The Fellowship of Econometrics: Selection and Diverging Views in the Province of Mathematical Economics, from the 1930s to the 1950s

Francisco Louçã and Sofia Terlica

The inception and development of econometrics is one of the most impressive success stories in twentieth-century economics. This essay discusses the founding of the Econometric Society, the defining moment of the story, in order to bring to light the debates over the role of mathematics in the new "social physics," as Ragnar Frisch called it. Using previously unpublished archival evidence from the founders of econometrics,[1] we conclude that the methodological disputes over the role and scope of mathematics reflected diverging views.

Some of these debates are well known, namely, those between John Maynard Keynes and Jan Tinbergen and his companions (1938 to 1940) and between Tjalling Koopmans and the institutionalist school (the 1947 "measurement without theory" debate), and we consequently do not deal

with them here. But other debates, although important, have been studied little at all; paradoxically, they are the debates that took place inside the econometric camp itself. They provide useful information about the hesitations and the divergences among the protagonists of the econometric revolution, and as such they are the focus of our study.

In the period considered here, from the founding of the Econometric Society in the early 1930s to the reconstruction of its international network after the end of World War II, a coexistence of distinctive views of econometrics and of the application of mathematics can be identified. But this coexistence was challenged, and we signal two moments when a dispute between different views emerged, in both cases over the criteria for selecting fellows: in 1931–33 and in 1953. The debate during both periods was essentially the same, although some of the actors had changed: in 1931 the dispute involved the founders of econometrics, as they were asked to select the first fellows, whereas in 1953 it was up to Oskar Morgenstern to ask for a clarification of the uses of mathematics in econometrics and to oppose the dominant practices.

The Founding of Econometrics

When, in 1926, Ragnar Frisch (1895–1973) defined econometrics for the first time, his aim was to transform economics into a positivist science: econometrics should turn "pure economics, as far as possible, in[to] a science in the strict sense of the word" (Frisch 1926b, 1). Without empirical verification, economics could not be a bona fide science, Frisch thought, and he devoted all his efforts to the making of that science.

For that purpose, in September 1926 Frisch approached François Divisia (1889–1964) with a bold proposal: the creation of an international association of "pure" economics and a new journal. Frisch and Divisia had corresponded at least once before, and the two shared similar views about the role that mathematics should play in the discipline. Divisia was a highly respected French economist working on monetary theory to whom, earlier that year, Frisch had sent a copy of his dissertation and a letter outlining his views on the future of mathematical economics. In June, Divisia (1926) replied, sharing with Frisch his own ideas on these topics: "First of all, I believe, as you do, that economic studies cannot today be restricted to the vague reasoning that the classical economists have offered, and that the help of mathematics is necessary; I even believe that economic studies must resort to more complicated mathematical notions than those gener-

ally used in sciences for which experimentation is possible." Although Divisia did not feel at ease with the more advanced mathematical methods, he nonetheless believed that they represented the way forward: "Mathematical economics has very few supporters in France; myself, I don't know much about it; nevertheless, I am to be counted among those who consider that economic phenomena must be studied by methods as precise as those used in the other more advanced sciences" (our translation).

This was hardly an encouragement and even less a commitment, but Frisch only wanted not to be opposed. Divisia, like so many economists of that period, was not exactly a neoclassical economist and was skeptical of the statistical methods and the economics of Léon Walras and Vilfredo Pareto.[2] Yet, like an even larger number, he was ready to accept the epistemological predominance and guidance of "pure sciences," and consequently he sympathized with Frisch's move toward a thorough mathematization of economics in order to create an empirically based science, although he felt himself to be in some danger in those deep waters. Consequently, Frisch rightly interpreted the letter as an invitation to proceed. The letter sent from Frisch (1926a) to Divisia that September took up the challenge and assumed that new steps could follow immediately and, moreover, that he would lead the effort:

> I enthusiastically welcome the idea of a list or some other form of communication between mathematical economists of the whole world. Myself, I had thought of creating an association with a journal discussing these questions. . . . I know quite a few mathematical economists in different countries, and I consider writing one of these days a letter to each of them in order to get their opinion about the possibility of an "Association Internationale d'Economie Pure" and the possibility of a journal. What do you say to an Econometrica (the sister of Biometrika)?

And so he did: on 1 November 1926, Frisch wrote to four colleagues, Ladislau von Bortkiewicz, Charles Jordan, Arthur Bowley, and Eugen Slutsky—no one from the United States. Slutsky, whom Frisch had already

2. "Nevertheless, I do not hide the fact that I am rather sceptical about the very principle of the method consisting of firstly analysing the elementary phenomenon in order thereafter to deduce the global phenomenon, I mean to study how the individual behaves (which is peculiar to choice theory)." Divisia (1926) further insisted that "in the treatment of the observation material, I believe the usual methods of statistical science to be often either too vague or tainted with arbitrariness" (our translation).

met in Oslo, was the most enthusiastic about the new association (Bjerk-holt 1998, 31–32), although he never joined it.[3] The same day, Frisch informed Divisia of the initiative of this letter.

Despite these early efforts, the decisive steps in creating the econometric movement were not taken until it became a European-American enterprise: when Frisch arrived in the United States, he immediately found a like-minded thinker in Charles Roos (1901–1958), then at Cornell University, and together they prepared a five-page memorandum, the main points of which Frisch (1970, 225) recounted in his Nobel lecture. The memorandum argued in favor of rigorous quantification and an empirically based science:

> Two important features in the modern economic development are the application of mathematics to abstract economic reasoning . . . and the attempt at placing economics on a numerical and experimental basis by an intensive study of economic statistics. Both these developments have a common characteristic: they emphasize the quantitative character of economics. This quantitative movement in our estimation is one of the most promising developments in modern economics. (Frisch 1927b)

This argument was championed by Frisch two months later, in his presentation to a roundtable at the joint meeting of the American Economic Association and the American Statistical Association:

> Quantitative economics is something more than economic statistics. There is a quantitative aspect of economics which is rational and in one sense more fundamental than the empirical manipulation of numerical data on economic phenomena; namely, that part of economic theory which is concerned with the logic of our quantitative notions . . . . We speak of one statistical procedure as giving a better result than

3. The contacts between the two men were intense, dealing in particular with the publication of Slutsky's paper on cycles emerging from random shocks. However, during their correspondence, both the society and the journal were still mere projects. The reasons why Slutsky did not become a member of the Econometric Society are unknown. He was both a friend and a regular correspondent with Frisch, and his 1927 paper (later published in *Econometrica*, in 1937, under the auspices of Frisch) was widely circulated and attracted much attention. But there is no indication in their correspondence about the reason for Slutsky's failure to participate in the society, although one may speculate that the evolution of the USSR in the late 1920s and his fear of the political consequences of being associated with a foreign institution eventually decided the issue.

another. . . . But I cannot get rid of the impression that we engage . . . in target shooting without any target to shoot at. The target has to be furnished by axiomatic economics. Clearing the ground in axiomatic economics is a job which will certainly not be accomplished within the first few years to come. (Frisch 1927a)

The responses to the memorandum and the call for a new movement were similar to Divisia's: curiosity and sympathy, but only tentative support. It was not until a couple of well-respected economists joined the movement that it became a force: Joseph Schumpeter and Irving Fisher. Schumpeter (1883–1950), who was by then preparing to leave Germany for his American exile, was twelve years older than Frisch, and Fisher (1867–1947) was his elder by twenty-eight years. Both were established and leading economists, and the idea was not new for them: Fisher had already unsuccessfully promoted the project of a new association of mathematical economists in 1912 (Darnell and Evans 1990, xvn). Yet the next initiative would not fail. In the autumn of 1927 Frisch met Schumpeter for the first time, at Harvard: their friendship and complicity in matters of the Econometric Society became a driving force behind the emerging movement. The American connection between the two Europeans was to be the core of econometrics.

In February of the next year, Frisch continued with his tour in support of econometrics and visited Irving Fisher at Yale, and then Charles Roos once again at Princeton: both would soon form part of the society's first managerial board. Later that month, on 29 February, Frisch met Schumpeter and Gottfried Haberler at the Colonial Club in Harvard: the abstract of the conversation, drawn up by Frisch, indicates that they prepared a new "list of [seventy-seven] econometric people" and discussed a name for the projected International Circle for the Promotion of the Econommetric [*sic*] Studies, suggesting Eranos Oekonommetrikos—a scientific corpus under the name of a student club.[4]

The answers from the "econometric people" were quite prudent. As we showed, Slutsky endorsed the idea but not the society. Georges Lutfalla (1928) wrote back to Frisch, advising him not to expect crowds at the door: he had had the experience of being unable to find four hundred subscriptions to create a journal of mathematical economics in France. Despite following the movement since its incipient days, Divisia (1930)

4. Also referred to in the letter from Schumpeter to Frisch, 19 September 1930.

remained prudent: "I believe the formula 'Economic Science' [in the title of the journal] would be too dangerous: it would mean we want to monopolise economic science. It may well be the essence of our thought, but I believe it is still not the moment to announce it" (our translation). Norbert Wiener, who attended the founding meeting in December of that year, was pessimistic about the whole enterprise, as Frisch (1970, 164n) recalled later. Others thought the same.

But Frisch did not give up: back in Europe, he went to Italy in June and discussed the matter with Corrado Gini, while developing an intense correspondence with many others about the future Econometric Society. Returning to the United States at the beginning of 1930 as a visiting professor at Yale, Frisch drew up a list of invitees to the founding meeting of the society, which was to be held in December, and sent out a circular letter on 17 June to twenty-eight people in the name of Roos, Fisher, and himself. The invitees were Hans Mayer in Austria; Harald Westergaard in Denmark; Umberto Ricci in Egypt; Clément Colson, François Divisia, Jacques Moret, and Jacques Rueff in France; Ladislau von Bortkiewicz and Joseph Schumpeter (who would come to Harvard a couple of years later) in Germany; Luigi Amoroso, Corrado Gini, Alfonso de Pietri-Tonelli, and Gustavo del Vecchio in Italy; Ragnar Frisch in Norway; Gustav Cassel and Bertil Ohlin in Sweden; Wladislaw Zawadzki in Poland; Arthur Bowley, John Maynard Keynes, and Arthur Pigou in the United Kingdom; Thomas Carver, John Bates Clark, John Maurice Clark, Griffith Conrad Evans, Mordekai Ezekiel, Irving Fisher, Henry Moore, Warren Persons, Charles Roos, and Henry Schultz in the United States; and Eugen Slutsky in Russia. Charles Jordan was no longer on the list, despite having been one of the first to be contacted after Divisia. Considering their answers,[5] the promoters of the society decided to go ahead with the inaugural conference.

The conference met as scheduled on 29 December 1930, at the Statler Hotel in Cleveland, Ohio, and was held under the presidency of Schum-

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5. Pigou, Dennis Robertson, Cassel, Slutsky, and J. B. Clark never joined the society: "Professors Pigou and Robertson of England have refused membership of the Society. Slutsky's name was removed from the list at the direction of Professor Fisher. I assume, but do not know, that Slutsky refused membership. Kondratiev, according to a rumour reported by Roos, is dead—executed by the Soviets. Hicks of England, Porri of Italy and Lange of Poland have never been formally proposed by anyone for membership of the Society" (Cowles 1932). Frisch added that Pigou would not accept the invitation unless he was elected a fellow, since he was "afraid of getting into the same class as all the ordinary members." Frisch was convinced Slutsky would accept if re-asked and should also be proposed as a fellow (Frisch 1932). Kondratiev was still alive, although imprisoned.

peter. Sixteen men, including some added to the preliminary list of invitations, decided to found the Econometric Society: from the United States, the meeting was attended by Harold Hotelling, Frederick Mills, William Ogburn, J. Harvey Rogers, Roos, Malcolm Rorty, Henry Schultz, Walter Shewhart, Carl Snyder, Norbert Wiener, and Edwin Wilson, and from Europe by Frisch, Oystein Ore (who was then at Yale), Ingvar Wedervang, Karl Menger, and Schumpeter. Despite their heterogeneity, this small number of economists, sociologists, and mathematicians, some of them neoclassical, others institutionalists, reunited to lay the foundation of one of the societies that would reshape economics. Divergence and skepticism amid convergence and enthusiasm: that marked the founding moment of econometrics, the mathematics of the new "social physics."

The Creation of the Fellowship (1931–33)

The conference elected ten men to the first council of the Econometric Society: Fisher, Roos, and Wilson from the United States, and Frisch, Schumpeter, Luigi Amoroso, Ladislau von Bortkiewicz, Arthur Bowley, Divisia, and Wladislaw Zawadzki from Europe. Fisher, who was not present at the meeting, was elected president, and Divisia vice president.

The task this small group set itself was immense in three different fields. First, its members endeavored to create a new discipline inside economics: quite originally, the Econometric Society was created precisely to define its own subject. Second, they wanted to emulate physics and established a constitutional goal to create a social science "to promote studies that aim at a unification of the theoreticalquantitative 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"—as stated by the society's constitution, drawn up by a committee composed of Frisch, Mills, and Roos.[6] Third, they intended to provide new solutions, rigorous and quantified as they should be, to traditional economic problems. Mathematics was therefore proposed as the language of the new project. Everything seemed to turn around math—but, as events eventually proved, the econometricians did not agree on what type of mathematics, and, even worse, it became obvious that some disdained the very centrality of mathematical formalism.

6. Frisch (1936) was responsible for the wording of the draft of the constitution, after consultation with several founders of the society.

From the very first day that the Econometric Society was created, the members of the council understood that its agenda was too demanding, since they shared only some general ideas and not a concrete response to any of these three goals. In fact, not only did each one pursue his own research agenda, but they also had different visions about the future of the society. This difficulty emerged immediately in 1931 as the society undertook its first task: to define the criteria for membership and, most important, for selecting fellows, the distinguished econometricians who should set the example. The differences, as it appears, concentrated on the value of mathematical achievements for defining a career in econometrics.

In any case, the selection of fellows presented an opportunity to provide the example of best practices in the new province of econometrics and to indicate its major avenues of research. In this sense, we use the term *fellowship* as a metaphor for the creation of the connections that led to a structure of authoritative scientific reference in the society, through a well-identified and thoroughly discussed process of selection. As the reader will verify, the term would be used by different econometricians in the second debate here discussed, that of the 1950s, for the same purpose of describing the accession to the status and level of fellows. Consequently, we do not refer either to a community sharing the same beliefs or to a club whose members coincided in words or deeds,[7] but to the assembling of a group of famous scientists whose choice would set the pattern for the new society.

For the definition of such criteria of selection, the founders nevertheless found themselves in opposing camps: Roos and Fisher argued for an open society, whereas Schumpeter, Frisch, and Bowley preferred a closed center of excellence with mathematically trained scientists.[8] As a consequence of his different view of the nature of the society, Schumpeter opposed a number of names proposed by Fisher (Bjerkholt 1998, 39–40). Fisher (1931a) even complained that mathematics was too emphasized: "I notice a tendency in the society to stress mathematics and forget economics."

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7. In a recent conference (2010), Alan Kirman quoted a 1988 letter by Robert Solow referring to the holders of the dominant views in the profession as a "brotherhood." The term *fellowship* is here used in the same sense, in reference to the creation of the *autoritas* of a select body of scientists inside the Econometric Society.

8. A discussant argued that Bowley was used to an open society, for example, the Royal Statistical Association, and that he could barely be taken as a defender of the alternative of a close forum. If this is certainly true for his participation at the statistical association, in the case of the Econometric Society he argued for a restricted criterion of participation.

The disagreement over the criteria for membership was first resolved by the statutory definition of two types of members: ordinary members and fellows. Despite these discussions, the society had grown by the next year: after the meeting of the sixteen founders in 1931, 153 new members joined the group, and some of the most influential economists of the time were among that number. Irving Fisher had drawn up a list of 261 mathematical economists, and many of them were approached by the society's founders (Bjerkholt 1998, 31).

The inevitable result was that the divergence was consequently translated into the choice of fellows, and the first years of the society were indeed dominated by the definition of criteria both for that selection and for the election process itself, which finally took place for the first time in February 1933. It took some months for the election to be held, and some members expressed their anxiety about it: the immediate election of fellows was instrumental in preventing the discontentment of young members, since, as Alfred Cowles (1932b) put it, "with the policies of the Society guided by a group of Fellows comprising outstanding econometricians, to exclude from ordinary membership those of lesser attainment in this field, who are nevertheless interested in furthering the aims of the Society, could only result in impeding the progress of econometrics. The creation of a group of Fellows should meet all the requirements of those who crave an esoteric atmosphere." In any case, the dominant concept was that fellows should represent the example of a scientific tradition distinguished by the achievements of its members rather than by the daily workings of the society.

Finally, in the first days of 1933, an agreement was reached on the criteria for the election, and Fisher formulated the requirements for the choice of a fellow, as recapitulated by Frisch (1933a):

1. The candidate must be an economist acquainted with economic theory.
2. He must have a mathematical foundation.
3. He must have some knowledge of statistics.
4. He must have done some original work.
5. Some of this original work must have been in economic theory.

Using these criteria, the votes were cast, and consequently twenty-nine fellows were elected to the council and notified in August: Amoroso, Oskar Anderson, Albert Aupetit, Pasquale Bonisegni, Bowley, Colson,

Gini, Haberler, Hotelling, Keynes, del Vecchio, Divisia, Evans, Fisher, Frisch, Nikolai Kondratiev, Wesley Mitchell, Moore, Ricci, Roos, Rueff, Erich Schneider, Schultz, Schumpeter, Tinbergen, Vinci, Wilson, Zawadzki, and Frederick Zeuthen.[9]

The diverging concepts about the nature of the Econometric Society were expressed in the discussion about the candidates. Frisch (1931b) wanted to include Tinbergen, "an absolutely charming personality," but also Vinci, Gini, Otto Weinberger, Otto Kuhne, and "perhaps" Leontief and Marschak.[10] Divisia (1931) opposed Aftalion. Fisher (1931b) favored Edwin Cannan ("one of the first to distinguish between a stock and a flow"), Thomas N. Carver ("the only one who has developed certain points in regard to the coordination of distribution"), Edwin Kemmerer ("has used a little bit of mathematics"), and William Ogburn ("familiar with the application of correlation to mathematics"), a description that indicates the fairly unimpressive state of the art of mathematical economics at that time.

Schumpeter (1931a) suggested Vito Volterra and preferred Frank Taussig to Carver (both from Harvard), although Frisch (1931a) opposed both of these and proposed John Black ("certainly has the *econometric attitude*, even if he does not master much of the mathematical technique"). The final list resulted from multiple compromises among these opinions and eventually expressed the prestige and influence of the main candidates: Wesley Mitchell (1874–1948) received the maximum number of votes possible, fifty-seven, despite not being involved in the endeavors of the society, not to mention his growing hostility toward the econometricians' works; Fisher, Frisch, Schumpeter, Divisia, and Roos, the founders, received fifty-four, whereas Keynes received fifty-two.

Nevertheless, as the result of the election of fellows was unsatisfactory for many, a new list was drawn up the same year. Frisch presented just one candidate, Marschak, since he had previously abandoned this proposal in view of a remark made by Divisia.[11] Taking into account other sugges-

9. The election was held according to a laborious method: the members voted, but corrections were made to the result of the vote to take account of country distribution and other criteria (consequently, Darmois was replaced by Colson, although he had received two more votes; Schneider and Kondratiev were elected, although they received fewer votes than Persons and Leontief, who were excluded).

10. Schumpeter (1931b) accepted Marschak, although later he went back on his recommendation.

11. "For my own part there is only one man whom I should now like to propose as fellow, namely Dr. Marschak. As you will remember, I was a little hesitant about Marschak in the first round, my hesitation being caused by Divisia's remark that probably Marschak did not know

tions, the society compiled a list that was put to the vote of the current fellows, after eliminating some of the possible candidates, among others Hicks, Piero Sraffa, Friedrich Hayek, and Morgenstern. The list included four who were elected (R. G. D. Allen, Constantino Bresciani-Turroni, Marschak, and Ezekiel) and thirteen who were rejected (Georges Darmois, Pietri-Tonelli, Marco Fanno, L.-V. Furlan, Alvin Hansen, Ralph Hawtrey, Leontief, Mills, Giorgio Mortara, Snyder, Otto Weinberger, E. J. Working, and Holbrook Working). Consequently, the election as fellows of two of those who had been present at the inaugural conference of the society, Mills and Snyder, was also rejected for the second time.

The next list of fellows was established only four years later, in 1937: Cowles, Hicks, Mortara, René Roy, and Hans Staehle were all elected. In 1938 it was the turn of Oskar Lange, Leontief, J. C. Stamp, and T. O. Yntema.[12] By the end of the first decade of the society's existence, forty-two fellows represented the Olympus of the "econometric people."

This was, in any case, an immense success: a couple of years after its creation, the society attracted already some of the most prominent economists and mathematicians: apart from those previously mentioned, in 1935 Emile Borel, Constantino Bresciani-Turroni, Jacques Hadamard, Friedrich Hayek, William Jaffé, Otto Kuhne, Emil Lederer, Erik Lindhal, Fritz Machlup, James Meade, Ludwig von Mises, Gunnar Myrdal, Lionel Robbins, Arthur Spiethoff, Sven Wicksell, and Vito Volterra were also members, among many others. Not many distinguished economists were absent from the econometric gathering—even if they were not econometricians.

The success of the enterprise was matched by the uniqueness of the convergence of different approaches and schools in economics, which were involved by this innovative program: the Econometric Society was born under the project of reuniting all available capabilities in economics, notwithstanding their divergences. Its pluralistic nature was highlighted by the careful choice of whom to invite as well as by the choice of president and vice president at the 1930 Cleveland conference: the Columbia

what a partial derivative was. That was the only thing that held me back from recommending Marschak" (Frisch 1933b). In previously considering Divisia's insinuation, Frisch (1933c) had for some time been convinced of Marschak's virtual errors, and, when he recommended one of his papers to be translated for *Econometrica*, he indicated to his managing editor that "here Marschak uses partial differentiation, and from my contact with him I would not be surprised if he had made a slip in considering one quantity as constant while it ought to be variable, or something of that sort." An end was soon brought to this injustice, and Frisch moved quickly to correct his own error of judgment.

12. Sraffa, unhappy at not being elected a fellow, withdrew from the Econometric Society.

school was conveniently represented and, as the editorship of *Economet-rica* was assigned to Frisch, Frederick Mills was also involved as associate editor as a de facto representative of the institutionalists. He was also asked to contribute to the redaction of the society's constitution. Mitchell was one of the five members of the advisory council of the Cowles Commission when it was formed. It was because of Mills's other obligations that he resigned the post of associate editor in 1934, not because he did not feel comfortable with the journal's editorial choices for its first year. On the contrary, at least during the society's first years, he played an active role in advocating the virtues of affiliation to the new movement: as Edmund Day, who worked for the Rockefeller Foundation, hesitantly approached Mills to weight the arguments for membership, he was convinced by a battery of reasons, including the certainty that the society would not cultivate mathematical "esoterism" or any "kind of separatism" (Day 1932; Mills 1932).[13]

The Econometric Society was moving forward. But it was self-centered in regard to several internal quarrels over vague concepts; diplomacy abounded, but no important steps were taken to establish the dominance of mathematical economics, which still remained illdefined. The fact was that Mitchell was in 1931 the most popular economist among the selected audience of the econometric people, but, despite being engaged in empirical work like very few others, he could not be taken as the promoter of a mathematically based "pure" economics, and still less so of a science that aimed at achieving the higher grounds of the positivist realm of social physics: he did not have the appropriate "econometric attitude," to adopt Frisch's insinuation against Black. At the same time, the econometric people were scarcely prepared for a battle over reconstructing economics: according to president-elect Irving Fisher, the ability to distinguish between a stock and a flow, the "use of a little bit of mathematics," and "familiarity with the application of correlation" were sufficient recommendations for membership.

Indeed, the development of the society required the creation of two instruments: an intense network of cooperation, emulation, and competi-

13. Mills (1932) strongly emphasizes that the founders of econometrics—and he named Frisch, Roos, and Evans—considered "mathematical economics [as] a discipline quite different from traditional economics of a nonmathematical type," although "I am personally agnostic concerning the possibilities of substantial accomplishment in this field by technicians who are not closely conversant with the actual economic process. Probably more important is the danger that mathematicians who are interested in economics may cultivate their art in isolation and that an esoteric, unrealistic discipline may develop."

tion, such as that provided by regular conferences, and the publication of a journal. Both instruments were at the center of the preparatory discussions among the society's founders, although one was easier to establish than the other: conferences required enthusiasm, but a journal required financing, and the large endowment of the former could not compensate for the scarcity of the latter. Consequently, the priority was to set up the assemblies of econometricians. The society organized an intense schedule of regular meetings both in Europe and in the United States: each September-October in Europe, whereas the U.S. meetings were held in December-January and June, frequently in association with other academic meetings.

A second tool, and a most valuable one, would be *Econometrica*. Since the early days of the 1920s, when Frisch first contacted Divisia about his plans for creating a new international association, the proposal to publish a journal had been constantly made. Indeed, it was intensely discussed, mostly with respect to its title. Indeed, a curious feature of this early correspondence is the discussion about the name of the future journal: Frisch favored *Oekonometrika* (admitting the influence of *Biometrika*), Divisia suggested *Oeconometrika* or *Oeconommetrika*, whereas Slutsky's choice was *Economometrika*. Fortunately, the final choice turned out to be the first suggestion, the more pedestrian *Econometrica*.[14]

The journal was central to the project: indeed, since its conception, the association of econometricians had been supposed to be defined both by the organized corpus of membership and attendance at the conferences and by *Econometrica* as the expression of their research. But it was much harder to create the journal, since it required more than just intense work, devotion, and imagination—it demanded financing in that disturbed period of the 1930s, amid the general depression and the imminent outbreak of war. Financing was even harder to attain given the general ignorance of econometrics and the widespread dismissal of its potentiality: even later, when the society was beginning to get an audience in the profession, the treasurer, Roos (1935), noted the difficulties in obtaining funding from public or other sources, since the referees of projects were very skeptical— mathematicians were rather critical and, if asked, Jacob Viner and Carl Snyder could be "quite unfavourable," whereas Mitchell's and Taussig's attitudes were supposedly unpredictable. The paradox of the situation was obvious: Snyder, Mitchell, and Taussig were members of the society

14. Letters written in 1927, undated May–June and 4 July; letter from Frisch to Divisia, 22 May 1927. By 1928 the name *Econometrica* had become stabilized. But some references were still made to the "econommetric" circles.

and yet were suspected of not favoring the financing of its projects and activities.

A miracle was needed to publish *Econometrica*, and it came in the form of a complete surprise: Alfred Cowles III (1891–1984), the son of a millionaire, the president of an investment counseling firm, Cowles and Co., and a competent statistician interested in stock market predictions, offered to pay twelve thousand dollars a year for the journal.[15] "An angel suddenly fell down from the sky," announced Fisher (1931c) to Frisch, asking for his opinion, since, as the "original founder," the decision was up to him, and he accepted after some hesitation.[16] *Econometrica*'s first issue appeared in January 1933, and it has been published ever since.

Frisch proposed Hotelling as editor, to avoid his own appointment, but he could not prevent it from happening: he finally took over editorial duties from the first issue until 1955. Associate editors were also appointed: first Alvin Hansen (replaced, in 1938, by Schumpeter), Frederick Mills (resigned in 1934), and Harold Davis: one economist, one statistician, and one mathematician.

At least for the first decade of the journal's existence, Frisch was the sole driving force behind its publication: he set the agenda, corresponded with the authors,[17] asked for articles, was the referee in most cases, discussed the papers and made suggestions, and, finally, decided on publication, changed the notation for coherence, and even corrected the galley proofs. He worked immensely hard, and his efforts were the ones that determined the journal's survival and development; at the same time, the

15. Alfred Cowles III had developed statistical research into stock market forecasting after the Great Depression. His main interest in econometrics arose from the bad record of stock predictors. His 1932 paper "Can Stock Market Forecasters Forecast?" examined three years of records kept by twenty-four leading financial services, including banks and investment companies, and he concluded that "as a group, these supposedly shrewd investors would have accomplished a comparable result through a purely random selection of stocks" (Cowles 1932a). The paper was discussed with Frisch and presented to the December 1932 meeting of the American Statistical Association in Cincinnati.

16. Roos was also enthusiastic. The term *angel* was certainly widespread in econometric circles: Mills (1932) refers to an "angel" Fisher had found in Denver.

17. The National Library of Oslo contains a collection of Frisch's correspondence that highlights his efforts to guide the development of the econometric movement and the journal: 175 letters from Cowles, 78 from Divisia, 111 from Fisher, 295 from Dickson Leavens, 42 from Marschak, 30 from Hotelling, 131 from Nelson, 27 from Roos, 33 from Schultz, 25 from Hansen, 16 from Gini, 13 from Haavelmo, 34 from Richard Khan, 12 from Keynes, 15 from Michal Kalecki, 12 from Koopmans, 17 from Morgenstern, 36 from Schumpeter, 51 from Tinbergen, as well as letters to and from many other protagonists in the history of economics in the twentieth century.

concentration of decision making generated delays, since the papers and proofs had to cross the Atlantic twice before each issue and, worse still, created new editorial problems.[18] The editor was the journal. *Econometrica* became one of the leading journals in the profession and fully accomplished its role as a pillar for the development of the econometric movement. Looking back, one can only be amazed by the depth and seminal influence of so many papers, by the diverse and far-reaching strategies of publication, by the attempts to motivate young colleagues, and by the strenuous efforts to combine historical memory with the promotion of technical expertise.

Compared with the *Economic Journal*, an older and well-established publication, *Econometrica* exhibits some revealing differences during this period. The number of general theoretical papers published in both journals is not significantly different, but the editorial strategies diverge notably in regard to the publication of articles about statistical theory and mathematics (irrelevant for *EJ* and quite substantial for *Econometrica*).[19] The other relevant differences were the publication of papers on the history of thought (*Econometrica* publishing twice as many as the *EJ*) and the publication of papers on empirical studies (*EJ* publishing three times as many as *Econometrica*). *Econometrica* was definitely more inclined to publish papers on mathematically based theoretical and applied research and on the history of economic thought, and less on empirical applications.

The numbers are telling, but they do not reveal the discussions taking place on editorial strategies, in particular those that decided the shape of the newborn journal. In fact, despite its dedication both to mathematics, the powerful formal logic at the epicenter of econometrics, and to statistical theory, *Econometrica*'s editorial policy was not insensitive to the difficulties of affording technical treatment to abstract topics, and several of its prominent leaders frequently emphasized the importance of there being an empirical counterpart in the choice of the papers. After a nasty dispute on the publication of a difficult mathematical piece by Frisch himself, he tried to address his critics' concerns by inviting Georges Bousquet, a

18. When the managing editor, Leavens (1948), left his job in 1948 after eleven years, he was bitterly critical of Frisch's editorship.

19. The editorial policy of the *EJ* was clearly stated in 1954: "We suggest that authors should aim at avoiding the use of advanced mathematics, except where it is necessary for supplying a rigorous proof, or where the nature of the subject inevitably requires it," to avoid the creation of "language barriers" (Editor's Note 1954, 2). From 1900 to 1960, the percentage of mathematical papers published in the EJ was very low (5 percent); in the period 1887–1924, 39 percent of such papers were produced by Edgeworth, the editor (Mirowski 1991, 150).

young economist, to prepare a paper for *Econometrica* in order to build a
bridge between the "more mathematically oriented men in our group and
the broader group of general economists who are interested in the econo-
metric approach but who do not have the time or the background to follow
the technicalities of our work" (Frisch 1934).

On the other hand, the journal was not supposed to be just a repository
of difficult mathematical papers. One of its most impressive early features
was the editorial concern to recuperate history. This highlighted the nature
of the movement, which was looking for legitimacy as the heir to the ideas
of giants: in its very first issue, *Econometrica* surveyed Augustin Cournot
and Wicksell; in 1934 it included papers on Johann Heinrich von Thünen,
F. Y. Edgeworth, William Stanley Jevons, and Walras; in 1938 the famous
paper on Pareto and another on Cournot, then the next year on Schultz and
again on Cournot; in 1946 a memorial on Keynes. In looking at the past,
Econometrica was pointing to the future.

Mathematics of Induction or of Deduction?
A 1953 Reconsideration of the Criteria
to Select the Fellows

By the end of its first decade, the Econometric Society had changed, and
the coexistence among different views of econometrics was ending.[20] The
epicenter of the mutation was the Cowles Commission, a think tank
funded by Alfred Cowles and reuniting a small group of full-time and
invited researchers, which became the brains of the operation, although it
was at first difficult to convince any prestigious econometrician to take the
post of director and to move to Colorado Springs, where its headquarters
was located. After much deliberation, Oskar Lange was selected and given
the job in 1937. But in 1939 the commission moved to Chicago, under the
influence of Schultz and Yntema, who took the job (both were at war with
the Columbia institutionalists), and the days of the happy convergence
were over (Mirowski 1989). As the commission was now established at a
major university and its prestige had grown, the post of director became a
highly desirable position: Marschak succeeded Yntema in 1943 and stayed
until 1948; Koopmans became director from 1948 to 1954.

20. One anonymous referee presented these changes as the passage of the Cowles Com-
mission from being a "benign promoter" (the early 1930s) to a "censor" in econometrics (the
1940s and 1950s).

Marschak was responsible for a major change in direction in the development of the commission. He recruited Trygve Haavelmo (July 1943) and Koopmans (July 1944) as research associates, as well as Kenneth Arrow, Lawrence Klein, Evsey Domar, Franco Modigliani, Don Patinkin, and Herbert Simon (Hildreth 1986, 8). This new team drove the commission toward structural estimation, following the seminar Marschak had previously held at the New School of Social Research in New York, which was attended by Haavelmo, Schumpeter,[21] Leontief, Modigliani, Abraham Wald, Koopmans, Paul Samuelson, and Arrow from 1940 until his move to Chicago. For the first time, the elite of U.S. econometrics was directly involved in the day-to-day research of the commission. A witness to that period, Lawrence Klein (1987, 413), marveled at the intensity and diversity of the working of the commission: "It was the most unusual group of people there. To think of having Marschak, Koopmans, Haavelmo, Hurwicz, Anderson, Patinkin and eventually Arrow, Herman Rubin, Roy Leipnik and Herman Chernoff, with many visitors like Jan Tinbergen and Ragnar Frisch. It was just a tremendous number of people who were unusually talented, and they all congregated in that one place."

Yet this program for the generalized use of probabilistic theory for structural estimation was soon exhausted. Despite massive efforts and the elaboration of sophisticated techniques, these did not lead to very different estimation results in relation to standard ordinary least squares. As a consequence, although since 1932 the commission's motto had been "Science is measurement" (Christ 1952, 61), structural estimation waned during the 1940s. Consequently, empirical research in structural estimation and "econometrics started to become a secondary interest of the Cowles staff as the 1940s ended" (Epstein 1987, 110). Simultaneously, Frisch's parallel research program on business cycles, avoiding the probabilistic approach, was also paralyzed: "By 1939 Frisch's original research program for the Institute was in shambles, as his high-profile business cycles/time series project had fallen apart" (Bjerkholt 2005, 520).

Furthermore, the relationship with the economics department at Chicago was tense and deteriorating. The department had always been a center of intense scientific innovation: during its first period, it was dominated by Thorstein Veblen, Mitchell, and J. M. Clark, who had moved to

21. Schumpeter's attitude was remarked by the young Kenneth Arrow (1978, 71), since he "treated the whole matter with the benevolent condescension of a lord among well-meaning and deserving but necessarily limited peasants." These paradigmatic shifts are discussed in Israel and Ingrao 1985.

Columbia; then, the duo Frank Knight and Jacob Viner transformed it into the counterinstitutionalism headquarters. The Chicago school of the 1920s was a peculiar mixture of atypical neoclassical economists of Austrian and Marshallian inclination but suspecting the efficiency of laissez-faire, supporting government intervention in recessions but later hostile to the Keynesian movement. It attracted mathematically trained economists such as Lange, Schultz, and Paul Douglas, who were partial to the Lausanne school of economics, but Knight himself did not bet on mathematics. When the Cowles people, with Marschak and Koopmans, came to the university, the tension was unavoidable. Koopmans had replaced Marschak in June 1948: it was time for the commission to turn to developments in general equilibrium theory and models, to the discontent of some econometricians. Koopmans generated a "metamorphosis" of the commission, both in structure and in research (Mirowski 2002, 249).

Although this turn of events is quite well known, the concomitant discussions inside econometrics itself are generally ignored, despite their importance, since the available evidence is buried in different archives. Oskar Morgenstern was the first to criticize the standard criterion used to select fellows, mathematical ability, and in this frameworkthis also represented an obvious expression of *malaise* in relation to Koopmans's strategy of promoting abstract generalizations based on general equilibrium models as the mode of thought of the econometric circles. Consequently, much of the discussion Morgenstern ignited centered on the value of the inductive versus the deductive approach to mathematics and statistical verification in economics.

At first, Morgenstern's 1953 comments were presented as simple proposals for two criteria for the choice of the next fellows: they should have proceeded to applied work (and not only abstract modeling) and created new methods. Morgenstern himself had provided such new methods with John von Neumann, in their 1944 book *Theory of Games and Economic Behavior*. Later, in 1959, in a project at the National Bureau of Economic Research, he developed these insights for an international monetary system, considering pairs of countries as a two-person game, with data from four countries: France, England, the United States, and Germany (Morgenstern 1959).

In any case, his proposal ignited an intense debate over the type of mathematical and statistical reasoning adequate to the society's purposes. Morgenstern's remarks were not explicitly directed against Koopmans or the Cowles Commission, but he offered an alternative view on the role of

theory, of empirical models, and of economic research. The correspondence (available at the Koopmans Archive at Yale University and the Morgenstern Archive at Duke University) reveals two methodological views on the course of econometrics, namely, on the articulation between the empirical and theoretical levels. The correspondence began by an apparently naive suggestion by Morgenstern to his fellows, in a letter to the executive director, Rosson L. Cardwell. Cardwell (1953) elaborated a report on this proposal and the following reactions to it:

> Recently a suggestion from Professor Oskar Morgenstern was circulated which said, in part, "in my view the Fellows ought to be persons who have done some econometric work in the strictest sense. That is to say, they must have been in one way or another in actual contact with data they have explored and exploited, for which purpose they may have even developed new methods."

This proposal for selection criteria valued those who developed empirical work or applied innovative methods, and it divided the fellows. Some supported Morgenstern's proposal and some rejected it: of the eleven economists whose answer is known, six agreed, with five opposed. To the first group belong Johan Henryk Åkerman, Oskar Anderson, Robert Charles Geary, Prasanta Chandra Mahalanobis, Charles F. Roos, and Erich Schneider,[22] and all of them interpreted Morgenstern's suggestion as a radical shift in standard practices inside the Econometric Society.

Johan Henryk Åkerman (1896–1982), a Swedish economist, worked on the theory of economic cycles. Considering Morgenstern's proposal, he favored empirical work as a criterion for selection. Besides the relevance of mathematical expertise and knowledge in economics, Åkerman emphasized the connection with empirical work:

> My comment is that Professor Morgenstern is fundamentally right. In later years one has received the impression that the requirements for an econometrist are absolutely to be a mathematician, knowledge of economics being desirable while contact with data is not necessary. It seems to me that these tendencies are more conspicuous in the content

22. In the Tjalling Koopmans' Collection, some handwritten notes were added to this letter: two columns were made, one bearing the sign +, the other a −, and beneath were inscribed the initials of those members of the Econometric Society who agreed with Morgenstern (+), and of those who disagreed (−), as if it were a dissension whose solution depended on the number of supporters on each side. This note by Koopmans illustrates a situation of scientific strife.

of *Econometrica* than in the election of Fellows. As statistical data ana-
lyzed by statistical methods form the basis of our science, economic
theory formulating rules of interdependence and mathematics consti-
tuting a language properly expressing some of its laws, the present stress
on mathematical forms as the Society's main theme may formalize our
science and estrange it from the trends of economic and social reality.
(Cardwell 1953)

This comment was developed in a letter by Åkerman, arguing that there
are two kinds of econometric work: one that is methodological and deduc-
tive, and another that is based on empirical analysis. The latter should be
developed: "If the content of Econometrica were to be divided in two
parts—one including contributions of a deductive kind as well as method-
ological studies and the other contributions based on facts—it is possible
that a new stimulus could be given to empirical work in the domain of
econometrics" (Åkerman 1953).

Another supporter of Morgenstern's proposal was Oskar Anderson
(1887–1960), but there is no statement explaining the reasons for this sup-
port: Cardwell just states "O. Anderson: 'I wholly agree with the sugges-
tions of Professor Oskar Morgenstern'" (Cardwell 1953).

Robert Charles Geary (1896–1983) was an Irish statistics expert respon-
sible for the division of national accounts in the United Nations, in New
York, between 1957 and 1960. Geary aimed to demonstrate the relevant
scientific significance of empirical work. For him, the choice should be
between work in descriptive statistics, with no connection to economic
theory, and the use of statistical approaches for econometric work:

I entirely agree with Professor Morgenstern. One of the main preoccu-
pations of directors of national statistical offices is that so large a part
of the vast output of their statistics is rarely put to any scientific use (and
sometimes to no use at all). In the world generally econometric work,
in Morgenstern's sense, is negligible in relation to the volume of statisti-
cal raw material. There is a perfectly clear distinction between math-
ematical statistics on the one hand and econometrical applications on
the other. The Society should accept as candidates for Fellowships only
those persons who made sophisticated applications of statistical data.
(Cardwell 1953)

In Morgenstern's archive at Duke University there is a copy of Geary's let-
ter specifying the reason for supporting his suggestion, in reply to Cardwell's

circular letter of 21 July 1953. To Cardwell's, Geary adds that theoretical development is much more advanced than empirical applications:

> The reason why there are one hundred research economists and ten mathematical statisticians to each econometrician is mainly because econometrics is a far more stern and onerous discipline than mathematics or theoretical economics. The econometrician knows that a long series of calculation may not yield a single useful result. Even negative results have, however, a definite value in sometimes showing the invalidity or inadequacy of economic assumptions. Theory is far ahead of practice. It would be no great harm if the mathematically-minded could impose a self-denying ordinance on themselves for a period as regards theory, and apply instead existing theory to actual data.
>
> The society should take a positive attitude on this important matter with a view generally to the development of genuine econometric work. It might, for instance, circularise the economic faculties of universities throughout the world suggesting that practical econometrics should be included in the courses of economics. This would mean that laboratory work (the laboratories being equipped with computing machines, books of reference, etc.) would become part of the training of economists.
>
> This comment goes far beyond Professor Morgenstern's suggestion. It is submitted because it is of vital interest to national statistical offices. I have sent a copy of this letter to Professor Morgenstern. (Geary 1953)

The Indian scientist Prasanta Chandra Mahalanobis (1893–1972) devoted himself to statistical application and founded the Indian Institute of Statistics. He had studied in England with R. A. Fisher, and his work was particularly praised by Hotelling. In what concerns Morgenstern's suggestion, Mahalanobis also considered the importance of empirical work as opposed to abstract mathematical formalism:

> I agree with Professor Morgenstern's views about the importance and need of actual contact with data. Abstract work and the use of mathematics are but means to an end. In my opinion, econometric work must seek justifications in the interpretation and utilization of information in the form of statistical data so that the value of such work should be assessed not by the abstract quality of the tools but the fruitfulness of the results. Due consideration should, of course, also be given to fundamental advances in technical methods when there are good prospects of using such methods in practice. (Cardwell 1953)

Charles F. Roos had been the director of the Cowles Commission between September 1934 and January 1937, and he was a founder of the Econometric Society, over which he presided in 1948. His work was mainly developed in dynamic economics and planning, as he distanced himself from the typical econometric work, and he also supported Morgenstern.

Erich Schneider (1900–1970) was a lecturer at the University of Kiel, in Germany, and director of the journal of the Institut für Weltwirtschaft. His training in economics was later complemented by the study of mathematics and physics. In 1932 he became associate lecturer in Bonn, together with Schumpeter, devoting himself to the theory of monopoly and to different forms of market structure. In 1936 he became a lecturer at Aarhus University, in Denmark, where he was one of the founders of the faculty of economics. He eventually became close to Scandinavian economists, particularly Frisch and Haavelmo. In 1946 he accepted a position as professor at the University of Kiel. He was a member of the Econometric Society, having organized a conference of the society in Kiel, in 1955 (Bombach 1971).

Schneider encouraged the study of statistics in economics courses in Germany, and this explains his approach: "I am in perfect agreement with prof. Morgenstern's remarks. A necessary condition to be able to be elected as a Fellow of the society should be that the candidate in his research work has been in actual contact with data and has done some significant econometric research work" (Cardwell 1953). On the other hand, the economists disagreeing with Morgenstern argued for keeping the empirical or statistical work under the vigilance of theory—a distinctive brand of the previous debate on "measurement without theory." Furthermore, they feared that, were the criterion of practical application to be applied, some valuable theoreticians would be excluded from the society. In this group, contesting Morgenstern's proposal, were Luigi Amoroso, Griffith Conrad Evans, René Roy, and essentially the tenors of the Econometric Society and the Cowles Commission, Tjallings Koopmans and Jacob Marschak.

The Italian Luigi Amoroso (1886–1965) referred to his own mathematical inclination to declare that

I, too, have more than once experienced the unpleasant feeling mentioned by Prof. Morgenstern in seeing pages crammed with formulae which, in general, apply only with limitations to the economic reality. And in this connection allow me to recall Edgeworth's golden maxim:

if discretion in the use of algebraic symbols constitutes an elegance for the mathematician, it constitutes a duty (or: a necessity) for the economist. I cannot, however, consent (or: agree) with the opposite thesis which, it seems to me, is implicit in the thought of my eminent colleague. I.e., I cannot agree that econometrics should consider as its principal task the mathematical analysis of statistical data, with the purpose of obtaining directly the empirical laws governing the phenomenon in question.

In reality econometrics (like Janus' temple) is two-faced: at the same time an inductive and deductive science. And these two lines must be harmoniously blended, avoiding any excess in one or the other direction.

By this criterion I let myself be guided in indicating candidates for fellowship; this I do within the limits of my possibilities which are bounded by my incomplete knowledge of the papers written by the individual authors. (Cardwell 1953)

Amoroso's interpretation assumes Morgenstern's suggestion to imply the need for more inductive statistical work. His rejection of Morgenstern's proposal is based on his nonacceptance of an exclusively inductive interpretation of econometrics practice, and in this sense he quoted Edgeworth, considering the use of mathematics essential to econometrics.

The American mathematician Griffith Conrad Evans (1887–1973) was a lecturer at Rice University, Texas, and by 1934 had moved to the University of California, in Berkeley. Responsible for the department of mathematics there, he recruited some European mathematicians and statistics experts who were crucial for the development of econometrics, such as Jerzy Neyman. Evans was recognized as one of the first researchers on dynamic economics (Morrey 1983). His comment to Morgenstern's suggestion reflects his essentially mathematical work, rejecting the proposal on the grounds that it might narrow econometric work down to statistical work:

I do not see surpassing merit in Professor Morgenstern's proposed restriction on candidacy for fellowship in the Econometric Society. It may well be that a person's contribution will depend fundamentally on something other than statistics and that a corresponding statistical problem will be treated by another. Perhaps I am influenced by the fact that I have myself only a neighbourly interest in statistics, but there are problems—e.g., the theory of money,—in which statistics seems to play a secondary role.

Certainly, sound statistical investigations are very important in themselves, as well as to serve as a brake or as an incentive to speculation, and it is a matter of congratulation that government statistical work, for example, is in so much better shape than it was a few years ago. The Econometric Society has not been without influence. But I believe that part of its influence on the progress of economics is due to the broad statement of Section 1 of its Constitution; and I hope that the concepts inherent in the founding of the Society will continue to be exemplified in its choice of Fellows as well as in its membership and its publication. (Cardwell 1953)

Together with François Divisia and Maurice Allais, René Roy (1894–1977) was part of the French marginalist school. Roy was one of the heads of the Institut international de statistique in Paris and specialized in transport economics. He argued that Morgenstern's proposed restriction might deprive the society of important theoretical analysis:

If it is desirable that econometricians not exclusively favor abstract investigations and if it is convenient to encourage research and publications oriented toward the use of concrete data, it would nevertheless be excessive to ask a candidate to exceed in such works in order to be a fellow, since in that case we would deprive the society of the active participation of people with original and sometimes important work in the field of theoretical analyses. (Cardwell 1953; our translation)

But the decisive reactions came from the dominant figures in econometrics at the time. Koopmans, the director of the Cowles Commission, did not agree with Morgenstern because he considered that work in theoretical economics was essential:

I wish to register disagreement with Professor Morgenstern's proposal that would limit nominations for Fellowship in the Society to scholars who have made studies involving actual processing of statistical data. While I would feel alarmed at the state of econometrics if not a large majority of the fellows of the Econometric Society were in this category, the progress of economic knowledge requires concurrent work in economic theory, statistical inference, and econometric work proper, and important contributions to the common purpose of these three activities have been made by individuals preferring to work in only one compartment. (Cardwell 1953)

Marschak took the same stance. He had been until recently the director of the Cowles Commission, and he considered the proposed criteria for selection a dangerous limitation for econometrics: "I disagree with the statement of Dr. Morgenstern as circulated. If his suggestions were adopted the following men could never aspire to the fellowship: John v. Neumann, Vilfredo Pareto, and Leon Walras'" (Cardwell 1953). The reference to von Neumann, who was by that time occupied elsewhere and indifferent to what happened in the province of economics, is obviously a cryptic criticism of Morgenstern, since they had coauthored *Theory of Games and Economic Behavior*, the book establishing their authority in the province of economics. But it missed the target, since of all people Morgenstern was aware of von Neumann's dissatisfaction in relation both to neoclassical economics and to econometrics (Louçã 2007), as he considered that never before had the theory been so dependent on purely abstract modeling in the framework of general equilibrium economics with scarce claims on reality. Under the prevailing econometrics of the 1950s, economics became a branch of mathematical logic.

In any case, the front of rejection eventually came to dominate, and nothing changed. As the argument goes, these econometricians stood for a theoretical choice: the integration of the different forms of analysis under the guidance of general equilibrium models. Instead, among those supporting Morgenstern's proposal there was greater diversity of opinion. His suggestion was indeed a suggestion or an appeal to further empirical work applied to economic reality. In the same sense, Åckerman, Mahalanobis, and Geary rejected the dominance of axiomatics, and Geary, even further, rejected theoretical analysis without application to reality. Furthermore, the dispute turned out to be a difference of evaluation of the role of induction and deduction in econometrics.

This debate calls into question the work of econometrics either as the reality of the model or as the model of reality: while structural econometrics, as represented by Koopmans, stood for generalizations from theory, Morgenstern and his fellow thinkers argued for empirical work devoted to researching real data (or new models of it, implying the search for new approaches) and not to constructing abstractions.

Much later, both Frisch and Haavelmo exhibited the same concerns about the danger of econometrics being trapped by abstraction. This was obvious in Frisch's (1970, 4) argument against "playometrics," or statistical exercises without regard to real economic problems. Haavelmo (1957,

352) also pointed out the limits of theoretical applications if ignorant of concrete facts: "Contrary to what many people seem to think, it is in the practical application of theories to facts, in attempts to draw conclusions on the concrete level, that the need for stringent logic and fancy mathematics really shows up."

It is not certain that, at that time, he was still understood by the fellowship.

Conclusion

The present essay has discussed the creation of the Econometric Society and the internal discussions among its members about the role of mathematics. In the first debate, some of the founders took opposing views of the degree of mathematical expertise required for participating in the society: Roos and Fisher argued for an open society for all economists, whereas Schumpeter, Frisch, and Bowley proposed a closed club of mathematical economists. The solution to the divergence was to define two levels of affiliation, that of regular members and that of fellows, the latter to be elected under strict requirements of demonstrated capacity in mathematics.

In any case, the very definition of mathematical expertise was pluralistic, as was the conception of economics itself. In the first years of the society Frisch and Tinbergen were there, but so was Keynes. Neoclassicals of different strands crossed paths with institutionalists, the Keynesians-to-be, and other economists. Schumpeter was one of the masterminds of the mathematization of the society, although he was totally innocent of mathematics. Mills, the institutionalist, was to be appointed associate editor of *Econometrica* in its first year, and, although he was twice rejected in the election for fellow, he always proved enthusiastic about the society.

When the question was reassessed twenty years later, in 1953, the landscape of econometrics had changed dramatically. The pluralistic views of the required mathematical skills had been superseded in the 1940s by the program of structural estimation, but that program was in shambles by the 1950s.

Morgenstern's attack on the criteria for selecting fellows can therefore be interpreted as an echo of his and eventually von Neumann's dissatisfaction with the course of econometrics, and certainly as a criticism of the society's current practices (asking for openness and for the treatment of real data) and of its dominant mathematical approach (asking for innova-

tion). In that, Morgenstern was opposed by Koopmans and Marschak, and the tenets of the abstract approach prevailed.

References

Åkerman, J. H. 1953. Letter to R. L. Cardwell, 30 July. Oskar Morgenstern Papers, Rare Book, Manuscript, and Special Collections Library, Duke University, Durham, North Carolina.

Bjerkholt, O. 1998. Ragnar Frisch and the Foundation of the Econometric Society and Econometrica. In *Econometrics and Economic Theory in the 20th Century*, edited by S. Strøm. New York: Cambridge University Press.

———. 2005. Frisch's Econometric Laboratory and the Rise of Trygve Haavelmo's Probability Approach. *Econometric Theory* 21:491–533.

Bombach, G. E. 1971. Erich Schneider, 1900–1971. *Revue de l'institut international de statistique* 39.2:250–51.

Cardwell, R. L. 1953. Letter to the Fellows of the Econometric Society, 18 September. Tjalling Koopmans' Collection, Manuscript and Archives, Yale University Library, New Haven, Connecticut.

Christ, C. 1952. History of the Cowles Commission, 1932–1952. In *Economic Theory and Measurement: A Twenty Year Research Report*. Chicago: Cowles Commission for Research in Economics.

Cowles, A. 1932a. Letter to Ragnar Frisch, 10 March. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

———. 1932b. Letter to Ragnar Frisch, 4 November. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

Darnell, A., and J. Evans. 1990. *The Limits of Econometrics*. Aldershot, U.K.: Elgar.

Day, E. 1932. Letter to Frederick Mills, 11 January. Papers of Joseph Alois Schumpeter, 1914–1960, Harvard University Archives.

Divisia, F. 1926. Letter to Ragnar Frisch, 1 June. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

———. 1930. Letter to Ragnar Frisch, 23 July. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

———. 1931. Letter to Ragnar Frisch, 15 August. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

Editor's Note. 1954. *Economic Journal* 64.253:2.

Epstein, R. J. 1987. *A History of Econometrics*. Amsterdam: Elsevier.

Fisher, Irving. 1931a. Letter to Edwin B. Wilson, 28 November. Papers of Joseph Alois Schumpeter, 1914–1960, Harvard University Archives.

———. 1931b. Letter to Joseph Schumpeter, 16 June. Papers of Joseph Alois Schumpeter, 1914–1960, Harvard University Archives.

———. 1931c. Letter to Ragnar Frisch, 18 October. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

Frisch, R. 1926a. Letter to François Divisia, 4 September. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1926b. *Sur les semi-invariants et moments employés dans l'étude des distributions statistiques*. Oslo: Akademi i Oslo.

——. 1927a. The Analysis of Statistical Time Series. Mimeo.

——. 1927b. Frisch-Roos memorandum, 27 October. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1931a. Letter to Irving Fisher, 3 July. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1931b. Letter to Joseph Schumpeter, 5 November. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1932. Letter to Alfred Cowles, 24 November. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1933a. Letter to Irving Fisher, 11 January. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1933b. Letter to Irving Fisher, 6 December. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1933c. Letter to William F. C. Nelson, 13 March. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1934. Letter to Georges Bousquet, 23 March. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1936. Letter to Arthur Bowley, 27 July. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

——. 1970. From Utopian Theory to Practical Applications: The Case of Econometrics. *American Economic Review* 71.6:1–16.

Geary, R. C. 1953. Letter to Rosson L. Cardwell, 24 July. Oskar Morgenstern Papers, Rare Book, Manuscript, and Special Collections Library, Duke University, Durham, North Carolina.

Haavelmo, T. 1957. The Role of the Econometrician in the Advancement of Economic Theory. *Econometrica* 26.3:351–57.

Hildreth, C. 1986. *The Cowles Commission in Chicago, 1939–1955*. New York: Springer-Verlag.

Israel, G., and B. Ingrao. 1985. General Economic Equilibrium Theory: A History of Ineffectual Paradigmatic Shifts. *Fundamenta Scientiae* 6.1:1–45, 6.2:89–125.

Klein, L. 1987. The ET Interview: Lawrence Klein. *Econometric Theory* 3.3:409–60.

Leavens, D. 1948. Letter to Ragnar Frisch, 24 August. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

Louçã, F. 2007. *The Years of High Econometrics: A Short History of the Generation That Reinvented Economics*. London: Routledge.

Lutfalla, Georges. Letter to Ragnar Frisch, 10 November. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

Mills, Frederick. 1932. Letter to Edmund Day. Papers of Joseph Alois Schumpeter, 1914–1960, Harvard University Archives.

Mirowski, P. 1989. The Probabilistic Counter-Revolution, or How Stochastic Concepts Came to Neoclassical Economic Theory. *Oxford Economic Papers*, n.s., 41.1:217–35.

———. 1991. The When, the How, and the Why of Mathematical Expression in the History of Economic Analysis. *Journal of Political Economy* 5.1:145–57.

———. 2002. *Machine Dreams: Economics Becomes a Cyborg Science*. Cambridge: Cambridge University Press.

Morgenstern, O. 1959. *International Financial Transactions and Business Cycles*. New York: National Bureau of Economic Research.

Morrey, C. B. 1983. *Griffith Conrad Evans, 1887–1973: A Biographical Memoir*. London: National Academy Press.

Roos, Charles. 1935. Letter to Joseph Schumpeter, 25 November. Papers of Joseph Alois Schumpeter, 1914–1960, Harvard University Archives.

Schumpeter, Joseph. 1931a. Letter to Ragnar Frisch, 28 October. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.

———. 1931b. Letter to Ragnar Frisch, 11 December. The Ragnar Frisch Archive, the Manuscript Collection, the National Library of Norway, Oslo.