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THE ET INTERVIEW: PROFESSOR OLAV REIERSØL

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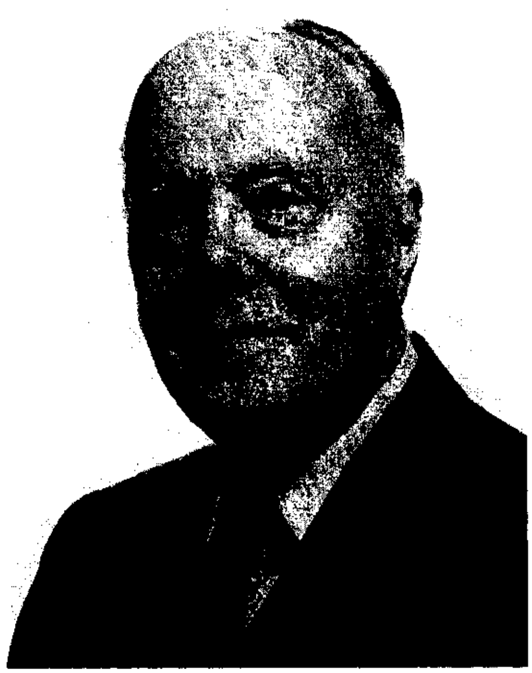

Olav Reiersøl

Professor Olav Reiersøl was born on 28 June 1908 in the countryside in Norway. He enrolled at Oslo University in 1928, where he studied chemistry, mathematics, physics, and botany. Choosing mathematics as his main subject, he received the cand. real. degree in 1935. As a graduate in mathematics and mathematical statistics he was employed as an assistant by Professor Ragnar Frisch.

During the Second World War Norway was occupied by the Germans in April 1940. Later on, in November 1943, the university was closed down. In this action the Germans arrested many university teachers and all the students they got hold of. Reiersøl, who had been active in the resistance, then fled to Sweden (in December 1943) and stayed there until June 1945. In the years 1946–1949 he spent most of the time abroad, mainly in England and the United States.

From 1949 he has been permanently attached to Oslo University. He was appointed docent in mathematics in 1950 and in 1958 professor of mathematics. In 1961 he was appointed professor of mathematical statistics, a position in which he served until his retirement in 1975. With this background in econometrics, mathematics, and mathematical statistics it is quite natural that Reiersøl in his scientific writings has touched many different areas. His works include the following: identifiability of structural models in econometrics and factor analysis, general estimation by means of instrumental variables, design of experiments, logit analysis, multiple comparisons, computational methods for matrices and stochastic processes, statistical problems in genetics, and the history of probability. Reiersøl has made several substantial contributions to econometrics and statistics. His works on identifiability and instrumental variables are standard references both in econometrics and statistics. The work on genetic algebras has been reprinted in a collection of benchmarks in genetics [i]. So undoubtedly, Reiersøl has made several fine contributions to mathematical statistics. Professor Reiersøl, Ragnar Frisch, Trygve Haavelmo, and Herman Wold made up the strong Scandinavian group whose contributions definitely helped lay the statistical foundation of modern econometrics. Therefore, I think it is Reiersøl's works on econometrics that are most interesting to the econometric society. Thus in the interview that follows I will concentrate on that part of his scientific work.

I met Reiersøl in autumn 1974. I remember clearly my first glimpse of him at an underground tram station: an unexpectedly stout, ruddy, and shy figure, almost fisherman-like, with a rucksack and an old woollen hat. I had then heard quite a lot about him during my stay in Uppsala and Gøteborg with Herman Wold. Wold always spoke of him with approval and respect. The stay with Wold had aroused my interests in soft modeling so I already knew his papers on identifiability quite well. Knowing, therefore, that Reiersøl was a prominent figure on latent models I quickly got in touch with him. Since then he has been extremely generous in the amount of time he has spent on answering questions, reading, and commenting on my papers. Therefore, it is a pleasure for me to conduct the present interview.

In order to get started, can you tell us about your educational background?

I attended the local primary school, where my father was the teacher. This was a small school with only two classes, so the pupils belonging to the same group had quite different ages. I studied books on algebra, geometry which I found at home. Another interest was in botany and the identification of flowers. I read with great interest an old Danish book on astronomy. From this and other books on astronomy I got an idea of the seemingly strange movements of the planets. On dark nights I could observe how the constellations changed positions depending on the rotation of the earth and the different seasons.

I was interested in science. In those days the education in science was quite comprehensive, and we had to study several topics. I studied botany, chemistry, mathematics, and physics at the University of Oslo. Then I chose mathematics as my main subject and decided, if possible, to specialize in probability theory and mathematical statistics. My knowledge of these subjects was at that time quite limited. I had read the greater part of E. Czuber's *Wahrscheinlichkeitsrechnung und ihre Anwendung auf Fehlerausgleichung, Statistik und Lebensversicherung* [iii]. I had also tried to read some of the articles appearing in *Biometrika*, but I quickly realized that I didn't have the prerequisite knowledge to understand them fully. When I was a student, there was no chair in mathematical statistics at the University of Oslo. Alf Guldberg, a professor of mathematics, gave lectures in mathematical statistics for students in actuarial science and lectures in the calculus of probability for students in economics. When I got in touch with Professor Guldberg he recommended R. von Mises's book *Wahrscheinlichkeitsrechnung und ihre Anwendung in der Statistik und theoretischen Physik* [x]. I also read Steffensen's book *Matematisk iagttagelseslære* [xii]. I finished with the cand. real. dissertation *Undersøkelser over momenter, halvinvarianter og deres frembringende funksjoner* (A study on moments, semi-invariants, and their generating functions) in 1935.

I guess Frisch was quite active at that time. Can you tell us about your contact with Frisch and his institute?

When I started my studies in mathematical statistics, I didn't know that Frisch was working in this field. But in the course of my studies I became familiar with his "Correlation and Scatter" [v]. In this paper Frisch makes extensive use of matrix theory, and I think this is one of the earliest applications of matrices in statistics. However, I hadn't learned anything about matrices in my studies of mathematics, so I had to read up this subject during my summer holidays in 1935.

Frisch and Wederwang were running an Economic Institute which was not part of the university but was financed by the Rockefeller Foundation and Norsk Varekrigs-forsikrings Fond. Early in 1936 I was employed by Frisch as his assistant. In his work on developing new statistical methods, Frisch worked partly with real and partly with artificial data. In order to help him in carrying out the numerous numerical calculations he had a group of young assistants. When I joined him, cand. oecon. Trygve Haavelmo was the supervisor of this group. Later on, Haavelmo left the institute, and I took over this position.

In January 1938 I was appointed lecturer in theoretical statistics for students in economics. This was a newly created lectureship at the university. From 1938 until 1945 I was the only teacher in statistics for students in economics. Occasionally, Frisch himself gave lectures on special topics. He lectured on time series analysis and later on regression theory and confluence analysis.

Frisch's book on confluence analysis [vi] appears in one way or another to be the origin of most of your works in econometrics. In modern parlance this book deals with latent variable models. Can you tell us why you think these models are important to study in econometrics?

Well, in econometrics as in any science for that matter, we wish to deduce stable relations. In order to be specific I think it is best to consider a simple example. Let $X_1, \ldots, X_n$ denote the observable variables while $Y_1, \ldots, Y_n$ denote the corresponding latent variables. Then consider the standard latent variable model given by the structural equation

$$Y_1 = \alpha_{10} + \alpha_{12} Y_2 + \ldots + \alpha_{1n} Y_n + U \quad (1)$$

and the measurement equations

$$X_i = Y_i + \varepsilon_i \quad (2)$$

where U is a random disturbance and ε_i are measurement errors.

If the Y's and the ε's are independent and both are multivariate normal, then the X's are also normal. Hence, if we take the regression of X_1 on $X_2, \ldots, X_n$, the regression equation

$$X_1^P = \beta_{10} + \beta_{12} X_2 + \cdots + \beta_{1n} X_n \quad (3)$$

gives the expectation of X_1 for given values of $X_2, \ldots, X_n$ and can be used to predict X_1. Similarly we can regress Y_1 on $Y_2, \ldots, Y_n$ to obtain the regression equation

$$Y_1^P = \alpha_{10} + \alpha_{12} Y_2 + \cdots + \alpha_{1n} Y_n. \quad (4)$$

However, we can question the use of (4) since this is a regression equation in unobservable variables.

The point is that we may be interested in (4) because it is more than a regression equation; it can be a structural equation. By a structural equation I mean an equation which, compared to a regression equation, has a greater stability. Here I don't think of the stability of the estimators in repeated samples but of the stability with respect to changes in the model. The use of equation (3) for making predictions presupposes that the joint distribution of the X's doesn't change. However, equation (4) can still be valid although the distribution of observable variables changes. We could say that equation (1) or (4) has a higher degree of autonomy.

In addition we can also be interested in equation (4) although we only consider it to be a regression equation. Even in this case it will be more stable than equation (3) since it is independent of the distribution of the measurement errors $\varepsilon_1, \ldots, \varepsilon_n$, while the regression equation (3) will depend on the variances of $\varepsilon_1, \ldots, \varepsilon_n$. I assume of course, that these errors are independent.

Again it is interesting to illustrate by an example. Moran [xi] considers a model with two X's and two Y's. In Moran's study Y_1 is the total annual stream flow of the river Kiewa, which passes the town Kiewa (in Victoria, Australia). The term Y_2 is the total annual precipitation in the catchment area. The variable X_2 is the annual precipitation measured at Bright, a town just outside the Kiewa catchment. If, instead of X_2, we had used a variable X_2^* based on the precipitation from different places within the Kiewa catchment, then we might obtain a smaller value of ε_2 and, therefore, another value of β_{12}.

An interesting result in Frisch's confluence analysis is that although the vector of structural parameters is not identifiable, one can under certain conditions deduce bounds on this vector by ordinary least square regressions. For the bivariate case the theorem is stated in Frisch [vi, p. 60], although the original result appears to go back to Gini [iv]. Nowadays, econometricians often refer to Kalman [vii] and Klepper and Leamer [viii] for the extension of this result to the multivariable case. Can you tell us about the history of this result?

Well, Gini wrote in Italian so perhaps Frisch didn't know about Gini's result. The first extension of this result to the multivariate case is given by Koopmans [ix, p. 101]. That the adjoint of the covariance matrix was positive or could be made positive by proper sign changes of the variables turned out to be an important condition for the validity of this result. In a way Koopmans proved the necessity part of this condition, and then I showed that it also was sufficient. The complete formulation and proof of this result are given in theorem 14 in my *Econometrica* article from 1941 [2].

So Koopmans is an important contributor in this connection. When did you first meet Koopmans?

Koopmans came to Oslo in autumn 1935 to study Frisch's confluence analysis. On the request of Frisch, Koopmans then gave a series of lectures on R.A. Fisher's theory of estimation and Neyman–Pearson's theory of testing hypotheses. I attended these lectures. Later on I studied the original works by Fisher, Neyman, and Pearson. That was a great experience, indeed.

You worked on your doctoral thesis during the Second World War when Norway was occupied by the Germans. I guess it was difficult to do scientific work under such circumstances. Can you tell us about your life and work in that period?

Yes, that was difficult times, indeed. In August 1941 the Germans started to confiscate people's radios. By that time we already had some illegal newspapers. But after this confiscation of radios several new illegal newspapers appeared. One of these was *Avantgarden*, to which I was connected. Together with a friend I was listening to news from London and Moscow (my friend was quite fluent in both Polish and Russian), which was afterwards published in this newspaper. I also did some work with distributing *Avantgarden*. In October 1943 two of my illegal contacts were arrested, and I had to go into hiding with one of my cousins living outside Oslo. In November that year the Germans went into the university, arrested students and teachers, and closed the university. After this event I decided to flee to Sweden. Equipped with a false identity card and fake papers I traveled by train and a lorry to Trysil (a place not far from the Swedish border). From Trysil I went on skis to Sweden.

Having arrived in Sweden, I went to Stockholm and was lucky enough to get a room at Cramér's institute for actuarial mathematics and mathematical statistics. I had almost completed my doctoral thesis, *Confluence Analysis by Means of Instrumental Variables*, when I left Oslo in December 1943. The manuscript was typed at an office run by the Students' Association. When I had to flee to Sweden the typewriting was only half finished, so I managed to bring with me only the first part of my dissertation. Later on, someone working at the Economic Institute delivered the typed second half of this manuscript to the Norwegian editor of the journal *Skandinavisk Aktuarietidsskrift.* He in turn sent it to professor C.O. Segerdahl, the Swedish editor of this journal. In this way I managed to get the complete dissertation to Sweden, and in May 1945 I was awarded the doctorate from Stockholm University.

Staying with Cramér in Stockholm I also remember that I attended his lectures on measure and integration theory. Cramér was then writing his book *Mathematical Methods of Statistics* [ii]. When I returned to Norway that year I had a copy of the first part of this book. The book was published in Uppsala in the autumn of that year.

Your doctoral thesis was an extension of Frisch's confluence analysis?

Yes, there I introduced the idea of an instrumental variable to the confluence analysis. Herman Wold was the "first opponent." I remember that there was a summary of the thesis in a Stockholm newspaper saying that it dealt with breeding of pigs. You see, on the last two pages I illustrated some of my methods by applying them on some Danish pig production data.

So the newspapers then were as reliable as they are today?

I think you can say that.

The method of instrumental variables has a place of prominence in modern econometrics and statistics. You are considered to be its originator. Can you tell us how this idea developed?

Well, a random variable is said to be an instrumental variable relative to a stochastic equation if it is uncorrelated with the disturbance of the equation and uncorrelated with possible errors of observation of the variables occurring in the equation, while it is correlated with some of or all the true values of the variables in the equation. This idea emerged during my work on deriving consistent estimators of the slope parameters in some models in the confluence analysis. There

I saw quickly that with the usual assumptions on the random errors, one could deduce consistent estimators by using lagged values of the appropriate observable variables. The idea is used in my *Econometrica* article from 1941 [2]. Originally I tried several different terms for this variable, but Frisch was not satisfied with any of my proposals. He thought that this was a good idea and should have a name that matched its importance. One day he told me enthusiastically that he had found the proper name and this variable should be termed *Instrumental variable*! I myself used this term for the first time in my doctoral thesis in 1945.

Can you recall using the idea of instrumental variables in other applications?

I think I have touched upon it several times, e.g., in [2] and [5]. In [5] I showed that the well-known Frisch–Waugh theorem easily generalizes to the instrumental variable case.

When you returned from Sweden you resumed your work as a lecturer in statistics for students in economics. But already the next year you went abroad again. Can you tell us about your travels abroad?

Yes, in 1946 I received a three months' scholarship which I spent in Cambridge, England. There I got a small office at the Department of Genetics, of which R.A. Fisher was then the director. Most of the time I spent studying some of Fisher's works, among them his papers in genetics. In the office in which I had my desk, Fisher also kept a huge pile of reprints. In this collection I happened to see some articles on genetic algebras by Etherington. These papers were interesting, but it wasn't until 1958 that I myself started to study seriously these algebras.

After Cambridge I got a scholarship from the Rockefeller Foundation which made it possible for me to go to the United States. So, the autumn of 1946 I spent at Columbia University. There I followed lectures on design of experiments given by A. Wald and lectures on multivariate analysis by T.W. Anderson. The following spring I spent at the University of North Carolina attending lectures on probability theory by H. Robbins and on multivariate analysis by the Chinese P.L. Hsu. This summer (1947) I stayed in Chicago. Here I followed lectures on identification and estimation of systems of stochastic difference equations, and there I also participated in the weekly Cowles Commission Discussion Meetings. I left Chicago for Berkeley in January 1948. In Berkeley I participated in the weekly seminars but didn't follow any lectures. For the academic year 1948–1949 I was visiting assistant professor at Purdue University after being recommended by Neyman for this position. There I gave an introductory course in mathematical statistics and a course in difference calculus. I also gave two seminars, one on a book by Wilks and one on Thurstone's factor analysis.

In 1949 I went back to Norway and did not revisit the States until 1964. In the academic year 1964–1965 I was visiting professor at Cornell University.

During your first stay in the United States you wrote three influential papers on the identifiability of structural models [7–9]. Can you tell us how these papers came into being?

The first one I wrote together with Koopmans. I already knew Koopmans from his stay in Oslo, and collaborating with him on identification problems was quite natural. Koopmans had worked on the identifiability of econometric models in the Cowles Commission while I had studied the identifiability of factor analysis models. Anyway the paper was published in *Annals of Mathematical Statistics.*

Tell us about your interests in factor analysis.

Although I am no psychologist I have some interest in psychology. When I read Thurstone's books [xiii, xiv] on factor analysis, I realized, immediately, the similarities between factor analysis models and models in Frisch's confluence analysis. There is also a formal resemblance between the reduced form of an econometric model and the structural form of a factor analysis model. Although, it should be kept in mind that the exogenous variables are observable in the econometric model while the factors, as well as the number of factors, are unobservable in the factor model. However it was clear that the methods deduced by the Cowles Commission to study identification of the econometric model should have a certain analogy to methods appropriate for the factor analysis. On one occasion Koopmans asked me to study the identifiability of the parameters in Thurstone's multiple factor analysis from the point of view of this analogy.

I met Thurstone in Chicago (1947) and later at a meeting in Oslo. Thurstone had Swedish parents, and Thurstone is an anglicization of Torsten, which is a very common Scandinavian first name.

Now you have told us about two of four works from this period. However, the paper "Identifiability of a Linear Relation between Variables Which Are Subject to Error" [9] is possibly the one which is the best known. Can you tell us about its emergence?

In those days a lot of work was going on in deriving consistent estimators of the regression parameters in latent variable models. Since identifiability is a necessary condition for estimability it was quite natural to study the identifiability of these parameters. Using the general definition of identifiability I approached this problem through the application of characteristic functions. The theory of characteristic functions I knew quite well from my education in mathematics and from the books by von Mises [x] and Cramér [ii].

Finally, I know that you have a lifelong interest in Esperanto. Can you tell us about it?

As a young boy I happened to find a book on Esperanto at home. Nobody there seemed to know anything about Esperanto, but this little book aroused my interest in this language. When I later on went to Oslo to attend schools, I quickly became a regular user of the public library. There I found textbooks and other written material on Esperanto, but still I didn't know anyone speaking this language. Nor did I know there was an international Esperanto Association which had a local society in Oslo.

However, in autumn 1932 a German lecturer in Esperanto came to Oslo to give an Esperanto course. I attended her lectures, and afterwards I managed to read Esperanto quite easily with a dictionary at hand. The Oslo Esperanto society regularly arranged meetings/lectures which offered good opportunities for me to practice the language. The Second World War restrained the activities of the Esperanto movement in Norway, but in Sweden the war had the opposite effect. When I arrived in 1944 the Esperanto movement there was very active. Some refugees from Central Europe staying in Sweden spoke Esperanto more easily than Swedish. I learned then that Esperanto was a good second language for people having very different native languages.

When, after the war, I went to England and later on to the States, I always took the opportunity to visit the local Esperanto societies. In 1950 I participated for the first time in "Universala Kongreso," which is held every year. That year the meeting took place in Paris. For me it was a great experience to witness that thousands of delegates coming from almost every country in the world were able to speak to each other and to understand lectures given in one language without any need for translations. In 1952 the Universala Kongreso was arranged in Oslo. I participated in the "Loka Kongresa Komitato" organizing the congress. The yearly "Universalaj Kongresoj" is also a good occasion to meet and to discuss professional questions with colleagues.

I was one of the founders of the international Esperanto association for mathematicians, "Internacia Asocio de Esperantistaj Matematikistoj." In 1976 I gave lectures on probability theory in Esperanto at a seminar in Antwerp, "Someraj Universitataj Kursoj." I have corresponded with statisticians and mathematicians from all over the world, in particular on the compilation of statistical and mathematical terminology in Esperanto. But I have also been interested in Esperanto from a linguistic point of view. I have worked on the use of the verbal forms in Esperanto and with criteria for language assessment of interlinguistics. Together with Professor Richard E. Wood I have published, in English, an article on the use of suffixes in English and Esperanto.

In 1951 I published an article, "Transiro per diferencialaj ekvacioj de probablodenso al karakteriza funkcio kaj inverse," in the Portuguese journal *Portugaliae Mathematica* [11]. After the publication I sent a reprint of this paper to a colleague in the States. He responded with kind words and remarked that he didn't know that it was so easy to read Portuguese!

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