake and in this kind of work it is very difficult to be sure that an author hasn't somewhere slipped over on himself through inadvertence. Only a most profound study of the subject including some original developments of my own would give me any real confidence in my ability to guarantee everything. I rate Samuelson very highly and think he has struck a good line here and believe he has done a thoroughly good job. He is aware that there are some criticisms to his approach to the subject and he has considered these criticisms carefully and unemotionally and I believe that it would be a good scheme to print his paper.'

Wilson's statement was more characterization of Samuelson than a referee statement. He didn't really say much about the content of the paper. Frisch accepted it immediately with no further comment apart for a remark that Samuelson should complete the references, in particular to his own work. The paper was published in the October issue, 1938. This was Samuelson's only paper in *Econometrica* before the war.

5.10 The Duncan case

INCOMPLETE

5.11 C.W. Cobb on Frisch's diagonal regression 1937-39

Cobb submitted the manuscript in July 1937 and the managing editor, D.H. Leavens, sent it to S.S. Wilks for review. Wilks wrote a brief reports which Leavens wrote into a letter to Frisch at the end of August 1937:

> 'I have read the paper over carefully and essentially all that the author does in it is to point out a few rather self-evident connections between the coefficients and other features of Frisch's diagonal regression equation and the classical least-square regression equations. There is nothing very original and fundamental in the paper. In my opinion the paper does not constitute a contribution of sufficient importance to be published in Econometrica.'

The rest was up to Frisch but he hesitated for long time in taking a decision. Leavens reminded Frisch again in December, January, February, March and then in June 1938. Frisch responded to the latter letter that he would settle the matter as soon as he came down from the mountains.

In July 1938 Frisch wrote briefly to Leavens:

> 'On second reading I find that Cobb's paper, contains a little more than I first saw. I think it is O.K. to publish it. But as you see I have made a little verbal change in the beginning which make it much less presumptuous. I have written Cobb; I am confident that this small change will meet with his approval.'

Eventually the article was published in January 1939. Frisch had thus dragged it out for a long and decided against the advice of the referee, but perhaps not unreasonably so.

But there is a tail to this story as Frisch a year and a half or so later received from Leavens a submission by Elliott B. Woolley titled *The method of minimized areas as a basis for correlation analysis*. It was sent in July 1940 but took its time on its way to Frisch in German occupied Norway. Leavens had let Schumpeter and also W.L. Crum (Harvard) review the paper while Norway was for some months unreachable by mail after the German attack in April 1940. In fact took over as de facto editor in this brief period.

Frisch responded by letter in December 1940, stating clearly that, 'The paper cannot be published in its present form. It does not fit up with previous work. As a matter of fact the author seems to be unaware of much of previous literature. I shall go over the MS carefully and send you my suggestions. The paper <u>does</u> contain some valuable material….Please communicate this to the author.'

But Woolley's was by then virtually in print. It appeared in January 1941. Leavens explained in January 1941 that the manuscript was accepted

> 'on the basis of Professor Schumpeter's recommendation. I am sorry this happened, but as in July we had to fill the issue or face a delay of undetermined length. Fortunately this is the only case in which Schumpeter's decision has disagreed with yours. I think I will not write the author your remarks unless and until I receive from you further suggestions. Then I will send them on to him, and it might be that he could write a supplement, acknowledging your criticisms and making such additions as seemed in order.'

A few days later towards the end of January Leavens wrote again:

> 'In further regard to the paper by Elliott B. Woolley, "The Method of Minimized Area as a Basis for Correlation Analysis," which was published in the January issue on the basis of recommendation by Professor Schumpeter (and also Professor W. L. Crum), Mr Haavelmo has pointed out to me that the first part was essentially your method of diagonal regression. And now a note has come from Professor Paul A. Samuelson, pointing out the same thing, and adding certain desiderata for regression methods. I enclose this, which he submits for publication, this is a carbon copy; so you can send a decision by air mail without returning MS.
>
> 'Probably you will already have returned the Woolley MS with your comments made before you knew the article was appearing in January. Presumably it might finally be in order to publish Samuelson's note; a note by you commenting on the Woolley paper as printed; the whole introduced by a footnote signed by me, saying that the Woolley paper was published without your having had the opportunity to see it, because enough material seen by you was not at hand. I can merely say that it had been recommended by two referees without bringing in the names of Schumpeter and Crum. It would seem best to hold everything till I hear from you in answer to this letter and close up the matter in one issue of the journal, instead of letting it string along.'

Thus Haavelmo and Samuelson immediately recognized that Woolley had rediscovered Frisch diagonal regression (which was not well published) and obviously not read Cobb's paper either

Samuelson quickly written note was published in January 1942.

(Leavens to Frisch, Aug. 2, Aug. 26, Aug. 31, Dec. 7, 1937; Jan. 17, Feb. 2, March 1, June 21, 1938; Jan. 23, 1941; Frisch to Leavens, July 7, July 11, 1938; Dec. 12, 1940).

5.12 Henry Schultz in memoriam 1939

Henry Schultz died at the end of November 1938. Leavens conveyed the sad facts to Frisch a few days later:

> 'You will be very sorry to hear about the death of Professor Henry Schultz, with his wife and two children, in an automobile accident near San Diego, on Saturday, November 26. His car, travelling rapidly, got off the road, and was hurled over a cliff. Two of them died instantly, the other two on the way to the hospital.' (Leavens to Frisch, Dec. 2, 1938).

Frisch and Schultz had been friends since they met in 1927. They had met quite a few times at Econometric Society meetings and kept in frequent contact by correspondence. Frisch had written to Schultz one month before the accident to tell him that Konüs' article and Schultz' comment on it, were both accepted.

A memorial session was arranged at the first opportunity which was the forthcoming meeting of the Society in Detroit at which a special session was held on the afternoon of December 29. Memorial articles by Paul H. Douglas, *Henry Schultz as Colleague*, and Harold Hotelling, *The Work of Henry Schultz*, were published in the April 1939 issue of Econometrica.

But it did go so smoothly. Hotelling may have dealt with the works of Schultz in exactly the same as he would have done if Schultz had been alive, and some reacted to that. Two weeks after the Detroit meeting McIntyre NAME-AFF who had been Secretary of the Program Committee wrote to Hotelling:

> ''Several persons came to me the morning following the joint session and indicated their surprise that a memorial program had been the occasion for a criticism of Professor Schultz' mathematical shortcomings and the weaknesses of his "dynamic" approach. It is no doubt true that the several references you made to your own excellent contributions in these fields tended to place a sharper emphasis than you planned on your implied criticisms of Schultz. You may have noted that even Professor Douglas placed this interpretation upon your remarks in his comments which followed yours. I have no doubt that the minority that so misunderstood your intention was small, but I wish to raise with you the question of whether you might wish to alter a phrase or two of you paper in order to avoid spreading these false impressions through the printed page.
>
> Entirely apart from the above considerations is the question whether the remarks of Schultz' former students deserve as much space as you have so graciously given them. Would you, for example, be willing to edit or have us edit those portions of Hartkemeier's remarks which describe in great detail the number of units in a regular course load at Chicago and the confusion of digits and worksheets?' (McIntyre to Hotelling, January 14, 1939.)

Frisch did not find anything in Hotelling's statement about Schultz as a mathematician which would make anybody take offense, and after accepting he left it to Leavens to see to it that some of the formulations could be changed (Frisch to Leavens, Jan. 31, 1939). Hotelling revised his remarks and Leavens passed on to Frisch that 'Hotelling took this entirely in the spirit in which it was sent' (Leavens to Frisch, Jan. 20, 1939).

Douglas also got into problems as he had used formulations some found unsuitable in Econometrica. Leavens. Only after Frisch had accepted Douglas' paper he received Leavens' comment that 'Douglas' remarks at the end about European dictators may possibly be a little strong for an international

society to publish'. Leavens added in a PS he had already mentioned to Douglas the possibility of toning down his final remarks and received his reply as follows: 'The remarks about dictators come from my heart, and I want the Europeans to see them. I, therefore, do not want to withdraw any of my comments unless I am forced to do so. I dislike being so blunt, but it really seems important to me as it would, I believe, to Henry Schultz.' (Leavens to Frisch, Feb. 4, 1939).

Frisch heard directly from Douglas about his formulations and took the view that '[Schultz] would like to have the phrasing in his Schultz paper as it stands, and if he believes that this was important to Schultz and a characterization of Schultz, it should be left in.' Frisch found the case 'exactly similar' to the Amoroso paper on Pareto, 'which sounded like a fascist propaganda, but which it would not be correct for the editors to object to because it was presented as a characterization of Pareto' (Frisch to Leavens, Feb. 2, 1939). This was of course a remark that served to defend Frisch for the criticism he received for the Amoroso article.

Leavens suggested again to Douglas to tone down some of the remarks but to no avail. He discussed the matter with Cowles and sent the Douglas MS to Schumpeter to ask his advice. Schumpeter expressed strongly that formulations in the article should be deleted, including the last paragraph which Leavens was prepared to keep. After talking to Cowles about again Leavens decided to defer publication until Frisch had opportunity to look at it again. Cowles talked to Douglas on the phone and next morning Douglas yielded, accepting Leavens' suggestion of

> 'substituting "dictators and militarists" for "Hitler, Mussolini, and Japan" in the third from the last paragraph, and also substituting "totalitarianism" for "fascism" there; he said nothing about the last paragraph, however. (Leavens to Frisch, March 14, 1939).

After one further round via Cowles Douglas agreed in deleting the last paragraph, as his own comment and better omitted in an international journal.

5.13 C.F. Roos and General Motors rejected 1939

Charles F. Roos who had played a central role in the foundation of the Econometric Society and became its first Secretary had joined the National Recovery Administration as director of research in 1933. He soon lost enthusiasm for NRA and left in 1934 and became research director of the Cowles Commission at Colorado Springs. He left that position at the beginning of 1937 and established the Institute of Applied Econometrics in New York City. The Institute was not so much devoted to promoting applied econometrics as the economic interests of Charles Roos. He attracted powerful corporate clients including General Motors. At the Econometric Society meeting in Detroit in December 1938 Roos took control and arranged two sessions under the common title of "The Dynamics of Automobile Demand", which was not planned by the program committee comprising Schumpeter and Francis McIntyre. As Leavens reported to Frisch the sessions had been 'gotten up by Roos and DuBrul, and represented in effect an ex parte presentation by General Motors Corporation and Roos' Institute (which has General Motors as a client) of a case against the suggestion by some of the New Deal economists that the automobile industry should make its prices more flexible.' The morning and afternoon sessions on automobile demand comprised five papers, two by Roos and his staff member Victor von Szeliski, two by General Motors representatives (Stephen M. DuBrul, Seward L. Horner) and one by Andrew T. Court of the Automobile Manufacturers Association. DuBrul who gave the concluding paper, argued on behalf of General Motors that the margin between the cost of production and the selling price was so narrow that even if the elasticity of the demand for automobiles was as large as 2, it would be impossible to increase volume by cutting prices without inviting insolvency.

Leavens found that on the whole the papers were good. He praised especially Court's paper on hedonic indexes. He conveyed to Frisch that Irving Fisher had been impressed by the two sessions. Roos had asked Leavens if Econometrica would care to publish the five papers, in full, in an enlarged issue, with the expense met by General Motors. Leavens evaded it by responding that such a decision

rested with Frisch as Editor. Leavens got hold of the only members of the advisory editorial board present, namely Fisher and Hotelling. Hotelling was opposed, because the papers were commercially sponsored. Irving Fisher, on the other hand, was inclined to favour publication. Leavens later gathered Schumpeter, Fisher, Hotelling and McIntyre for lunch and discussed the matter. They came to the conclusion that it was best to be on the safe side and avoid any danger of accusation that *Econometrica* was supporting a case for a commercial organization. Leavens informed Roos, and that was it!

Frisch praised Leavens for making the correct decision by not accepting the Roos-General Motors papers. He wondered, however, 'if it would have been possible to arrange for publication in Econometrica of these papers as well as of papers from the other side. If the papers had really been of high quality - preferably quite a bit mathematical - it would have been a very great stimulus to the econometric cause. It would have opened the eyes of many to the fact that exact econometric methods are needed on many problems that are of great public concern.' He encouraged Leavens to take this up with those with whom he had already discussed the matter.

Leavens passed copies of Frisch's letter to Fisher, Hotelling, and Schumpeter and discussed the matter with each one of them. He reported back to Frisch that the idea of publishing the papers from both sides was good, but it might have been difficult to get such papers written in a reasonable time. As it was, the five papers presented were already being published by the General Motors Corporation in a pamphlet, and copies would be sent by them to all members of the Society.'[45]

5.14 The Lotka-Preinreich controversy

INCOMPLETE

Leavens to Frisch, January 10, 1939 ,

'At the evening meeting on December 27, Dr. Lotka presented a paper, and Dr, Preinreich was a discussant. In the correspondence last summer, I rather preferred Preinreich's attitude to Lotka's. At the meeting, however, Lotka showed up better. Preinreich made some rather snippy remarks about Lotka's work. After the meeting I spoke to Lotka for a minute; he apparently still feels that I did not treat him properly in handling the controversy last summer. '

MORE The conflict broke out 090938

6. Some concluding comments

The paper is not really concluded. The ambition is to continue with the remaining fifteen Frisch years, subdivided as the war years 1940-45 and the post-war years 1946-54. It cannot be continued beyond that on the basis of similar source material. Extraordinary circumstances have provided the rich source material of which only the tip of the iceberg have been displayed in this paper. I don't think similar editorial records can be compiled for later periods, although the rich archival material after outstanding Econometric Society members gathered in various archives may be helpful.[46] The correspondence files of editors and managing editors after Frisch may have been lost.

What does this paper convey about the promotion of econometrics through *Econometrica*? A reader may well find that the paper is filled with an enormous amount of details, overshadowing the larger picture. This may well be true. Can we point to articles in *Econometrica* in the period under scrutiny of really path breaking importance for the emergence of econometrics as a full bodied discipline? Perhaps not.

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[45] Leavens to Frisch, January 10, 1939; Frisch to Leavens, January 27, 1939; Leavens to Frisch, February 16, 1939.

[46] Economist's Papers Project at the Center for the History of Political Economy, Duke University seems to a particularly rich source.

But what do we find in the 193-39 period pointing towards the later success of econometrics and, indeed, *Econometrica*? The establishment of the Econometric Society was generally very positively received both in the United States and in the world at large. The number of countries with Econometric Society members is impressive, coming up beyond 40 at the end of the 1930s. The total number of members is perhaps less impressive. It remained a relatively small community for many years.

The record displayed in the paper shows a number of notable things. One is that there was a relatively small group of core members who devoted a lot of their attention and time to Econometrics and the ES meetings. Among these were in addition to Frisch from the beginning also Jan Tinbergen, Jacob Marschak, R.G.D. Allen, and others. A little later Gerhard Tintner entered the scene and after him even later arrivals in the US but devoted to the econometric program from the very beginning, such as Trygve Haavelmo, Abraham Wald and others. Some submitted little or nothing to Econometrica but where highly active at meetings, and perhaps as referees and in other ways in the activities of the Econometric Society. Schumpeter is in this category, after his programmatic article in 1(1) he submitted virtually nothing.

Another notable aspect is that a number of the economists and statisticians who had the highest activity in econometric work before 1933 refrained from transferring wholly or partly from transferring their publishing activity to Econometrica but retained their previous publication loyalties. Much has been written, e.g. by Karl A. Fox and others about the American agricultural economists, some of them affiliated with USDA and its Graduate School who had given USA a lead in the number of people who actually conducted empirical econometric studies. The record shows that they submitted few papers to Econometrica, although they did attend the meetings. The Working brothers (Elmer and Holbrook) submitted no paper in the period we have studied, neither did Louis Bean submit to Econometrica, Mordecai Ezekiel submitted only the paper discussed in section 5.1. Henry Schultz turned down invitations to serve as associate editor. Even Henry Schultz who was founding member of the Society submitted nothing to Econometrica apart from the article he submitted already in 1932 and the posthumous paper about Konüs in 1939.

But in the years we have studied, a number of young talents joined the Econometric Society and took an interest in *Econometrica*. Some of them where refugees (as Abraham Wald), others were just stranded due to the war or voluntary exiles (a group which incidentally comprised both Haavelmo, Koopmans and Marschak). A number of young American graduates were also attracted by the econometrics during the war years. The build-up period during the 1930s, not least the Econometrica volumes, is likely to have played a major role in the recruitment of members during the war years.

FINANCIAL ISSUES, SOLICITING AND REFEREEING PRACTICES TO BE DEALT WITH LATER

As the paper ends the Econometrica history already in 1940 we conclude this paper by a glimpse into the records of early post war period. In 6.1 below is the briefing letter of the managing editor to the editor-in-chief in the early post war period about the performance of all associate editors and members of the advisory editorial board. The second document, in 6.2 below, is a straightforward case of rejection after refereeing, in this specific case of a paper on the usefulness of game theory by no other than O. Morgenstern.

6.1 D.H. Leavens summing up the editors and the editorial board

(Leavens to Frisch November 27, 1946)

Mr Marschak and I think it might be a good idea for you to consider revision of the list of the Advisory Editorial Board of Econometrica.

I list below the American members, with comments.

Eugen Altschul: I have called on him only once or twice to referee, and he kept the article for months, in spite of reminders.

Irving Fisher: I call on him from time to time and he is always helpful.

Oscar Lange: He of course served as acting editor for two or three years, and we transferred him to the Board when he became ambassador. He of course cannot be active now but probably should be left on in the hope that he will in due course return to the faculty here.

Jacob Marschak: He of course has always been very helpful and at present is taking the responsibility for American articles until we get sufficient back log of them to send you.

Frederick C. Mills: I have not called on him in recent years. He was a pioneer in the teaching of statistics but has not kept up with modern mathematical developments.

Wesley C. Mitchell: He has helped out once or twice but is very busy and getting along in years, and I don't like to bother him.

Oystein Ore: I have not had occasion to refer any MSS to him as his field of mathematics is not very closely related to ours.

Charles F. Roos: Have not consulted him often of recent years, but think he should be kept on for one man in touch with business world.

Paul A. Samuelson: He was added to the Board during the War in consultation with Lange, and is very cooperative. Has refereed about 10 papers in recent years.

Gerhard Tintner: Same remarks as for Samuelson.

Abraham Wald: Added by Lange, and called on from time to time for mathematical advice.

Edwin B. Wilson: I have called on him only once or twice. He is cooperative. Has now retired from Harvard but I imagine will continue active in many lines.

Harold Hotelling: He is busy but cooperative.

The three "Associate Editors" are:

Schumpeter: He was very helpful the early years of the war, making final decisions for me informally before Lange was officially appointed acting editor.

Tinbergen: I of course have not had occasion to consult him, but you will.

Davis: Is always ready to help on mathematical papers and I call on him frequently.

My feeling would be that there would be no harm in dropping from the list Altschul, Mills, Mitchell, Ore, and possibly Wilson. Just how to do it is another matter. It would hardly do to drop them from list with no notice. It would probably be necessary for you to write a tactful letter, saying that the Board was being reorganized after the war, and that you were trying not to burden people who were too busy, and to keep only those who were able to put some time on it, and give them opportunity to withdraw gracefully.

Marschak also suggests that you consider putting on the Board, Trygve Haavelmo, Leonid Hurwicz, and Tjalling Koopmans. Each of them has refereed about 10 papers in recent years, and are always helpful.

I also call on a variety of young men for refereeing, but there is none of them whom I would recommend as yet for the board. I find it is often more satisfactory to get a young man to do refereeing, as he feels more responsibility and will do a more thorough job than an older man with his reputation already made and many things pressing on his time.

As to the foreign members of the Board, I have had no dealings with them. You can best tell which ones you will want to call on for help, and which are not of much use. The names are Amoroso, Bowley, Darmois, Del Vecchio, Divisia, Frechet, Rist, Schneider, Zeuthen.

I think we have gotten past the stage where we need a list of figurehead names, and that we should do better with a comparatively small board, not over 15 or 20, of people who will actively help.

This is not a matter that needs to be rushed but I think it would be worth your consideration to see how some of the present members could be retired and some new ones added.

Incidentally, physical limitations of the type arrangement on cover page ii of Econometrica preclude increasing the list greatly, unless the style is changes, as by omitting addresses and listing only names.

Sincerely yours,

Dickson H. Leavens'
[Frisch's remark in the margin (translated): 'We cut out the entire editorial board']

6.2 Morgenstern on game theory 1949

Oskar Morgenstern presented a paper titled *Economics and the Theory of Games* at a symposium on the theory of games, held September 7-10, 1948, jointly with the Institute of Mathematical Statistics (Econometrica 17(1), 71-74). The symposium comprised five other papers, including papers by John von Neumann, George B. Dantzig and John W. Tukey. In the paper Morgenstern stated and argued in favour of his conviction that the theory of games was destined to play an increasing part in the further development of economic theory, although the paper did not far in the direction of actually applying game theory beyond pointing the way. He submitted the paper to *Econometrica* shortly after the symposium. Simpson solicited a referee report from Kenneth Arrow and sent the paper with the report to Frisch:

> I enclose the MS of a paper "Economics and the Theory of Games" by Oskar Morgenstern. It was presented at the Madison meeting of the Society, and was submitted for publication in *Econometrica* at my suggestion. Although the comments of the referee, which I enclose, are certainly against its being published, I feel that the paper merits publication as one of the nontechnical survey type articles which *Econometrica* may occasionally wish to sponsor. If approved by you, I would like to use it as the introductory article for the April issue, copy for which is now in preparation.' (Simpson to Frisch, Nov. 16, 1948).

Arrow's referee report was as follows:

> This is to simultaneously acknowledge receipt of Morgenstern's paper for refereeing and to give my comments as a referee. 'It is to be understood that the following comments relate solely to the referee's opinion of the merits of the paper. It is a matter of editorial judgment whether papers which have been written by well-known authorities and which have been designated for publication under the auspices of the Econometric Society should be refused.
>
> However, as far as the merits of the paper are concerned, there seems to be no reason to accept it. There is exactly nothing whatever new in it; it is essentially a series of mentions of different topics in the work of Von Neumann-Morgenstern, following each mention being a statement that this topic will be of use in some undefined sense in economics. Anything this author might have had to say in this matter has already been said, in any case, in his recent A.E.R. paper. [remark in the margin: "not strictly correct. WBS'] There is no actual exposition of a game theory per se in the paper, so that it cannot even be maintained that it is a less mathematical presentation of material which might be too difficult for the average economist to read. In any case, the work of Von Neumann-Morgenstern has been reviewed about as extensively as any book in the field of economics has ever been, and the fundamental nature of the reasoning and the results of the book are as familiar to economists as they are likely to be without intensive study of the book itself.
>
> The only part of the theory of games referred to in the present paper which has not been stressed by the various reviewers is the composition and decomposition of this concept. The

> best he can say is that, "There is no doubt that similar applications to economic phenomena could readily be suggested." This kind of indefinite reference is typical of the paper. Thus, in referring to the proposition that the social maximum benefit is always reached when complete information prevails, Morgenstern ends a cursory discussion by saying, "It is impossible to discuss this proposition here any further, but it seems sufficiently significant to direct attention to the statement and its implications." Similar statements abound.
>
> For these reasons, it seems to the referee that there is no reason to publish the enclosed paper. (Referee report from Kenneth J. Arrow to William B. Simpson, Nov. 1948).

Frisch responded immediately with an idea:

> Regarding Oskar Morgenstern's paper…I subscribe entirely to the opinion of the referee. It is not the type of positive contributions which we should endeavour to make the main content of Econometrica. I wonder whether it could be an alternative for me to write to Morgenstern asking for a joint paper by him and von Neumann giving an actual application of parts of the theory to the analysis of an important current problem. That would serve the twofold purpose of actually showing what the theory can be used for and (if attempt was successful) of making a valuable contribution to the field in question….In the meantime I am having the paper read by a second referee on this side. (Frisch to Simpson, Nov. 22, 1948).

Frisch gave the Morgenstern's manuscript to Haavelmo (accompanied with a carbon copy of his answer to Simpson). Haavelmo didn't use much time nor many words. Within 4-5 days he returned a statement in Norwegian (scribbled by hand in the bottom left corner of the above mentioned carbon copy and meant just for Frisch's eyes). In translation it went as follows:

> He throws out a lot of ideas, but <u>he does not do anything himself</u>. We are all moving around with more or less vague ideas about that one "ought to revolutionize the entire system." But to suggest explicitly a real step forward, that is something completely different. I <u>learned</u> nothing from reading this MS. I think Morgenstern should send this paper to "The American Economic Review." (Haavelmo to Frisch, Nov. 30, 1948)

Haavelmo's assessment is in spirit very similar to Arrow's and the same kind irritation emerges from both. Haavelmo seemed unlike Arrow to be unaware that Morgenstern had a paper on game theory in the papers and proceedings issue of *AER* in the same year. It is interesting that despite the time it took for game theory to get firmly entrenched in the economist's toolbox. Arrow held that 'the results of the [von Neumann and Morgenstern 1944] book are as familiar to economists as they are likely to be without intensive study of the book itself.' That was certainly true for the Cowles Commission staff but not obviously for all of *Econometrica*'s readers.

Frisch asked Simpson further whether there might anything solicited about the application of game theory and asked him to check with Cowles and Koopmans. Simpson doesn't seem to have pursued it. may not have pursued. Frisch also asked what had been indicated to Morgenstern and Simpson had to put the cards on the table:

> You have asked as to the extent that *Econometrica* has been committed toward including the paper by Professor Morgenstern. I suggested to Morgenstern at Madison, following his presentation of the paper, that he submit it to *Econometrica* for refereeing, it being made clear in the letter which acknowledged the manuscript that you, of course, had the final decision. An inquiry from him as to delay in receiving his proofs would seem to indicate, however, that he considered acceptance of the manuscript as a foregone conclusion. My own enthusiasm is here to be blamed. I am writing him to say that you have not reached a final decision in the matter. If, after hearing from your own referee on the paper, you are still inclined to reject it, you might write directly to Morgenstern as you suggested in your letter, and ask that he collaborate with von Neumann on an actual application of parts of the theory to the analysis of an important current problem. (Simpson to Frisch, Dec. 12, 1948).

Frisch did not write to Morgenstern until the beginning of March 1949 wrapping the rejection up as gently as he could:

> Your paper has been read by two referees. There is a general feeling that since the theory of games has been rather fully reported on in a number of journals and the importance of the general theory is so widely recognised that there is no risk of an important idea being lost, - it would seem a better utilization of the scarce resources of Econometrica to contribute now to the publication of a special application of the theory of games rather than to help bringing the general idea across. I therefore wonder whether you would consider - possibly in joint authorship with Professor von Neumann - to prepare a brief article giving an example of the application of the theory of games to one of the specific problems of the day in economic theory. If it could be done by using actual figures (however roughly estimated) it would help tremendously in bringing out the essence of the method. Such a study may be done on an ever so narrowly specified field, but it must be specific in the sense of definitely linking it up with a problem whose economic significance today will impress the readers. I do hope that you will see your way, possibly in cooperation with Professor von Neumann, to making such a contribution. Anything along such lines will be very welcome in Econometrica. But I am afraid that at present we will not be able to bring your general MS as now submitted. (Frisch to Morgenstern, March 7, 1949).

Morgenstern submitted his paper to *Kyklos* which immediately accepted it (Morgenstern 1949).

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