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THE ET INTERVIEW: PROFESSOR SIR RICHARD STONE

Interviewed by M. Hashem Pesaran

Sir Richard Stone, knighted in 1978 and Nobel Laureate in Economics in 1984, is one of the pioneering architects of national income and social accounts, and is one of the few economists of his generation to have faced the challenge of economics as a science by combining theory and measurement within a cohesive framework. He was awarded the Nobel Prize in Economics for his "fundamental contributions to the development of national accounts," but he has made equally significant contributions to the empirical analysis of consumer behavior. His work on the "Growth Project" has also been instrumental in the development of appropriate econometric methodology for the construction and the analysis of large disaggregated macroeconometric models.

Throughout his long and productive career, stretching over more than half a century, Stone has been an inspiration to applied econometricians all over the world. His influence goes well beyond his written work. He has made a lasting impact on the large number of (now prominent) economists and statisticians who visited the *Department of Applied Economics* when he was its Director. He is a scientist, a scholar, and above all, a gentleman. He gives generously of himself and is always willing to help the cause of applied econometrics. He has been a Fellow of King's College, Cambridge since 1945 and has served as the President of the Econometric Society (in 1955) and the President of the Royal Economic Society (during 1978–1980). In the interview that follows, Richard Stone gives us a delightful account of his time as a student at Westminster School, his early introduction to economics at Cambridge University, and he shares with us his memories and thoughts on a long and productive career. The interview was conducted in Stones' magnificent private library in Cambridge, and I hope that readers enjoy reading the interview as much as I enjoyed recording it.

Further details of Richard Stone's biography and research activities can be found in:

Deaton, A. Stone, John Richard Nicholas. In J. Eatwell, M. Milgate and P. Newman (eds.), *The New Palgrave: A Dictionary of Economics*, Vol. 4, pp. 509–512. London: Macmillan, 1987.

Stone, J.R.N. An autobiographical sketch. In *Les Prix Nobel 1984*. Stockholm: Almquist and Wicksell International, 1985.

Perhaps you could start by telling us about your early schooling. I believe you went to Westminster School with the intention of preparing for a profession in law.

Yes, I went to Westminster in 1926 and left at the end of 1930. My father, who was a barrister, wished me to become a lawyer too, so I was put on the classical side because that was supposed to be the right formation for lawyers.

My school career was wholly undistinguished. I had not done badly at my prep school, Cliveden Place, so when I entered Westminster I hoped to start a little way up the school. In fact, to my disappointment, I found myself in the bottom form. Not only that, but I spent my second year there too. After that I made a wild dash up the school, changing form every term and sometimes twice a term, with the result that I never finished any of the courses. All this was bewildering and rather boring, but I took life as I found it and was not unduly bothered. Perhaps I should have been more interested if I had been on the scientific side. Eventually, late in 1929, I found myself in the form from which one could take School Certificate, which I duly took with good marks. The subject in which I excelled was Greek New Testament, don't ask me why. I left school too early to take Higher Certificate.

Have you been back to Westminster since?

Yes. When I was awarded the Nobel Memorial Prize in 1984, I was invited to visit the school and ask for a half-holiday, "beg a play" in the local jargon. I was also supposed to make a speech so I told the assembled school, now girls as well as boys, about my own school career in the hope that it would encourage any who were not shining in theirs. We then all had lunch in hall. It was a jolly occasion and the food was much better than it had been in my day.

I understand that you left Westminster early to join your father in India. How did this come about and what did you do during the year that you spent in India?

I left Westminster in the middle of the Michaelmas, "Play" term of 1930. The reference here is not to a holiday but to the Latin play, usually Terence or Plautus, which the King's Scholars put on each year at the end of that term. The nibs, that is to say our parents, sat in the stalls and we were in the gallery, each group provided with a prefect who waved a wand at any particularly outrageous joke as a sign for us to clap. As we were going out I heard with secret amusement somebody say to my father: "It's wonderful how they bring the boys on here; you know, they didn't miss a joke."

Well, to return to your question. In 1930 my father was appointed a high court judge in Madras. In the summer before he was due to leave, he and I called on my headmaster. "What," my father asked, "are we going to do about Dick." "If I were you," the headmaster replied, "I should take him with you, he doesn't seem to be doing much good here." So I went to India and had a good time. I was going up to Cambridge in the following year but nobody had given me any books to read so I danced, played tennis, and traveled a bit round India with my parents. In my father's long vacation we made a trip down the coast of Malaya to Singapore and then across to Java. There I had my first encounter with volcanoes, humming birds, and Javanese puppets. It was all highly enjoyable.

What subjects did you take when you went to Cambridge and who were your supervisors?

For my first two years I read law in accordance with my father's plan. My college was Gonville and Caius and my tutor was Emlyn Wade, a distinguished constitutional lawyer, and I also remember supervisions in international law with Arnold McNair and what I recollect as mass supervisions in Roman law with Charlie Ziegler at Pembroke. We were lectured to on Roman law by W.W. Buckland. Buckland had lectured to my father about a quarter of a century earlier and I remember the stack of well-thumbed lecture notes which he slapped on the desk before he began to speak.

In my second year, 1932–1933, I did a foolish and rather unkind thing; I was determined, if possible, to switch to economics. I never opened a law book after Christmas 1932. What I read was Irving Fisher, Marx, Freud, Lenin and popularized science. This might well have cost me the rather poor first I got that year in part 1 of the law tripos: only a I.2. We were very exactly classed in those days. After what seemed like interminable discussions, it was finally agreed that I should change to economics.

This time I was given some homework for the long vacation. No, not the *Wealth of Nations* but Marshall's *Principles* and Joan Robinson's *Economics of Imperfect Competition*. At that time there was no economist at Caius so I was sent to Richard Kahn at King's for supervision. This lasted for two terms and I then went to J.W.F. Rowe of Pembroke. In my last year I was supervised by Gerald Shove of King's.

What drew you to economics in the first place? Which economists in Cambridge influenced you most?

As we had been sinking into the Great Depression for some years, youthful inexperience and innate optimism combined to make me think that if there were more economists the world would be a better place. I think a number of my contemporaries thought the same and, whatever they had been doing before, switched to economics.

Of my teachers, the two best on the theoretical side were Richard Kahn and Joan Robinson, but without doubt the greatest influence on me came from Colin Clark, who at the time was teaching statistics to economists in Cambridge. He had written a small book on the national income in 1932 and was attempting to reconcile his income totals with the estimates of expenditure made by Feavearyear for the same year (a final reconciliation was effected by Myra Curtis, with the help of the authors, in 1935). At the same time he was engaged on a much larger book on the national income and outlay, which came out in 1937. For this work he brought together estimates of income, output, consumers' expenditure, government revenue and expenditure, capital formation, saving, foreign trade and the balance of payments. Although, as he told me a few years ago, it did not occur to him to set up all this information in an accounting framework, it is clear that all the building blocks for the national accounts were there and that the main totals were fairly consistent.

I found all this fascinating and Colin and I became great friends. I often had dinner and spent the evening with him in his pleasant rooms in a terrace in Peas Hill which has since been pulled down to make way for an extension of the Guildhall. He had a good plain cook who had formerly been a van driver for the railway and lost his job when horse-drawn vehicles were replaced by motors.

Colin was a very amusing companion, unstuffy to a degree. He was also a hardy fellow. I remember vividly a canoeing holiday we took together on the Severn. After about a week, in the course of which we had been attacked in our canoe by angry swans and visited in our tent by a cow, we camped on what turned out to be wet ground and getting wetter all the time. I got up at dawn and staggered about with a high temperature but Colin seemed completely unaffected. I told him at breakfast that I should have to drop out. "That's all right," he said "I shall go on for a bit and will see you when I get back to Cambridge." This was on a Friday. On the following Monday I met him walking about the streets of Cambridge. "But I thought you were going on," I said. "I did," he replied, "but the boat sank so I decided to abandon her."

During a time when most prominent economists in Cambridge were preoccupied with economic theory, what attracted you to focus on empirical analysis?

My interest in economics was from the beginning in its applications. I thought that the economics I was taught was insufficiently quantitative and that theory and facts were too widely separated. This is why I was so attracted by Colin Clark's work. It seems to me that the development of a science requires attention to both facts and theories and I agree with Marshall that economic theory is as mischievous an imposter when it claims to be economics proper as is mere crude unanalysed history. I cannot imagine why anyone should think otherwise or why economists should tend to put theorists on a pedestal. The real difficulty is to combine the two so that theory can be used to interpret facts and facts can show what has to be interpreted.

I tried my hand at this sort of thing while still an undergraduate in 1934 or 1935. I remember fitting a Cobb-Douglas function to fairly long British time series of output, labor, and capital with a residual trend. The results were never published and not a trace of them remains today but they were fairly typical of their time: the exponents of labor and capital were about 3/4 and 1/4, respectively, and the residual trend representing technical progress was about 2.25 percent per annum. I was quite excited about this, my first calculation of a least-squares regression which I carried out on a small hand Monroe given me by parents as a twenty-first birthday present. I tried to interest Pigou in this piece of work, without conspicuous success. Actually, being young and very shy I would not have dared do this had I not been egged on to it by my friends. Pigou received me kindly, looked at my diagrams, and composed himself to listen. Half-way through my explanation he remarked: "I don't understand a word of this, but carry on." When I had finished he said, not unkindly: "Doubtless it is all very interesting but I still don't understand. Good bye, Mr. Stone, come and see me again. Always glad to help."

In my undergraduate days I also had an encounter with Keynes, who had asked me to give a talk at one of his Monday evenings. My subject was effective demand *versus* production frictions such as, say, the difficulty of moving a labor force from one region to another or of changing its skill content to meet different production requirements. Given a productive system suffering as the British one was from many such imbalances, could demand be effective in the Keynesian sense or was it not more likely to lead to shortages and hence to increased prices? If so, was it not better to try and redress the imbalances before stimulating demand? Looking back, I see that my choice of subject was tactless, to say the least, and indeed it did not go down at all well with my audience except for Keynes himself. He was very nice about it and paid me the compliment of appearing to take me seriously. Whether he really did I cannot tell.

You completed your university education with flying colors, obtaining a double first in law and in economics. What did you do next?

When I went down in 1935, Caius offered me a research studentship and, while I was greatly tempted to take it up, I did not in fact do so. There were two reasons for this.

In the first place, I had only studied economics for two years and did not feel confident that I could do myself credit as a graduate student. In the second place, my father was very anxious to see me in a job and said he would find me one. He did, and I joined the Lloyd's broking firm of C.E. Heath and Co. I suppose my position was really that of an apprentice. I started on £50 a year and it may have risen to double that by the time I left in 1939. The purpose of the exercise from my father's point of view was that having worked in this way for five years I could become a member of one of Lloyd's underwriting syndicates at a much reduced price. However, I never reached that point. I learned the business and a good deal about life, but I was not and never should have become a business man, and eventually I managed to persuade my father of this.

While you were on the staff of C.E. Heath and Co. you managed in your spare time to produce a statistical monthly called "Trends." Could you tell us about this venture? How did you become involved in this?

"Trends" was not a magazine in its own right but a feature which appeared regularly in the monthly *Industry Illustrated*. It had been started by Colin Clark, and when in 1937 Colin went to Australia he asked me if I would like to take it on. I naturally agreed, and from June 1937 to May 1939 ran it with the help of my first wife, whom I had married in 1936 and who was also an economist. Each month we produced a set of graphs of British economic time series with a commentary on their recent movements, an article on a topical subject, such as the American stock exchange or economic recovery in Germany, and, from time to time, a survey of statistics of, for instance, building plans or regional employment [2,4,7]. It could be considered as a very modest forerunner of the British official monthly *Economic Trends* which began to appear after the war. It was interesting but quite a lot of work. After doing it for about two years we decided that we should be paid more than the £5 a month which had been originally offered. The editor refused and we gave it up. I do not know what happened next.

During that period, my wife and I also contributed articles to economic journals. One was on the marginal propensity to consume and the multiplier for several countries, obtained by various methods [3]. Another compared alternative indices of British industrial output [5]. And a third was on pitfalls in assessing the state of trade; this was based on the "Trends" material and gave our estimates of the number of working days in each month in the years 1928–1938 as a percentage of the average number in the period 1921–1934, together with the seasonal patterns in our series [6].

The war broke out on 2 September 1939, and you joined the economic staff of the Ministry of Economic Warfare. What did you do at the Ministry and did you find your work intellectually rewarding?

I had been asked by Noel Hall earlier in 1939 if, in the event of war, I would join the Ministry of Economic Warfare in which he was going to be Director of Neutral Countries Intelligence. I said I would and September 2 found me at the gathering point, the London School of Economics. As I had been at Lloyds, it was assumed that I knew all about shipping. This was quite untrue since, though it had a small marine department, Heath's dealt essentially with nonmarine business. As I was also supposed to be a statistician, I was given the job of keeping a record of imports into neutral countries.

This led to a rather curious episode. Without going into details, it soon became obvious that the only ships worth recording were tankers. One knew their speed and capacity and they carried a limited range of products of great importance in war time. As I was now concentrating on tankers, I went to work with a man in the oil department called Stafford Byers. Although *Lloyd's List*, giving daily shipping movements, lasted for a few days into the war, it soon packed up and we were dependent on information from secret sources. We filled in my tanker index day by day and it became clear that Italy, which had not yet entered the war, was a big importer. Suddenly, in the second half of May a dramatic change took place. The Italian tankers, which up to then had been moving in a predictable way, changed course and began to steer north or south. They must be making for the nearest neutral ports, we thought. So we bought a large map of the Atlantic on which, with the help of colored pins, we could plot the course of each ship. We had to guess which port each ship was making for but we knew their speeds and decided by the end of May that they would all have reached their chosen destination by 10 June. On that day we thought that Italy would declare war.

This seemed to me a very interesting piece of information but when I reported it to the appropriate authorities I found myself in the doghouse. The Italophile section of the Foreign Office refused to believe me and I was reprimanded for my presumption.

Unfounded suspicions, they said. Italy was a delightful country and a firm friend. What if it imported a lot of oil; it was a Catholic country and needed a lot of paraffin for altar candles. However, Italy did declare war on the day we had foreseen.

Would you say that this was your first attempt at prediction?

Yes, it was; and perhaps my most successful one, objectively speaking, even though it was not a great success at the time. Much later I told this story to

Keynes. "If you had said all this to Churchill something might have happened," he said. But I was much too junior then to dream of doing such a thing.

In the summer of 1940 you left the Ministry of Economic Warfare to join James Meade at the Central Economic Information Service of the Offices of the War Cabinet to work on national income accounts. Was this your first involvement with national accounting?

Yes. Meade had been working on the information required for dealing with war finance and said that if he was to translate his ideas into numbers he would need someone to get out the figures, and Austin Robinson suggested me. That was the beginning not only of the British national accounts but also of a friendship that is still going strong. James and I got on well and by the end of 1940 we had produced a small system of three accounts which we filled in for 1938 and the four quarters of 1940. At this point we showed our accounts to Keynes, who was then at the Treasury. He was naturally interested since he had made use of accounts for 1938–1939 in *How to Pay for the War*, which had appeared early in 1940; his estimates had been prepared by Erwin Rothbarth by updating Clark's figures. He now encouraged us to publish and suggested at first an article in the *Economic Journal.* Finally it was decided that our tables should be annexed to a paper he was writing on the analysis of war finance which was to be circulated as a White Paper (Cmd.6261) with the Budget of 1941.

Our accounts attracted a good deal of attention among economists. Although the chancellor said that he could not promise that they would be continued, they in fact established themselves. Around that time our department was split into two: the Economic Section, to which James was attached, and the Central Statistical Office, to which I was attached. Here, thanks again to Keynes' intervention, I continued to work on the national accounts, producing every year at budget time a White Paper with up-to-date figures. The last I worked on appeared in 1945 and covered the years 1938–1944. These thin White Papers were the forerunners of the Blue Books on *National Income and Expenditure* in which our small beginnings have been immensely magnified.

To return to 1941, after the appearance of the first White Paper, James and I did publish in the *Economic Journal* a methodological article on the construction of the accounts [8]. And in 1944 we brought out a little textbook illustrated with estimates for Britain and America which turned out very successful [16].

How did your work on national income accounting in the UK compare with the estimates of national income and expenditure that were being prepared in the United States and Canada? When did attempts at coordinating these activities start and what was your role in these attempts?

On the income side, both the Americans and the Canadians had a start on us, as official national income estimates had been made before the war in both countries. In the United States these had been started in the early thirties by Simon Kuznets with his estimates of income produced and income paid out. He also worked unofficially at the National Bureau of Economic Research on complementary topics such as capital formation. Gradually estimates were made of other elements in the national accounts. I cannot remember exactly when all this happened, but a history of U.S. national income and product accounts, 1932–1947, is given by Carol S. Carson in the *Review of Income and Wealth*, series 21, no. 2, 1975; and a detailed history of private and public estimates in Canada appears in O.J. Firestone's book *Canada's Economic Development, 1867–1953*, published by The International Association for Research in Income and Wealth in 1958.

What I do know is that in 1942 I was able to publish a fairly complete set of national accounts for the United States back to 1929 [9]. I presented them in the form proposed by Meade and me in our 1941 *Economic Journal* article, trying to make the classification as far as possible comparable with that of our official estimates. At that stage in the American accounts consumers' purchases of nondurable goods and services were not estimated independently but were obtained by difference. In 1943, Milton Gilbert, who was then head of the national income unit of the Department of Commerce, contributed to the *Economic Journal* a paper on U.S. income statistics, commenting sympathetically on my estimates and raising a number of controversial points which I answered in the same issue [13].

By now we were all casting about for the best way to set up the national accounts, and in the autumn of 1944 I was sent over the Atlantic to try to reach agreement with the Americans and Canadians on national accounting taxonomy and presentation.

I went first to Ottawa and met my opposite numbers in the Dominion Bureau of Statistics (now Statistics Canada). From there I went down to Washington with George Luxton, who unhappily died shortly afterwards, to discuss matters with Milton Gilbert and his team. This included George Jaszi, who eventually succeeded Gilbert, and Edward F. Denison, who is well known for his subsequent work on growth accounting. Our meetings were extremely friendly and constructive; they were reported by Denison to the American Conference for Research in Income and Wealth and appeared in volume 10 of their *Studies* in 1947.

After the war you became the first director of the Department of Applied Economics in Cambridge. But at the same time you started a long association with international organizations on the development of national accounts. Before we get on to the department could you tell me something about that work?

Well, it began in a way I hadn't expected. At the end of the war I was invited by Winfield W. Riefler, who was a member of the American Board of Economic Warfare stationed in London, to visit the Institute for Advanced Study in Princeton after the war. I decided to take advantage of this invitation before I entered actively into my duties at the Department and so September 1945 found me in Princeton. At the Institute I met Alexander Loveday, the head of the Intelligence Department of the League of Nations. He had been there with his department during the war and one of the first things he said to me was that he had been looking for someone to write a report on national income statistics for the League of Nations' Committee of Statistical Experts and would I like to take the job on. My answer was an emphatic yes, as for the past five years I had been wanting to write up the subject properly but had never found the time and, indeed, was intending to do it here in Princeton. "Well," he said, "if you can get out a draft in the next two or three months I'll organize a committee here and we can finalize the whole thing." I did this and the committee met from 17 to 20 December. I was in the chair and the other eight members were H.P. Brown (Australia), J.B.D. Derksen (Netherlands), C.M. Isbister (Canada), George Jaszi (U.S.A.), Hildegard Kneeland (Inter-American Statistical Institute), Raul Ortiz Mena (Mexico), Arne Skaug (Norway), and Julius Wyler (Switzerland). Though not a member of the committee, Agatha Chapman (Canada) also took part in the discussions. The report was published by the United Nations, Geneva, in 1947 [24].

The next development was connected with the OEEC (since 1961 the OECD), which was set up after the war to administer Marshall aid to Europe. Richard Ruggles was involved in its creation and I think it was his idea that there should be a National Accounts Research Unit which would formulate a standard system of national accounts, review the estimates of member countries and help to train statisticians from those countries in national accounting. This unit was set up in Cambridge under my direction for the three years 1949–1951, after which it was merged in the division of statistics and national accounts at the headquarters of the organization in Paris. By then, Milton Gilbert was in charge of statistics. The *Standardised System of National Accounts* was published by the OEEC in 1952.

Meanwhile the UN Statistical Commission had also been thinking about national accounting and decided that they needed a standard system. So July 1952 found me again in New York as a member of an expert group consisting of Loreto M. Dominguez (Pan American Union), Kurt Hansen (Denmark), George Jaszi (U.S.A.) and M.M. Mukherjee (India). I was again elected chairman and, despite the heat (we worked at night and slept by day), we turned in our report by the end of the month. For drafting purposes we broke into two groups. Jaszi and I spent a long weekend at his house in Washington writing the first part of the report down to the end of the tables;

and Hansen and Mukherjee wrote the second part dealing with definitions.

Not surprisingly, the new report, the original SNA, was very similar to the OEEC's standardized system. In the following year I was again in the United States and was invited to attend the meeting of the UN Statistical Commission on condition that I did not open my mouth. The report was very well received (except, as was inevitable at that time, by the member for Russia) and was published in 1953.

After that, the SNA continued for about ten years with only two minor revisions before it was felt that something more was needed. In 1963 I happened to run into Pat Loftus, who was in charge of statistics at the headquarters of the UN, in the lift of my hotel in Rome. "Ah, the very man I was looking for," he said, "we are planning a major revision and extension of the SNA and wonder if you would help us?" Well, certainly I would; and when I got home I started to consider how we could incorporate all the information now demanded into the system. For it was intended that the new version should include input-output and flow-of-funds data as well as balance sheets and there were other matters to be borne in mind such as distribution and regional statistics as well as constant price estimates.

This was a large assignment. Furthermore, the situation had changed since 1953. By 1963, most countries were producing national accounts and so not only was a more representative expert group desirable but it would be necessary to explain the new proposals in different parts of the world if they were to stand a reasonable chance of proving acceptable and being acted on.

In all this I had the help of Abe Aidenoff, who had long been on the staff of the UN Statistical Office and was deputy director from 1970 to 1975. He not only attended the expert group meetings but organized conferences at the regional commissions of the UN (ECAFE, ECLA etc.) to discuss the new system. In 1968 he and I wrote the final report in Cambridge, by agreement with Loftus. I did the first four chapters and Abe did the remainder. It was published in the same year.

What was the nature of these revisions; and, looking back, do you think that you would have done the task differently?

The principal changes were a distinction between industries and commodities and the introduction of input-output and flow-of-funds tables and of balance sheets. At the time I do not think we could have done more than we did. But looking back I think it was a pity that we did not complete our disaggregation of the domestic sectors by disaggregating households (the consumption account) and discuss the analyses that this would make possible. The UN Statistical Office did take up this question and I returned to it in a paper on the disaggregation of the household sector in the national accounts which I wrote for a World Bank conference in 1978 [165].

titative work on prices and prime costs, in which he was assisted by Brian Tew, Y.N. Hsu, and intermittently me. At the same time P.R. Marrack was working on the British demand for imports, and Erwin Rothbarth on consumption and its composition in relation to the level of activity. Work was planned on foreign trade and the balance of payments. Keynes himself intended to direct an investigation into the measurement of saving, and with the help of Piers Debenham made a start on it in the summer of 1939. Some publications arose from this series of projects, but little could be finished before it was all swept away by the war. However, enough had been going on to show that the program had a strong econometric flavor.

From an administrative point of view the Cambridge Research Scheme was highly informal. By way of providing a formal link between the group and the University, the Faculty Board of Economics and Politics, doubtless at Keynes' instigation, proposed to the General Board of the Faculties in the summer of 1939 the establishment of a Department of Applied Economics. This was accepted in the course of the Michaelmas term of that year. But in view of the outbreak of war the project had to be put into cold storage.

Keynes did not forget about it, however, and as the war drew to a close, arrangements were put in train to get the Department going as soon as possible. In 1944 the Faculty Board appointed a Committee of Management consisting of Keynes (chairman), David Champernowne, Austin Robinson, Joan Robinson, Gerald Shove, and Piero Sraffa, with Dennis Robertson as a coopted member, and in November of that year the Committee decided to offer the directorship to me.

When the offer came I was surprised and delighted. But before I could actually enter into my functions two things had to happen: the office of Director of the Department had to be approved by the University and I had to obtain my release from the Civil Service. The University took some months to make up its mind; I remember a letter from Gerald Shove in which he informed me that "this project is not quite dead." Eventually, on 15 May 1945, the University gave its consent, my release from the Civil Service was obtained without difficulty, and I was appointed Director as from 1 July of that year.

And so in the summer of 1945 the work began of formulating a research program, raising finance, finding accommodation, and recruiting staff. For the record, our first grant came from the Rockefeller Foundation, primed by Keynes. Without their help we might never have got off the ground. I also owe them my gratitude for renewing the grant in subsequent years. Another early supporter was the Nuffield Foundation.

Given the deep involvement of Keynes in the establishment of the DAE (with the idea of promoting research in the areas of measurement of savings and investment), how do you explain Keynes's adverse reactions to Tinbergen's macroeconomic work on business cycles?

Richard Stone, circa 1945.

A further revision is due to appear this year. Although I am no longer a player but a mere spectator, I hope in Adam Smith's phrase an impartial one, I am looking forward to the new report with great interest.

Perhaps we should now get back to the Department.

In 1945 you became the first director of the Department of Applied Economics in Cambridge. Were you also involved in the foundation of the Department?

No. The value of such a department was recognized before the war but the proposal only got through all the University hurdles at the end of 1939. Whether the first idea originated with Keynes himself, it is now impossible to tell, but what is certain is that he took it up with enthusiasm; and when Keynes took something up, sooner or later it was sure to happen.

Its forerunner was a short-lived initiative known as the Cambridge Research Scheme. The chairman of the steering committee was Keynes and the finance was found by the National Institute of Economic and Social Research. It may amuse those who now have the task of raising funds for research to know that the grant to the Cambridge Scheme in 1938–1939 was £600 and that not all of it was spent.

The general subject of research was the process of economic change in the United Kingdom. One of the initial items in the program was Kalecki's quan-

This is a difficult question to answer. I discussed it at some length in my Keynes lecture to the British Academy in 1978 [167] and perhaps the best thing I can do is to quote from that. Up to a point the explanation may lie in Keynes's state of health. In 1937 he had had a severe heart attack; and the summer of 1938, when he received Tinbergen's proofs, must have been a particularly bad moment for him to be faced with an approach to economics so very different from anything he was accustomed to. But this does not justify the virulence of his remarks. While not pretending to know the full answer to the puzzle, I have three suggestions to offer.

First, Keynes suffered from an irresistible urge to overstate. He recognizes it himself in *A Treatise on Probability* where he says:

> In writing a book of this kind the author must, if he is to put his point of view clearly, pretend sometimes to a little more conviction than he feels. He must give his own argument a chance, so to speak, nor be too ready to depress its vitality with a wet cloud of doubt. It is a heavy task to write on these problems; and the reader will perhaps excuse me if I have sometimes pressed on a little faster than the difficulties were overcome, and with decidedly more confidence than I have always felt.

This *caveat* should always be kept in mind when reading Keynes, even though he himself may have forgotten it. Both by temperament and by training he was heir to the great rhetoricians of the nineteenth century. This style has its splendours and its fun, but it also has its dangers, and Keynes seems to me to fall very often into the trap of overstatement, that it works up the feelings of the writer quite as much as those of the reader.

Second, by the thirties Keynes's mathematics had become pretty rusty. Although he introduced some algebra into *The General Theory*, he did not do it in a way that added much to the argument. And in the Tinbergen review we come across the following passage:

> Is it possible that there could be a cyclical fluctuation in a system, all the ultimate independent determinants of which had fixed regression coefficients and were in linear correlation with their consequences, except in the case where one of the ultimate determinants is itself a periodic function of time (*e.g.*, sun-spots)? Where and how does the element of *reversal* come in?

He had forgotten the equation describing the motion of a simple pendulum which, as Jeremy Bray has pointed out, appeared in part I of the mathematical tripos examination for 1905. In fact, Keynes never seems to have relied much on his mathematics and when it came to econometrics he can hardly be said to have been conscious of doing any. As a consequence, the subject was one on which his judgment seems to have been uncertain and he tilted at knights and windmills alike with the gusto of a Don Quixote.

Third, in my experience, Keynes' reaction to anything new was to look for the weak spots and shoot them full of holes. This was not the end of the matter but only a way of gaining time, as he usually thought things over and either came up with some really good arguments or changed his mind. In the latter case he seldom said so in so many words, but one discovered that the insuperable objections to the frightful rot one had been talking the other day had somehow melted away and were never mentioned again.

It is my belief that in the end his views had changed considerably. I remember him saying to me towards the end of the war *à propos* of something I had submitted to *The Economic Journal* that he had touched up the text a bit but left my nefarious econometrics alone. But by then this was a joke which no one could possibly have resented.

I think, in fact, that by then he had become clearer in his mind about his own position *vis-à-vis* econometrics. In the middle of 1943 Alfred Cowles wrote to him expressing the wish of the Council of the Econometric Society that he would accept the presidency. Keynes was evidently pleased to be asked but a little hesitant, since under wartime conditions he did not think there would be very much that he could do and, as he continued in his reply, "whilst I am interested in econometric work and have done something at it at different times in my life, I have not recently written anything significant or important along these lines, which would make me feel a little bit of an impostor." However, he did accept the presidency and held it through 1944 and 1945, by which time all his asperity toward econometrics seems to have evaporated. In his last letter to Cowles, dated 23 July 1945, he recounts his extreme satisfaction at renewing contact with Tinbergen, who had been visiting England and whom he had entertained in Cambridge. "I felt once more," he writes, "as I had felt before, that there is no-one more gifted or delightful or for whose work one could be more anxious to give every possible scope and opportunity." This sounds to me like the authentic Keynes and deserves to be remembered more than the incident of 1939.

What were your guidelines as the new director?

They were extremely liberal and congenial. Although in principle the Committee of Management were in charge, they left me amazingly free. The director was expected to initiate the Department's main program of research. As regards the higher posts, he would naturally have the right to propose the people he wanted, would be expected to exercise it fully, and his views would carry very great weight with the Committee. As regards the lower posts, the proposals would normally come from him and be accepted straightaway by the Committee. It was understood that in addition to conducting its own program of research the Department should cooperate with members of the teaching staff.

Could you tell us in more detail about the research activities of the Department in its early days?

This is rather a long story. An account for the first ten years while I was director is given in the first four reports of the Department, but I will try to give a summary here. My idea was to set up an econometric program which would embrace work on facts, work on theories, and work on econometric and statistical methods needed to analyze the facts in the light of the theories.

As examples of the factual I may mention the work on the interwar national accounts which I had started during the war at the National Institute of Economic and Social Research. After the war I continued it in Cambridge, concentrating on consumers' expenditure with the help of Deryck Rowe at the Institute, and we produced the first volume of *The Measurement of Consumers' Expenditure and Behavior* [56]; the second came out in 1966 [123]. In the early days Alan Prest and Arthur Adams took the expenditure back to 1900 and Agatha Chapman and Rose Knight produced a book on interwar wages and salaries. Later Phyllis Deane, John Utting, Kurt Maywald, and Charles Feinstein worked on the project and in 1972 Feinstein carried the whole work, including the parts that had not been published separately, back to 1855 in *National Income, Expenditure and Output of the United Kingdom 1855–1965*.

Another example is the index of industrial production published by Charles Carter, Brian Reddaway (both from the Faculty as distinct from the Department), and me in 1948, the first in the Department's series of monographs [37]. The London and Cambridge Economic Service had published such an index before the war and this one took its place after the war. In addition to a description of the calculations (by Carter), the book contains a discussion of the problems of constructing such an index (by Reddaway) and a short survey of historical statistics of production back to 1907 (by me).

We had two projects on regional accounting in Britain. Phyllis Deane produced aggregated accounts for the regions of Britain. These were not published at the time but I included estimates for 1954 in a paper I wrote in 1960 [70].

In my Newmarch Lectures, published as *The Role of Measurement in Economics* [32], I discussed the question of the introduction of statistical design into the collection of social accounting data. The idea was to replace data that happened to be available from a variety of usually administrative sources with data derived from samples of different sectors of the economy. This idea was tried out on Cambridgeshire by John Utting and Dorothy Cole with a number of assistants. We had considerable success with households, farms, government, and the university world. We were, however, finally defeated by business. There were reasons for this: there was hardly any large-scale enterprise in Cambridgeshire; in the early 1950s businesses were not so used to cooperating over statistics as they have since become; and we were not set up to deal with public relations in an adequate way. We wrote up the successful bits but they were too incomplete to make a monograph though they made a number of papers.

In the autumn of 1950, at the request of the Colonial Office, we began work on the national income of Nigeria. This work gave rise to many problems that were new to us. It was carried out by Alan Prest and Ian Stewart with an African assistant, G.E.A. Lardner. Their report, which related to 1950-1951, was published officially in 1953.

As for the theoretical work we conducted at the time, much of it was in the field of consumers' behavior and was undertaken as a basis for empirical application. There were two major studies. First, the work of Prais and Houthakker on cross-section data, specifically prewar working-class and middle-class budget studies, from which they calculated a variety of forms of Engel curve for a wide range of goods and services, estimated unit-consumer scales, quality variations in consumer patterns and some of the social, occupational, and regional factors in consumption. Second, there was my work on the theory of consumers' behavior and the econometric problems of analyzing time series and combining them with cross-section data which helped to reduce collinearity in the determining variables.

In addition to these studies a number of papers on demand appeared at the time. Alan Prest analyzed the demand for a number of commodities over the period 1870-1914. Michael Farrell noticed that in Prest's study, little of the variation depended on income and prices and introduced the idea of irreversible demand functions in which the upward and downward movements of demand were differently determined; in these the role of income and prices was much increased. Geer Stuvel (from the Netherlands) and S.F. James contributed a cross-section study of the demand for food in Holland in which a number of variables were introduced in addition to income. Finally, I shall mention that for a time James Tobin was a visitor and produced an excellent study of the demand for food in the United States based on time series and cross-section data and also, in cooperation with Houthakker, a paper on the demand for rationed foodstuffs.

After the war was over, the National Institute set up a committee of accountants and economists to report on the treatment of accounting terms and concepts by the two professions. I was a member and so was Frank Sewell Bray, the head of a London accounting firm with a passion for research. He was willing to accept a one day a week appointment at the Department and was immensely helpful to me as at the time I was engaged in work on social accounting with international organizations. Bray wrote a monograph and a number of papers while he was with us. He founded and edited an excellent journal, *Accounting Research*. He left us when I ceased to be director in 1955 but in the meantime he had become Stamp-Martin Professor at Incorporated Accountants' Hall.

We were fairly active in those days. I have given a sample of our activities, though not quite a random one.

How did you consider the work carried out in the Department by young statisticians/econometricians such as Durbin, Watson, Cochrane, and Orcutt for the development of applied work that interested you?

Very highly, and I made considerable use of it in my book on consumers' behavior. It was as a result of the work of Cochrane and Orcutt that I fitted equations to first-differences in my analysis of demand; if not ideal, it certainly helped over serial correlation of the residuals. Similarly I have always given the Durbin–Watson statistic in my time series analyses.

You had a large number of visitors, mainly from abroad, including Brumberg, Prais, Houthakker, Tobin, Anderson, Klein, Leontief, Koopmans, and Frisch to the Department. How did they fit in and what attracted them to the Department?

I traveled a good deal in those days and knew all the people you mention. Incidentally, Brumberg, Prais, and Houthakker were on the staff of the Department and the others were visitors. In my opinion they fitted in very well and I hope they thought so too. As to the last part of your question, there was not much organized econometric research in this country at the time and so we were perhaps the natural group to visit.

Turning now to your other research activities at the time; perhaps you could start by telling us about your work on the analysis of consumer behavior. Your first paper on this subject, "The Marginal Propensity to Consume and the Multiplier," written with your first wife, W.M. Stone, and which appeared in 1938, deals with macroeconomic issues of special interest at the time. What stimulated your later work in this area which was more microeconometric in character?

I continued to write papers on spending and saving functions until the early 1970s [54,60,89,122,145]. They gradually became more elaborate. I introduced a term in wealth, I divided income into permanent and transient components, I distinguished wages and transfers from other income, and I introduced a variable to represent government measures to influence consumption. I had two versions of the model; in one, consumption was confined to perishables, and in the other, net investment in durables was added to saving and the consumption of durables was excluded from income and consumption. I also carried out the calculations for 1949–1966 and then added successive years, ending up with 1949–1970 in order to see how much the parameter estimates were affected. The results came out well; they appeared in a paper I wrote in honor of Tinbergen in 1973 [145]. In it I also examined the saving ratio as a function of the growth rate of income and the transient path of the adjustment of consumption to a maintained unit increase in income.

As for my analysis of consumers' behavior, I suppose you could say that my initial motivation was that of the child who takes a watch to pieces to see how it works. Its first result was my paper on market demand which came out in 1945 [17], was followed by two more papers [31,44] and eventually led to *The Measurement of Consumers' Expenditure and Behaviour.* Then I had the idea of applying the Klein–Rubin model to data on demand and the result of that was the linear expenditure system [53].

Your book, *The Measurement of Consumers' Expenditure and Behaviour in the United Kingdom, 1920-1938*, published in 1954, is by any standards a classical work in applied econometrics. It presents a unified treatment of the problems of theory, measurement and statistical analysis, and it is rich in details. In preparing this work what developments in theory and in statistical methods influenced you most?

Thank you for your kind remarks about my book. I was greatly impressed with the work of Henry Schultz and had in mind to produce something on similar lines. The economic theory in my book is fairly traditional, the main influences in my case being, I think, Pareto and Hicks. The statistical analysis owed much to my colleagues and particularly to Durbin and Orcutt. It is perhaps surprising that I did not discuss Haavelmo's simultaneous equation system. In principle I fully agreed with it but in practice I thought that, with the many other difficulties in time series regression analysis, this one could perhaps be left over for the time being. In my work it got left over forever, I am sorry to say.

In your early work on market demand (published in *The Journal of the Royal Statistical Society*, 1945) and also in your 1954 book you make extensive use of the bunch maps proposed by Frisch to deal with the errors of measurement problem. How do you explain the profession's negative reaction to Frisch's ideas in this field? Did you consider the application of the instrumental variable method to your data?

Yes. Bunch maps figured largely in my early work as a safeguard against the appearance of more than one relationship in my small samples. I do not know how widely they were used even in the early days. I suppose I was persuaded that they were not worth the considerable amount of work involved as I gave them up after a time.

Yes, I did experiment with instrumental variables but I never had any luck with them and they never appeared in my published works.

You also made use of the principal component method in your paper on the interdependence of blocks of transactions. What led you to the technique and how do you view its potential in economic applications?

One of the purposes of statistical methods is to reduce the number of variables with which we begin, and so reduce the labor involved in analysing them. In the present case [26] I began with 17 components of the national income and expenditure of the United States over the years 1922–1938 and showed that 97.5 percent of the combined variances could be accounted for by three orthogonal factors. I also showed that these factors could approximately be identified with the national income, its rate of change, and a time trend. So if we could explain these three variables we could explain the 17 components to a considerable degree of accuracy.

This was the idea, but to gauge how generally useful it is one must realize that my sample was a small one and that other influences may be at work at different times and places. More experience is needed to judge the usefulness of the method in applied economics.

In your paper on the linear expenditure system which appeared in *The Economic Journal* in 1954, you are one of the first to attempt a direct confrontation of theory with the empirical evidence. What stimulated you to take this novel line of research, particularly considering that in your book published in the same year you follow the more traditional approach of estimating double logarithmic demand functions for each commodity one at a time?

In *Consumer's Expenditure and Behaviour* I introduced the prices of competing and complementary commodities into a number of analyses. This improved matters but it was not systematic. I discovered that if we start with the idea that expenditures on individual commodities are homogeneous linear functions of total expenditure and all the prices, and if we then require that the expenditure on all the commodities be equal to total expenditure, that each demand be homogeneous of degree zero in income and all the prices, and that the matrix of elasticities of substitution be symmetric, then we obtain the linear expenditure system.

By getting all the prices into each demand equation without the need to introduce an excessive number of parameters we can study the influence of each price on the demand for each commodity. The model has the great disadvantage that it can only accommodate competitive commodities; complementary and inferior commodities are ruled out. It is therefore necessary to arrange commodities into competitive groups. As shown in *Social Accounting and Economic Models* [67], the model can be extended to a dynamic form from which short-term and long-term elasticities can be calculated.

Later I worked out a model of the components of demand which would have fitted into the consumption function described earlier. This was a linear expenditure system with time-varying parameters [105]. I fitted this system to British data for the period 1900–1960 but I did not put the two models together; and having worked out the consumption model for eight groups, I did not go further and subdivide these groups, though up to a point the model lends itself to this kind of elaboration.

As far as I know the LES was the first complete demand system. It is now a little passé, its main weakness being the assumption of additive preferences, as Angus Deaton pointed out in 1974 in the *Economic Journal.* There are now better models, notably that proposed in 1980 by Deaton and Muellbauer in their splendid book *Economics and Consumer Behavior.*

Geary had also similar ideas. Did you talk to him about your ideas on the linear expenditure system?

No. Geary did derive the utility function underlying the LES, but he and I were in daily contact some years before I got going on this problem and I do not remember that we ever discussed it.

You also did some work on modeling consumption of durable goods with D.A. Rowe.

Around 1960 Rowe and I devoted a certain amount of time to modeling the market demand for durable goods. These are often expensive and render services for a number of years. We divided purchases into a consumption component, which we equated to the reducing balance depreciation on the commodity, and an investment component equal to the remainder. The usual demand equations determine the equilibrium stock or consumption level and perishable goods fit into this system as a special case [63].

A defect of this method is that the annual depreciation rate has to be given; and while in certain cases this can be obtained, our examples related to groups of commodities such as clothing or household durables and so it would have been better if we could have extracted these rates from our data. Marc Nerlove suggested an ingenious way of doing this but when we applied his method we found that the apparent durability of consumers' durables was very small; they were almost reduced to perishables. So we went back to our original method which not only gave reasonably good results but showed that prewar and postwar data could be analyzed by the same equation [72].

In 1966 we brought out the second volume of *Consumers' Expenditure and Behaviour.* We had hoped to use the LES and our model of durables to provide a systematic analysis of the whole of consumption. Unfortunately this never happened: there are no analyses in volume II. I was tied up with the major revision of the SNA and with the Cambridge Growth Project. So we contented ourselves with producing data for the remaining groups and linking all groups to the work of Prest and Adams so as to have uniform series from 1900 onward.

In 1955 you gave up the Directorship of the Department to become the P.D. Leake Professor of Finance and Accounting at Cambridge University. How did this affect your research?

Very little: it really worked out for the best from my point of view. At first I was a little put out as I had come to think of the Department as mine. But it had been agreed that I should continue to work in the Department, and as my chair was entirely concerned with research, that I should have a group to work with me. I soon realized that this was an ideal arrangement since it enabled me to get on with what I was interested in without having to worry about administration.

How did the Growth Project develop?

Alan Brown, who had been working with me at the Department since 1952 [59,88,108] and played an important role there until he left in 1965 to take

European Meeting of the Econometric Society, Kiel, 1955, from left to right: Richard Stone, Ragnar Frisch, Jan Tinbergen.

up the new chair of Econometrics at Bristol, thought it might be a good idea to pull together the Department's work on social accounting, input-output and consumers' behaviour and build a model of the British economy. We discussed the matter, in fact we composed a set of lectures to explain what we had in mind. These appeared in *A Computable Model of Economic Growth* [85] which was published in 1962 and was the first in our Green Book series "A Programme for Growth." The second, which came out a little later in the same year, set out in progressive stages of disaggregation our social accounting matrix for 1960, which we christened SAM [86]. We continued this series, which dealt with various aspects of the model, until number 12 appeared in 1974. We then started a new series, "Cambridge Studies in Applied Econometrics," of which five issues have appeared.

Some of the issues in both series described our projections. The first, *Exploring 1970* (no. 6 in the old series) was written by Alan Brown and appeared in 1965. It was not a forecast but looked at 1970 on the assumption that the economy grew at the rate of 4 percent or a little more during the 1960s compared with rather less than 3 percent during the 1950s. Several variants were given and a do-it-yourself kit was provided which would enable the reader to work out the consequences of changing our assumptions.

Our next *Exploring* volume (no. 9 in the old series) appeared in 1970 and related to 1972. It was written by Terry Barker and Richard Lecomber, who in 1967 had written a report relating to 1972 for the National Ports Council. A new look at the future of the economy seemed desirable since apart from the devaluation of 1967 there were the consequences of the "Kennedy Round" tariff reductions, the effects of the selective employment tax, and the development of North Sea gas.

After this our next forward-looking volume, relating to 1980 and edited by Terry Barker, appeared in 1976 with the title *Economic Structure and Policy* (no. 2 in the new series). This was far more than a set of projections, it was the final volume on the static model, the first stage of our project.

In the meantime, Terry had developed a dynamic version which thereafter replaced the static one. A full description of the new version, *A Multisectoral Dynamic Model of the British Economy* (no. 5 in the new series), edited by Terry Barker and William Peterson, appeared in 1987.

The model has changed a great deal in 30 years. It was never very small but it is now very large, with 5686 variables of which 507 are exogenous (mostly tax rates), leaving 5179 equations. Given the data for a base year, the data for certain preceding years in the case of lagged endogenous variables, and the values of the exogenous variables for the period after the base year, the computer program calculates the endogenous variables year by year into the future. Initially only consumers' expenditure was sensitive to prices, now most of the relevant variables are. Many variables which were previously exogenous have been endogenized. Financial relationships are coming to play an increasing role.

All this has been the work of relatively few people. The team working on the project at any one time was never more than ten, usually six or seven, but it kept renewing itself. Many left to take up chairs or lectureships, among them Alan Brown, Graham Pyatt, Angus Deaton, and Jack Revell. I have rather lost count, but over the years there must have been about 30 people in all attached to it in one way or another.

The project's first sponsor was the Ford Foundation. Later we were supported mainly by the British SSRC (now the ESRC) with sporadic contributions from various government departments. In 1978 it was suggested that we might raise some money ourselves by selling our services, so we set up a company, Cambridge Econometrics, to provide forecasts based on the model, the main research being carried on by the group at the DAE. In 1980 I retired and the project continued for several years at the DAE under the direction of Terry Barker. It is now no longer part of the University and the development of the model has been entirely taken over by Cambridge Econometrics, still under Terry's direction.

Did the Growth Project have the impact you had anticipated?

It is difficult to say. In the 1960s it was clear that the British economy was falling behind its neighbors and competitors and so there was a considerable interest in the idea of indicative planning as a means of enabling the country to improve its economic performance. We had a Department of Economic Affairs and a national plan which aimed to raise the national output by 25 percent between 1964 and 1970; this amounts to 3.8 percent a year over the period, very much the figure suggested in our first *Exploring* exercise. However, the pound had been overvalued since Winston Churchill as Chancellor of the Exchequer decided in 1925 to maintain the prewar parity with the dollar. After a series of devaluations a further one took place in 1967. Following this, as we have seen, we made a projection for 1972 which led to relatively optimistic conclusions. Then the 1970s brought their difficulties, including the oil crisis, and these were reflected in our projections for 1980.

The Treasury and other government departments followed our work and made use of it for specific projects; the report for the National Ports Council I mentioned above is a case in point. But other people were producing forecasting models and the government itself began to set up its own. I don't know how much any of these owed to ours.

In any case it would appear that large econometric models of the economy have ceased to be fashionable and have lost the popularity they had a generation ago. Needless to say, I wholly dissent from this view. I was shocked to be told not long ago that our model was too big, since for policy purposes it seems to me that you need all the detail you can get and that a highly aggregated model is not really suitable. It is foolish to give up the advantages of disaggregation just when computer technology has made it so much easier to cope with.

In constructing the Growth Project model you made extensive use of input–output techniques, and what has now come to be known as the Social Accounting Matrix (SAM) approach. Do you consider the I–O approach to be complementary to the econometric approach?

I have already dealt with the SAM approach, which started in the Project. As regards input–output techniques, which we have always used in our model, I must confess that I am not up-to-date in terminology as I have always thought that input–output techniques were an integral part of econometrics.

So far we have been discussing purely economic questions, but the fact of the matter is that there are social, demographic, and environmental issues that affect the economy. You have made important contributions in the area of demographic accounting. What stimulated your research in this field? Was it merely the extension of your earlier work on national income accounting?

My work on demographic accounting was prompted by the desire to put education and manpower into the Growth Model [99]. This never happened in the way I intended. In the late 1960s I wrote a number of papers on the subject [111,115,116,127,128] and in 1971 the OECD published my *Demographic Accounting and Model Building* [134], which was illustrated by a numerical example relating to the educational system of Britain in the postwar period. In the 1970s this work became merged in the work I was doing for the UN.

This dates from a UN meeting I attended in Geneva in 1969, when I was asked to prepare a report on social and demographic statistics for their Statistical Office. I wrote a first draft for the Committee in 1970 and this was reproduced by my friend and committee member, Prasanta Mahalanobis, in his journal *Sankhya* [136]. Successive drafts went the rounds, like the SNA, in the charge of Aidenoff. The final report was published in 1975 under the title *Towards a System of Social and Demographic Statistics* (SSDS). The report is divided into three main parts: the first deals with the system as a whole; the second with individual sequences and subsystems; and the third with examples and applications. In the last section considerable use is made of models based on absorbing Markov chains.

Although it was made clear that the SSDS was only intended to help countries which were trying to develop socio-demographic statistics and that no country was expected to cover all the topics in the report, the system did not catch on in the way the SNA had. I think I can see the reasons why. Unlike the SNA, it had not been introduced in gradual, easily digestible stages. From the point of view of official statisticians it was long and full of unfamiliar stuff, the taxonomic proposals were very elaborate, and there was a lot of mathematics, which is still apt to turn people off. To my knowledge, the only statistical office that has followed it up is the Central Bureau of Statistics of the Netherlands; their first full-scale report, *Sociaal-demografische Rekeningen*, was published in 1989.

In academic circles, the SSDS was well received by a group of American sociologists who knew of my earlier work in this field, and earned me an invitation to a very interesting conference in Washington [176]. But with this and a few other exceptions it was ignored. I think it fell between three stools, that is, between the three specialisms of economics, demography, and sociology.

The SSDS contains very little that is relevant to the environment but I did write a paper intended to show how far a country should divert resources from the production of regular goods to cleaning up pollution [141]. Meade produced in *L'industria* (1972, pp. 145–1252) a better version of this model, in which it was recognized that the consumer is interested not so much in the amount of cleaning up as in the state of the world after the cleaning up has been carried out. I have always maintained that environmental statistics, along with the national accounts and socio-demographic statistics, were one of the three pillars on which the study of society should rest.

From early on you were very much concerned with the precision of national income estimates and, in 1942, together with David Champernowne and James Meade published a paper on this topic in the *Review of Economic Studies*, and recently you have taken it up again. Could you tell us about your current work in this field?

If one tries to estimate the national accounts it is immediately obvious that with the data available they will never balance without adjustment. So from the beginning I was anxious to devise a technique for balancing them, and David Champernowne, Meade, and I developed a means based on the method of least squares which was published in the paper you refer to [10]. Later in the decade Durbin, while he was at the Department, extended our work to deal with systematic errors and the adjustment of a sequence of years rather than an individual year; unfortunately his study was never published.

The adjustment procedure was not used for many years though I published small constructed examples from time to time. The first application I heard about was by Ray Byron to Malaysian data in 1978 but I later learned that Oleg Arkhipoff of INSEE had adjusted data for the French Cameroons in 1969. In 1981, I balanced a set of 14 accounts for Britain over the years 1969–1979 [183]. Although these were balanced one year at a time and not as a single operation, the results were reasonably satisfactory. In 1982, I looked into the absolute as opposed to the relative accuracy of the adjustments in my 14 account system, again with what I considered satisfactory results [187]. Members of the Growth Project, Frederick van der Ploeg and Martin Weale in particular, have contributed a number of papers on the subject, including one with Barker in the *Review of Income and Wealth* (1984) in which they balanced the Project's system of 262 accounts for 1975.

In order to apply the method it is necessary to obtain measures of the relative errors in the initial estimates. These can best be obtained by the compilers, who can gauge the reliability of the data they collect. Fortunately, the adjustment problem has now been taken up by official statisticians both in Italy and in this country. The 1982 input-output table for Italy was balanced for ISTAT by Paola Antonello and published in 1987. In Britain, a paper entitled "An investigation into balancing the national and financial accounts, 1985–87," appeared in *Economic Trends* for March 1989.

Apart from your work on balancing national income estimates, what other areas have you been working on during the past decade?

Mainly historical topics. In 1986, I gave the Mattioli Lectures in Milan [198]. I chose as my subject "Some British empiricists in the social sciences" and discussed the life and work of 12 individuals from William Petty in the seventeenth century to Charles Booth at the end of the nineteenth. I am now engaged in expanding these lectures for publication.

In 1987 James Meade had his eightieth birthday and in the course of the celebrations I gave a talk about the finance of the War of the League of Augsburg as estimated by Gregory King in 1695 [199]; not so odd a subject as it may sound, since it was the problems of war finance that had brought James and me together in 1940. I ended with a comparison of war finance and expenditure in England over the periods 1689–1698 and 1939–1945. It turned out that King's war, had it lasted to 1698 (it actually ended in 1697), would have cost, reckoned at 1938 prices, £2.29 per head per annum, or about *one-tenth* of the corresponding national income per head; whereas our war cost £47.07 per head per annum, or over *one-third* of the corresponding national income per head.

In addition, I have written two papers with the general title "Some Seventeenth Century Econometrics." The first, on consumers' behavior, gives Engel curves for ten groups of consumers' expenditure at the end of the seventeenth century, the data being taken from King's notebook [200]. The second is concerned with various aspects of public finance in the seventeenth century based on the data of Petty, King, and Davenant [201].

Your work has greatly emphasized the role of measurement. How do you view the role of theory in empirical analysis?

Measurement is important in economics, which is largely a quantitative subject. But left to themselves facts are not very coherent, they need interpretation by the investigator. Theory helps him to do this in a way which makes them consistent with what he knows. But since he does not know everything, he can be quite sure that before long someone will come along who, with a new theory, will make the original observations consistent with a wider range of facts. So one can push a subject on but not reasonably expect to have the last word.

Perhaps you would permit me to end this interview by asking your views about the future of economics and econometrics. What sort of developments would you like to see in our subject?

I will do my best, but I find these general questions rather difficult, because whatever you say, it will seem to some people the most blatant platitude and to others the most arrant nonsense. I think economics and econometrics in particular have come along very well in the past generation. I hope they will continue to progress but with a few changes of emphasis.

In the first place, I hope they will become more empirical because, while theory is essential, its purpose is to help us to interpret and understand the world we live in. Spinning theories is good fun, especially when they are expressed mathematically; testing them quantitatively is a swot but is the only way of finding out whether they have any validity. I know that theorizing is considered a nobler pursuit than number crunching and is therefore held up as the highest achievement to all who aspire to fame. As a consequence, thousands of theoretical papers are published every year. I doubt whether thousands of worthwhile theories are produced every year.

In the second place, I hope that economics will become readier to accept the relevance of other disciplines in the social sciences, and it goes without saying that I hope those disciplines will come to feel the same about economics. Speaking as an economist, I suspect that in quite a few cases the failure of our models may be due to our disregard of noneconomic factors.

Finally, I hope that politicians and administrators will learn to make better use of economics, particularly econometrics. But here I may be entering Utopia, so I had better stop.

1936

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