Key names: Karl Pearson, R. A. Fisher, J. Neyman, A. Wald, H. L. Moore, H. Hotelling, M. Ezekiel, H. Schultz, R. Frisch, T. C. Koopmans, T. Haavelmo.

# The Econometricians' Statisticians 1895-1945


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**Abstract**

Histories of econometrics describe what econometricians did with the ideas of statisticians–correlation, regression, maximum likelihood, testing statistical hypotheses, etc. The transfer of these ideas was made possible by institutions, practices and personal contacts between statisticians and econometricians. This paper complements the usual history of econometrics with an account of these transactions from the statisticians' side. I concentrate on four statisticians and their interactions with econometricians: Karl Pearson whose influence was exercised in the years before the First World War, Ronald Fisher who began to be influential in the late 1920s, Jerzy Neyman who appeared in the late 30s and Abraham Wald in the early 40s.



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# 1 Introduction

"Modern statistical theory originated in England, and is today advancing faster there than in any other country." So Harold Hotelling (1930, p. 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 Moore, Ezekiel, Schultz, Koopmans and Haavelmo especially. I also notice 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 travelled will be my concern; more broadly, I will be considering the way statisticians and econometricians worked together, or, 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 (1936/8), Box (1978) and 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 Epstein (1987), Hendry & Morgan (1995), Morgan (1990) and Qin (1993)–see also Gilbert & Qin's (2006) recent brief account–and Bjerkholt's (2005 & -7) more specialised studies of Haavelmo and Frisch. Bjerkholt's researches on the Frisch network have been invaluable and I often seem to be writing footnotes to his footnotes. Viewed from the history of statistics this essay belongs to "reception studies" with connections to S. M. Stigler (1986) writing on Pearson's work on correlation and regression and Aldrich (1997 & 2005) on Fisher's work on maximum likelihood and regression. There are especially close links with Stigler's "epic story of maximum likelihood" (2007): 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, e.g. Bjerkholt (1995) and Stigler (1996).

The plan is this. §§2-3 consider Pearson's correlation/regression and how it entered economics. §4 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 (1930 & -31a) contributed. By the mid-1930s ideas were racing round a network created by Ragnar Frisch and people were travelling too. §5 describes the network and §6 one of the travellers, Tjalling Koopmans who took foundations and maximum likelihood from Fisher.

Neyman was a member of the original network centred on Oslo while Wald joined the network as it was reconstituted in the United States; their interactions with the econometricians are the subject of §§7-10. §11 is a coda on statistical inference and structure and §12 sums up.

Pearson and Fisher were English statisticians, Neyman was a Pole, who after being co-opted into English statistics, moved to California, while Wald went from Vienna and the Continental tradition in statistics to America to 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 (Yule) and a Dutchman in Chicago (Koopmans). The associations emphasise 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.

## 2 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, p. 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. 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 (2001/7).

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 Galton and after some input from Edgeworth was given definitive form in Pearson (1896); the paper also treated bivariate and trivariate regression. Pearson wrote on biometry and published in mathematical journals but correlation and regression went straight into to the economics journals taken by Pearson's student/collaborator, Udny Yule (1871-1951). Stigler (1986, Part Three) describes the background in statistical theory and biometry and Hepple (2001) supplies additional background on Yule and his activities. The regression story is carried forward into the twentieth century in Aldrich (2005).

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, volume 2, p. 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. The point is elaborated in Aldrich (2008).

Yule promoted Pearson's statistics among the economists/statisticians on a broad front by publicising it (Yule (1897)), applying it (Yule (1896a)) and developing useful extensions.

Expositions and results appeared in the *Economic Journal*: he (1895) showed economists how they could use correlation to analyse pauperism and then he (1896) introduced them to his new concept of partial ("nett") correlation. Justifications and details (1897a & -99) appeared in the *Statistical Journal*. An engineer-physicist by training, Yule, studied the standard economics work, Marshall's *Principles* (1890), and could quote from it effectively as on mutual determination in his (1895, p. 605) or on demand curves in his (1915, p. 305). One of the topics Yule worked on with his friend R. H. Hooker (1867-1944) 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 or 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 §5 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; for more on the activities of Yule and Hooker see Aldrich (1995) and Klein (1997).

Putting aside the question of Yule as an econometrician, what of him as an econometricians' statistician? He did what was needed by demonstrating the new methods and by producing a guide book, the *Introduction to the Theory of Statistics* (1911), and yet his impact 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.

## 3 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 Chuprov and Slutsky were among those impressed by it. Slutsky wrote a book on correlation in economics and consulted Pearson; he even offered a paper which appeared in the *Statistical Journal* as his (1913)–no article on econometrics ever appeared in *Biometrika.*

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 was soon (1898) writing about Yule's "Theory of correlation" (1897). Fisher expounded Yule's bivariate analysis; he did not mention partial correlation or multiple regression and in fact never used them. Fisher's econometric work is discussed in Aldrich (2007).

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, p. 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. G. J. Stigler (1962, p. 18) sums up his achievement, "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." Moore's work is further considered by Mirowski (1990) and Morgan (1990, passim). In the programmatic "Statistical complement of pure economics" Moore (1908, p. 2) named four investigators who have conceived "an inductive statistical complement of the pure science": Cournot, Jevons, Edgeworth, and Pareto. Moore made contact with the two living but nothing followed. Moore's unhappy experience with Edgeworth (and Marshall) are well-known but something can be added about his relations with the statisticians. Pearson figures in the "Statistical complement," not as prominently as Edgeworth but more prominently than Lexis and von Bortkiewicz, the leading representatives of the continental turn in statistics. 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 the forty something Moore visited London and attended Pearson's lectures. Pearson's influence can be seen in Moore's later books. Yule (1912 & -15) reviewed two of them, favourably though 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–learnt them from reading Pearson, or more probably his followers, without personal contact with the master: the authors on correlation that Persons (1910) suggests are 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; S. 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, p. 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 G. J. Stigler (1962) and Mirowski (1990 and -94) 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 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; his career is described in Darnell (1981). 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 in 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 & Burnett-Hurst (1915), one of a series of surveys of poverty Bowley conducted. Obtaining data–through the condition of labour surveys and the construction of national income estimates–was Bowley's main occupation through the 20s and 30s. The survey data was not used for econometric analysis; that has only been done very recently–see Hatton & 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 §7 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.

The failure of econometrics to take 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:

> [I]f 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) vol. 2, p. 307.)

When Marshall *was* young–in 1866–he had asked Todhunter 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 vol. 3, p. 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 4th edition (1898, p. 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 S. M. Stigler (1986, pp. 356-7). In 1910-11 there was the well-known controversy between Pearson and the economists over the effects of parental alcoholism; see Stigler (1999a). The economists followed Irving Fisher's business cycle analysis and monetary economics but they did not imitate his methods: Pigou (1927, pp. 194-5) reached similar conclusions using graphical comparisons instead of distributed lag correlation analysis. Of the Cambridge economists Keynes was the most involved with statistics; his complex relations with the statisticians are examined in Aldrich (2008). One constant was hostility to Pearson and another was that correlations had to be handled very cautiously. Of course, Keynes's later controversy with Tinbergen is well covered in the history of econometrics literature; see e.g. Hendry & Morgan (1995, Part IV).

Drawing on what he saw in 1929 Hotelling (1930, p. 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–though it may be an indicator of his lack of presence that Hotelling should mistake him for one–but the spirit is 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 30s but the *Economic Journal* did not repeat the Yule experiment of 1895-6.

## 4 R. A. Fisher in America

Ronald Fisher (1890-1962) was, according to Hald (1998, p. 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; Fisher the biologist appears in §11 below. For more on Fisher see Aldrich (2003/7).

From 1919 Fisher had a base at Rothamsted Experimental Station near London; there, as well as continuing his old researches, he developed methods for analysing and designing agricultural experiments. Like Pearson, Fisher wrote difficult journal articles but his *Statistical Methods for Research Workers* (1925) took his ideas to a larger public. The "research workers" Fisher aimed at were biologists or 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 (1925, p. 2):

> 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.

In the 5th 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, p. 2.)) Aldrich (2008) speculates 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) 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% of ASA members were also members of the American Economic Association (Biddle (1999, p. 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), Hunter (1996) and Stigler (1996a and -b) but less for their research than for their role in setting up the Institute of Mathematical Statistics and the *Annals of Mathematical Statistics*; institutions which came to life only in the late 1930s following an infusion of European ideas–see §§5 & 9 below. There were more statisticians than in Britain and more book titles. The mathematical textbooks by H. L. Rietz (1928), J. E.

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 Sheynin's (2005) history. In Britain the outstanding enthusiast for continental statistics was Keynes (1921) but that did its cause no good at all: see Aldrich (2008). For another perspective on American statistics in the 1920s see Aldrich (2007a, §7).

Hotelling's (1927, p. 412) assessment of Fisher's book 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 (1927, p. 412) 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." The econometricians found Student's $t$-test and its application to regression especially valuable; for Fisher on these topics see Aldrich (2005).

Having failed to get Fisher to Stanford (Stigler, 1999a), 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, p. 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. For further biographical information see Smith (1978), Darnell (1988) and Arrow & Lehmann (2005).

Some of Fisher's impact 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" (1927) 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 (1927, p. 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 his (1925, p. 10) verdict. Fisher's hostility to inverse probability, or Bayesian inference, is described in Aldrich (2008).

Hotelling's first post-Fisher paper was written with Holbrook Working, an agricultural econometrician at the Food Research Institute. Working & Hotelling (1929) was the first publication to adopt Fisher's regression framework; the framework is described in Aldrich (2005). The article (1929, p. 81) 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 $x$; Scheffé (1959, p. 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.

Hotelling was the first modern statistical theorist to come out of America and almost the first Fisherian–Wishart on the strength of his (1928) could claim to be that. 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 (1930) and on $T^2$ (1931) come 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 §5–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 & Lehmann (2005, p. 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–Wilks, Wald and 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 *The Theory and Measurement of Demand* (1938) the most ambitious American econometric study of the decade.

Ezekiel's *Correlation Analysis* was in Fox's (1989, p. 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, p. 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 & 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 April 23rd 1930 he wrote asking for help in understanding Fisher's (1928) paper on the distribution of the multiple correlation coefficient; he could manage the $t$ material from 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." 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). The work–for both of them–was over by June and on the 26th 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 in Schultz's writings of the 1920s (they culminate in the *Statistical Laws of Demand and Supply* (1928)) of much influence from those directions, 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 30s Schultz moved decisively towards 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*. 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 -36; in 1936 he gave lectures at the Cowles Commission. When Box (1978, ch. 12) describes these trips she emphasises 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; see Aldrich (2000). There are small traces of Fisher's encounters with the econometricians in his writings: a reference to Working & Hotelling was added in the 1936 edition of the *Statistical Methods* and a reference to Schultz to 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 §6 below.

## 5 Frisch: network and project

Hotelling promoted Fisher without himself practising 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 centre of a great network where mathematical economics, econometrics and mathematical statistics met. 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 Louçã (2007).

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. Stigler (1996) has described the birth of the *Annals* and Bjerkholt (1998) the birth and early history of *Econometrica*. 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 have met above, were all involved; Hotelling, Fisher's choice as editor was on the journal's advisory editorial board along with the statisticians, Bowley, Darmois and E. B. Wilson. One of the three associate editors was 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 (1935) provided a streamlined version of the distribution theory underlying his tests and there were contributions from Shewhart (1933), Darmois (1934), Wilks (1935) and 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." In Continental Europe this was a minority interest and the Nordic countries themselves had a strong statistical tradition; see Schweder (1980). Frisch's (1926) thesis was in statistical theory, on Thiele's semi-invariants. Thorwald Thiele (1838-1910) has recently been re-evaluated by 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, though ironically one source of tension between him and Fisher was the latter's refusal to admit that his cumulants (of 1930) were semi-invariants *re*-discovered; some of the correspondence is reprinted in Bennett (1990, pp. 314-7).

In his years of post-graduate 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 (1926, p. 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 (1926, p. 87) indicated that he would. In the thesis he did not reveal what he had done on the inverse problem beyond saying what he disagreed with the Russian statistician A. A. Chuprov (1874-1926) and offering (p. 88) 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 theory 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 has 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 by Bjerkholt (2005, p. 526)). These reflections resemble Keynes's criticism of the English statisticians' preoccupation with "techniques." Frisch knew Keynes's *Treatise on Probability* (1921) for he (1923, p. 1031) mentions it in a technical context 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 favoured 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." There he (1926a, pp. 27ff) objected to Yule's regression theory because it did not treat the variables symmetrically. Frisch (1926a, p. 28) 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 which can be applied to the very application of the method of least squares to problems which do not fall within the proper scope of the theory of errors of observation.

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* (1934) were his main contributions. Frisch had first used matrices in his (1926a) and by the end of the decade his mastery of the algebraic/computational side of least squares/correlation theory was probably unsurpassed; 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; see Hendry & Morgan (1989) for an exposition and history of confluence analysis.

Frisch was a new phenomenon, the 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–which persisted for a number of years: in confluence analysis Frisch had a technique which 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 probabilisation 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, p. 500)) but by February 1937 he was telling Wilks (see below §§6 & 9) 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 (p. 526)) 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) but another, which kept to the original framework of equations in variables measured with error, was Tintner's (1945, -46) work on rank and multicollinearity; there is more about the first line of development in §§8-10 below while the second, less influential, line is traced in Aldrich (1993).

Trygve Haavelmo (1911-1999) appears as a principal in §§8-10 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 travelled the Frisch network and where he went reflected Frisch's priorities at the time; his journeys are described by Bjerkholt (2005 & -7).

# 6 Koopmans: specification and maximum likelihood

Frisch travelled, invited people to Oslo and carried on a huge correspondence (the list of correspondents runs to 80 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, p. 222). The Institute of Economics was well funded by the Rockefeller Foundation and it had many visitors; see Andvig & Thonstad (1998, pp. 9-10). Tjalling Koopmans (1910-1985) from the Netherlands was one of the visitors. In the 40s Koopmans would be responsible for the basic identification and estimation theory for the simultaneous equations model–see §10 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, pp. 328-330) and Wilks's comments (to Frisch) are summarised by Bjerkholt (2005, p. 526). Samuel Wilks (1906–1964), at Princeton, did his PhD under Rietz but his intellectual home was the Pearson department at University College and its journal *Biometrika*; for 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 inference theory 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, p. 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); for this see Aldrich (1997) and Stigler (2005). Koopmans (pp. 2-4) quotes extensively from the part (1922, sections 2 & 3) dealing with the "problem of specification," investigating the "hypothetical infinite population" appropriate to an economic setting in which variables are measured with error; the relevant passages are reproduced in Hendry & Morgan (1995, Part IV). 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 econometrician's use of what Frisch called "sampling theory" could find a place.

For estimation Koopmans used Fisher's method of maximum likelihood; for a history of this see Stigler (2007). The theory presented in Fisher (1922 and -25a) combined a large sample distribution theory, an optimality theory and an interpretation of the method in terms of the extraction of information–all for i.i.d. observations. Fisher used the method in genetics and in the theory of errors, the latter becoming modern regression analysis–see Aldrich (2005) for details. 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 (1930) or Darmois (1936) which tried to express Fisher's ideas in more rigorous and intelligible terms; this literature is described in Stigler (2007). 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, pp. 59-63) carefully notes the estimation possibilities for all of the many parameters involved. He (p. 61) 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"; he assumes the ratios are known. Koopmans did not an overwhelming case for maximum likelihood: he (p. 62) argues that under his conditions the estimator will be consistent but then (pp. 63-5) explains that little of Fisher's original argument for its superiority over estimators can be transferred to the present situation and finishes by stating, "maximum likelihood estimation is here adopted simply because it seems to lead to useful statistics."

On October 26 1937 Fisher wrote congratulating Koopmans 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, pp. 328-330))

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; see Pesaran (1991) and Gilbert (1991) for Stone and the DAE. Stone (1945, p. 311n) 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; for them see Phillips (1985 and 1988).

## 7 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. Fisher, rather than Karl, was the influence on the partnership's first production, Neyman & Pearson (1928) which proposed the (maximised) likelihood ratio test. A series of important papers followed with the fundamental principles of the mature "Neyman-Pearson theory" of testing appearing in their (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 KP'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, p.

73) described *his* reaction to reading Fisher, "What an interesting way of asking and answering questions, but can't I do differently?" He (p. 74) then sketched a "theory of mathematical statistics which would be based solely upon the theory of probability (thus independent of the conception of likelihood)." In this theory the "frequency of errors in judgment" would be central, not information as in Fisher's theory. Fisher's (1935a, p. 82) reply was civil, "It has been, naturally, of great interest 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 Zabell (1992, pp. 385-6).

Neyman showed how he would do things differently in his "Outline of a theory of statistical estimation based on the classical theory of probability" (1937). The theory outlined is for "estimation by interval" rather than "estimation by unique estimate"; Neyman had first treated interval estimation in his (1934). Maximum likelihood is discussed in the preliminary review of the 1937 paper (p. 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. Stigler (2007) looks at Neyman's attitude to maximum likelihood–and to Fisher–more closely.

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 (1937a, b, 1938 and 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, pp. 509-10). From the printed summary it seems that the talk was not a survey of what had been done but a programme. All the elements of Neyman's thinking about econometrics are here. "Two paths of approach, the empirical and the a priori" are characterised and it is argued that the latter is the more appropriate. Neyman (1937a, p. 368) 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." 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 re-appear in Koopmans's "Measurement Without Theory" (1947, p. 161), where Mitchell of the National Bureau is identified 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" (1927) 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 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 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 (1937a, p. 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 (p. 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 emphasise 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 State, meeting some of the same people as Fisher; Schultz (1938, p. 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 the conference on "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 (1938, p. 114) said is one of "many similar ones" he mentioned 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." Bjerkholt (2005, p. 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, pp. 115-124) used the 2 person/sector dynamic model of exchange from Frisch's "Circulation planning" (1934a, pp. 261-272) 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

$$
\begin{aligned}
x_t &= (a+\alpha)y_{t-1} \\
y_t &= (b+\beta)x_{t-1} \\
a &= \alpha_0 + \alpha_1 t \\
b &= b_0 + b_1 t
\end{aligned}
$$

where $\alpha$ and $\beta$ 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 & 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 & -44) to the simultaneous equations model, came from confluence analysis; how is described in Aldrich (1989 and -94).

*Lectures and Conferences* was reviewed in the statistical journals by 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, p. 570) evidently thought so too, "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 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, p. 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–eventually creating a new University College–and in 1939 Poland was invaded. He returned to the econometric field with Neyman & Scott (1948) and Neyman (1951) but these were not continuations of the 1937-9 work but explorations of the pathology of maximum likelihood in the errors in variables model and models like it. See Stigler (2007) for a perspective on Neyman's work on maximum likelihood.

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 e.g. Koopmans (1941, p. 173)–but they did not refer to his remarks on econometrics: Haavelmo (1944, pp. 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.

## 8 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 & Morgan (1995, Parts VII-VIII), 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, pp. 810-2) 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 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. There are other Haavelmo recollections from the 80s–summarised by Morgan (1990, p. 242n)–but the most vivid is the passage in the Nobel lecture (1989, p. 285) 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.

Morgan (1990, p. 242) sees the significance of these encounters in Haavelmo's "conversion to probability reasoning." Bjerkholt (2007, pp. 784-5 & 825) concurs on their significance but argues that the meetings took place in England in 1936. Bjerkholt (2005, p. 13) 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 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 -39, there was a lot of room for Haavelmo and Neyman to talk past one another. The decision Neyman emphasised 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 towards accepting the probability approach, i.e. recognising the desirability and feasibility of probabilising confluence analysis; see §5 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 -39 and so it is difficult to make sense of the story. My guess is that the recollections reflect meetings in both 1936 and -39; 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 Neyman-Pearson theory and its application to econometrics. Haavelmo went to learn about the theory and chapter IV distilled what he had learnt. 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, p. 831)) Haavelmo's relations with Wald, the last of my quartet of statisticians, were more straightforward.

## 9 A "unique knowledge of modern statistical theory"

As an econometricians' statistician Abraham Wald (1902-50) was unique for he was a statistical authority *and* a working econometric theorist. After producing a PhD 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 (1952) and Morgenstern (1952).

In Europe Wald had not quite belonged to the Frisch network: Bjerkholt (2005, p. 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 Frisch. The Cowles Commission had always been a node of the network but now in Chicago and from 1943 under Jakob Marschak's leadership Cowles in Chicago replaced the Institute of Economics in Oslo as the centre 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 1940-6 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" (§2 above) and Fisher warned against the economists (§4 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, Hermans Rubin and 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; see Hildreth (1986) for Rubin and Chernoff and Phillips (1986) for Anderson.

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. When the two met in the United States in 1939 Wald, like Haavelmo, 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, p. 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 acknowledgement was to Frisch—for "ideas.") One of the areas in which Wald contributed mathematical expertise was the "problem of arbitrary parameters," i.e. Haavelmo's theory of identification; see Aldrich (1994) and Bjerkholt (2007, passim) 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 Wolfowitz (1952, p. 2) "probably his most important paper." The theory grew out of the Neyman-Pearson theory of testing and Neyman's theory of confidence intervals. Fisher Wald knew 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 IV has Neyman's. If Wald had a big idea it was decision theory but this only makes a small appearance in Haavelmo's (1944, ch. VI) discussion of prediction, itself a peripheral topic. Wald with his "unique knowledge of modern statistical theory" could provide technical support and re-assurance about foundations: in the case of i.i.d. observations 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, pp. 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" he (1943, pp. 7-10) 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 this question and in a footnote Haavelmo (1944, p. 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" (1940a) 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, viz. that the confidence interval derived from the maximum likelihood estimate is shortest in the sense of Neyman (1937). Wald's involvement with maximum likelihood is discussed in greater depth by Stigler (2007).

Haavelmo's footnote exposed the different allegiances of the econometricians. Koopmans's response, as reported by Bjerkholt (2007, p. 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 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 & 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.

## 10 Wald and Koopmans in econometric theory

The relationship between Koopmans and Wald was quite different from that between Haavelmo and Wald: 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 (§6 above) was a part of the setting for Wald's "The fitting of straight lines if both variables are subject to error" (1940); 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. This 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. Wald's paper is discussed further by Bjerkholt (2007) and Stigler (2007).

"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 realised. Wold (1938, p. 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) 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, pp. 3-5) is a multiplier-accelerator model which has the form of a restricted bivariate autoregressive process. Haavelmo (1943, pp. 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 & Wald's "Linear stochastic difference equations" published later in the same year: there (1943, p. 175), "It is shown that the maximum-likelihood estimates are consistent and their limit distributions normal." 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 & 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 5 years later–Koopmans, Rubin & 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, chapter 4) and Aldrich (1994).

"Measuring the equation systems" with 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 and -44) was much more durable than the first (§6 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–§§4-5 above–was reopened in the 1960s; see Qin (1996).

In the Introduction I mentioned "structure" and "identification" as concepts which 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.

## 11 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–cf. §6 above. Under the heading "*"Population" versus "structure"*" Koopmans and Reiersøl (1950, p. 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* (1925, p. 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; Goldberger (1972) has examined the case.

## 12 The econometricians' statisticians 1895-1945

We have watched some ideas passing from statisticians to econometricians. Usually they travelled because the econometricians went and got them on realising, 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 (§7 above) and so, in a larger way, did Yule in his econometric phase (§2 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 some 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 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. Towards 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 programmes.

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 §§7-10 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, p. 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 amongst the econometricians, Yule's venture with a different ending and with Harald Cramér in the role of Pearson; for Wold see the interview with Hendry & Morgan (1994).

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