The Econometricians' Statisticians, 1895–1945

John Aldrich

"Modern statistical theory originated in England, and is today advancing faster there than in any other country." So Harold Hotelling (1930a, 186) informed the American Statistical Association after a visit in 1929. Hotelling traced the origins to Karl Pearson and attributed the current progress to R. A. Fisher. A few years later there were two new theorists to watch, Jerzy Neyman and Abraham Wald, Fisher's main rivals/successors in the theory of statistical inference. I will be considering these four and how they interacted with the econometricians—with H. L. Moore, Mordecai Ezekiel, Henry Schultz, Tjalling Koopmans, and Trygve Haavelmo especially. I also note some statisticians who might have been, but were not, the econometricians' statisticians.

Research in the history of econometrics has concentrated—naturally enough—on the distinctive concepts developed by econometricians, "structure" and "identification" being outstanding instances, but the history of econometrics is also a history of using ideas from statistics. These ideas and how they traveled will be my concern; more broadly, I will be considering the way statisticians and econometricians worked together, or,

Correspondence may be sent to john.aldrich@soton.ac.uk. I would like to thank Olav Bjerkholt for discussion and for providing me with documents. Kevin Hoover commented on an earlier draft. Karl Pearson's correspondence is part of the Pearson Papers at University College London. R. A. Fisher's letters are at the University of Adelaide. Some of the letters have appeared in volumes of correspondence—Bennett 1990 is the relevant one—and some are available on the library's Web site.

History of Political Economy 42:1 DOI 10.1215/00182702-2009-064

more exactly, the *ways*, for there was no one pattern. Pearson and Fisher were primarily biometricians, ready to provide instruction but not involving themselves in the special problems of economics. Neyman was happy to be consulted on the theory of testing but he had his own ideas on how econometrics should be done. Wald was different again; he was an economic statistician before he was a statistical theorist and, as well as acting as a consulting statistical theorist, produced econometric theory of his own. The econometricians sought different things from the statisticians: techniques for data analysis from Pearson, techniques and foundations from Fisher, and new foundations from Neyman and Wald. The emphasis on foundations may seem odd in a branch of applied statistics, but some of the econometricians took theory in all forms very seriously. Wald spent nearly half of his short career in statistics with the econometricians, but for the others—even Neyman—their transactions with the econometricians were small episodes in big lives, and the otherwise admirable biographies by E. S. Pearson (1936–38), Joan Fisher Box (1978), and Constance Reid (1982) do not register them.

The present essay tries to fill a gap between the history of econometrics and the history of statistics. On the econometrics side it complements the surveys by Roy J. Epstein (1987), David F. Hendry and Mary S. Morgan (1995), Morgan (1990), and Duo Qin (1993)—see also Christopher L. Gilbert and Qin's (2006) recent brief account—and Olav Bjerkholt's (2005, 2007) more specialized studies of Haavelmo and Ragnar Frisch. Bjerkholt's research on the Frisch network has been invaluable, and I often seem to be writing footnotes to his footnotes. Viewed from the history of statistics, my essay belongs to "reception studies," with connections to Stephen M. Stigler's (1986) writing on Pearson's work on correlation and regression and my own writings (Aldrich 1997, 2005) on Fisher's work on maximum likelihood and regression. There are especially close links with Stigler's (2007) "epic story of maximum likelihood": maximum likelihood went early to econometrics, and all of my statisticians had some role in its story. The one significant common character with Stigler's (2002) survey of economist-statisticians is Hotelling; he appears at critical turns in the present story, although he cannot really be counted as an econometrician and possibly not even as an econometrician's statistician. Because channels of communication are under examination, the literature on societies and journals is relevant, for example, Bjerkholt 1995 and Stigler 1996.

The plan is this. Sections 1 and 2 consider Pearson's correlation/regression and how it entered economics. Section 3 relates how Fisher's techniques became part of American econometrics, a process to which Hotelling's visit and the subsequent public relations campaign—including two *JASA* articles (1930a, 1931b)—contributed. By the mid-1930s ideas were racing round a network created by Ragnar Frisch, and people were traveling too. Section 4 describes the network and section 5 one of the travelers, Koopmans, who took foundations and maximum likelihood from Fisher. Neyman was a member of the original network centered on Oslo while Wald joined the network as it was reconstituted in the United States; their interactions with the econometricians are the subject of sections 6–9. Section 10 is a coda on statistical inference and structure, and section 11 sums up.

Pearson and Fisher were English statisticians; Neyman was a Pole, who after being co-opted into English statistics, moved to California; Wald went from Vienna and the Continental tradition in statistics to America, where he became one of the leaders of mathematical statistics. The dates in my title, 1895 and 1945, are associated with an Englishman, or rather a Scot, in London (Udny Yule) and a Dutchman in Chicago (Koopmans). The associations emphasize that the stories below are details in larger narratives: the rise of the English statistical school and of its western successor; the rise of econometrics in the United States and Continental Europe and its arrested rise in England; the movement of econometric theory from the Continent to America.

1. Pearson and Yule

Karl Pearson (1857–1936) was one of the "extremely small number" of "extraordinary individuals" who made "things go differently," wrote Raymond Pearl (1936, 653). To a grand vision of the statistical method as a universal method Pearson added charisma and ambition for his subject and for himself. He also had an establishment, a department at University College London, and, from 1901, a journal, *Biometrika*—"a journal for the statistical study of biological problems," it said on the masthead. Pearson prepared the ground for Fisher and Neyman and their dealings with the econometricians: he developed the basic techniques for investigating relations, he created the expectation that significant statistical work would come out of England, and he created the infrastructure at University College.[1]

1. Of course there was statistical theory before Pearson and applications to economics before 1900; for the former, see Stigler 1986 and Hald 1998 and, for the latter, Morgan 1990 and Aldrich 1992. For a guide to the literature on Pearson, see Aldrich n.d.b.

Pearson had many ideas but for the econometricians the most important were correlation and regression. The idea of a measure of correlation had been introduced by Francis Galton, and after some input from F. Y. Edgeworth, Pearson gave it definitive form in an 1896 article that was part of a series of "mathematical contributions to the theory of evolution" published in the *Philosophical Transactions of the Royal Society*; the article, "Regression, Heredity, and Panmixia," also treated bivariate and trivariate regression. Pearson wrote on biometry and published in mathematical journals, but correlation and regression went straight into the economics journals taken by Pearson's student/collaborator, Udny Yule (1871–1951).[2]

Yule used two channels, the *Journal of the Royal Statistical Society* and the *Economic Journal*. The Royal Statistical Society served those interested in numerical facts about society by running meetings and publishing a journal. Economists were prominent, although by 1895, when Yule joined, they had their own society, the British Economic Association (now the Royal Economic Society), which published the *Economic Journal*. Soon after the new society was formed Marshall expressed the hope that the new and old would "ultimately amalgamate" (Whitaker 1996, 2:81). Even in 1892 this was unrealistic, for statistics was more than economics in figures. However, the modern notion of the statistician as one with special techniques applicable to any kind of material, social or not, only became common after the Second World War.[3]

Yule promoted Pearson's statistics among the economists-statisticians on a broad front by publicizing it (Yule 1897a), applying it (Yule 1896a), and developing useful extensions. Expositions and results appeared in the *Economic Journal*: he showed economists how they could use correlation to analyze pauperism (Yule 1895), and then he introduced them to his new concept of partial ("nett") correlation (Yule 1896b). Justifications and details appeared in the *Statistical Journal* (Yule 1897b, 1899). An engineer-physicist by training, Yule studied the standard economics work, Marshall's *Principles* (1890), and could quote from it effectively, as he did on mutual determination (1895, 605) and on demand curves (1915, 305). One of the topics Yule worked on with his friend R. H. Hooker (1867–1944)

2. Stigler (1986, pt. 3) describes the background in statistical theory and biometry, and Leslie W. Hepple (2001) supplies additional background on Yule and his activities. The regression story is carried forward into the twentieth century in Aldrich 2005.

3. The point is elaborated in Aldrich 2008a.

came from the *Principles*, the relation between demographic and economic variables.

Yule went on preaching correlation to the economists but after 1900 he had new projects, including the development of a parallel theory for attributes, and here the applications were not to economics but to biology and medicine. The most common meaning of *econometrics* today is sophisticated statistical analysis of economic data, and Yule's correlation and regression studies would be considered mainstream econometrics. On Frisch's interpretation of econometrics (see section 4 below) they would not count at all. They did not use economic theory and did not contribute to the two great projects of making demand theory numerical and creating a quantitative business cycle theory. Yule's econometric phase—if it can be called that—was over by 1914, although his important work on time series analysis came after the war; Hooker also left economics, in his case for agricultural meteorology.[4]

Putting aside the question of Yule as an econometrician, what of him as an econometrician's statistician? He did what was needed by demonstrating the new methods and by producing a guidebook, the *Introduction to the Theory of Statistics* (1911), and yet his influence was limited: his 1899 multiple regression exercise long remained the only multiple regression exercise. The econometricians did not study with him, consult him, or invite him to visit them. He had a reputation in the club of the Statistical Society but less weight outside. In the years between 1899 and 1912 he had no university position apart from the part-time Newmarch Lectureship at University College. From 1912 he was the lecturer in statistics at the Cambridge University School of Agriculture; he taught and published but did not attract.

2. English Statistics and American Economics

Yule was the only person who went out from University College to teach the economists, but there were visitors who came to learn. There were no visitors from Russia, but Pearson's work was read there, and Alexander Chuprov and Eugen Slutsky were among those impressed by it. Slutsky wrote a book on correlation in economics and consulted Pearson; he even offered a paper that appeared in the *Statistical Journal* in 1913—no article on econometrics ever appeared in *Biometrika*.

4. For more on the activities of Yule and Hooker, see Aldrich 1995 and Klein 1997.

The Pearson archives also have letters from the American economists Irving Fisher (1867–1947) and H. L. Moore (1869–1958). Fisher and Moore were first-generation American PhDs in the age when the metropolis was still across the Atlantic; they read the European literature and visited England and the Continent early in their careers. Fisher first knew Pearson from his *Studies in Evolution* (1897). Fisher sent Pearson a copy of his review, telling him, "I admire them greatly and have found them very helpful." His only disagreement was over the desirability and inevitability of socialism. The studies made no use of correlation, but Fisher (1898) was soon writing about Yule's "Theory of Correlation" (1897b). Fisher expounded Yule's bivariate analysis; he did not mention partial correlation or multiple regression and in fact never used them.[5]

Fisher did not hurry to adopt the method of correlation—he first used it in his *Purchasing Power of Money* (1911)—but his colleague J. P. Norton used it in his PhD thesis, *Statistical Studies in the New York Money-Market* (1902); Yule (1909, 727) considered Norton had used the method "in a very able manner." In 1905 Fisher invited Pearson to Yale to give some lectures, mentioning that Norton had made a "special study" of his methods; a postscript mentioned the possibility of a more permanent arrangement. Pearson would not have been Fisher's first visitor from England—Edgeworth had visited in 1902—but he turned down the invitation and never visited the United States. The rest of my quartet all went west.

H. L. Moore is a greater figure in the development of statistical economics than Norton, or even Fisher. George Stigler (1962, 18) sums up his achievement as follows: "One can say that Moore was as much a founder of this movement [statistical economics] as any one man is likely to be a founder of a great movement toward which a science has been steadily moving."[6] In the programmatic "Statistical Complement of Pure Economics," Moore (1908, 2) named four investigators who have conceived "an inductive statistical complement of the pure science": Antoine-Augustin Cournot, William Stanley Jevons, Edgeworth, and Vilfredo Pareto. Moore made contact with the two living but nothing followed. Moore's unhappy experience with Edgeworth (and Marshall) is well known (see Stigler 1962), but something can be added about his relations with the statisti-

5. Fisher's econometric work is discussed in Aldrich 2007b.

6. Moore's work is further considered by Philip Mirowski (1990) and Mary S. Morgan (1990).

cians. Pearson figures in the "statistical complement," not as prominently as Edgeworth but more prominently than Wilhelm Lexis and Ladislaus von Bortkiewicz, the leading representatives of the Continental turn in statistics. The stability of statistical ratios, the major theme with the Continental writers, did not worry Moore to the same extent. Moore's knowledge of Pearson's statistics came from the chapter on evolution in the second edition of the *Grammar of Science* (1900). In 1909 and 1913 Moore, then in his early forties, visited London and attended Pearson's lectures. Pearson's influence can be seen in Moore's later books. Yule (1912, 1915) reviewed two of them, favorably although with reservations; he was the American correlationists' biggest supporter in England.

Moore's *Forecasting the Yield and Price of Cotton* (1917), a multiple regression study and his most purely Pearsonian production, was the subject of a plaintive appeal to Pearson in August 1918. Moore's courses were in danger from possible restructuring and he wanted Pearson's opinion of his book and of his work in general:

> But I candidly confess that I have long had a desire to know how you regard the use I have made of what you taught me. If I have done good work, the knowledge that you regard the work as good will increase my strength and fortify my purpose. If I have not been wise in devoting so much time to this particular phase of science, the sooner I realise my mistake the better.

What effect the appeal had is not known; there are no letters from Pearson in the Moore archives at Columbia, and the Pearson collection has only one further letter (from 1920) in which Moore writes introducing a Columbia student to Pearson.

In an intensely emotional way Pearson was Moore's statistician, but it is clear from this letter that they were not close, and there is no suggestion in any of Moore's books that Pearson was involved in their writing. The other American economists who adopted Pearson's techniques—Norton, Fisher, and Warren Persons—learned them from reading Pearson, or more probably his followers, without personal contact with the master: the authors on correlation whom Persons (1910) suggests are A. L. Bowley, Elderton, Hooker, and Yule.

I have mentioned two other English statisticians who were well qualified to be the economists' statistician. F. Y. Edgeworth (1845–1926) and A. L. Bowley (1869–1957) were better qualified than Pearson or Yule in that they were economists as well as statisticians; Stephen M. Stigler (2002) classes Edgeworth as one of the very few "masters" among economist-statisticians and Bowley one of the many in the "middle class." Stigler (1978) has reviewed Edgeworth's extensive and impressive statistical work, and Mirowski (1994, 59) has tried to account for the "paradox that the first economist who readily qualifies as a statistical theorist can in no sense be promoted as the first econometrician." Nor was Edgeworth in any sense the econometricians' statistician; the accounts of his relations with Moore in Stigler 1962 and Mirowski 1990, 1994, bring out his lack of sympathy for the enterprise. The only project in statistical economics into which Edgeworth put any great effort was the stochastic approach to index numbers (see Aldrich 1992). As editor of the *Economic Journal*, Edgeworth published Yule, he discussed index numbers with Irving Fisher, and he advised Bowley on statistical theory, but he is not otherwise visible as a statistical consultant to economists.

Bowley had all the econometric skills—economic statistician, statistical theorist, and mathematical economist.[7] For a time it seemed that Bowley coming from economics and Yule from statistics would join forces, for there is an introduction to correlation in Bowley's *Elements of Statistics* (1901), the first English textbook on the subject. There was a more serious excursion into correlation in the 1920 edition of the *Elements* where correlation was applied to data from Bowley and A. R. Burnett-Hurst's *Livelihood and Poverty* (1915), one of a series of surveys of poverty Bowley conducted. Obtaining data—through the condition of labor surveys and the construction of national income estimates—was Bowley's main occupation through the 1920s and 1930s. The survey data was not used for econometric analysis; that has only been done very recently—see Hatton and Bailey 2002. Bowley was a professor (from 1915) at the London School of Economics but with a very small staff: E. C. Rhodes (1892–1964) joined in 1924, and R. G. D. Allen (1906–1983), in 1928. Rhodes came from Pearson's laboratory and started by doing statistical theory of an Edgeworthian cast before switching to applied statistics; he appears in section 6 below. Allen worked with Bowley on *Family Expenditure* (1935), an econometric study in the Frischian sense, and he did some work on estimation when there are errors in variables, but most of his effort went into consumer theory. Under Pearson and his successors University College was a mecca for statisticians and econometricians, but LSE under Bowley was never that. Bowley retired in 1944.

7. His career is described in Darnell 1981.

The failure of econometrics to take root in England after its spectacular start around 1900 may be worth further comment. The negative attitude of the Cambridge economists must have been a factor when economics was so dominated by Cambridge. When Bowley sent Marshall a copy of the *Elements* (1901), his old teacher told him:

> If I were younger I would study the abstract mathematical doctrine of correlated curves. . . . I think it may occasionally be helpful in determining a controversy as to whether two movements have a causal connection. But at present, we are not ripe for that, I think. (Whitaker 1996, 2:307)

When Marshall was young—in 1866—he had asked Isaac Todhunter, the Cambridge mathematician, about the wisdom of using least squares in economics "without a careful inquiry into the wind" and back came a negative answer, or so he recalled to Keynes in 1910 (Whitaker 1996, 3:266). Bowley (1901), like Yule and Hooker, used correlation to investigate the interaction of economic and demographic variables, but the treatment in the *Principles* stayed frozen in the pre-correlation age, not changing after the fourth edition (1898, 268). The younger Cambridge economists were no more enthusiastic. Marshall's successor, A. C. Pigou, delivered a negative verdict on Yule's work on pauperism (see Stigler 1986, 356–57). In 1910–11 there was the well-known controversy between Pearson and the economists over the effects of parental alcoholism (see Stigler 1999b). The economists followed Irving Fisher's business cycle analysis and monetary economics, but they did not imitate his methods: Pigou (1927, 194–95) reached similar conclusions using graphical comparisons instead of distributed lag correlation analysis. Of the Cambridge economists, Keynes was the most involved with statistics.[8] One constant was hostility to Pearson and another was that correlations had to be handled very cautiously.[9]

Drawing on what he saw in 1929, Hotelling (1930a, 190) reported, "Apart from Yule and Bowley, British economists have not shown any strong tendency to introduce new statistical methods." The detail is wrong, for Yule was not an economist—although it may be an indicator of his lack of presence that Hotelling should mistake him for one—but the spirit is

8. His complex relations with the statisticians are examined in Aldrich 2008a.

9. Of course, Keynes's later controversy with Tinbergen is well covered in the history of econometrics literature; see, for example, Hendry and Morgan 1995, pt. 4.

right and the econometricians we meet from now on are Americans or Continental Europeans. The Statistical Society provided an arena for the battles between Fisher and Neyman in the 1930s, but the *Economic Journal* did not repeat the Yule experiment of 1895–96.

3. R. A. Fisher in America

Ronald Fisher (1890–1962) was, according to Anders Hald (1998, 738), "a genius who almost single-handedly created the foundations for modern statistical science." From his undergraduate days when he was learning to be a conventional Cambridge applied mathematician, Fisher was interested in biometry, genetics, and evolution, fields in which he also achieved great distinction.[10]

From 1919 Fisher had a base at the Rothamsted Experimental Station near London; there, as well as continuing his old researches, he developed methods for analyzing and designing agricultural experiments. Like Pearson, Fisher wrote difficult journal articles, but his *Statistical Methods for Research Workers* (1925a) took his ideas to a larger public. The "research workers" Fisher aimed at were biologists and agricultural scientists, but others could—indeed, should—pay attention. Economists were in the latter class; indeed, the conception of the statistician as an economist who works with numbers had a special horror for him:

> Statistical methods are essential to social studies, and it is principally by the aid of such methods that these studies may be raised to the rank of sciences. This particular dependence of social studies upon statistical methods has led to the painful misapprehension that statistics is to be regarded as a branch of economics, whereas in truth economists have much to learn from their scientific contemporaries, not only in general scientific method, but in particular in statistical practice. (Fisher 1925a, 2)

In the fifth edition (of 1934) Fisher changed "painful" to "unfortunate," but he kept on complaining that in universities statistics "is often absurdly confused with economics" (Bennett 1990, 2). In an earlier paper (Aldrich 2008a) I speculate on where Fisher's "pain" came from.

Fisher's book was not embraced by the British statisticians. In the United States it was noticed because Harold Hotelling (1895–1973)

10. Fisher the biologist appears in section 10 below. For more on Fisher, see Aldrich n.d.a.

reviewed it on his own initiative for the *Journal of the American Statistical Association*. Before describing the passage of Fisher's ideas to America, it will be useful to sketch the scene there. The American Statistical Association resembled its British counterpart in antiquity, composition, and objectives. Hotelling announced Fisher to a society dominated by economists: in 1928, 69 percent of ASA members were also members of the American Economic Association (Biddle 1999, 631). Historians of statistics have not taken much notice of what American statisticians did in the immediate pre-Fisher era: some are noted by Neyman (1976), Patti W. Hunter (1996), and Stigler (1996) but less for their research than for their role in setting up the Institute of Mathematical Statistics and the *Annals of Mathematical Statistics*, institutions that came to life only in the late 1930s following an infusion of European ideas—see sections 4 and 8 below. There were more statisticians than in Britain and more book titles. The mathematical textbooks by H. L. Rietz (1927), J. L. Coolidge (1925), and Arne Fisher (1922)—this Fisher was the Danish-American actuary—looked more to Continental Europe than to Britain, reflecting the pattern in American mathematics generally. Some of the work from Germany, France, Russia, and Scandinavia is discussed by Stigler (1986), but it is much more prominent in Oscar Sheynin's (2005) history. In Britain the outstanding enthusiast for Continental statistics was Keynes (1921), but Keynes's own position was not solid enough for him to support others: see Aldrich 2008a.[11]

Hotelling's (1927b, 412) assessment of Fisher's *Statistical Methods* was that it "is of revolutionary importance and should be far better known in this country." The book's special feature was its attention to small samples, and Hotelling noted, "Common occurrence in economic and other statistics of short series will make the work valuable to a larger class of research workers than the biologists for whom it was primarily intended" (412). The econometricians found Student's *t*-test and its application to regression especially valuable.[12]

Having failed to get Fisher to Stanford (Stigler 1999b), Hotelling went to Rothamsted as a "voluntary worker" for the second half of 1929. Hotelling was Fisher's first and loudest follower: in 1931 Arne Fisher was complaining of his excess to Ronald Fisher: "According to Hotelling, you are the saviour to lead the statisticians out of the wilderness" (Bennett 1990,

11. For another perspective on American statistics in the 1920s, see Aldrich 2007a, sec. 7.
12. For Fisher on these topics, see Aldrich 2005.

310). Hotelling was trained as a pure mathematician but worked at Stanford as a statistician, first in the Food Research Institute and then in the mathematics department. Hotelling also published on economic theory, and in 1931 he went to Columbia as professor of economics to replace Moore.[13]

Some of Fisher's influence on Hotelling can be seen by comparing what Hotelling was writing on fitting equations to data in 1927 and in 1929. Hotelling's "Differential Equations Subject to Error, and Population Estimates" (1927a) used an equation for population growth to extrapolate and interpolate population size. Hotelling's sources were diverse: the logistic trend curve was from Pearl and Reed; there are references to the work of Moore and Irving Fisher; Yule is mentioned and so is E. T. Whittaker's Bayesian method of interpolation. Hotelling's interpolation analysis (1927a, 312) gave the Bayes posterior for the logarithm of population size assuming a uniform prior and conditional on the known values of population at an earlier and a later time. The Bayesian approach would go, presumably under Fisher's influence: "The theory of inverse probability is founded upon an error, and must be wholly rejected," was Fisher's (1925a, 10) verdict.[14]

Hotelling's first post-Fisher paper was written with Holbrook Working, an agricultural econometrician at the Food Research Institute. Working and Hotelling's "Applications of the Theory of Error to the Interpretation of Trends" (1929) was the first publication to adopt Fisher's regression framework.[15] The article gives an interval for the expected value of Y (the "trend") associated with a given year x and then presents a "graphic representation of the range of error of a trend"—a simultaneous "interval" for the entire line—by taking the envelope of the hyperbolae appropriate to each value of x (81). Henry Scheffé (1959, 68) calls it "the earliest non-trivial example" of the S-method of multiple comparisons. It was appreciably more sophisticated than the confidence interval construction Neyman (1934) introduced a few years later. Hotelling's work on interval inference was not followed up by the econometricians or by the statisticians; it is discussed in more detail in Aldrich 2000 in the context of Fisher's development of fiducial inference.

13. For further biographical information, see Smith 1978, Darnell 1988, and Arrow and Lehmann 2005.

14. Fisher's hostility to inverse probability, or Bayesian inference, is described in Aldrich 2008a.

15. The framework is described in Aldrich 2005.

Hotelling was the first modern statistical theorist to come out of America and almost the first Fisherian—John Wishart (1928) could claim priority. Hotelling almost became Fisher's pure mathematician, for he and Fisher projected a *Statistical Methods* with proofs (see Stigler 2007). Hotelling's papers on regression with Working, on maximum likelihood (Hotelling 1930a), and on T^2 (Hotelling 1931a) came directly from reading Fisher, but Fisher's influence on the papers that followed was much less. The paper with Working was not followed by more, and Hotelling promoted Fisher among the econometricians without becoming a Fisherian econometrician. Hotelling did not do econometrics; his contribution to the econometric project and to *Econometrica* (see section 4) took the form of mathematical economic theory. The statistical techniques he devised were not used by economists but by psychologists, and most of Hotelling's small body of empirical work was in demography. Hotelling was a great figure in building the discipline of statistics in the United States: Arrow and Lehmann (2005, 11) write, "It is no exaggeration to state that during the 1930s and early 1940s, Hotelling nearly single-handedly brought American statistics into the modern age and laid the foundation for the extraordinary development of the subject after the Second World War." Numerous individuals were guided into modern statistics by Hotelling—Samuel Wilks, Abraham Wald, and Meyer Abraham Girshick are ones who will appear below.

Mordecai Ezekiel (1899–1974) and Henry Schultz (1893–1938) attended the ASA meeting where the Working and Hotelling paper was presented. They were adopting Fisher's methods, and through their works the methods were widely circulated. Ezekiel's *Methods of Correlation Analysis* (1930) was the standard regression textbook of the 1930s, and Schultz's *Theory and Measurement of Demand* (1938) was the most ambitious American econometric study of the decade.

Ezekiel's *Correlation Analysis* was, in Karl A. Fox's (1989, 67) words, "by far the most comprehensive work on applied regression analysis published up to that time." Ezekiel was an agricultural econometrician, and the applications drew on his own empirical studies and those of others at the Bureau of Agricultural Economics, but what was being applied came from England. "During the last two decades, the English statisticians 'Student' and R. A. Fisher have been developing more exact methods of judging the reliability of conclusions, particularly where those conclusions involve correlation or are based on small samples," wrote Ezekiel (1930, vi). His was the first textbook after Fisher's *Statistical Methods* to present t- and z-tests; the first British textbook to incorporate them was Yule and

Kendall 1937, Maurice Kendall's revision of Yule's *Introduction*. Bowley never adopted them.

At a late stage in the writing of his book Ezekiel consulted Fisher. On 23 April 1930 he wrote asking for help in understanding Fisher's (1928) paper on the distribution of the multiple correlation coefficient; he could manage the material on *t* in Fisher's writings. Ezekiel needed an early reply: "As I would like to make use of this latest development of your methods for judging the reliability of observed multiple correlations in some material which I am about to publish, I am anxious to make exactly the right interpretation of your conclusions."[16] The work—for both of them—was over by June, and on the twenty-sixth Fisher was congratulating Ezekiel "on the skill with which you have dealt with a very difficult subject." Ezekiel visited Fisher at Rothamsted in October 1930.

Schultz had studied with Moore before the war and after war service he spent two terms at the London School of Economics and at University College; he attended many lectures, including some by Bowley and Pearson. However, there is no sign of much influence from those directions in Schultz's writings of the 1920s (they culminate in the *Statistical Laws of Demand and Supply* [1928]); rather Schultz drew on a miscellany of writers who had worked on the problem of estimating relations where the variables are measured with error. In the 1930s Schultz moved decisively toward Fisher, giving up the errors in variables formulation for Fisher's regression scheme where the error is in the equation. Schultz (1929) grumbled that much of Fisher was already in Gauss, but he adopted Fisher's small sample theory and presented it in his *Theory and Measurement of Demand* (1938). Schultz died soon after finishing the book; see Hotelling 1939 for a memorial.

Fisher was not the remote authority Pearson had been. He visited the economists on trips to the United States in 1931 and 1936; in 1936 he gave lectures at the Cowles Commission. When Joan Fisher Box (1978, chap. 12) describes these trips, she emphasizes Fisher's meetings with agricultural scientists and biologists, reflecting what were probably Fisher's own priorities: he was not interested in the econometricians' problems. The exchange with Ezekiel was important for Fisher, for it made him reflect on his own problems. Ezekiel was a presence in Fisher 1930 but not in Fisher's later accounts of the origins of the fiducial argument

16. The story of how Ezekiel engaged Fisher and stimulated him to formulate the fiducial argument, which appeared in Fisher 1930, is told in Aldrich 2000.

(see Aldrich 2000). There are small traces of Fisher's encounters with the econometricians in his writings: a reference to Working and Hotelling was added in the 1936 edition of the *Statistical Methods* and a reference to Schultz appeared in the 1938 edition—Fisher had visited Schultz in Chicago in 1936—but these were not on points where Fisher's thinking was influenced. Of course Fisher's econometrician contacts helped spread the Fisher gospel. Fisher's regression had gone into econometrics; maximum likelihood and his conception of the statistician's task moved a little later, described in section 5 below.

4. Frisch: Network and Project

Hotelling promoted Fisher without himself practicing Fisherian econometrics; Frisch's choreographing of econometricians and mathematical statisticians was a similar feat on a larger scale. Ragnar Frisch (1895–1973) is generally considered the central figure in the econometrics of the 1930s. He was central because of his work as the first econometric theorist, because of his personal influence, and because he was the center of a great network where mathematical economics, econometrics, and mathematical statistics met.[17]

Until the 1930s econometricians could only publish in their local statistical and economic journals and theoretical statisticians only in their local statistical journals and in *Biometrika*. Both groups were marginal except in *Biometrika*, which was itself changing from a biology journal into a statistical theory journal. The situation was transformed by the appearance of the *Annals of Mathematical Statistics* in 1931 and *Econometrica* in 1933.[18] The Econometric Society, of which *Econometrica* was the organ, was dedicated to the "advancement of economic theory in its relation to statistics and mathematics." The mission, the term *econometrics*, and the idea of the journal were Frisch's, and he was the first editor (see Bjerkholt 1998). Irving Fisher, Schultz, and Hotelling, whom we met above, were all involved. Hotelling, Irving Fisher's choice as editor, was on the journal's advisory editorial board along with the statisticians Bowley, G. Darmois, and E. B. Wilson. One of the three associate editors was

17. For overviews of Frisch and his activities, see Arrow 1960 and the introduction to Bjerkholt 1995; the Frisch centenary volume, Strøm 1998, has some useful historical essays, and there is now a book-length treatment by Francisco Louçã (2007).

18. Stigler (1996) has described the birth of the *Annals*, and Bjerkholt (1998), the birth and early history of *Econometrica*.

a statistician, F. C. Mills, in the early days. However, as Bjerkholt (1998) makes clear, Frisch decided what went into the journal, and *Econometrica* was as much his personal journal as *Biometrika* was Pearson's.

Among the things that went into *Econometrica* were surveys of statistical theory. R. A. Fisher (1935c) provided a streamlined version of the distribution theory underlying his tests, and there were contributions from W. A. Shewhart (1933), Darmois (1934), Wilks (1935), and Paul R. Rider (1936)—not that these provided alternatives to Fisher, for he was the author most often cited. Whatever their nationality, these *Econometrica* authors all represented "modern statistics," and in Continental Europe that was a minority interest. The Nordic countries themselves had a strong statistical tradition (see Schweder 1980). Frisch's (1926b) thesis was in statistical theory, on Thiele's semi-invariants. Thorwald Thiele (1838–1910) has recently been reevaluated by Steffen L. Lauritzen (2002) and has come through as a very impressive figure. "When Did the Scandinavians Slip behind the British?" is the subtitle of an unpublished note by Schweder, and his answer is, when Thiele died and Fisher came on the scene. This was clear to Frisch in the 1930s, although ironically one source of tension between him and Fisher was the latter's refusal to admit that his cumulants (of 1930) were semi-invariants rediscovered.[19]

In his years of postgraduate study Frisch acquired a very good knowledge of statistics, including the work of the English school in its pre-Fisher phase—thus in 1925 he published a paper in *Biometrika* in which he used his results on moments to improve upon a solution given by Pearson. Mathematical statistics, as Frisch (1926b, 5) saw it, has two parts, a "rational" part in which implications are drawn from specified stochastic schemes and an "empirical" part concerned with the inverse problem. Frisch did not publish anything on the inverse problem, although at the end of his thesis he indicated that he would. In the thesis he did not reveal what he had done on the inverse problem beyond saying that he disagreed with the Russian statistician A. A. Chuprov (1874–1926) and offering these reflections on the general problem:

> The inverse problem: how to reconstruct from an empirical distribution the scheme which has given birth to the observed distribution is a problem of a rather different kind. To deal with it in depth one cannot avoid entering into philosophical issues and in particular into the the-

19. Some of the correspondence is reprinted in Bennett 1990, 314–17.

> ory of knowledge. It seems to us that too often the scholars in statistics and mathematics have refused to enter into these philosophical issues, instead confining themselves only to deal with technical questions. That is the reason in our opinion why the critical interpretation of the foundation and the methods of statistics have not kept in step with the development of techniques and the increasing range of applications of our discipline in the social as well as in the natural sciences. (translated and quoted by Bjerkholt [2005, 526 n. 56])

These reflections resemble Keynes's criticism of the English statisticians' preoccupation with "techniques." Frisch knew Keynes's *Treatise on Probability* (1921), for he mentions it in a technical context (Frisch 1923, 1031), but what he thought of its philosophical position is unknown. Over the next few years Frisch chose to extend the techniques rather than establish better foundations. A decade later, when he was calling for foundations for those techniques, the foundations were to be sought in "sampling theory" (i.e., frequentist theory) rather than in the Bayesian theory favored by Keynes. Bayes had no place in the new statistical world of Fisher and Neyman.

The techniques that interested Frisch were for dealing with relationships between variables. The direction is already clear in his first contribution to *l'économetrie*, "On a Problem in Pure Economics," originally published in French in 1926 ("Sur un problème d'économie pure"). There he objected to Yule's regression theory because it did not treat the variables symmetrically (Frisch [1926] 1995, 27). Frisch acknowledged that his own procedure left something to be desired:

> This procedure for determining a mean regression line is an entirely mechanical one which cannot be justified by *a priori* considerations. But this is a remark that can also be applied to the very application of the method of least squares to problems that do not fall within the proper scope of the theory of errors of observation. (28)

Frisch's main object in econometric theory was to extend least squares/correlation theory so that it would be useful in econometrics. "Correlation and Scatter" (1929) and *Confluence Analysis* (1934b) were his main contributions. Frisch had first used matrices in his "Sur un problème d'économie pure," and by the end of the decade his mastery of the algebraic/computational side of least squares/correlation theory was probably unsurpassed.[20]

20. Some of the history of this side of least squares theory is given in Aldrich 1998.

Confluence analysis, unlike the regression theory of Pearson, Yule, and Fisher, was designed for data where there was more than one relationship between the variables. In an experiment a single relationship could be contrived, but the passive observations available to economists usually reflected more than one relationship.[21]

Frisch was a new phenomenon, an econometric theorist, for his techniques were not separate gadgets, like those found in Moore's work, but part of a system. Associated with this system was a situation—it cannot be called a crisis—that persisted for a number of years: in confluence analysis Frisch had a technique that he thought did justice to the complexities of economic data, the statisticians did statistics by using "sampling theory," and the two did not connect. In a general way Frisch encouraged links between econometricians and statisticians because he believed that was the way to make economics a science, but he also had a specific project for them, the probabilization of confluence analysis. Over the years Frisch changed his mind about the feasibility and desirability of such a project; in 1934 in *Confluence Analysis* (p. 88) he argued that the development of sampling theory was not a promising line (see Bjerkholt 2005, 500), but by February 1937 he was telling Wilks (see below sections 5 and 8) that Koopmans had taken a first step in supplying the "missing link" between the confluence and sampling approaches and was encouraging Wilks to build "a more embracing theory in this field" (Bjerkholt 2005, 526 n. 53). The invitation to Wilks—which was not taken up—indicates a more active approach than encouraging statisticians to publish articles on sampling theory in *Econometrica* and letting things happen. Eventually things did happen, with the econometricians making them happen. One happening was Haavelmo's "Probability Approach" (1944); another, which kept to the original framework of equations in variables measured with error, was Gerhard Tintner's (1945, 1946) work on rank and multicollinearity; there is more about the first line of development in sections 7–9 below, while the second, less influential, line is traced in Aldrich 1993b.

Trygve Haavelmo (1911–1999) appears as a principal in sections 7–9 below, but here we can note that from 1933 he was Frisch's assistant and he was sent on a series of quests to learn useful things: he traveled the Frisch network, and where he went reflected Frisch's priorities at the time; his journeys are described by Bjerkholt (2005, 2007).

21. See Hendry and Morgan 1989 for an exposition and history of confluence analysis.

5. Koopmans: Specification and Maximum Likelihood

Frisch traveled, invited people to Oslo, and carried on a huge correspondence (the list of correspondents runs to eighty pages). He knew, or at least wrote to, everybody; in 1934 he was writing to Hotelling about a visit to R. A. Fisher: it was "very stimulating to get into personal contact with him" (quoted by Louçã [2007, 222]). The Institute of Economics was well funded by the Rockefeller Foundation and it had many visitors (see Andvig and Thonstad 1998, 9–10). Tjalling Koopmans (1910–1985) from the Netherlands was one of the visitors. In the 1940s Koopmans would be responsible for the basic identification and estimation theory for the simultaneous equations model—see section 9 below—but already in 1935 he was emerging as a significant figure. Bjerkholt (2005) describes Koopmans's visit to Oslo and how he was made part of the network. His lectures on sampling theory were duplicated and circulated; Fisher's report is reproduced in Bennett 1990, 328–30, and Wilks's comments (to Frisch) are summarized by Bjerkholt (2005, 526). Samuel Wilks (1906–1964), at Princeton, did his PhD under Rietz, but his intellectual home was Pearson's department at University College and its journal *Biometrika*; for more on Wilks, see Anderson 1965.

For Koopmans, as for Ezekiel and Schultz, Fisher was the authority but, where they simply transferred Fisher's regression techniques to econometrics, he engaged Fisher's ideas at a more fundamental level. This was a first in another respect, for no econometrician had tried to get into Pearson's theory of inference in the same way. Koopmans's *Linear Regression Analysis of Economic Time Series* (1937) was his PhD dissertation; it was supervised by the theoretical physicist Hans Kramers with some input from Tinbergen. Koopmans (1937, 2) described it "as an application of the theoretical concepts of the English school of mathematical statistics to the special situation prevailing in economics." The key work of the English school was Fisher's "On the Mathematical Foundations of Theoretical Statistics" (1922).[22] Koopmans (1937, 2–4) quotes extensively from the part dealing with the "problem of specification" (Fisher 1922, secs. 2, 3), investigating the "hypothetical infinite population" appropriate to an economic setting in which variables are measured with error.[23]

22. On this, see Aldrich 1997 and Stigler 2005.

23. The relevant passages are reproduced in part 4 of Hendry and Morgan 1995.

Koopmans saw a different side of Fisher from Frisch: Frisch saw agricultural experiments—in 1934, when Frisch met him, Fisher was preparing his book *The Design of Experiments*—but Koopmans saw a general framework for statistical inference in which the econometricians' use of what Frisch called "sampling theory" could find a place.

For estimation Koopmans used Fisher's method of maximum likelihood.[24] The theory presented in Fisher 1922 and 1925b combined a large sample distribution theory, an optimality theory, and an interpretation of the method in terms of the extraction of information—all for independent, identically distributed observations. Fisher used the method in genetics and in the theory of errors, the latter becoming modern regression analysis.[25] The *Statistical Methods* presented maximum likelihood only through an example from genetics, and Koopmans worked from Fisher's difficult original papers apparently unaware of contributions like Hotelling 1930a or Darmois 1936 that tried to express Fisher's ideas in more rigorous and intelligible terms.[26] Like Hotelling, Koopmans has a place in the history of maximum likelihood; the one produced the first theoretical exploration after Fisher, and the other, one of the first new applications.

In his discussion of the errors in variables "specification" Koopmans (1937, 59–63) carefully notes the estimation possibilities for all of the many parameters involved. He notes without elaboration that estimation of the ratios of the variances and covariances of the erratic components is "opposed by difficulties of a fundamental nature" (61); he assumes the ratios are known. Koopmans did not make an overwhelming case for maximum likelihood: he argues that under his conditions the estimator will be consistent (62) but then explains that little of Fisher's original argument for its superiority over estimators can be transferred to the present situation and finishes by stating that "maximum likelihood estimation is here adopted simply because it seems to lead to useful statistics" (63–65).

On 26 October 1937 Fisher wrote to Koopmans, congratulating him on the published thesis: "You have done a magnificent piece of work, which should be the basis of a large part of future applications of a theoretical statistics to economic problems." Fisher also praised Koopmans to others: in 1940 he told E. B. Wilson, "The best sense, I think, that can be made of Frisch's notions [on confluence analysis] was made by a Dutchman, Koopmans" (Bennett 1990, 305).

24. For a history of this, see Stigler 2007.

25. See Aldrich 2005 for details.

26. This literature is described in Stigler 2007.

After 1937 Fisher seems to have had no further dealings with econometricians. He was in Cambridge (as professor of genetics) in the brilliant first decade of Richard Stone's Department of Applied Economics but they could have been on different planets.[27] Stone (1945, 311 n) refers distantly to the "well-known works of R. A. Fisher and M. Ezekiel." The most important British econometricians of the next generation, J. D. Sargan and J. Durbin, were students at Cambridge in this period but did not encounter Fisher.[28]

6. Neyman: Test Theory and "Problems in Economics"

Fisher, like Pearson, let the econometricians come to him, but Jerzy Neyman (1894–1981) went to them and, for a while at least, contemplated their problems. Neyman, like Fisher, had a base in agricultural statistics, but he was more interested in economics than Fisher—Rothamsted was more applied chemistry than applied economics. Also, like Fisher in his early insecure days, Neyman went looking for audiences. Neyman had first visited England in 1926 to study with Karl Pearson, but he formed a very useful partnership with Pearson's son, Egon. And it proved to be Fisher rather than Karl Pearson who influenced the partnership's first production, "On the Use of Certain Test Criteria for Purposes of Statistical Inference" (1928), which proposed the (maximized) likelihood ratio test. A series of important papers followed, with the fundamental principles of the mature "Neyman-Pearson theory" of testing appearing in Neyman and Pearson 1933. Meanwhile Karl Pearson had retired and was succeeded as Galton Professor of Eugenics and head of the Galton Laboratory by Fisher. Fisher did not inherit all of Karl Pearson's empire, for the college put Egon Pearson in charge of a separate statistics department. Neyman's career began to be made in 1934 when he left Poland to join this department.

Relations between Fisher and Neyman were initially good, but in 1935 they collapsed. In December 1934 relations were still cordial. At a Statistical Society meeting Neyman (1935, 73) described his reaction to reading Fisher: "What an interesting way of asking and answering questions, but can't I do differently?" In Neyman's new theory the "frequency of errors in judgment" would be central (74), not information, as in Fisher's theory. Fisher's (1935b, 82) reply was civil: "It has been, naturally, of great interest

27. See Pesaran 1991 and Gilbert 1991 for Stone and the DAE.
28. For more on them, see Phillips 1985, 1988.

to me to follow the attempts which Drs. Neyman and Pearson have made to develop a theory of estimation independently of some of the concepts I have used." How Fisher and Neyman went from being different to being enemies is briefly recounted by S. L. Zabell (1992, 385–86).

Neyman showed how he would do things differently in his "Outline of a Theory of Statistical Estimation Based on the Classical Theory of Probability" (1937b). The theory outlined is for "estimation by interval" rather than "estimation by unique estimate"; Neyman had first treated interval estimation in "On the Two Different Aspects of the Representative Method" (1934). Maximum likelihood is discussed in the preliminary review of the 1937 paper (1937b, 345), and the story runs from Karl Pearson and a particular application, through Fisher with his insistence on the general principle and his statement of "several important properties" to the present and authors like Hotelling, who "proved" Fisher's statements "partly in a modified form." The discussion is perplexingly neutral, for Neyman does not say how important, or not, he judges these statements.[29]

Neyman joined the Econometric Society in 1934. Contact with the econometricians helped disseminate his ideas on statistical inference, but Neyman had ideas about econometrics too which he presented on a number of occasions (1937c, 1937a, 1938, 1939). In September 1936 he gave a "survey of recent work on correlation and covariance" at the Econometric Society conference in Oxford; the conference is described by Bjerkholt (2005, 509–10). From the printed summary it seems that the talk was not a survey of what had been done but a program. All the elements of Neyman's thinking about econometrics are here. "Two paths of approach, the empirical and the a priori," are characterized, and it is argued that the latter is the more appropriate. Neyman (1937c) describes the effort in the former as consisting "in *guessing* the appropriate formula . . . so that it might fit the observations. That is what is being done in the empirical approach to social and economic phenomena" (quoted in Phelps Brown 1937, 368). In the latter there are "hypotheses concerning not the functions representing the observable facts, but the machinery which may have produced those facts." In the history of astronomy the second approach is associated with Newton and the first with his predecessors. The distinction and the historical identifications reappear in Koopmans's "Measurement without Theory" (1947, 161), where Mitchell of the National Bureau is identified

29. Stigler (2007) looks at Neyman's attitude to maximum likelihood—and to Fisher—more closely.

with the empirical approach and the Cowles econometricians with the a priori; Neyman was not mentioned in connection with the analogy. Because Neyman associated the special problems of economics with time series analysis, his second theme was the need for a "stochastic calculus" for dealing with time series probabilistically; in this connection he noted Hotelling's "Differential Equations Subject to Error" (1927a) and work by Sergei Bernstein, one of his teachers. The third was the Neyman-Pearson theory of testing and how it would figure in the new approach. Frisch was impressed by the Neyman-Pearson theory as an intellectual structure and believed that it would play a part in testing economic theories.

The theme of the empirical versus the a priori runs through Neyman's long comment on E. C. Rhodes's (1937) use of factor analysis to extract an index of business activity; the occasion was a Statistical Society meeting in December 1936. Neyman's (1937c, 50) eight pages begin with the declaration, "I think that the problem of determining the business activity index, as it has been attempted by Dr. Rhodes, could not be solved at all." After formulating and illustrating what is essentially the identification problem for factor analysis models (Thomson 1935 is cited), Neyman (1937c, 56) makes a suggestion:

> I have the impression that if the structure of the observable variables can be found at all, it must be by some method based on economic considerations determining a priori the pattern of the structural equations and the character of the factors involved. This a priori information, combined with the empirical data, could be perhaps sufficient to determine the structure of the variables.

The term *structural equation* was Frisch's—although Neyman does not refer to him—and there had been plenty of structural talk at the Oxford conference (see Aldrich 1989 and Bjerkholt 2005). Neyman's remarks emphasize his enigmatic position in econometrics. He never situated himself in relation to the literature, most probably because he did not know much of it.

People came to study with Neyman and he went on trips. In early 1937 he visited the United States, meeting some of the same people as Fisher; Schultz (1938, 733) recalled his visit to Chicago in March. Neyman's big engagement was a series of "lectures and conferences on mathematical statistics" in Washington at the Department of Agriculture. In his "Time Series Analysis and Some Related Problems in Economics" Neyman expanded on what he had said in Oxford and about Rhodes. He repeated his critique of Rhodes's approach, which he said is one of "many similar ones," citing the work of Frisch: "If you consider the method of the so-called confluence analysis advanced by Ragnar Frisch you will find that it is open to almost identical criticism" (Neyman 1938, 114). Bjerkholt (2005, 510) notes that after the Oxford meeting Neyman and Frisch had talked about the common points between factor analysis and confluence analysis. In Washington Neyman declared the identity without going into particulars but he provided a sample of a priori analysis.

Neyman (1938, 115–24) used the two-person/sector dynamic model of exchange from Frisch's "Circulation Planning" (1934a, 261–72) into which he inserted time varying random coefficients. In the model x_t is the amount of money the shoemaker spends on farm products and y_t the amount the farmer spends on shoes:

$$x_t = (a + \alpha)\, y_{t-1}$$

$$y_t = (b + \beta)\, x_{t-1}$$

$$a = \alpha_0 + \alpha_1 t$$

$$b = b_0 + b_1 t,$$

where α and β are normal variables. Neyman considers how to test the hypothesis of parameter constancy $\alpha_1 = 0$. The equations do not have the error in equation form of Haavelmo 1943 or Mann and Wald 1943 and so the model looks strange, but it is an application of the sampling theory approach to Frisch's economics. Here was probability and structure but not simultaneity and identification. The latter, which informed Haavelmo's contributions (1943, 1944) to the simultaneous equations model, came from confluence analysis; how is described in Aldrich 1989, 1994.

Lectures and Conferences was reviewed in the statistical journals by W. G. Cochran (1938) and Wilks (1938). They did not mention the part on econometrics, but they thought the book a good introduction to Neyman's ideas; Fisher (1938–39), who was by now contemptuous of Neyman's ideas, agreed: "There is not enough original material to justify publication as a book, and too much that is really trivial" (quoted by Stigler [2007]).

The final piece from Neyman's econometric phase was a review of Herman Wold's *Study in the Analysis of Stationary Time Series*. The awaited "stochastic calculus" had arrived and Neyman wrote welcoming it; the book contained surprises, among them that Khinchin and Yule "were studying essentially the same thing." Naturally Neyman (1939, 297) noted how some of the most important problems remain unsolved: Wold "describes them as sampling problems, but really they are problems of testing hypotheses and of estimation." The review marked the end of Neyman's econometric adventure—in print at least. He had other concerns: in 1938 he had moved to Berkeley—reproducing Pearson's department in the West—and in 1939 Poland was invaded. He returned to the econometric field with Neyman and Scott 1948 and Neyman 1951, but these were not continuations of the 1937–39 work but explorations of the pathology of maximum likelihood in the errors in variables model and models like it.[30]

Nothing in the literature of the time indicates that Neyman's intervention in econometrics had any effect. Econometricians acknowledged his publications on testing handsomely—see, for example, Koopmans 1941, 173—but they did not refer to his remarks on econometrics: Haavelmo (1944, 41, 77) mentions *Lectures and Conferences* but only for its exposition of test theory. It seems that in econometrics Neyman expressed the spirit of the age and was a bit in advance of it but that his work was parallel and separate. And yet Neyman is a name in the history of econometrics as a protagonist in the most dramatic exchange between statistician and econometrician we encounter. The econometrician was Trygve Haavelmo.

7. Talks with Haavelmo

Today Haavelmo is the best-known econometrician from this era. He was awarded the Nobel Prize in 1989 "for his clarification of the probability theory foundations of econometrics and his analyses of simultaneous economic structures," achievements based on "The Probability Approach in Econometrics" (1944) and "The Statistical Implications of a System of Simultaneous Equations" (1943). These writings are treated in the standard works—Hendry and Morgan 1995, pts. 7–8; Morgan 1990; and Qin 1993—while Bjerkholt (2007) has carefully reconstructed the process of their composition. "The Probability Approach" was a new title for what was substantially Haavelmo's 1941 dissertation, "On the Theory and Measurement of Economic Relations"; Bjerkholt (2007, 810–12) describes how the new title came about. The new title, being more specific, may better indicate the nature of the work, but it is open to the misunderstanding that the main issue is the choice between the probability approach and a non-probability approach when it is rather to figure out

30. See Stigler 2007 for a perspective on Neyman's work on maximum likelihood.

the implications of adopting the probability approach in the conditions of economics.

Haavelmo recalled his meetings with Neyman and explained their significance only much later, after Neyman's death. The most vivid of Haavelmo's recollections from the 1980s (summarized by Morgan [1990, 242 n]) is the passage in the Nobel lecture recalling his visit to the United States in 1939:

> I then had the privilege of studying with the world famous statistician Jerzy Neyman in California for a couple of months. At that time, young and naive, I thought I knew something about econometrics. I exposed some of my thinking on the subject to professor Neyman. Instead of entering into a discussion with me, he gave me two or three numerical exercises for me to work out. He said he would talk to me when I had done these exercises. When I met him for that second talk, I had lost most of my illusions regarding the understanding of how to do econometrics. But professor Neyman also gave me hopes that there might be other more fruitful ways to approach the problem of econometric methods than those which had so far caused difficulties and disappointments. (Haavelmo 1989, 285)

Morgan (1990, 242) sees the significance of these encounters in Haavelmo's "conversion to probability reasoning." Bjerkholt (2007, 784–85, 825) concurs on their significance but argues that the meetings took place in England in 1936. Bjerkholt (2005, 513) describes how after the Oxford conference Haavelmo stayed on until the beginning of December, attending Neyman's lectures on testing statistical hypotheses.

It is difficult to coordinate Haavelmo's recollections with contemporary information from either 1936 or 1939. The two meetings with Haavelmo mirror the two parts of "Time Series Analysis and Some Related Problems in Economics." These were, of course, available in 1939 and perhaps already mapped out in 1936 when Haavelmo was with Neyman. However, both in 1936 and 1939, there was a lot of room for Haavelmo and Neyman to talk past one another. The decision Neyman emphasized was between "the empirical and the a priori," not between using "sampling theory" and not using it. The latter was not an issue, for that was how statistical inference was done. In 1936 Frisch was moving toward accepting the probability approach, that is, recognizing the desirability and feasibility of probabilizing confluence analysis; see section 4 above. For Neyman the essential first step forward was to forget confluence analysis and so he would offer no help in this project. On Haavelmo's side there was no sign of any collapse in the credibility of confluence analysis—"lost illusions"—in 1936 or 1939, and so it is difficult to make sense of the story. My guess is that the recollections reflect meetings in both 1936 and 1939; there were misunderstandings at the time, but later Haavelmo saw what Neyman was getting at. From "Time Series Analysis and Some Related Problems in Economics" it was clear what Neyman wanted to say and life, or the memory of life, imitated art.

The "Probability Approach" has a chapter on the Neyman-Pearson theory and its application to econometrics. Haavelmo went to learn about the theory and chapter 4 distilled what he had learned. But the "Probability Approach" has no references to Neyman's ideas on econometrics, or to any contacts with him. Neyman responded to the published work by thanking Haavelmo "for giving a considerable amount of attention to my work," but he distanced himself by adding that he felt sure that Haavelmo deserved the compliments he had heard from others (Bjerkholt 2007, 831). Haavelmo's relations with Wald, the last of my quartet of statisticians, were more straightforward.

8. A "unique knowledge of modern statistical theory"

As an econometrician's statistician Abraham Wald (1902–1950) was unique, for he was a statistical authority and a working econometric theorist. After writing a PhD dissertation and many articles in pure mathematics, Wald began publishing in probability, economic theory, and economic statistics in 1936. His first big effort in statistics was a book on seasonal adjustment (1936); this had a distinctly "Continental" character with roots in the work of Oskar Anderson. In 1938 Wald moved to America and after a short time with the Cowles Commission joined Hotelling at Columbia. With Hotelling guiding him in "modern statistical theory" Wald was soon making fundamental contributions to statistical theory. At the same time he was becoming familiar with econometric theory; Wald would be most closely involved with econometrics in the years 1939–45. Wald stayed at Columbia, becoming professor of mathematical statistics in 1945. He was still in the economics department but in 1946 he was made chairman of the new department of mathematical statistics. He died in a plane crash in 1950; there are memoirs by Hotelling (1951) and Oskar Morgenstern (1951).

In Europe Wald had not quite belonged to the Frisch network: Bjerkholt (2005, 528) describes how Frisch asked him to visit Oslo on his way to the United States but that Wald was in too much of a hurry to accept. Yet Wald's career in statistics and econometrics represents the triumph of the network. When war broke out in Europe in 1939, many members of the network found themselves in America, and the network reconstituted itself there; Frisch was in letter contact until 1941, but then the network had to do without him. The Cowles Commission had always been a node of the network, but now in Chicago and from 1943 under Jacob Marschak's leadership Cowles in Chicago replaced the Institute of Economics in Oslo as the center of the network. The *Annals of Mathematical Statistics* was changing. In 1939 Wilks, an American member of the network, had become editor of the *Annals*, which was beginning to threaten *Biometrika* as the leading statistical theory journal. In the war years *Econometrica* and the *Annals* seemed to converge in the most remarkable way. In the early days Hotelling had been the only common factor, but in the years 1940–46 many authors contributed to both journals, not only Wald but Court, Dodd, Geiringer, Girshick, Hotelling, Koopmans, Mann, Reiersøl, Samuelson, Tintner, and Waugh. They were not all members of the Frisch network and they were not all econometricians, but the way the two communities seemed to merge is astonishing—or perhaps not, given Frisch's cultivation of the mathematical statisticians. The world had changed since Pearson founded a journal for the "statistical study of biological problems" (section 1 above) and Fisher warned against the economists (section 3 above). Although the conjuncture of econometric theory and mathematical statistics that Wald personified was unique to the war period, its effects persisted so that the mathematical statisticians, Herman Rubin and Herman Chernoff, went on working in econometrics into the 1950s and T. W. Anderson, Wilks's student and successor as editor of the *Annals*, made a career there.[31]

Coming back to Wald, he was a different kind of econometrician's statistician from Pearson, Fisher, or even Neyman—a close collaborator rather than a more or less remote authority. Between Wald and his major partner, Haavelmo, there was less of an age difference or authority imbalance than between Haavelmo and Neyman or between Haavelmo and Frisch; Neyman was born in 1894, Frisch in 1895, Wald in 1902, and Haavelmo in 1911. When the two met in the United States in 1939, Wald, like Haav-

31. See Hildreth 1986 for Rubin and Chernoff and Phillips 1986 for Anderson.

elmo, was still learning and had no mass of achievement and reputation behind him; he was not sought out as the authority on his subject as Pearson, Fisher, and Neyman had been. In the customary way, Haavelmo attended Wald's lectures—Hotelling was on leave and Wald was substituting for him—but their involvement went much further: Bjerkholt (2007) has documented their friendship, detailing their meetings and explaining what was behind the public acknowledgment in the preface to the "Probability Approach" (1944, v):

> My most sincere thanks are due to Professor Abraham Wald of Columbia University for numerous suggestions and for help on many points in preparing the manuscript. Upon his unique knowledge of modern statistical theory and mathematics in general I have drawn very heavily. Many of the statistical sections in this study have been formulated, and others have been reformulated, after discussions with him.

(The other major acknowledgment was to Frisch—for "ideas.") One of the areas in which Wald contributed mathematical expertise was the "problem of arbitrary parameters," that is, Haavelmo's theory of identification; see Aldrich 1994 and Bjerkholt 2007 for further information on this point.

Wald, the econometric theorist, is considered in the next section; here we consider the authority on statistical theory. Wald's first publication in the *Annals* was an announcement, in effect, that he would be a major contributor to basic inference theory. Wald (1939) advanced the basic ideas of the decision theory approach to inference that he would develop after 1945; this was for J. Wolfowitz (1952, 2) "probably his most important paper." The theory grew out of the Neyman-Pearson theory of testing and Neyman's theory of confidence intervals. Wald knew Fisher only through a Hotelling filter: the original was too obscure and non-rigorous.

It seems that Wald's influence was everywhere in the "Probability Approach" and . . . nowhere, for there is no part of it that has Wald's stamp the way that chapter 4 has Neyman's. If Wald had a big idea, it was decision theory, but this only makes a small appearance in Haavelmo's (1944, chap. 6) discussion of prediction, itself a peripheral topic. Wald with his "unique knowledge of modern statistical theory" could provide technical support and reassurance about foundations: he had a more sophisticated argument for maximum likelihood than anything descending from Fisher; he was the first to present distribution theory for the case of serially correlated observations. Here I discuss the technical support Wald may have provided and his view of foundations and in the next section his treatment of maximum likelihood in dynamic models.

Haavelmo had known about maximum likelihood since at least 1935, but he first used it in his own research in 1941. In the "Probability Approach" (1944, 103)—this part was written in 1941—he used maximum likelihood to estimate the reduced form of a just-identified demand and supply model and transformed the estimates to obtain estimates of the structural parameters; in the "Statistical Implications" (1943, 7–10) he wrote down the first-order conditions for the maximum likelihood estimates of the structural form parameters. This was all new territory for Haavelmo: the difficulties were a combination of producing a relevant specification and formulating the likelihood function correctly. He completed these tasks with Wald's aid or, at least, with his approval; there is no information on how the two worked together.

The Oslo reservations about the use of "sampling theory" were not doubts about the validity of maximum likelihood as an estimation method when one knew what to estimate. Wald, however, was interested in its validity, and in a footnote Haavelmo (1944, 103) picked up on this point and indicated how new research sanctioned the use of maximum likelihood:

> The method of maximum likelihood, commonly used by statisticians, was originally founded more or less upon intuition, but recently it has been shown by A. Wald that the method, under certain conditions, can be justified on the basis of modern theory of confidence intervals.

Wald may have talked privately about "intuition," but in the "New Foundation of the Method of Maximum Likelihood" (1940b) to which Haavelmo refers he says only that the restriction to asymptotic normality associated with the notion of efficiency is a "serious one" and that a property stronger than efficiency is wanted, namely, that the confidence interval derived from the maximum likelihood estimate is shortest in the sense of Neyman 1937b.[32]

Haavelmo's footnote exposed the different allegiances of the econometricians. Koopmans's response, as reported by Bjerkholt (2007, 813), was that the efficiency of the maximum likelihood estimates as compared with other estimates with asymptotically normal distributions was known long before and was still the most conspicuous and most easily formulated reason for preferring maximum likelihood estimates. Koopmans

32. Wald's involvement with maximum likelihood is discussed in greater depth by Stigler (2007).

was a follower of Fisher rather than of Wald (or Neyman). Wald's "new foundation" and the Neyman interval estimation technique which inspired it did not become the econometric orthodoxy. Despite these differences about the best foundations for maximum likelihood, the same technique of large sample normality and large sample confidence intervals went into Mann and Wald 1943 and into Koopmans, Rubin, and Leipnik 1950; see the next section. The animosities between Fisher and Neyman were not passed on to the econometrician followers, but neither were the intellectual differences. The parts of Fisher that Neyman objected to, likelihood as a fundamental principle, information as a basic concern, and the fiducial argument, were not transmitted to econometrics. Also lost from transmission was Neyman's optimality theory for confidence intervals.

9. Wald and Koopmans in Econometric Theory

The relationship between Koopmans and Wald was quite different from that between Haavelmo and Wald: for Koopmans, Wald was another econometric theorist rather than an authority in statistical inference. I will describe two of Wald's pieces on econometric theory and how they reflected and affected the work of Koopmans; Wald's entire econometric output is reviewed by Tintner (1952). Koopmans's thesis (section 5 above) was a part of the setting for Wald's "The Fitting of Straight Lines If Both Variables Are Subject to Error" (1940a); this was Wald's first publication in econometric theory, although it appeared in the *Annals*. Koopmans and his use of maximum likelihood make only a routine appearance in the literature review, but the point of the paper is to provide a consistent estimate in a case where maximum likelihood is inconsistent. The point about the inconsistency of maximum likelihood is not stated in the paper, but Stigler (2007) quotes from a letter from Wald to Neyman in which Wald makes the observation; in the case treated by Koopmans with the ratio of the error variances known maximum likelihood is consistent.[33]

"The Fitting of Straight Lines" appeared when econometricians were turning to dynamic models of the kind used by Tinbergen (1939); Neyman's wish for a statistical inference complement to Wold's stochastic analysis was beginning to be realized. Wold (1938, 143) had already seen in Tinbergen's (and Frisch's) macrodynamics a possible application of the theory of stationary stochastic processes, and when Koopmans (1942)

33. Wald's paper is discussed further by Bjerkholt (2007) and Stigler (2007).

treated testing in the first-order scalar autoregressive model, it was because that was the simplest Tinbergen-like model. Wald's most important contribution to econometrics, his *Econometrica* article with H. B. Mann on the vector autoregressive model, was part of this development, although it grew more immediately from Haavelmo's work. The main example in Haavelmo's "Statistical Implications" (1943, 3–5) is a multiplier-accelerator model that has the form of a restricted bivariate autoregressive process. Haavelmo (1943, 9–10) describes the estimation of this model by maximum likelihood and indicates how a general vector autoregressive model may be estimated. Haavelmo did not investigate the properties of the estimator. This was the business of Mann and Wald's "Linear Stochastic Difference Equations" published later in the same year: there, "it is shown that the maximum-likelihood estimates are consistent and their limit distributions normal" (1943, 175). This was an unprecedented and difficult undertaking that required developing a central limit theorem for dependent random variables. The paper also gives large sample confidence intervals and formulae for prediction.

Results from Mann and Wald 1943 were used in "Measuring the Equation Systems of Dynamic Economics," a paper by Koopmans and two young mathematical statisticians, Herman Rubin and Roy Leipnik, presented at a Cowles conference in Chicago in early 1945. The version published five years later—Koopmans, Rubin, and Leipnik 1950—is a comprehensive 180-page account of the simultaneous equations model covering identification, maximum likelihood estimation, and the associated computational procedures. The distribution theory extends that of Mann and Wald by taking into account exogenous variables, but the most innovative part was the treatment of identification; its combination of streamlined technique and general results helped it supersede Haavelmo's effort in the "Probability Approach" (see Qin 1993, chap. 4; and Aldrich 1994).

"Measuring the Equation Systems" and the supplement "When Is an Equation System Complete for Statistical Purposes?" by Koopmans alone make an appropriate conclusion to the present story of how the econometricians digested the ideas of the statistical masters: Koopmans was no longer looking over his shoulder at Fisher, and his papers came themselves to serve as references in high theory. These papers constituted Koopmans's second attempt to formulate a specification for general use in econometrics. This new specification based on Haavelmo's work (1943, 1944) was much more durable than the first (section 5 above), for it dominated econometric theory for decades rather than years. The new econometric theory was more obviously complete than Frisch's of fifteen years before; there was no inborn gap. The econometricians next went back to the inference specialists when the problem of inverse probability (sections 3–4 above) was reopened in the 1960s (see Qin 1996). In the introduction I mentioned "structure" and "identification" as concepts that have attracted special attention from historians of econometrics. Koopmans gave special thought to these concepts, and his final perspective on them makes a nice coda to the story of how the concepts of statistical inference came to be incorporated in econometric theory.

10. Coda: Beyond Fisher

Neyman and Wald thought they had advanced beyond Fisher in statistical inference. Koopmans describes an advance of a different kind in his and Reiersøl's "Identification of Structural Characteristics" (1950). Their paper provides a general scheme in which results on identification in the simultaneous equations model, the errors in variables model, and the factor analysis model could be placed. The paper appeared in the *Annals*—of course.

The starting point is Fisher 1922 again—see section 5 above. Under the heading "'Population' versus 'Structure,'" Koopmans and Reiersøl (1950, 165) write:

> In a fundamental paper [1922] R. A. Fisher distinguished as the first group of problems in mathematical statistics the "specification of the mathematical form of the population from which the data are regarded as a sample." . . .
>
> In many fields the objective of the investigator's inquisitiveness is not just a "population" in the sense of a distribution of observable variables, but a physical structure projected behind this distribution, by which the latter is thought to be generated. It is the purpose of this article to suggest a reformulation of the specification problem, appropriate to many applications of statistical methods, and to point out the consequent emergence of a new group of problems, to be called identification problems.

The "reformulated" specification problem is the specification of "structure." Statistical inference was satisfied with inference from sample to population.

The "structures" in econometrics are markets or economies, but Koopmans and Reiersøl make a broad sweep of fields where inference to structure is important. Biology and Sewall Wright's (1934) path analysis are mentioned but not Fisher and his 1918 paper "The Correlation between

Relatives on the Supposition of Mendelian Inheritance," which was probably the most influential example of structural inference in any field. It is no surprise that Fisher's papers in genetics were missed, for they were even more inaccessible to econometricians than his work in statistical theory; however, there is an easily understood example of identification failure in the exposition of maximum likelihood in the *Statistical Methods* (1925a, 24); it is discussed in Aldrich 2002. The case of Wright and his lack of impact on econometrics is much more puzzling, for he, like Yule, appeared to do all the right things; Arthur S. Goldberger (1972) has examined the case.

11. The Econometricians' Statisticians, 1895–1945

We have watched some ideas passing from statisticians to econometricians. Usually they traveled because the econometricians went and got them on realizing, or suspecting, that the statisticians had produced something they could use. Moore, Ezekiel, Schultz, Koopmans, and Haavelmo were the acquisitive/receptive econometricians, but the traffic was encouraged by Hotelling and Frisch. Less often the statisticians took the initiative and went to the econometricians. Neyman tried (section 6 above) and so, in a larger way, did Yule in his econometric phase (section 1 above); their experience suggests that supply does not create its own demand. In the later years there were partnerships: Koopmans, Rubin, and Leipnik was one, and there was teamwork involved in the "Probability Approach," although Wald did not appear as a coauthor.

The phrase "the econometricians' statisticians, 1895–1945" rather than, say, "the econometricians and the statisticians, 1895–1945" suggests that there was something special about the relationships between the people concerned or about the period. There was something special in the way the econometricians related to Pearson, Fisher, Neyman, and Wald rather than to Bowley, Oskar Anderson, or Wilks. The period was special too. These particular statisticians were the final authorities on their own creations, but at times they were the only authorities. When the original papers are the only literature and the author the only authority, the alternatives are to read the papers or to get help from the author. With correlation, exact distributions, and the Neyman-Pearson theory of testing, the econometricians got help. Most of the econometricians did not attempt maximum likelihood, and Koopmans, who did, mastered the original papers and contacted Fisher only after he had completed his main work. Toward the end of the period there were more competent people, an acceleration in the rate at which ideas were absorbed, and the appearance of advanced textbooks, such as Wilks 1944. When Ezekiel prepared the first edition of *Correlation Analysis* in 1930 he went to Fisher for help, which only Fisher could give; for the second edition in 1941 he got help from Girshick, a colleague at the Department of Agriculture. Girshick reinterpreted the original probability intervals as confidence intervals; to get the confidence interpretation in 1935, say, would have required a consultation with Neyman.

The phrase "the econometricians' statisticians" may also suggest that the statisticians belonged to the econometricians and to nobody else. Of course this was not so, and yet there is something in the notion. The econometricians were not the primary audience for Pearson, Fisher, Neyman, or even Wald: Pearson and Fisher wanted to reach biometricians, and Neyman and Wald, statistical theorists. There is no comparative study of the impact of statistical ideas on secondary audiences—on the different varieties of "metricians"—but the statisticians' ideas were available to economists almost as soon as they were available to anyone. It seems likely the early support for their ideas would have mattered for the statisticians at the time even if that early success was eclipsed by success in fields that mattered more to them and the dealings with the econometricians left no trace on their own research programs.

A caveat: this has not been a survey of all the statistical ideas that entered econometrics in that half-century: there were more ideas from the English statistical school as well as ideas from other directions. Time series analysis made an appearance in sections 6–9, but much more could be said (see Morgan 1990, Aldrich 1995, and Klein 1997). The English statisticians were involved in time series analysis, but the econometricians also looked elsewhere for inspiration. Bjerkholt (2005, 513) describes how Haavelmo was sent to look at the harmonic analysis Karl Stumpff was doing at the Meteorologisches Institut in Berlin; the mission did not yield any useful results. Herman Wold's 1938 book has been mentioned several times, but looking at his entire career Wold could be considered a probabilist-statistician who went native among the econometricians, Yule's venture with a different ending and with Harald Cramér in the role of Pearson.[34]

34. For Wold, see the interview with Hendry and Morgan (1994).

References

Aldrich, J. 1989. Autonomy. *Oxford Economic Papers* 41:15–34.

———. 1992. Probability and Depreciation: A History of the Stochastic Approach to Index Numbers. *HOPE* 24:657–86.

———. 1993a. Cowles Exogeneity and CORE Exogeneity. Southampton University Economics Discussion Paper.

———. 1993b. Reiersøl, Geary, and the Idea of Instrumental Variables. *Economic and Social Review* 24:247–74.

———. 1994. Haavelmo's Identification Theory. *Econometric Theory* 10:198–219.

———. 1995. Correlations Genuine and Spurious in Pearson and Yule. *Statistical Science* 10:364–76.

———. 1997. R. A. Fisher and the Making of Maximum Likelihood, 1912–22. *Statistical Science* 12:162–76.

———. 1998. Doing Least Squares: Perspectives from Gauss and Yule. *International Statistical Review* 66:61–81.

———. 2000. Fisher's "Inverse Probability" of 1930. *International Statistical Review* 68:155–72.

———. 2002. How Likelihood and Identification Went Bayesian. *International Statistical Review* 70:79–98.

———. 2003. The Language of the English Biometric School. *International Statistical Review* 71:109–30.

———. 2005. Fisher and Regression. *Statistical Science* 20:401–17.

———. 2007a. "But you have to remember P. J. Daniell of Sheffield." *Journal électronique d'histoire des probabilités et de la statistique* 3:1–58.

———. 2007b. Irving Fisher and the Econometrics of the Dancing Dollar. Unpublished paper.

———. 2008a. Keynes among the Statisticians. *HOPE* 40:265–316.

———. 2008b. R. A. Fisher on Bayes and Bayes' Theorem. *Bayesian Analysis* 3:161–70.

———. n.d.a. A Guide to R. A. Fisher. www.economics.soton.ac.uk/staff/aldrich/fisherguide/rafreader.htm.

———. n.d.b. Karl Pearson: A Reader's Guide. www.economics.soton.ac.uk/staff/aldrich/kpreader.htm.

Allen, R. G. D., and A. L. Bowley. 1935. *Family Expenditure: A Study of Its Variations*. London: King.

Andvig, J. C., and T. Thonstad. 1998. Ragnar Frisch at the University of Oslo. In Strøm 1998, 3–25.

Anderson, T. W. 1965. Samuel Stanley Wilks. *Annals of Mathematical Statistics* 36:1–23.

Arrow, K. J. 1960. The Work of Ragnar Frisch, Econometrician. *Econometrica* 28:175–92.

Arrow, K. J., and E. L. Lehmann. 2005. Harold Hotelling, 1895–1973. *National Academy of Sciences* 87:1–15.

Bennett, J. H., ed. 1990. *Statistical Inference and Analysis: Selected Correspondence of R. A. Fisher*. Oxford: Oxford University Press.
Biddle, J. 1999. Statistical Economics, 1900–1950. *HOPE* 31:607–51.
Bjerkholt, O., ed. 1995. *Foundations of Modern Econometrics: The Selected Essays of Ragnar Frisch*. 2 vols. Aldershot: Edward Elgar.
———. 1998. Ragnar Frisch and the Foundation of the Econometric Society and *Econometrica*. In Strøm 1998, 26–57.
———. 2005. Frisch's Econometric Laboratory and the Rise of Trygve Haavelmo's Probability Approach. *Econometric Theory* 21:491–533.
———. 2007. Writing "The Probability Approach" with Nowhere to Go: Haavelmo in the United States, 1939–1944. *Econometric Theory* 23:775–837.
Bowley, A. L. 1901. *Elements of Statistics*. London: King.
———. 1920. *Elements of Statistics*. 4th ed. London: King.
Bowley, A. L., and A. R. Burnett-Hurst. 1915. *Livelihood and Poverty*. London: Bell.
Box, J. F. 1978. *R. A. Fisher: The Life of a Scientist*. New York: Wiley.
Chuprov, A. A. [A. A. Tschuprow]. [1925] 1939. *Grundbegriffe und Grundprobleme der Korrelationstheorie*. Translated by M. Kantorowitsch as *Principles of the Mathematical Theory of Correlation*. London: W. Hodge & Co.
Cochran, W. G. 1938. Review of *Lectures and Conferences on Mathematical Statistics*, by J. Neyman. *Journal of the Royal Statistical Society* 101:758–59.
Coolidge, J. L. 1925. *An Introduction to Mathematical Probability*. Oxford: Clarendon Press.
Darmois, G. 1934. Developpements recents de la technique statistique. *Econometrica* 2:238–48.
———. 1936. *L'emploi des observations statistiques: Méthodes d'estimation*. Paris: Hermann.
Darnell, A. C. 1981. A. L. Bowley, 1869–1957. In *Pioneers of Modern Economics in Britain*, edited by D. P. O'Brien and J. R. Presley, 140–74. London: Macmillan.
———. 1988. Harold Hotelling, 1895–1973. *Statistical Science* 3:57–62.
Epstein, R. J. 1987. *A History of Econometrics*. Amsterdam: North-Holland.
Ezekiel, M. 1930. *Methods of Correlation Analysis*. New York: Wiley.
———. 1941. *Methods of Correlation Analysis*. 2nd ed. New York: Wiley.
Fisher, A. 1922. *The Mathematical Theory of Probabilities and Its Application to Frequency Curves and Statistical Methods*. 2nd ed. New York: Macmillan.
Fisher, I. 1898. Statistical Methods: Comment on G. Udny Yule's Paper, "The Theory of Correlation." *Yale Review* 7:103–5.
———. 1911. *The Purchasing Power of Money*. New York: Macmillan.
Fisher, R. A. 1918. The Correlation between Relatives on the Supposition of Mendelian Inheritance. *Transactions of the Royal Society of Edinburgh* 52:399–433.
———. 1922. On the Mathematical Foundations of Theoretical Statistics. *Philosophical Transactions of the Royal Society*, ser. A, 222:309–68.
———. 1925a. *Statistical Methods for Research Workers*. Edinburgh: Oliver & Boyd.
———. 1925b. Theory of Statistical Estimation. *Proceedings of the Cambridge Philosophical Society* 22:700–725.

———. 1928. The General Sampling Distribution of the Multiple Correlation Coefficient. *Proceedings of the Royal Society of London*, ser. A, 121:654–73.

———. 1930. Inverse Probability. *Proceedings of the Cambridge Philosophical Society* 26:528–35.

———. 1934. *Statistical Methods for Research Workers.* 5th ed. Edinburgh: Oliver & Boyd.

———. 1935a. *The Design of Experiments*. Edinburgh: Oliver & Boyd.

———. 1935b. The Logic of Inductive Inference [with discussion]. *Journal of the Royal Statistical Society* 98:39–82.

———. 1935c. The Mathematical Distributions Used in the Common Tests of Significance. *Econometrica* 3:353–65.

———. 1936. *Statistical Methods for Research Workers.* 6th ed. Edinburgh: Oliver & Boyd.

———. 1938. *Statistical Methods for Research Workers.* 7th ed. Edinburgh: Oliver & Boyd.

———. 1938–39. Review of *Lectures and Conferences on Mathematical Statistics*, by J. Neyman. *Science Progress* 33:577.

Fox, K. A. 1989. Agricultural Economists in the Econometric Revolution: Institutional Background and Leading Figures. *Oxford Economic Papers* 41:53–70.

Frisch, R. 1923. Sur un problème du calcul des probabilités (problème de Simmons). *Comptes rendus* 179:1031–33.

———. 1925a. Recurrence Formulae for the Moments of the Point Binomial. *Biometrika* 17:165–71.

———. 1925b. Sur les semi-invariants de Thiele. *Comptes rendus* 181:274–76.

———. 1926a. Sur un problème d'économie pure. *Norsk matematisk forenings skrifter*, ser. 1, no. 16:1–40.

———. 1926b. Sur les semi-invariants et moments employés dans l'étude des distributions statistiques. *Skrifter utgitt av Det Norske Videnskaps-Akademi i Oslo, II Historisk-Filosofisk Klasse* 3:1–87.

———. 1929. Correlation and Scatter in Statistical Variables. *Nordic Statistical Journal* 1:36–102.

———. 1934a. Circulation Planning: Proposal for a National Organization of a Commodity and Service Exchange. *Econometrica* 2:258–336.

———. 1934b. *Statistical Confluence Analysis by Means of Complete Regression Systems*. Publication no. 5, University Institute of Economics, Oslo.

———. [1926] 1995. On a Problem in Pure Economics. Translated by J. S. Chipman. In Bjerkholt 1995, 1:3–40.

Gilbert, C. L. 1991. Richard Stone, Demand Theory, and the Emergence of Modern Econometrics. *Economic Journal* 101:288–302.

Gilbert, C. L., and D. Qin. 2006. The First Fifty Years of Modern Econometrics. In *Econometric Theory*. Vol. 1 of *Palgrave Handbook of Econometrics*, edited by T. C. Mills and K. Patterson.

Goldberger, A. S. 1972. Structural Equation Methods in the Social Sciences. *Econometrica* 40:979–1001.

Haavelmo, T. 1943. The Statistical Implications of a System of Simultaneous Equations. *Econometrica* 11:1–12.

———. 1944. The Probability Approach in Econometrics. *Econometrica* 12 (supplement): iii–vi, 1–115.

———. 1989. Econometrics and the Welfare State. The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 1989 Prize Lecture. In *Les Prix Nobel*. Stockholm: Imprimerie Royale.

Hald, A. 1998. *A History of Mathematical Statistics from 1750 to 1930*. New York: Wiley.

Hatton, T. J., and R. E. Bailey. 2002. Native and Migrants in the London Labour Market. *Journal of Population Economics* 15:59–81.

Hendry, D. F., and M. S. Morgan. 1989. A Re-analysis of Confluence Analysis. *Oxford Economic Papers* 41:35–52.

———. 1994. The ET Interview: Professor H. O. A. Wold, 1908–1992. *Econometric Theory* 10:419–33.

———, eds. 1995. *The Foundations of Econometric Analysis*. Cambridge: Cambridge University Press.

Hepple, L. W. 2001. Multiple Regression and Spatial Policy Analysis: George Udny Yule and the Origins of Statistical Social Science. *Environment and Planning: D, Society and Space* 19:385–407.

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

Hotelling, H. 1927a. Differential Equations Subject to Error, and Population Estimates. *Journal of the American Statistical Association* 22:283–314.

———. 1927b. Review of *Statistical Methods for Research Workers*, by R. A. Fisher. *Journal of the American Statistical Association* 22:411–12.

———. 1930a. British Statistics and Statisticians Today. *Journal of the American Statistical Association* 25:186–90.

———. 1930b. The Consistency and Ultimate Distribution of Optimum Statistics. *Transactions of the American Mathematical Society* 32:847–59.

———. 1931a. The Generalization of Student's Ratio. *Annals of Mathematical Statistics* 2:360–78.

———. 1931b. Recent Improvements in Statistical Inference. *Journal of the American Statistical Association* 26 (supplement): 79–87.

———. 1939. The Work of Henry Schultz. *Econometrica* 7:97–103.

———. 1951. Abraham Wald. *American Statistician* 5:18–19.

Hunter, P. W. 1996. Drawing the Boundaries: Mathematical Statistics in 20th-Century America. *Historia Mathematica* 23:7–30.

Keynes, J. M. 1921. *A Treatise on Probability*. London: Macmillan.

Klein, J. L. 1997. *Statistical Visions in Time: A History of Time Series Analysis, 1662–1938*. New York: Cambridge University Press.

Koopmans, T. C. 1935. On Modern Sampling Theory. Lectures delivered at Oslo, autumn 1935. Mimeo.

———. 1937. *Linear Regression Analysis of Economic Time Series*. Haarlem: Bohn.

———. 1941. The Logic of Econometric Business-Cycle Research. *Journal of Political Economy* 49:157–81.
———. 1942. Serial Correlation and Quadratic Forms in Normal Variables. *Annals of Mathematical Statistics* 13:14–33.
———. 1945. Statistical Estimation of Simultaneous Economic Relations. *Journal of the American Statistical Association* 40:448–66.
———. 1947. Measurement without Theory. *Review of Economics and Statistics* 29:161–72.
———. 1950. When Is an Equation System Complete for Statistical Purposes? In Koopmans, ed., 1950, chap. 9.
Koopmans, T. C., ed. 1950. *Statistical Inference in Dynamic Econometric Models*. New York: Wiley.
Koopmans, T. C., and O. Reiersøl. 1950. The Identification of Structural Characteristics. *Annals of Mathematical Statistics* 21:165–81.
Koopmans, T. C., H. Rubin, and R. B. Leipnik. 1950. Measuring the Equation Systems of Dynamic Economics. In Koopmans, ed., 1950, chap. 2.
Lauritzen, S. L. 2002. *Thiele: Pioneer in Statistics*. Oxford: Oxford University Press.
Louçã, F. 2007. *The Years of High Econometrics: A Short History of the Generation That Reinvented Economics*. London: Routledge.
Mann, H. B., and A. Wald. 1943. On the Statistical Treatment of Linear Stochastic Difference Equations. *Econometrica* 11:173–220.
Marshall, A. 1890. *Principles of Economics*. Vol. 1. London: Macmillan.
———. 1898. *Principles of Economics*. Vol. 1. 4th ed. London: Macmillan.
Mirowski, P. 1990. Problems in the Paternity of Econometrics: Henry Ludwell Moore. *HOPE* 22:587–609.
———, ed. 1994. *Edgeworth on Chance, Economic Hazard, and Statistics*. Lanham, Md.: Rowman & Littlefield.
Moore, H. L. 1908. The Statistical Complement of Pure Economics. *Quarterly Journal of Economics* 23:1–33.
———. 1917. *Forecasting the Yield and Price of Cotton*. New York: Macmillan.
Morgenstern, O. 1951. Abraham Wald, 1902–1950. *Econometrica* 19:361–67.
Morgan, M. S. 1990. *A History of Econometric Ideas*. New York: Cambridge University Press.
Neyman, J. 1934. On the Two Different Aspects of the Representative Method [with discussion]. *Journal of the Royal Statistical Society* 97:558–625.
———. 1935. [Contribution to the Discussion of Fisher 1935b.] *Journal of the Royal Statistical Society* 98:73–76.
———. 1937a. [Discussion of Rhodes 1937.] *Journal of the Royal Statistical Society* 100:50–57.
———. 1937b. Outline of a Theory of Statistical Estimation Based on the Classical Theory of Probability. *Philosophical Transactions of the Royal Society*, ser. A, 236:333–80.
———. 1937c. Survey of Recent Work on Correlation and Covariance. Paper presented at the September 1936 meeting of the Econometric Society.

———. 1938. *Lectures and Conferences on Mathematical Statistics and Probability.* Washington: Graduate School USDA.
———. 1939. Review of *A Study in the Analysis of Stationary Time Series*, by Herman Wold. *Journal of the Royal Statistical Society* 102:295–98.
———. 1951. Existence of Consistent Estimates of the Directional Parameter in a Linear Structural Relation between Two Variables. *Annals of Mathematical Statistics* 22:497–512.
———. 1976. The Emergence of Mathematical Statistics: A Historical Sketch with Particular Reference to the United States. In *On the History of Statistics and Probability*, edited by D. B. Owen. New York: Dekker.
Neyman, J., and E. S. Pearson. 1928. On the Use of Certain Test Criteria for Purposes of Statistical Inference. Pts. 1 and 2. *Biometrika* 20A:175–240, 263–94.
———. 1933. On the Problem of the Most Efficient Tests of Statistical Hypotheses. *Philosophical Transactions of the Royal Society* 231:298–337.
Neyman, J., and E. L. Scott. 1948. Consistent Estimates Based on Partially Consistent Observations. *Econometrica* 16:1–32.
Norton, J. P. 1902. *Statistical Studies in the New York Money-Market, Preceded by a Brief Analysis under the Theory of Money and Credit with Statistical Tables, Diagrams, and Folding Chart.* New York: Macmillan.
Pearl, R. 1936. Karl Pearson, 1857–1936. *Journal of the American Statistical Association* 31:653–64.
Pearson, E. S. 1936–38. Karl Pearson: An Appreciation of Some Aspects of His Life and Work. *Biometrika* 28:193–257; 29:161–247.
Pearson, K. 1896. Mathematical Contributions to the Theory of Evolution. III. Regression, Heredity, and Panmixia. *Philosophical Transactions of the Royal Society*, ser. A, 187:253–318.
———. 1897. *Chances of Death and Other Studies in Evolution.* 2 vols. London: Edward Arnold.
———. 1900. *The Grammar of Science.* 2nd ed. London: A. & C. Black.
Persons, W. M. 1910. The Correlation of Economic Statistics. *Journal of the American Statistical Association* 23:287–322.
Pesaran, M. H. 1991. The ET Interview: Professor Sir Richard Stone. *Econometric Theory* 7:85–123.
Phelps Brown, E. H. 1937. Report of the Oxford Meeting, September 25–29, 1936. *Econometrica* 5:361–83.
Phillips, P. C. B. 1985. The ET Interview: Professor J. D. Sargan. *Econometric Theory* 1:119–39.
———. 1986. The ET Interview: Professor T. W. Anderson. *Econometric Theory* 2:249–88.
———. 1988. The ET Interview: Professor James Durbin. *Econometric Theory* 4:125–57.
Pigou, A. C. 1927. *Industrial Fluctuations.* London: Macmillan.
Qin, D. 1993. *The Formation of Econometrics: A Historical Perspective.* Oxford: Oxford University Press.

———. 1996. Bayesian Econometrics: The First Twenty Years. *Econometric Theory* 12:500–516.
Reid, C. 1982. *Neyman—from Life*. New York: Springer.
Rhodes, E. C. 1937. The Construction of an Index of Business Activity [with discussion]. *Journal of the Royal Statistical Society* 100:13–66.
Rider, P. R. 1936. Annual Survey of Statistical Technique: Developments in the Analysis of Multivariate Data. Pt. 1. *Econometrica* 4:264–68.
Rietz, H. L. 1927. *Mathematical Statistics*. Chicago: The Mathematical Association of America.
Scheffé, H. 1959. *The Analysis of Variance*. New York: Wiley.
Schultz, H. 1928. *Statistical Laws of Demand and Supply, with Special Application to Sugar*. Chicago: University of Chicago Press.
———. 1929. Applications of the Theory of Error to the Interpretation of Trends: Discussion. *Journal of the American Statistical Association, Supplement: Proceedings of the American Statistical Association* 24:86–89.
———. 1930. The Standard Error of a Forecast from a Curve. *Journal of the American Statistical Association* 25:139–85.
———. 1938. *The Theory and Measurement of Demand*. Chicago: University of Chicago Press.
Schweder, T. 1980. Scandinavian Statistics: Some Early Lines of Development. *Scandinavian Journal of Statistics* 7:113–29.
———. n.d. Early Statistics in the Nordic Countries: When Did the Scandinavians Slip behind the British? Unpublished paper.
Shewhart, W. A. 1933. Annual Survey of Statistical Technique: Developments in Sampling Theory. *Econometrica* 1:225–37.
Sheynin, O. 2005. *Theory of Probability: A Historical Essay*. www.sheynin.de/download/double.pdf.
Slutsky, E. E. 1913. On the Criterion of Goodness of Fit of the Regression Lines and on the Best Method of Fitting Them to the Data. *Journal of the Royal Statistical Society* 77:78–84.
Smith, W. L. 1978. Harold Hotelling, 1895–1973. *Annals of Statistics* 6:1173–83.
Stigler, G. J. 1962. Henry L. Moore and Statistical Economics. *Econometrica* 30:1–21.
Stigler, S. M. 1978. Francis Ysidro Edgeworth, Statistician [with discussion]. *Journal of the Royal Statistical Society* 141:287–322.
———. 1986. *The History of Statistics: The Measurement of Uncertainty before 1900*. Cambridge: Belknap Press of Harvard University Press.
———. 1996. The History of Statistics in 1933. *Statistical Science* 11:244–52.
———. 1999a. The Foundations of Statistics at Stanford. *American Statistician*, 53:263–66.
———. 1999b. Karl Pearson and the Cambridge Economists. In *Statistics on the Table*. Cambridge: Harvard University Press.
———. 2002. Statisticians and the History of Economics. *Journal of the History of Economic Thought* 24:155–64.
———. 2005. Fisher in 1921. *Statistical Science* 20:32–49.

———. 2007. The Epic Story of Maximum Likelihood. *Statistical Science* 22:598–620.

Stone, R. 1945. The Analysis of Market Demand. *Journal of the Royal Statistical Society* 108:286–391.

Strøm, S., ed. 1998. *Econometrics and Economic Theory in the 20th Century: The Ragnar Frisch Centennial Symposium*. Cambridge: Cambridge University Press.

Thomson, G. H. 1935. Group Factors in School Subjects. *British Journal of Educational Psychology* 5:194–99.

Tinbergen, J. 1939. *Statistical Testing of Business-Cycle Theories II: Business Cycles in the United States of America*. Geneva: League of Nations.

Tintner, G. 1945. A Note on Rank, Multicollinearity, and Multiple Regression. *Annals of Mathematical Statistics* 16:304–8.

———. 1946. Multiple Regressions for Systems of Equations. *Econometrica* 14:5–36.

———. 1952. Abraham Wald's Contributions to Econometrics. *Annals of Mathematical Statistics* 23:21–28.

Wald, A. 1936. *Berechnung und Ausschaltung von Saisonschwankungen*. Beitrage zur Konjunkturforschung, vol. 9. Vienna: Springer.

———. 1939. Contributions to the Theory of Statistical Estimation and Testing Hypotheses. *Annals of Mathematical Statistics* 10:299–326.

———. 1940a. The Fitting of Straight Lines If Both Variables Are Subject to Error. *Annals of Mathematical Statistics* 11:284–300.

———. 1940b. A New Foundation of the Method of Maximum Likelihood in Statistical Theory. *Cowles Commission for Research in Economics, Report of Sixth Annual Research Conference on Economics and Statistics*.

———. 1941. Asymptotically Most Powerful Tests of Statistical Hypotheses. *Annals of Mathematical Statistics* 12:1–19.

Whitaker, J. K. 1996. *The Correspondence of Alfred Marshall, Economist*. 3 vols. Cambridge: Cambridge University Press.

Wilks, S. S. 1935. On the Independence of *k* Sets of Normally Distributed Statistical Variables. *Econometrica* 3:309–26.

———. 1938. Review of *Lectures and Conferences on Mathematical Statistics*, by J. Neyman. *Journal of the American Statistical Association* 33:478–80.

———. 1944. *Mathematical Statistics*. Princeton: Princeton University Press.

Wishart, J. 1928. The Generalised Product Moment Distribution in Samples from a Normal Multivariate Population. *Biometrika* 20A:32–52.

Wold, H. 1938. *A Study in the Analysis of Stationary Time Series*. Uppsala: Almqvist & Wicksells.

Wolfowitz, J. 1952. Abraham Wald, 1902–1950. *Annals of Mathematical Statistics* 23:1–13.

Working, H., and H. Hotelling. 1929. Applications of the Theory of Error to the Interpretation of Trends. *Journal of the American Statistical Association, Supplement: Proceedings of the American Statistical Association* 24:73–85.

Wright, S. 1934. The Method of Path Coefficients. *Annals of Mathematical Statistics* 5:161–215.

Yule, G. U. 1895. On the Correlation of Total Pauperism with Proportion of Out-Relief. Pt. 1, All Ages. *Economic Journal* 5:603–11.

———. 1896a. Notes on the History of Pauperism in England and Wales from 1850, Treated by the Method of Frequency-Curves; with an Introduction on the Method. *Journal of the Royal Statistical Society* 59:318–57.

———. 1896b. On the Correlation of Total Pauperism with Proportion of Out-Relief. Pt. 2, Males over 65. *Economic Journal* 6:613–23.

———. 1897a. Note on the Teaching of the Theory of Statistics at University College. *Journal of the Royal Statistical Society* 60:456–58.

———. 1897b. On the Theory of Correlation. *Journal of the Royal Statistical Society* 60:812–54.

———. 1899. An Investigation into the Causes of Changes in Pauperism in England, Chiefly during the Last Two Intercensal Decades. Pt. 1. *Journal of the Royal Statistical Society* 62:249–96.

———. 1909. The Applications of the Method of Correlation to Social and Economic Statistics. *Journal of the Royal Statistical Society* 72:721–30.

———. 1911. *An Introduction to the Theory of Statistics*. London: Griffin.

———. 1912. Review of *Law of Wages*, by H. L. Moore. *Journal of the Royal Statistical Society* 75:551–53.

———. 1915. Review of *Economic Cycles*, by H. L. Moore. *Journal of the Royal Statistical Society* 78:302–5.

Yule, G. U., and M. G. Kendall. 1937. *An Introduction to the Theory of Statistics*. 11th ed. London: Griffin.

Zabell, S. 1992. R. A. Fisher and the Fiducial Argument. *Statistical Science* 7:369–87.