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THE ET INTERVIEW: PROFESSOR H.O.A. WOLD: 1908–1992

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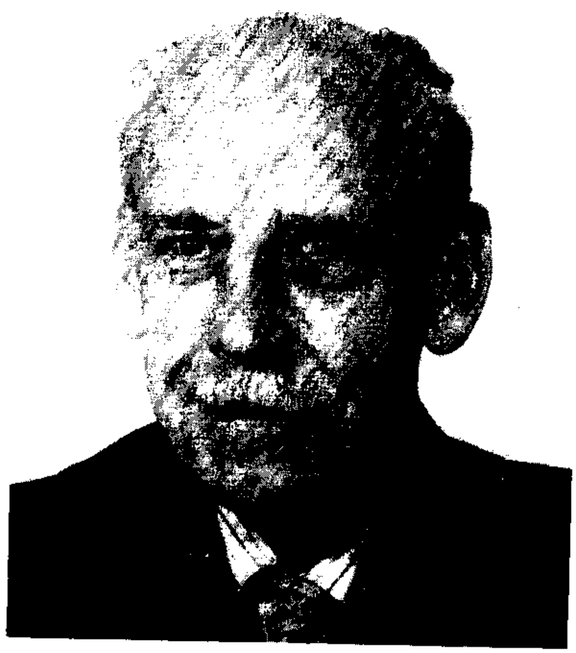

This interview draws on two interviews we held with Professor Wold on September 1, 1980, during the World Congress of the Econometric Society at Aix-en-Provence, assisted by our mutual friend Ted Anderson, and on June 1, 1989, in Uppsala. We have also drawn on Professor Wold's autobiographical piece (1982) to fill one or two gaps that emerged when we put the two interviews together.

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Sadly, Herman Wold died on February 16, 1992, before agreeing to the final version of this interview. We are indebted to his son, Professor Svante Wold, for his kind permission to publish. We hope that this record of our discussions with Herman Wold, who, together with his two great Norwegian compatriots Ragnar Frisch and Trygve Haavelmo, helped lay the statistical foundations of modern econometrics, will contribute to his memory. From our personal perspective, Herman Wold was an enthusiastic supporter of our early incursions into the history of econometrics (see *The History of Econometric Ideas* by Mary Morgan, 1990), and we know that there are many like us who will greatly miss his stimulating contributions.

Herman Wold was born on Christmas day, 1908, at Skien, Norway. His family moved to a small town outside Stockholm in 1912, and he lived in Sweden for the remainder of his life. He enrolled at Stockholm University in 1927 to study physics, mathematics, and economics but switched to studying statistics with Harald Cramér. After his undergraduate degree, he studied the theory of risk with Cramér, then worked for an insurance company for a period, returning to Stockholm University in 1936. His doctoral thesis of 1938, *A Study in the Analysis of Stationary Time Series*, embodies the famous Wold Decomposition theorem. In 1942, he moved to the Chair of Statistics in Uppsala and held that post until 1970, when he went to Gotëborg for five years, finally becoming Professor Emeritus at Uppsala in 1975.

He became a Fellow and later President of the Econometric Society; was Vice-President of the International Statistical Institute; a Foreign Honorary Member of both the American Economic Association and the American Academy of Arts and Sciences; an Honorary Fellow of the Royal Statistical Society; a member of the Swedish Royal Academy of Sciences, serving on the Nobel Prize Committee in Economics from 1968 until 1980; and was the recipient of several honorary doctorates. In retirement, he was Professeur Invité at the University of Geneva until 1980.

Can we start by asking how you came to be interested in statistics?

Yes. I had an excellent teacher in mathematics where I lived, so I had good school mathematics and had very good marks in statistics. We lived in a small town outside of Stockholm, where my father was a furrier and made leather coats lined with fur. When I came to enroll at the University of Stockholm in 1927, I tried to study physics, mathematics, and economics. I had a very good professor in economics, Karl Leman, and then Harald Cramér was also an influence.[1] I was greatly impressed by him and interested in his work. I was really interested just in statistics, and the fact that Cramér was interested in my studying with him were the reasons I started doing statistics.

So it was Cramér who got you intrigued in the subject. What sort of training did he give you in probability and statistics?

We took up elements of risk theory, and that was influenced by Filip Lundberg who had written a book on risk theory. It was a very difficult field. Cramér was interested in it, not as an applied theory that might be used by insurance companies, but as a very mathematical theory on the intellectual aspects of how risk could be studied. This was part of the story that led me to this field.

And what about the actual probability theory? Was it quite usual for statistics courses to start with probability theory and derive everything that way, or start with data descriptions like histograms and means? Was it very formal or somewhat less formal?

It was both sides at the same time.

It was a three-year degree, and you did math and statistics all the way through for three years?

Yes, by the standards of that time it was a very thorough training in probability and statistics.

On graduating in 1930, you went to work for an insurance company. Presumably, this is related to your having studied risk under Cramér.

I also met the Chief Actuary, Filip Lundberg, and he was outstanding. He had two sons, Erik[2] and Ove, and both of them were my friends.

Did you enjoy working in the insurance industry?

It was my job to compile rainfall statistics for Sweden and design a tariff for rainfall insurance.[3] It was not my intent to say in insurance, although I continued to act as a consultant in that field for some years. For example, in 1936 I designed a tariff rate for the local fire insurance companies. What made me decide to return to academic life was that I wanted to do a doctorate and I was greatly interested in statistics.

Just pure intellectual interest in the development of the subject?

Yes. Actually I am interested in music and I remember when I came to Stockholm and heard the Fifth Symphony by Beethoven and I remember thinking this is my life, I must stay in Stockholm. I've played the pianoforte for many years. I was eight years old when we got the first piano, and then later on, when we married, we had a Bechstein piano, which we still have at home.

Your doctoral thesis, *A Study in the Analysis of Stationary Time Series*, was awarded in 1938. How did you decide what topic to study?

Yes, Cramér taught a graduate course in 1933–1934 on stochastic processes which included presentations of Kolmogoroff's work on probability theory and time-series analysis. I was attracted by the field and took up various problems of stationary stochastic processes for my research. Since Harald Cramér was the dominant statistician in Sweden at the time and I had a very high regard for his work, I was pleased to be working with him.

In your doctoral thesis, you pay tribute to earlier work by Udny Yule and Eugen Slutsky. Relative to their work, what was the most important development that you added; for example, the formalization of the problem, the generic role of least squares, sequential conditioning, or the decomposition theorem itself?

The most important thing was the decomposition theorem. I am not sure that Yule and Slutsky, though they had worked on autoregressions and moving averages, actually found the decomposition theory; that was new to me.[4] But the role of least squares, that was important, too, so the most important things were least squares and the decomposition theorem.

They are intimately related in the way you derive the decomposition theorem. In the course of the thesis you make use of some examples of economic data, in particular, two sets of price data due to William Beveridge and Gunnar Myrdal, respectively. But there are also examples from other fields. Were you thinking of your thesis as econometrics at the time you were doing it, or did you think of it as statistics with relevance to economics, or were you thinking of it much more broadly?

My theory did not influence econometrics at the time; this is my understanding now. People were interested, but it was not followed up by other econometricians, so I was known at the time for my work on demand.

Can you remember who examined your thesis?

Yes, it was Ragnar Frisch. (You see, if you look around, there is a picture of Frisch, which was taken later.) He did not like my thesis, so he was very angry at me and tried to find reasons to show that I was wrong. He had a big point and was very fierce, but I insisted on my point and gradually had my way. Actually, the examination went on from ten in the morning til four in the afternoon—a long, long time!

And do you think his disagreements with you in this public defense were based on fundamental views about the role of probability in econometrics, or were there more specific disagreements with you?[5]

My understanding now is that he had a wrong feeling about what happened. It was a mistake for him to think this way, but he did. He disagreed with the theorem, and we had more fundamental disagreements.

We gathered from our reading of the subject that Frisch did not like probability arguments in economics—and your thesis is based very much on probability theories.

That is true.

Was it this that Frisch disliked? Or was it your time-series approach, which differed so much from his changing harmonics idea? Or was it the assumption of stationarity that upset him?

I am no longer sure.

At the time you were doing your research on time series, there was a tremendous amount of work by Ragnar Frisch and Jan Tinbergen on dynamics and business cycles. In Frisch's propagation and impulse article in 1933, random shocks play a part. Did you see your work as connected to that, because in some ways you are spelling out the impulse side of that theory, or did you see your work as a theorem in statistics?

Well, I did mention their work on cycles as examples of random shocks in my thesis, and when Jan Tinbergen made his important papers in 1937–1939, I gave very positive statements in his support. And in my book on demand I refer to Tinbergen's important work. It is difficult for me to say that this is something different; it was closely related.

We suspect your notions of probability were formed during your work on stochastic processes during the 1930s. But in econometrics as such, rather than statistics, drawing that fine division, the role of probability only really becomes important with Haavelmo's monograph which was not published until 1944. Do you see that as a major difference between the econometrics and statistics of the 1930s?

There were major differences between Haavelmo and myself. He started with interdependent systems and used maximum likelihood—which was not good. I used recursive systems and least squares. But in any case, he confused interdependent systems and simultaneous systems. Then in my own work I made a change, so I did not use Haavelmo's method. I changed to interdependent systems and so I got to partial least squares.

You used the term *structure* to apply to the behavior of your stochastic processes, rather than as Frisch did to apply to the theoretical behavioral relations which econometricians sought to uncover. Was yours a different sense of the word than Frisch's?

Yes, Frisch's notion was close to Jöreskog's idea, meaning a sort of a model. But I emphasized the data more and had another notion of structure in the time series which was different from his.

After your doctorate and its publication you began to work on demand.

Yes, the Swedish government commissioned me to work on statistical demand analysis, and clearly a lot of work had been done in econometrics on this topic before the thirties by Moore and Schultz, Allen, and Bowley. But really I felt I was starting down a new track in combining the analysis of family budget data with those of market statistics.

The book wasn't published in English until 1952.[6] Why was it so long between doing the work and its publication? Was it just the war, even though Sweden was a neutral country?

Yes, Lars Juréen, my research assistant on the project with whom I am still in contact, helped me do the applied work and he did a lot of the actual empirical estimation. This we did quite quickly and published the report in Swedish in 1940. The book published in 1952 was more of a textbook in econometrics, and the empirical work plus some fresh material were used as examples.

What do you see as being the main contribution of your work on demand?

Well, it seems to me that the decomposition theorem came through everywhere and that was important; but as I mentioned, it did not influence other economists.

Not yet, at that stage. At the same time as your demand work had been undertaken in the late 1940s, you were obviously developing your ideas on causal chains and the recursive form model. What led you to take up that analysis?

The decomposition theorem was important for causal chains and for recursive forms because these are dynamic models, and when you match them to data, you get a stochastic process. So the decomposition theorem is still the important thing. In this context, the probability theory for stochastic processes was developed in Russia. Here is a picture of Kolmogoroff. I heard him lecture in Poland and he was excellent in this field. So I wrote to him and this was during the war, and he did not reply directly as a safety play. Eventually he replied.

You wrote to us in 1980 and mentioned in your letter that Jim Durbin had invented the term "causal chain versus interdependent system" to replace what you described as your sedate phrase "recursive versus nonrecursive system," during a visit to the LSE in 1951. Were you doing a lot of international traveling in the early fifties?

Jim Durbin was a very good friend of mine, a very nice man. I had excellent contacts with him. I have no special memories of my international visits of those days—it was only later on that I started to go more often. So from 1972 I was in the United States every year.

So that comes a bit later in that story. Can we go back to the famous debate between you and the Cowles Commission about the nature of the economic system to be modeled? There was a meeting of the Econometric Society in Uppsala in 1954, and there is an account of the interchange between you, Haavelmo, and the Cowles econometricians in which none of them seemed to be willing to take up your challenge.[7]

I have excellent memories of the Econometric Society meeting in Uppsala. In the public discussions we did not enter upon such details. Personal discussions were more friendly than the account suggests.

And do you think it was a very fundamental difference between the two groups, the view of recursivity versus simultaneity?

Well, one of the problems was even discussing the idea of causal chain models. You see, the influence of Bertrand Russell had created a climate of opinion in the thirties and into the fifties in which causal terminology was difficult to use. Tjalling Koopmans, for example, was influenced in this way. I did not accept Russell's arguments about causality, but felt that it was useful to talk about causes and causal chains.

Now you attempted to test causal chain models against Cowles Commission simultaneous equation models with applied examples, and published several attempts. How did the tests work out, in general?

It seems to me that Cowles simultaneous equations are not in my class of interdependent systems but were something different.

We also came across a report that you built an 85-equation recursive model for the Econometric Institute.[8]

I remember that the 85-equation recursive model worked very well. But it wasn't published as far as I know.

The next question concerns your article for the *Journal of the Royal Statistical Society* in 1956, in which you do not seem sympathetic to the Neyman-Pearson testing approach, but instead suggest testing models using other data sets, prediction tests, and so on. Why was this? Did you think the econometric profession was on the wrong track here?

Well, it seemed to me that Neyman–Pearson methods were appropriate for experimentally obtained data where you had some control over the variation in the data and knowledge of the properties of your samples. In econometrics, you have observational data and do not know much about its statistical properties.

You also seemed to have little sympathy with maximum likelihood methods, which was R. A. Fisher's approach, and instead you advocated methods that were rather less dependent on distributional assumptions. Why was this?

Yes, I have a picture of Fisher, too; he gave it to me. As I said, with economic data you don't know much about the distributions, so maximum likelihood is not so useful as least squares. R.A. Fisher did something important in ordinary statistics concerned with if you can use one method or if you can use two methods. If you use one method, you get no check on the results, but if you can use two methods and they agree, then this increases their importance very much. But then came the new maximum likelihood method, and he was so influenced by it that he forgot the older methods. It's very curious.

Yes, it is odd that the method of moments was dropped. So you were not arguing against maximum likelihood, but in favor of using several methods at once in tackling problems.

Yes, yes.

Shortly after this exchange you began to work on fix-point methods. Some econometricians would view your fix-point methods, in which you allow the equations to be interdependent in some respects, as being somewhat inconsistent with your causal chain models, in which you argued forcibly for a recursive formulation of the system. How do you see the relationship between the two approaches?

Both methods use least squares. The fix-point method is a recursive least-squares method, and the causal chain model also uses recursive least-squares method. Partial least squares includes fix-point modeling as a special case, so partial least squares and fix-point are interrelated.[9]

Indeed, they are a natural development. So you don't see it as a different direction but just as a development of that direction. Would it be fair to see the least-squares principle as the consistent link through your research contributions in statistics, econometrics, and time series?

Yes, I think it is. I agree with that very much.

And was this conscious—that you felt least squares was right and you kept applying it as you tackled a problem?

Yes, yes, yes . . .

How do you view the more recent developments in time-series econometrics since your work—approaches that base themselves on the decomposition theory, like Box–Jenkins methods, vector autoregressive modeling, the analysis of nonstationarity, and so on?[10]

Well, Box invited me to the United States, so I spent a term there, but it seems to be a different sort of development which is against partial least squares and the direction I took.

We have talked mostly about your research. What about other aspects of your work? Did you get involved in computing?

I never did any computer programming, but I did use computers a great deal and did a substantial amount of computational work. I never relegated this purely to research assistants, and at one time I was one of the main computors. In the thirties and forties we mainly used calculating machines; later it was computers based on programs written by others.

How about teaching?

I very much enjoyed teaching statistics and felt that I had a penchant for doing so.

Concerning your contemporary econometricians, did you see much of Frisch after your thesis defense, or Haavelmo?

I was actually born in Norway and I came to Sweden with my father and my mother, but my two eldest brothers are still in Norway, so I often go to Norway. When Ragnar Frisch got the Nobel prize, I saw him, but that was later. And then there is Haavelmo. I knew him but I have no personal contact with Haavelmo, and it seems that he is a singular person.

And what about researchers at the Cowles Commission, like Tjalling Koopmans or Ted Anderson?

Ted Anderson I know very well and he was over here not a long time ago, so we are great friends. He traveled a lot and sent me the story of his adventures in India, and there was actually an edition of my own memories from there. I was invited by Mahalanobis, in 1949 or some time like that, and my wife also came out after two months. After my work, he invited me to tour India, and so we did and that was very, very nice. Then, the next year, another one was invited and he also received the same invitation, but he decided to fly and unfortunately his aircraft crashed. It was easy for me to remember because people often mixed us up and confused our work. You must know now who it was. It was Abraham Wald.

Yes, that was a tragic accident. In your autobiographical article in *The Making of Statisticians*, you comment that you received the most inspiration from applied statistical work. That seems to be the driving force, rather than economics as such. Do you think that it was an advantage or a disadvantage not to be formally trained as an economist?

My main inclination was to statistics and not to econometrics, so that is still my view. I don't feel at a disadvantage. In Stockholm there is the Academy of Sciences; the members are in different classes and I am in class no. 10, which includes statistics and economics; and as I mentioned, I have other contacts in econometrics.

NOTES

1. Cramér was appointed to a new chair in actuarial science and mathematical statistics at the University of Stockholm in 1929.

2. Erik Lundberg – the Stockholm School economist who pioneered dynamic sequence analysis of the macro-economy.

3. Wold's report formed his licentiate thesis awarded 1933.

4. Udny Yule and Eugen Slutsky had formalized autoregressive and moving average processes, respectively, and Wold extended their work by demonstrating that any stationary time series could be decomposed into a deterministic component and a (possibly infinite but one-sided) moving average of white noise errors.

5. Frisch [2] was not keen on probability analyses in econometrics; see our article in *Oxford Economic Papers* [4].

6. Prior to his move to Uppsala, Wold had commenced his study of *Demand Analysis* for the Swedish Government. Shortly before the outbreak of the Second World War, he assisted in planning for rationing in the event of hostilities in Europe. This was the basis for his book with Lars Juréen.

7. *Demand Analysis* emphasized time-related "causal models" based on economic analysis and estimated by least squares, rather than simultaneous relationships which were becoming the dominant form of analysis in econometrics following Haavelmo's *Econometrica* Supplement in 1944 [3]. Wold noted that in simultaneous systems, least squares does not yield unbiased and efficient estimators, whereas it does in causal chains, which he argued was the more fundamental specification. On this debate and its many ramifications, see Morgan [6].

8. The model was apparently built for clients of the Econometric Institute Inc., an econometric consulting company set up by Charles Roos.

9. The fix-point method is discussed in his text *Interdependent Systems* with J. Mosbaek in 1970. This is a special case of partial least squares which Wold advocated as applicable to models with latent variables, to nonlinear settings, and to analyses where theoretical, or a priori, knowledge was scarce.

10. See *Prediction and Regulation* by his student Peter Whittle [7] and many of the papers in *Long-Run Economic Relationships* by Rob Engle and Clive Granger [1].

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