

Tilburg University

The ET interview

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Published in:
Econometric Theory

Publication date:
1987

[Link to publication in Tilburg University Research Portal](#)

Citation for published version (APA):
Magnus, J. R., & Morgan, M. S. (1987). The ET interview: Professor J. Tinbergen. *Econometric Theory*, 3(1), 117-142.

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THE ET INTERVIEW: PROFESSOR J. TINBERGEN

Interviewed by:
Jan R. Magnus and Mary S. Morgan



Jan Tinbergen (ca. 1985).

Jan Tinbergen is one of the founding fathers of econometrics, publishing in the field from 1927 until the early 1950s. This was the frontier age of econometrics when the distinction between mathematical economics and econometrics, let alone between theoretical and applied econometrics, did not yet exist. Tinbergen's approach to economics has always been a practical one. This was highly appropriate for the new field of econometrics, and enabled him to make important contributions to conceptual and theoretical issues, but always in the context of a relevant economic problem. The development of the first macroeconomic models, the solution of the identification problem, and the understanding of dynamic models are perhaps his three most important legacies to econometrics. Tinbergen was

awarded the first Nobel Memorial Prize in economics in 1969 (jointly with Ragnar Frisch) for his contributions to econometrics.

Tinbergen's desire to communicate his ideas to others is matched by a talent for clear and direct writing. This gives his econometric work great appeal and an apparent simplicity which should not be underestimated. This talent was also fruitfully applied to the development of pedagogical tools for teaching econometrics to his students.

Since the early 1950s Tinbergen's interests have moved on and he has made notable contributions to such diverse fields as the theory of economic policy, development planning, and income distribution. Tinbergen's political and pacifist views have always been an important element in his economics, and even, as this interview shows, his econometrics. His overriding aim has been to improve the welfare of the less fortunate in this world.

It is now 60 years since Tinbergen's first article in economics appeared, yet he shows no signs of retiring. We met him on May 27, 1986, in the study of his house in The Hague, where he has lived for most of his working life and which bears the hallmarks of continued study and writing. Most of the discussion during the afternoon concerned his econometric work published in the 1930s and 1940s. He gave us his views of those early developments—both what he thought then and how he sees them now. What follows is an edited transcript of the conversation. We hope that this interview will bring alive to the readers of the 1980s the issues and difficulties faced by econometricians in the 1930s, as well as Tinbergen's characteristic response to those problems. One of Tinbergen's attributes is a considerable modesty about his own achievements; the reader should bear this in mind when reading his remarks.

Over the last sixty years you have been one of the most prolific, diverse, and profound writers in the economics profession. In 1921, when you started your studies in physics at the University of Leiden, what were your ideals and hopes for the future?

They were somewhat queer. I wanted to finish quickly and then to switch to economics. Now you can ask why I chose physics at all, but it was clearly my real interest. I liked physics and mathematics very much and I also felt that that was the thing I was perhaps strongest at, but at the same time I had already come to the conviction that I could probably be more useful to society by being an economist. I think most of the bigger political discussions were economic rather than physical, and so I hoped to get sufficient capability to handle things with mathematics and perhaps take physics as an example of a more developed science than economics.

So in 1929, when you received your doctorate in physics, see [42], your interest had already shifted from physics to economics. Could you tell us how you became interested in economics?

I was interested in the problem of unemployment, the problem of poverty generally, and being a socialist and a member of the Socialist Party, I felt that I

could be more useful as an economist than as a physicist. I discussed this with a few of the leading people in the party and they agreed. My interest in economics was not primarily scientific; it was typically social.

Was there a tradition at Leiden University in mathematical statistics from which you benefitted?

Not in the least. Economics was just a part of the Law Faculty and was hardly given any attention, and statistics was not taught at all. There was no actuarial statistics either. In Rotterdam, of course, there was, and that was the way I came into Rotterdam later as a lecturer.

How then did you learn mathematical statistics?

I have never been very strong at it, you see, and I didn't like it much either. I used it as a tool and I tried to know the most important things, but I made almost elementary mistakes. Haavelmo pointed out that estimating a system of equations by least squares for each equation separately is mistaken. That already illustrates my relatively weak interest in mathematical statistical questions.

You are one of the founding fathers of econometrics. Nowadays the field of econometrics is well defined, and young economists who go into that field know what is in store for them. But in the 1920s, when you started working in econometrics, it was a very new field although there had been some pioneering work, particularly on the demand for agricultural goods in the U.S.A. Were you aware of and influenced by the existing econometrics literature and what did you consider to be the major problems?

I certainly was interested and also influenced. I knew, of course, the work done on agricultural problems. At the time of the great depression, a lot of work started, especially in the United States, on the agricultural markets. They seemed the best examples for estimating, for instance, a demand curve because there were large changes in supply, and usually these supply changes were determined by natural factors and therefore independent of demand. So that was one starting point.

My first job was at the Central Bureau of Statistics and I stayed there for more than ten years. I was responsible for the periodical *De Nederlandsche Conjunctuur*. It started in 1929, but the War made an end to it. My official responsibility as editor left me quite some freedom. The man supervising my work was in charge of building up something like the London and Cambridge Economic Service, concerned with the cyclical prospects of the Dutch economy. In this periodical you will find a few indicators that were used at that time, using the so-called barometer based on the A-B-C curves. Now that was all very much crudely empirical. It was Wesley Clair Mitchell who started that, if I remember correctly, and my feeling was that there was

something lacking in between.¹ On the one hand, there was mathematical economics using demand curves, supply curves, and a few production functions, and on the other, there was this purely empirical idea of forecasting the cyclical position with the aid of these so-called barometers. So you find that first I tried to expand the techniques of finding demand curves, and then also to think about the coherence of these various relationships. Later on, of course, that brought me to the idea of models. But models at that time were an unknown matter.

Your earliest work was on the problem of economic dynamics. One of your important contributions in this field was your 1930 paper analyzing the structure of the potato flour industry in the *Zeitschrift für Nationalökonomie* [11]. This paper introduced a full treatment of the cobweb model. How did you arrive at this model formulation?

Actually, the man who deserves credit here is Dr. Hanau, who at that time was working at the Institut für Konjunkturforschung in Berlin. He was the first to study more carefully the pork market and he actually found what you could call the skeleton of the forces at work.² In a more popular journal of the Socialist Party I wrote an essay about market forces and other things [9], and I came at this possibility of getting cycles as a consequence of a lag, in this case a supply lag. When I was reading the proofs of that article, the study of Hanau appeared and so you will find a footnote that at the moment of correcting the proofs I found a real example of something that I had supposed to be possible in that article. So it was a simultaneous discovery, I from the theoretical side, and Hanau from the practical and empirical side.

Of course, it was slightly more complicated in that it was not only the price of pork that played a role but also the price of various grains that were eaten by the hogs. Nevertheless, the main features of the model were determined by the supply lag. That was all very primitive as you will have seen. The attractive thing in the case of potato flour was that so many details were known that are not generally known. In this case it was an industry where the number of enterprises was perhaps no more than twenty or so, probably less. Only about five or so were really important and one knew something about the costs. So there was, at the same time, the possibility of trying out this Cournot idea where you had not a monopolistic but an oligopolistic market. I think the available data permitted something to be done that one would always have liked to do.

This same 1930 paper has remained little known, yet it also contained the first formal discussion of the conditions necessary to identify both demand and supply parameters in a two-equation model. I am thinking of the early part of that paper where you deal with a system of simultaneous equations and you do what every mathematician would do, namely, to find the reduced form and solve for the unknowns. How did you set

about doing this, since nobody had dealt with the identification problem in this way before?

Is that correct? I wonder whether or not among the agricultural economists in the United States there might have been people who had done so as well. Henry Schultz was the most theoretically oriented among them. It could very well have been that the idea had grown up naturally. After all, we were teaching that the prices and the quantities bought were determined by the demand and the supply functions, so it was quite natural to try to find out whether that worked in fact. But then, of course, you needed the data on what you would call the co-factors: the supply factor which, together with the price, determines supply and the demand factor which, together with the price, explains the quantity demanded. Then a bit later the estimation of a system of equations became a well-known problem, and the finest and simplest example was to take just one market and show how it worked. So it was also a question that could very easily be used in my courses to explain the operation of the market to the students, and it was also nice to have something as a complete illustration. I think it was a natural consequence of the way we talked about supply and demand determining the price and quantity sold.

I may add something else here. My most influential teacher in Leiden was Paul Ehrenfest and I think the main characteristic of his teaching was that he always tried to find the simplest case in which something occurred, the simplest case to explain a certain concept. This idea has been very important for me and in this case, with one market, it could be applied very nicely.

You already suggested that when you formulated the identification problem mathematically and worked out the solution, you weren't really aware that the statistical estimation of the system was a separate issue which Haavelmo addressed later on in the 1940s.³

Yes, I had not the slightest idea of that. As soon as he said so it was clear to me, but I had not myself come to the idea; after all, this happened during the War and so I only heard about it much later. But he was, of course, perfectly right. Interestingly enough, Herman Wold has tried to defend the method by which each equation would be estimated by ordinary least squares.⁴ I think that was very kind of him, but it did not really apply. It did apply in the cases that he defined and that was very interesting, but I felt I had to admit simply to Haavelmo and the Cowles Commission that they were right.

I think we have learnt a lot, especially from the people in the Cowles Commission who developed the ideas of identification, and also hit upon the possibility to estimate by least squares if you first solve the two equations for their unknowns. I think that in my teaching, I developed standard terms and I did define not only a demand equation and a supply equation but also, after the solution, the price equation and the quantity sold equation.

Reading through another of your early papers, the one on shipbuilding [13], it's interesting to see that you distinguished very clearly between factors which are endogenous and factors which are exogenous to that cycle, even though the exogenous factors might be the general trade cycle. Why did you think that distinction was important?

Well, first of all it appeared again that very good data were available. We were, of course, very much dependent on the availability of data, and the shipbuilding world is statistically well taken care of, both in England and in Holland. Then also it appeared that if we took moving averages of three years or so, the correlations were very high, so we got the impression that we had the most important factors already in our set. If you see the original Dutch article [12], you'll find that indeed we showed these as an additional proof that it looked like something fairly solid.

Also, it was clearer perhaps even than in the pork cycle, that there should be such external factors, and we tried out some that eventually proved not to be so very important. But it was quite clear in the shipbuilding case that there had to be some external factors. This was again a good example to show a somewhat complicated market, actually two markets: the market for freight and the market for the ships themselves [16].

In 1936 you published the first macroeconomic model (now known as the Dutch model) as part of your analysis of the economic policy problems facing Holland in that year.⁵ Were there political motivations for you to undertake this task?

The immediate reason why I did this was that I was invited by the Dutch Economic Association to discuss the policy problem at their annual meeting. It was understandable—since The Netherlands were in a very bad economic situation because they had stuck to the gold standard—that a question like this was chosen for discussion.

At the same time, being a member of the Socialist Party, I also cooperated with something called the "Labour Plan." That was a proposal for an alternative policy package. In the discussions I had there with my friends, a number of instruments were suggested for a better policy and so the two things could very much support each other. In the article I tried five or six policies and these were simply a reflection of the daily discussions in newspapers and among politicians.

One particular characteristic of that period was that an association was established called "The Association for Stable Money." They were in favor of a devaluation of the guilder and opposed the government policy. Most of my colleagues—I was at the time the younger colleague of a number of economists in Rotterdam—were members of this association except one or two who were immediately linked with the government, and that's why I also considered devaluation as one of the possible policies.

The Dutch Labour Party was somewhat more interested in a sort of Keynesian approach—they wanted to spend a substantial amount on public works—and that's why you find this as another of the possible options. And then, of course, there was the usual discussion about wages: should the wages be lowered further and would that help, so that is also reflected in the choice of the various alternatives that I studied. Certainly my choices were partly determined by politics.

Was the Dutch government influenced by your findings, which were mainly policy oriented?

I don't think there was much influence on the Dutch government. I was a young man at that time. There may have been some effect on my fellow economists but certainly not on politicians. Moreover, we were among the three countries that stayed on the gold standard, because, as the then Prime Minister said, devaluation was like breaking a contract. So the political discussion was very much determined by ethics and not so much by economic considerations. In fact, Britain was the first to devalue, I think, already in September 1931, and there the depression was only very slight, so it was quite clear that Britain had chosen the better way.

You said later, in the introduction to your League of Nations work [3], that your macroeconometrics was a splicing of the Swedish development of "sequence analysis" and statistical economics.

Sequence analysis was nothing but dynamic economics, that is economics using relationships with time differences one way or another. I followed Frisch's definition of dynamics which was more precise than the loose way in which the word dynamics was used at that time for anything that moved. Frisch pointed out very clearly that you could also have movement in static systems that were influenced by some external factor.⁶ I followed the Swedish policy because they were rather successful in getting rid of the depression somewhat earlier than we were in Holland and also because in the Scandinavian countries the Socialists had more influence. It was natural that in Socialist circles we were much interested in what was done there. At the same time, Swedish economists had a very good name, of course. So it was more or less natural, I think, to give attention to what was being done in Scandinavia.

The Dutch model also contained a type of Phillips curve relationship. How did you arrive at that?

It seemed to me that it was quite a natural thing to expect that wages would move under the pressure of employment or unemployment, and, of course, that led typically to a non-static approach. In statics you would expect wages to be determined by prices and some other factors, but in this particular case it was not so much the level of wages as the movement of wages that was

determined by the situation in the labour market. Quite a few people thought of that possibility independently in those days.

How would you say that your work on macroeconometrics in the 1930s was related to that of other economists and econometricians active at the same time?

Our way of thinking was very much influenced by the Econometric Society which had just been created in Europe in 1931, and I think a year before in the United States. Our European meetings were very pleasant because there were only about thirty people participating, so you could actually have profound discussions. Frisch was the leading man there; he was automatically recognized as such. There were many interesting people at that time, for instance, Hans Staehle and Jacob Marschak, who were then still in Europe, and it was at these meetings that I think most progress was made.

All this had started in 1931 and it was only in 1936 or 1937 that I came with the Dutch model, and since that was written in Dutch it couldn't influence the others. But my being invited to Geneva to continue Haberler's work was, of course, a big impulse to me. That was in 1936 and then, by coincidence, I got an invitation to lecture in Sweden. I visited Sweden for the first time in 1937 and there was a whole group of very interested people. The discussions in Sweden with Ohlin, Myrdal, and some others were very stimulating.

In 1936 you moved to Geneva to work for the League of Nations to undertake, at their request, the statistical testing of the business cycle theories surveyed in Haberler's book *Prosperity and Depression*.⁷ Your work and results were published in two reports in 1939 [3]—the first a study of investment and the second a macroeconomic model of the U.S.A. Would you agree that the idea of a model was more clearly developed in your work for the League of Nations than it had been before?

The whole idea of a model fell somewhat from the air, but the concept of a model came to play a rather important role. Haberler had discussed various authors one after the other and each of them had presented something that you could call a model. But upon inspection they usually appeared not to be complete. If you had tried to express them in a set of equations you would have found that there were not enough equations.

I think the best way of introducing a model is to start out by taking just one variable, say the price level, and ask yourself how it has to be explained. You may write down an equation which indicates what factors determine the fluctuations in prices. Then, of course, some of these factors themselves, say the national income, have to be explained also in order to understand the phenomenon as a whole. And so you add an equation, and you go on until you have a system where there are as many equations as unknowns. That could be a clarification of how the idea of a model comes in almost by necessity.

The idea of models was lacking in Haberler's book and had to be added by the men responsible for the testing part. I had some difficulty in trying to

persuade Mr. Loveday, who was the Chief of the division where I worked at the League of Nations Secretariat, of this. It was not so easy for him to judge whether this was serious work and whether this was the way to do it. So he sent me to England to discuss my work with Professor Henderson, who at that time was a well-known man, and also with Dennis Robertson, whom I knew already as a very good friend; interestingly enough, not with Keynes. In a sense the idea of a model developed in my work for the League of Nations and I did present a model for the United States. But at the time I did this work, I saw the concept of models with less clarity than I now see it; nor did I realize how absolutely necessary they were.

There seem to be two things here: first, as you just outlined, the need to make complete and consistent models, as opposed to the verbal ones which were mostly incomplete; and secondly, the need to design models that could then be applied to data. I think that Frisch in his "Propagation and Impulse" paper (1933) was mostly concerned with the latter, which was why he brought in the concept of shocks to the system.⁸ You discussed Frisch's paper in your 1935 survey of business cycle theories for *Econometrica* [18].

Of course, Frisch's work was not a verified model. It was only a theoretical model and I did not understand the role of the shocks as well as Frisch did. But I think he was perfectly right, and of course one could indicate some of the exogenous variables playing the role of shocks. The most natural ones would be harvests or crops, and in fact they move as a random series. But there were other shocks as well. Too little effort has been made to identify which were the most important shocks in certain concrete cases. Theoretically, it was a very important concept. I suggested that perhaps the so-called Kitchin cycles were mainly due to agricultural shocks and I think also that has never been verified.

On the other hand, I think that what interested economists most was not the shocks but the mechanism generating endogenous cycles, and it might very well be that we have overestimated the role of the mechanism. Maybe the shocks were really much more important. This problem has never been solved, because the War came along and after the War we were not interested in business cycles anymore.

I do remember that I published an article in a Danish journal in 1944 [34] in which I took up another aspect of cycles that had been discussed by some of the English economists: that was the role of upper and lower bounds. The upper limit was, of course, full employment, but whether there was a lower limit was less clear.

How did you organize your work at the League of Nations?

Well, first of all we decided that we would stick to one single equation. As you look at it with the knowledge we now have about investment, we didn't choose a very good equation, because it is the one which has been the most difficult to catch. Then, the second volume dealing with the United States, did bring the

question of a model and there I had the important help of Marcus Flemming. He was my first and my most important assistant. He was oriented towards monetary and financial problems, and those equations that deal with banking, the stock market and so on, have been very much discussed with him. We had no difficulty understanding each other. I learnt a lot from him and we also tried to "translate" the operation of the banking and monetary system into a number of equations.

You will also find some of these equations in my study of the United Kingdom in 1951 [6]. It was only much later that I saw that the material I had collected for the United Kingdom was, in some respects, not so very good. I think Mr. Feinstein collected much better material and it might have been interesting to do this study again with his data. A very important man in this field was Dick Stone. Much later, in 1981, he gave a series of lectures in Geneva, in which he set out an enormous model that he had for Britain.

Jacques Polak was also my assistant at the League of Nations. Later he became the Director of Research at the IMF and is now one of its Executive Directors. He and Tjalling Koopmans continued the work at the League of Nations for a bit after I left in 1938.

How did you set about calculating regressions without electronic computers? Did you have an army of assistants to help you?

We had some assistants and they were very hard working people. They did have calculating machines so that some of the operations could be done somewhat more quickly than by hand. I developed a simplified technique to deal with the calculation of multiple regression equations. For instance, we would just look at the graph and then decide how many decimal points to take. Everything you could not see was considered waste. This, of course, simplified all the multiplications and shortened our work. But in comparison with today, it was all very primitive. We took at most two decimal points. In some cases that may be disastrous because you may miss the essence of a relationship, but in most cases it doesn't matter very much.

So you relied a lot on your graphing technique.

Yes indeed. I was rather proud of my way of putting the curve as observed at the top of the graph along with the explained part of it, and then the components of the equation below. I thought that was very helpful because you would see at once which factor was responsible for some maximum or some minimum, and so it helped to shape your economic understanding. I always recommended the inclusion of such a graph.

This has been replaced by most of my colleagues, correctly up to a point, by indicating each t -value. It was only later that I learned to work with that statistic and understood its importance. I used the correlation coefficient rather more, as a measure of the goodness of fit, and had not heard of t -values even when I left Geneva.

One detail I always propagated is that when you have an index to a certain variable you should use the capital letter as its upper limit. For example, $i = 1, \dots, I$ and $j = 1, \dots, J$. I think that this has now become more or less customary; before, it was usual that the upper limit was indicated by a different letter. But I thought it was just a little detail that could help you a lot to see through things.

Much of the work in econometrics prior to your League of Nations study was undertaken with the aim of verifying economic theories. But you wrote in the introduction to your first report, and indeed you stated earlier [8], that econometric work can never prove an economic theory. So when you were asked to test Haberler's theories, were you literally setting out to disprove them, or to try and verify them?

Well, I think in a sense that is correct. I think all of us would agree that you do not prove anything by very favorable values of R^2 and of the t -values. You only say that you give some sort of a green light to the man who has formulated the variables used in the regression, and so the proof can only, if there is such a thing, be given by economic reasoning. Or perhaps by just enquiring, asking people questions: "What are you motivated by?", "What stimulates you?", and so on. It cannot be done by statistical testing and I think we would all agree that this is so.

But it does constitute progress if you can say certain things are not correct. I had one example in my experience that I think was a good one which dealt with the acceleration principle. It appeared in one of my smaller articles published in 1938 in *Economica* [23]. I showed that in the original presentation of the acceleration principle, which you can find quoted by Haberler, investment was proportional to the rate of increase in production. But upon inspection, you will find that it did not apply to reality. That was perhaps an argument to look also at profits as one of the factors determining investment. Here we have an example of where we disproved the acceleration principle: it was a good start, but it had to be refined before it could be called a real explanation.

I wrote a review around that time, 1937, of one of Harrod's books [21], where he worked with two relationships, the multiplier and the acceleration principle (but he calls it "the Relation"). I praised him for a lot of things but I said that one difficulty was that if you did not have a lag in one of the equations, you cannot explain cycles; and I found that to be a weakness of his book. I do remember it as a case in which the advantages of mathematical economics were clear; namely, that in this case there was much need for a dynamic set-up—that is, for at least one lag. Of course you could choose some more complicated dynamics, but typically the definition of dynamics in the Frischian sense applies here. You must have relations that connect with each other and events at different time points.

You mentioned as early as 1927 [8] that although correlation is implied by causality, the reverse is not true. This gives you a way to disprove causal relationships but not to confirm them. Was it an attempt to get around this problem which led you to develop your arrow schemes (or causal chain models)?⁹

The arrow scheme was definitely one of my teaching tools. I thought it helped quite a bit to clarify the system of relations. In particular, it also helps to distinguish dynamic theories from static theories. But, of course, it was no more than a clarification and had no role by itself. It was not part of the analysis but only illustrated it.

One of the characteristics that clearly stands out in your League of Nations reports compared to the other work of the 1930s and even compared to the work of today, is the number of different tests you employed. You test for structural change, normality of residuals, multicollinearity, linearity of trends, and a whole host of other things. This was particularly unusual in the 1930s. Did you have problems in developing and using such a wide range of tests?

First of all, I was not primarily interested in these things and I have to admit that at present they are done much more seriously than they were at that time. But of course, as we just discussed, I did feel the necessity of looking for criteria to help determine whether something can be proved or disproved.

Also, at that time Frisch was very actively looking at these questions; for instance, he developed the so-called "bunch maps."¹⁰ For some time we took that, or at least Frisch's friends took that, as a new and important criterion. It showed whether or not the points in multidimensional space were situated in a lower dimension than the equations suggested. On the other hand, there had been a profound discussion, especially by Tjalling Koopmans in his dissertation, on the question Frisch raised.¹¹ Tjalling was much more on the side of the official mathematical statisticians and I think he was right.

I myself did very simple things such as trying to determine what happened if you left out one of the variables in order to get at least an impression of what seemed to explain most of the observed movements. These were attempts to develop practical tests, but you can see from this that it was not that part which interested me most. What interested me most were the economic relationships, but I felt that whenever there was doubt one should try to reduce that doubt as much as possible. For instance, comparing the same relation for different countries was one of the ways of doing this. It is a sort of commonsense method, one that you could use in a conversation to convince your colleagues, saying: "It was not only found for Britain but also for Germany." In the opposite case you can also use it to disprove things.

Your first report to the League of Nations was discussed at a special conference in Cambridge in 1938. Although neither Keynes nor Frisch

were at the conference, they appeared to be your main critics. On rereading Frisch's "Memorandum" on the matter, his meaning still remains obscure. Would you tell us about the meeting and how you understood the "Memorandum"?¹²

I must confess that I only vaguely remember that there was such a thing and I think Frisch was not entirely against me. Keynes was more doubtful of the whole thing than Frisch was. But I don't remember whether he had some particular points. The discussion with Keynes took place in the *Economic Journal*.¹³ By the way, it was no longer possible for the *Economic Journal* to pay a fee at that time. So Keynes gave me a life subscription and I'm still receiving the *Economic Journal*. I've never been paid that generously.

From Keynes' criticisms of your League of Nations first report, it seems fairly clear that he knew very little of the developments in econometrics over the 1920s and 1930s, despite being on the editorial board of *Econometrica*. He appears to criticize you unjustly for not doing certain tests you did in fact carry out (structural change tests for instance). Your own public reaction was restrained. How did you feel about Keynes' criticisms at that time and did they influence your second report?

Indeed, I did feel that, at least on certain points, he was badly informed. The best illustration is that he thought that a trend was determined by connecting the first and the last observation. It was a bit strange to me because he had written the *Treatise on Probability*, so I thought he was somewhat familiar with statistics.



JAN TINBERGEN (ca. 1945).

At first I was a bit disappointed, because I thought that he would be especially happy with my work, since we had very largely followed his main macro-theories. But all that seemed not to impress him very much. I had the privilege of meeting him later, just once in 1946. On that occasion I told him we had done quite a bit of research on the price elasticity of exports and that we had really found that the elasticity is about 2, the figure that he uses in his famous book about German reparation payments. I thought that he would be very glad that we had found that figure, and "that he had been right." But he only said: "How nice for you that you found the right figure." That was a most funny experience.

In 1952 you referred to Keynes as "the non-socialist Keynes" [40]. How did you feel personally about him?

It was quite clear that in a discussion between Socialists and non-Socialists it was important to say if you could have Keynes on your side. Then the fact that he wasn't a Socialist made it all the more convincing that your argument was right. So that's why, at that moment, I probably emphasized that he was not a member of the Labour Party.

When you left the League of Nations in 1938, you returned to the Central Bureau of Statistics in The Hague. Then in 1940, Holland was at war with Germany. Did you continue to hold the pacifist beliefs of your youth, and did you manage to continue with your econometric work during the war period?

First of all, it was quite clear that, against a man so typically aggressive as Hitler, pacifist ideas were not very helpful. On the other hand, at the present moment we are continually asking ourselves whether or not we need to consider the Soviet Union as an aggressor. So we are again confronted with the same problem. I find it extremely difficult. I must say that I am more than ever convinced that war is an instrument that cannot be used and should not be used anymore. I have just completed the text of a book *Warfare and Welfare* in which I try to go into more details. I do this together with Dietrich Fischer, who is a young Swiss at New York University [49]. So I'm again in the middle of questions of war and peace.

During the war I was indeed able to continue my work, although from time to time I had to hide myself, because I was on certain lists. Once I was dismissed and was told I could no longer teach in Rotterdam. Then the German business cycle specialist, Ernst Wagemann, interfered with the German authorities. He told them they were crazy and that I was a good scientist, and then I got a message after three or four months that I could teach again. So, interestingly enough, men like Wagemann sometimes interfered in favor of people like myself. I was both a social democrat and a pacifist and the Nazis disliked me of course. But that sort of experience was possible then as well.

Later there were these measures that all men below 40 had to work in Germany. It so happened that I was just over 40 but I did hide myself. At a certain time rumours spread that there was a razzia and that all young men were taken. So I found a nice hiding place and it was only a couple of days later that I heard that if I had been caught I would have been released at the same time since I was just over 40. But that was the way you lived at that time. For many it was much worse!

Interestingly enough, the leadership of the Statistical Bureau had persuaded the German authorities that the operation of the Bureau was necessary for agricultural policy and so it was an institution that could continue part of its work. In 1942 I even published an article in the *Weltwirtschaftliches Archiv* [33]. I have, of course, been criticized for that, but I had built in a sort of test. It was that I had quoted a large number of Jewish authors among my friends. If the editors had asked me to delete these quotations, I would have withdrawn the whole article. In fact, the editors at that time were not Nazis in the real sense, or at least not well informed about some people being or not being Jewish.

You must have been isolated from the main developments in econometrics, particularly those in the U.S.A. during the 1940s. When, for example, did you learn of Haavelmo's 1944 paper on the "Probability Approach" and the work of the Cowles Commission which led to their Monographs 10 and 14? Did you think these developments were important and were you influenced by them? Did you feel that their methods made a substantial difference which was evident, for example, in Klein's work on the U.S. model?¹⁴

That was only after the war. We were all very curious about what had happened in the meantime and, of course, much influenced by the developments. As I already said, the Haavelmo discovery was the most important thing for me. I immediately felt that he was right. Afterwards, of course, the production of the Cowles Commission was unbelievable. I learnt with a lot of interest about the new ideas that had been developed, such as identification. Certainly we were influenced by them. We felt that we had to be retrained and we were happy to be retrained.

I learnt to identify equations if there was any doubt. Also we tried to work with reduced forms which was the simplest way of getting rid of the more difficult methods. Some of us were more interested than others. Theil became one of our really great men and I sometimes feel that he has been a bit forgotten. If you look at Theil's work he has done an enormous number of original things.

You ask about Klein's work. I was, of course, very happy to learn that he had done something similar to my own research on the U.S.A. I would like to mention here that the other models of the United States business cycle do not give as much attention to the crash of 1929 as my model does. You will, of

course, find this very sudden change of direction in my model, although my attempt is perhaps slightly artificial. I do get a very precise explanation, but I also have to make rather *ad hoc* assumptions and this has never been criticized. On the other hand, I feel that other modelers have never brought in that there was an essential difference in that cycle, especially in the behavior of share prices. As a consequence of the Great Crash, the conditions on which you could get short-term credit for speculating were changed forever. The old rules were rather vital for the whole intensity of change in direction that took place in 1929. Looking at it afterwards, I don't feel completely happy about my treatment either. I think both my treatment and those that do not allow for this structural change miss something.

The Dutch Central Planning Bureau was established in 1945 and you became its first director. What did you set out to accomplish?

The Central Planning Bureau was established by a friend of mine, Hein Vos, who was the Minister of Economic Affairs at that time. He was a bit more pro-planning than I was. My own proposal had been to establish a Bureau for economic policy, but under the influence of Russian planning, Hein Vos, who invited me to propose a constitution for the Planning Bureau, wanted the word "planning" to be maintained, and so it was. The Bureau has done work more along the lines that I had in mind, but we have always remained good friends.

We did attempt, first of all, to define the position we were in. The country had lost an enormous part of its capital, labour productivity had gone down enormously, and so we had to establish where we stood. Then we had to ask ourselves how much time it would take to rebuild the economy and generally the distinction was made between reconstruction and development. The guesses that we made were very heroic because we had so little data, but after a few years we could gradually do the things that typically fitted in with my work in 1936. We built models and when I now look back at the history of the Central Planning Bureau I think we succeeded.

It's interesting to see that the Bureau is much more used now than it was in my time. Today, for example, I got a little book in which the programs of the three main political parties have been calculated through with regard to the consequences of their policies over the next five years. The director of the Planning Bureau is on TV from time to time. I think the Bureau has found its place and I am really very happy about the way it works. The staff has produced an enormous number of models for different purposes: short term, longer term, sectoral, and so on.

You were really satisfied then at the Central Planning Bureau.

Well, I was somewhat handicapped by the fact that the Prime Minister was not interested at all. He was very doubtful about the use of any sort of planning and forecasting. I must say that I am also rather sceptical about forecasting. I

think, I see much more in indicating how a certain evil can be abolished—that is, to say what the optimal policy is at a certain moment—than to forecast what will happen. That is much more difficult and you have to know many more things. I think the real task for econometricians is what I would call the policy part, that is to indicate what sort of policy has to be followed.

You might say that it is only a partial forecast that you need, namely, the change that takes place as a consequence of the policy. That can be calculated more or less reliably in a number of cases. But general forecasting means that you must also know the effect and even the appearance of a number of factors that you have not counted upon. It's much more difficult to forecast that Mr. Gorbachev will meet Mr. Reagan or that there will be a bad harvest and things like that.

In 1955 several changes in your working life occurred. First, in that year you and Henri Theil set up the Econometric Institute in Rotterdam.

In fact it was Theil who did it. He asked me to join him and I was happy to do so. But Theil actually ran the institute for quite a few years. It had been mainly J. G. Koopmans who had fought in favor of introducing mathematical economics, although he himself was not using much mathematics. Since the War some mathematics courses were already being given, but I think these were not obligatory.

Theil's initiative was a sort of consolidation of what had already been partly prepared. Since Theil was a very energetic man and also took part very actively in the development of econometrics, it was excellent that he headed the institute. I think it was all a very positive development.

The institute immediately attracted many bright young econometricians, mainly from the U.S.A., such as Goldberger, Zellner, Rothenberg, and Jorgenson. This must have been an exciting time for research at the institute.

Yes. Since econometrics generally is more international than economics, it became one of the centers for foreign contacts. I'm happy to say the same thing about the new sub-department of development planning with which I became involved at that time. There we also had many foreign visitors. But although most of the development planners also had to be econometricians up to a point, they were not specialized as much as the people at the Econometric Institute. I think correctly so.

At this time you also spent a sabbatical year at Harvard. How did you find the American economics community?

Actually Haberler had his sabbatical and he was the one who invited me to take over. So I had to teach foreign trade and I met a lot of well-known people. Among those good friends were Leontief and Smithies. It was really quite an adventure for me to be with them. It was a wonderful time. Also, I felt that my

style of teaching was accepted quite easily and that it was close to the American style. There was a lot of discussion with the students during the course and I liked it all very much.

The most important change in 1955 was that you changed jobs and fields. Why did you in 1955 leave the Central Planning Bureau?

At that time Professor Gonggrijp was the man who taught about "colonial (later: dual) economies" in Rotterdam and he retired. I was asked to take over his job, and I said that I would be happy to do so if the subject was changed into "cooperation with developing countries." Gonggrijp was an interesting man because he himself had had responsibilities in the colonial system. It was a shame, both in his view and mine, that while the poor population of Indonesia could be very much helped by cheap textiles, this market was reserved for the Dutch industries. That was the way it worked, and he had already somewhat modernized the views of the Dutch people who were typically colonialists before the War. I was also happy to take over Gonggrijp's job because in the meantime I had discovered that these problems were very much more serious than the social problems within this country. So, I felt the time had come for me to do something else.

You mentioned earlier your 1942 paper on long term trends in economic activity in the *Weltwirtschaftliches Archiv* [33]. Do you consider that to be the beginning of your interest in development economics?

No, that was simply a complement to my business cycle research. We had to begin by eliminating the trend, and all these theories of cycles were about deviations from the trend. But I also felt that the trend itself needed some explanation and that is why I would rather consider this paper as the last chapter of my work on business cycles.

My interest in development cooperation was again very typically social, but now on a world level. We felt that the real poor were the people in the developing countries. This followed my visit to India in 1951 which really was appalling to me. From then on I accepted several other engagements overseas.

We followed what at that time was the most usual approach, namely, to determine the need for investments in order to attain a certain rate of development and then to try and find out what they could finance themselves and to finance the shortage by foreign aid. So that was the line of reasoning that was followed by people like Hans Singer from Sussex University and others. All this, of course, was in close cooperation with a number of people at the United Nations.

I've very often been in New York and this started in a somewhat interesting way by accepting a small advisory task from the World Bank. I had to write a little book which could serve as an introduction to people who had to prepare operations at the Bank, and that little book was finished in, say, half a year or less and was ready in 1955. But it contained the idea that it would be good to

have part of the economy in the public sector and the President of the Bank did not like that; it was a bit too leftist. So the book was not published immediately; it was published only three years later [44] when the man who set up the development institute where people were trained, I think his name was Adler, took it as one of the textbooks to be used.

But it was a funny experience and completely natural that the United States thought that the Bank had to finance the infrastructure, but that the superstructure would have to come from private initiatives. I had, as a counterexample, the case of the state mines here in Holland, which had been established in 1902 at a time when there were no Socialists in the parliament, or at most one. But simply because there was insufficient private capital and initiative to start coal mining, the government had to step in. I had another example from Turkey where Atatürk, the modernizer of Turkey, also established a number of so-called economic state enterprises because there was insufficient private initiative. But, of course, an American could not understand this, so it was all quite funny to see the book held up, but also satisfactory that in 1958 it was published after all.

Would you agree that the strength of your models lies in their simplicity and flexibility? Is simplicity a conscious feature of your model design?

When I tried to develop simple things it was mainly for teaching purposes. But for advising countries in concrete cases you must be careful not to oversimplify. We already discussed this a little when we talked about supply and demand questions. And, of course, if you can keep something simple that's helpful, but it should not be at the price of missing a few main points.

If I may take one example: your educational model with Bos [47]. This is a very simple model. But the strength of it, I think, lies in its flexibility. Later Ph.D. students have generalized the model and applied it to Spain, Turkey, and Greece. The flexibility and robustness of the model ensured that it could be extended in that way without damaging its essential features.

It's very kind of you to say so. I was not aware of this flexibility, as you call it. I think flexibility anyway is a good thing. I wasn't aware of the fact that my models were more flexible than other people's models. But, of course, if it cannot be applied then it is not very useful. I must say it was quite fascinating to bring planning into the education sector, since it has some very unexpected consequences—especially for developing countries which usually want to get rid of the foreign teachers as soon as possible. But then the message from the model is that they should start with attracting even more foreign teachers because that will be the quickest way to have the necessary number of national teachers.

The declaration made by Frisch in the founding issue of *Econometrica* was full of hope that econometrics would take over economics and be the way forward. How do you feel now about that optimistic start?

It goes a bit too far. There are also some purely qualitative elements and by definition these cannot be a subject for econometrics. I've often repeated these last few years that if you have a hierarchy of authorities, say central government, provincial government and local government, if you have a number of levels on which decisions can be made, then for each problem there exists an optimum level and that optimum level must be as low as possible, but it must also be such that there are no external effects. This, of course, is a very simple philosophy, but it is helpful in a number of cases, especially if you think of world problems, because then you have many more levels, not only a national level, but also a continental and a world level.

I think that to explain to people why certain things must not be decided by national authorities but at higher levels is extremely important, not only for environmental policies—for which this applies quite clearly—but most of all for security problems. One of the ways in which this new book of ours [49] reformulates the main point is that a security policy cannot and must not be an element of national sovereignty. Also, when we discuss European integration a number of things must not be decided by national governments, but at the next higher level. Here you have something that you can discuss without using any figures.

On the other hand, I think that for a good deal of economic theorizing it is true that econometrics must take over. The clearest case we met during our discussion this afternoon was the case where theories are found to be incomplete. So often, that failing has been discovered only after we have taken up the econometric approach. I think that is an extremely strong argument which implies that a lot of economics is being dealt with better by econometricians than by what I would call verbal economists. In that sense I would see something in Frisch's idea.

How do you feel about the way econometrics has developed over the last twenty years or so? In 1952 you stated [40] that you feared that techniques would take over from attention to human needs and problems in the field of economics. Do you feel this fear was justified?

I'm afraid, yes. But, of course, that always has two sides. One side of it is my insufficient interest in and knowledge of difficult mathematics. This means that automatically you tend to neglect the more subtle mathematical/statistical issues involved and that means also a considerable portion of econometrics. So, my interest is typically purely economic and sometimes I feel that so much refinement of methods of testing is perhaps not necessary. But I'm not quite sure, so I say it tentatively. I simply cannot read the larger part of *Econometrica* anymore; but this is perhaps also true for many others, simply for lack of time. Production has increased tremendously. There was a time when I would read *Econometrica* as a whole, also probably because I was younger and had fewer responsibilities. But I've a vague feeling that I would have liked somewhat more applications and somewhat less pure theory.

What do you feel are your most important contributions to economics?

That is a difficult question. But I must try to answer it because then you can tell me that you don't agree. That will be interesting to hear. It seems to me that my contributions cluster around a few books that I have written—*On the Theory of Economic Policy* [43] is the first or maybe the second, if I take the book on Business Cycles first. Then I concentrated for some time on income distribution and that also appeared in book form later on [48].

At present, I'm working on this internalization of security policy into economic policy and here my old anti-militarist feelings help me a lot. But at the same time there are more objective reasons for it, as everybody knows. I mean the potentialities of a nuclear war are such that we now have to be pacifists in a sense, and in this new book I try to make some contributions. One of them is about these levels of decision making. There are also one or two smaller things I'd just like to mention. It gave me much pleasure to introduce the idea of non-tradeables into the simple Keynesian scheme. I did so in an article in the *Weltwirtschaftliches Archiv* [46], and it's interesting that with very simple algebra one can show that the orders of magnitude of certain coefficients become quite different, and there may even be a point where one might criticize the IMF.

One thing I am a little bit unhappy about is this. I don't know if you have ever read Sam de Wolff's *Business Cycles*. At the time it appeared, I wrote a review [10] that was a terrible disappointment to him. I went into details that were not so important as some of the main elements that he had taken up. Later a book appeared to honour him [24], I think on his 60th birthday, and there I showed something in support of at least part of his ideas. That honoured him and I am happy to know that he read it and was also pleased about it.

That was an example of a person you have perhaps offended a little bit at some stage. I don't think you have offended many people in your career. Do you make a particular effort?

Yes. I am not very religious, but I'm a member of a very small church of which the main principle is tolerance. And so if one applies that, I think you don't offend many people.

Is there a paper which you would have liked to have written but did not write, and vice versa is there a paper that you have written but you have regretted since?

Let me answer the last part first. It is not exactly in that form, but I am a bit careless, so almost all my publications have an error somewhere, sometimes a small error, sometimes a big one. In that sense I have written things that I have to deplore now, but that is simply because I was not formulating my idea exactly enough.

The first question is more interesting. For some time I have been interested in the distribution of a population over cities and villages, in short over centres of different size, and I tried to develop a theory, but that theory was either incomplete or too simple. It was mainly based, I think, on the demand side. So what I wrote about it [45] was unfinished and I think that the subject, although it's terribly interesting, is very difficult, too difficult for me. Perhaps Jean Paelinck will be able to solve that problem. But this is an area in which I would have liked to make more progress than I did.

NOTES

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4. This refers to Herman Wold's recursive least squares model and method (see his Estimation of Economic Relationships. *Econometrica* 16 (1948): 33–36, introduced in response to the simultaneous equations model).
5. The original paper of 1936 is available in English under the title: *An Economic Policy for 1936* in Tinbergen [7]. For a revised version in 1937, which concentrates on econometric aspects, see Tinbergen [2].
6. See Frisch, R. Statikk og dynamikk i den økonomiske teori. *Nationaløkonomisk Tidsskrift* (1929): 321–379, and On the Notion of Equilibrium and Disequilibrium. *Review of Economic Studies* 4 (1935): 100–105.
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This is a selected bibliography of Tinbergen's work. It contains in Section I: his books on econometrics, and in Section II: a selection of his more important papers in that field from the period 1927–1952. There is a huge number of papers in econometrics omitted from this list,

written mostly in Dutch, English, German, and French, which merit attention and are for the most part relatively easily accessible (see [54–56]). The interested reader is also referred to the journal *De Nederlandsche Conjunctuur*. Other books and papers mentioned during the interview are referenced in Section III.

There is no complete bibliography for Tinbergen. If there were it would probably run to over 50 pages! Several partial bibliographies have been published and these are referenced in Section IV, along with biographical sources and appraisals of Tinbergen's work.

I. ECONOMETRICS BOOKS

1936

1. *Grondproblemen der Theoretische Statistiek*. Haarlem: F. Bohn.

1937

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1939

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1942

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1951

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1959

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1939

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