

KEYNES LECTURE IN ECONOMICS

KEYNES, POLITICAL ARITHMETIC AND  
ECONOMETRICS

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*Read 3 May 1978*

I

IN considering how I could express my thanks to the Academy for the honour they have done me in inviting me to give this lecture, I came to the conclusion that my best course would be to take two of Keynes's manifold interests, namely political arithmetic and econometrics, and discuss his attitude to them. It is in these fields that I have spent most of my professional life; it was in them that Keynes and I worked together when, as a member of the Central Statistical Office, I assisted him during the last four years of the war; and together they constitute an area around which misunderstandings of what Keynes really felt still seem to linger.

The first thing I propose to do is to give my impressions of Keynes's salient characteristics as an economist and try to show the part they played in determining his attitudes at different stages of his life. This might help us to interpret certain pronouncements and initiatives of his which cannot be properly understood out of context. Everyone changes through life to a greater or lesser extent and Keynes was no exception. Furthermore, he was less interested than most people in keeping up an appearance of consistency. It is important, therefore, to know what was in his mind at any one time and not to assume a fixed background of attitudes represented by an average of the changing attitudes over his life span. For instance, the fact that he chose to read mathematics as an undergraduate does not mean that the mathematical aspect of economics held a particular attraction for him at a later stage, when his energies were directed to solving immediate practical problems. In the light of all this I shall go on to discuss his own contributions to econometrics and political arithmetic and the encouragement he gave to the pursuit of these subjects in the world at large.

It is stating the obvious to say that Keynes was gifted with exceptional breadth of interests, agility of mind and power of intuition. My point in saying it is that these qualities led him to expect as much of other people, and in particular of other economists. He expresses his views very forcefully in his obituary of Marshall in *The Economic Journal* for September 1924, where he stresses the variety of talents, as opposed to the exclusive mastery of any one of them, that his model economist should possess. He says:

The study of economics does not seem to require any specialised gifts of an unusually high order. Is it not, intellectually regarded, a very easy subject compared with the higher branches of philosophy and pure science? Yet good, or even competent, economists are the rarest of birds. An easy subject at which very few excel? The paradox finds its explanation, perhaps, in that the master-economist must possess a rare *combination* of gifts. He must reach a high standard in several different directions and must combine talents not often found together. He must be a mathematician, historian, statesman, philosopher—in some degree. He must understand symbols and speak in words. He must contemplate the particular in terms of the general, and touch abstract and concrete in the same flight of thought. He must study the present in the light of the past for the purposes of the future. No part of man's nature or his institutions must lie entirely outside his regard. He must be purposeful and disinterested in a simultaneous mood; as aloof and incorruptible as an artist, yet sometimes as near the earth as a politician. [18, pp. 321–2/173–4]

Whether one takes these resounding words as a declaration of faith or as an heroic-size self-portrait, they are significant in that they show what Keynes's ideal was and why, perhaps, he so often appeared overbearing and contemptuous of other people's endeavours. The ideal was so high, however, that he himself could not live up to it, in the sense that, although he did cultivate at one time or another each of the talents he enumerates, he did not find the way to harmonize them. This is made abundantly clear by his attitude to economic theory and to mathematics.

Keynes's attitude to economic theory is not easy to assess. His youthful reputation was not based on a series of path-breaking theoretical papers. As Austin Robinson has said, 'In the twenties we who were his pupils of the younger generation in Cambridge thought of him, indeed I suspect that he thought of himself, primarily as one who had in unusual degree the capacity to apply to the economic problems of the day the

*corpus* of economic thinking which he had inherited from Marshall' [49, p. 35].

Some insight into Keynes's feelings can be gained from the passages in the obituary in which he refers to Marshall's double nature, as he calls it. Marshall, he says, was at the same time a scientist who sought knowledge for its own sake and a 'preacher and pastor of men' who 'subordinated abstract aims to the need for practical advancement. The piercing eyes and ranging wings of an eagle were often called back to earth to do the bidding of a moraliser'. Keynes obviously did not like the moraliser. Yet a few pages later, in setting forth the reasons for Marshall's delay in publishing his methods and theories, he gives as a 'good' reason the fact that Marshall 'arrived very early at the point of view that the bare bones of economic theory are not worth much in themselves and do not carry one far in the direction of useful, practical conclusions. The whole point lies in applying them to the interpretation of current economic life.' But soon he changes tack again:

I have very early memories [he says] of the sad complaints of my father, who had been able to observe as pupil and as colleague the progress of Marshall's thought almost from the beginning, of Marshall's obstinate refusal to understand where his special strength and weakness really lay, and of how his unrealisable ambitions stood in the way of his giving to the world the true treasures of his mind and genius. Economics all over the world might have progressed much faster and Marshall's authority and influence would have been far greater, if his temperament had been a little different. [18, pp. 321, 342, 345/173, 196, 199]

Thus after much wavering Keynes ends up with the obviously sensible view that one cannot object to a total dedication to theoretical work. Furthermore, this kind of work requires 'the piercing eyes and ranging wings of an eagle', 'treasures of mind and genius', and so on.

I think the wavering is largely due to the fact that, like Marshall, he saw the relationship between theory and practice as one of conflict and, unlike Marshall, he was better at practice than at theory. Indeed, he seems to have been pretty uncertain as to the meaning he wanted to attach to economic theory. At times he seems to reduce it to a bunch of simple principles, at times to exalt it into an extremely complex discipline; yet other times he equates it with its mathematical content, at which point he sees red. And this brings me to the central theme of my lecture.

## 3

Keynes clearly had a 'thing' about the use of mathematics in economics which is more difficult to understand now than it would have been fifty years ago. His attitude is often considered a continuation of Marshall's, as expressed, for example, in the preface to the first edition of the *Principles*, but this seems to me too simple an explanation, especially when one remembers how Marshall himself had altered his point of view by the time he wrote the preface to his eighth edition [42]. For a fuller understanding of Keynes's feelings we had better take a look at his early academic career.

While at Eton, Keynes had shown particular interest in mathematics, classics and history and he came up to Cambridge with a reputation, especially a reputation in mathematics. However, then as later his curiosity was very wide and he engaged in many pursuits besides his tripos subject. It became apparent, as Austin Robinson has said, 'that by the exacting standards of Cambridge he was not in that narrow superlative class which alone can hope to achieve fame in the field of pure mathematics' [49, p. 9]. And again, in the words of Roy Harrod: 'He had no specific genius for mathematics; he had to take pains with his work; while showing efficiency and good style within his range, he did not seek out those abstruse regions which are a joy to the heart of the professional mathematician' [9, p. 57]. Indeed, as the date of the tripos grew near, Keynes wrote to his friend Bernard Swithinbank: 'I am soddening my brain, destroying my intellect, souring my disposition in a panic-stricken attempt to acquire the rudiments of the Mathematics' [9, p. 101].

In 1905 he was ranked twelfth wrangler in part I of the mathematical tripos, a creditable performance you may say but perhaps a little damping to one already accustomed to unqualified success. However that may be, he did not go on to part II of the tripos but instead took, in 1906, the examination for entry into the Civil Service, passed in second and was appointed to the India Office. Oddly enough his worst marks were in mathematics and economics, a result which he characteristically attributed, perhaps quite rightly, to the ignorance of his examiners.

At about this time he started to work on his fellowship dissertation for King's, which was concerned with probability viewed as a logic of partial belief. It was first submitted in 1908 and though the outcome was touch and go, Keynes to his great

chagrin was not elected. However, in the following year he was successful, having in the meantime resigned from the India Office and become a lecturer in economics at Cambridge. He continued with his book on probability until 1912, when he laid it aside, as it turned out for nearly ten years: *A Treatise on Probability* was not published until 1921.

The maths tripos and the work on probability are Keynes's principal mathematical achievements. I have mentioned them here for three reasons. In the first place, he was very young in his mathematical period: the *Treatise* was virtually completed before he was thirty. In the second place, each of them contained elements of disappointment: both were good performances of which most people would have been proud, but Keynes was self-demanding, ambitious and sensitive and at the time he felt some discontent. In the third place, neither seems to have given him much pleasure: as an undergraduate he found mathematics so uncongenial that he dropped out in mid-tripos; and the *Treatise* was made to give precedence to policy preoccupations for nearly a decade.

And so it is not difficult to understand why, with his multiple gifts and great opportunities, he became more and more attracted by the active as opposed to the speculative life and ended up quite differently from the way he had begun. His concern with economic policy, which was to become a predominant passion, may be said to date from his years in the India Office and to have been consolidated in his period at the Treasury during the First World War. This passion left him less and less leisure for theoretical and mathematical thinking and in the course of time he forgot much of his mathematics.

What is surprising, or would be in a less self-centred and passionate man, is the violence of his hostility to the use of mathematics in economics and his eagerness to belittle both its difficulties and its potential usefulness. The obituary is peppered with snide remarks about mathematical economics which, though attributed to Marshall, have an unmistakable Keynesian ring and are pressed home with typical Keynesian insistence. I shall give one example. 'Marshall', he says, 'as one who had been Second Wrangler and had nourished ambitions to explore molecular physics, always felt a slight contempt from the intellectual or aesthetic point of view for the rather "potty" scraps of elementary algebra, geometry and differential calculus which make up mathematical economics.' He then proceeds to emphasize the point with two notes, the second of which is worth quoting because a revealing story hangs upon it.

The footnote begins: 'Professor Planck of Berlin, the famous originator of the Quantum Theory, once remarked to me that in early life he had thought of studying economics, but had found it too difficult! Professor Planck could easily master the whole corpus of mathematical economics in a few days . . .' [18, p. 333/186]. The rest of the story is told by Harrod:

I happened to sit next to Keynes at the High Table of King's College a day or two after Planck had made this observation, and Keynes told me of it. Lowes Dickinson was sitting opposite. 'That's funny,' he said, 'because Bertrand Russell once told me that in early life he had thought of studying economics, but had found it too easy!' Keynes did not reply. [9, p. 137]

He did not reply, but Russell's high-table witticism seems to have struck a sympathetic chord. In any case he evidently thought it too good to be lost and so he passed it on to the public apropos of Planck. In order to do so it was necessary to give a perverse twist to the word 'economics', but this kind of thing never worried Keynes much. Like Humpty Dumpty he believed in paying his words extra.

His animosity had not abated in the thirties. In *The General Theory*, after a page of invective against 'blind manipulation' and 'symbolic pseudo-mathematical methods' we come to the well-known sentence: 'Too large a proportion of recent "mathematical" economics are mere concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols' [23, p. 298/298].

Such intemperate words would not have mattered had they not appeared in an important book by an important man, whose emotional utterances have been received and transmitted as gospel by so many people. Keynes did have a point. Building mathematical models can indeed be turned into a game of little value to anybody except the player. But it is unscientific, I would almost say frivolous, to generalize from the abuses of the few.

One could say in extenuation of Keynes that not even he could have foreseen his posthumous apotheosis. Had he foreseen it he might have realized the responsibility it entailed and chosen his words more carefully. I can hear those high-table wits exclaiming that he would have made them stronger still; for all I know, he would. Be that as it may, his words as they stand have caused a great deal of obscurantist propaganda, as petty and ill-judged as the present reaction against him is.

This propaganda has poisoned the air both in the field of mathematical economics and in that of econometrics, which in many people's minds are inextricably confused. It seems obvious to me that in the thirties Keynes himself did not distinguish very clearly between the two. We find the same animosity venting itself in his review of Tinbergen and in the increasingly short-tempered correspondence with Harrod that led up to it [25].

In 1938 Keynes was sent the proofs of Tinbergen's two studies for the League of Nations, *A Method and its Application to Investment Activity* and *Business Cycles in the United States of America* [57, 58]. His immediate reaction to both was negative: 'I confess that I have the utmost difficulty in making head or tail of them' he wrote in his reply [26]. His accompanying critique dealt mainly with the logic of the method, faulty in his opinion in that it brushed aside a number of basic difficulties such as the heterogeneity of the material through time, the inconstancy of the coefficients, the limitations of linearity, and so on. In his review of the first volume, which appeared in *The Economic Journal* for September 1939 [27], he emphasized the same points, harking back to his strictures on statistical inference in part V of *A Treatise on Probability*. The review is a model of testiness and perverseness. Apart from a number of misunderstandings on mathematical questions, Keynes seems positively to resent all attempts to overcome what he recognizes as 'the frightful inadequacies of most of the statistics employed'. Sensible procedures for fitting trends or arriving at time lags are 'arbitrary'. 'A free choice of regression coefficients', by which I take him to mean the choice among alternative or associated determining variables, can lead, he says, 'to strange results', rather like the number of the Beast in Revelation. He seems to have learnt absolutely nothing from his reading, even on topics that were up his street such as the dependence of investment on the difference between profit rates and interest rates.

Tinbergen replied to the review in *The Economic Journal* for March 1940 [59] and in the same issue Keynes commented briefly on this 'very valuable reply'. At the invitation of the editors, Tinbergen had also contributed a paper on econometric business-cycle research to *The Review of Economic Studies* for February 1940 [60]. A year later Koopmans joined in the debate with a paper on the logic of business-cycle research in *The Journal of Political Economy* for April 1941 [34]. In the eyes of the younger generation these exchanges, counterproductive from Keynes's point of view, doubtless increased the interest in

econometrics which *The General Theory* had unwittingly stimulated.

For there is no doubt that in its day Keynes's book had done probably more than any other to encourage the systematic estimation of national accounts magnitudes and the construction of econometric models. It is odd that Keynes, a Fellow of the Econometric Society since its inception in 1930, should have shown so little awareness of econometric literature and of Tinbergen's already substantial contributions to it, and should have failed to recognize in the new method just what was wanted to quantify the multiplier and other parameters of *The General Theory*.

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. [17, p. 427/467]

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, as I have said, 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 [1], 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 judgement 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's 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 his saying to me towards the end of the war apropos 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 it 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 towards 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 wrote, '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.' Nothing could show better the difference between Keynes's first impersonal impressions and his considered view based on personal experience.

## 4

Keynes's dislike of mathematics did not extend to political arithmetic. His capacity for observing and assimilating facts was remarkable, and facts for him included numbers. He liked to get a feel of the order of magnitude of the problems with which he was dealing and acted on Dr Johnson's saying: 'That, sir, is the good of counting. It brings everything to a certainty, which before floated in the mind indefinitely.' Like many others in the twenties and thirties he deplored the state of British official statistics and tried hard to improve them. But the times were not propitious, as the following episodes show.

In 1919 the Royal Statistical Society sent a petition addressed to the Prime Minister requesting that an inquiry should be made into the existing method of collecting and presenting public statistics [50]. It contained many distinguished signatories, among them Keynes. A reply eventually came in 1921 in the form of a printed report by a committee appointed by the Cabinet [61]. In preparing it no expenditure was incurred other than £30 for printing and publishing, and it can best be described as a complete brush-off. The committee denied that a case existed 'for an enquiry, whether by a Royal Commission or a Parliamentary Committee, into the present method of collection and presentation of official statistics'. The committee was opposed to the establishment of a central statistical office designed to provide a complete statistical conspectus of such subjects as the national income and wealth, even if this were done without radical interference with the existing organization of departmental statistics. They emphasized that: the purpose to be served by statistics must be recognized as of national value; the cost must be reasonable in relation to that purpose; any inquiries needed must not be such as to excite irritation and resentment; and any compulsory powers required must be such as Parliament may be willing to grant. They did, however, recommend that 'for the purpose of ensuring more effective

co-operation and co-ordination between the different departments in their statistical work, a permanent Consultative Committee of statistical officers should be established'.

Subsequent efforts to set up an economic general staff were not conspicuously successful, as exemplified by the history of the Committee on Civil Research in the twenties and the Economic Advisory Council in the thirties. These efforts have been admirably documented by Howson and Winch [10].

An indicator of a different kind is provided by the Inland Revenue's *Report on National Income* which was printed in 1929 and sent to the Chancellor of the Exchequer but not published. The reasons given for this lack of openness are scarcely convincing: the estimate for 1924 was slightly less than the figure already published by Bowley and Stamp and this was partly due to a lower estimate of the national wages bill. I have heard it said that the opposition to publication came from the Federation of British Industries, though how they could have known about it is a bit of a mystery since it is marked confidential, I have never seen any reference to it and I have never known anyone outside the official world who was aware of its existence. However, it is a most interesting document and last year, with permission, I published it [64].

In spite of the adverse climate of opinion, Keynes did not throw up the sponge, as can be seen from the recommendations on the subject, usually associated with his name, in the Liberal Yellow Book of 1928 [36, pp. 121-5] and in the Macmillan Report of 1931 [62, pp. 174-85].

The Yellow Book, after expressing the opinion that 'the nationalising of knowledge is the one case for nationalisation which is overwhelmingly right', points out that 'the deficiency of vital information and the ineffective publication of the information which we have are—as those who have conducted this Enquiry have learnt to their cost—scandalous and disgraceful'. 'How', the report asks, 'can economic science become a true science, capable, perhaps, of benefiting the human lot as much as all the other sciences put together, so long as the economist, unlike other scientists, has to grope for and guess at the relevant data of experience?' The report goes on to recommend the fuller and more speedy publication of data already collected and the extension of existing areas of collection to cover 'stocks of commodities, the state of the order books in leading industries, the volume of trade, the volume of goods transported, index numbers of wage rates and of hours actually

worked, indexes of the quantity of electrical power in use, and a large mass of banking and monetary statistics which are at present veiled in deep mystery and gloom'.

The relevant chapter in the Macmillan Report, chapter V, opens with a paragraph which is sufficiently interesting and forward-looking to be quoted in full:

Exact quantitative knowledge concerning the chief elements of the monetary and financial system is, we consider, of the utmost importance, both to provide the necessary data on which to base the management of the system and also for the purpose of making gradually possible a more definitely scientific treatment of these problems than the existing state of our knowledge of the facts allows. There are, moreover, many matters of importance which are now the subject of controversy, yet need not be so if they could be put to a statistical test. We should aim, therefore, at obtaining a complete inventory of the economic life of the community under its several aspects in such a form that we could cross-check the accuracy of our information by being able to work up to the final totals from more than one direction.

This programme of action is followed by a plea for substantially increasing the expenditure on statistical departments. The Report then goes on to make detailed recommendations concerning monetary and financial statistics on the one hand, and general industrial and economic statistics on the other. The first heading covers, among other matters, the foreign balances and the balance of trade; and the suggestion is made that the volume of cheque transactions should be so classified as 'to make it possible to separate, so far as practicable, the figures arising from Stock Exchange and money market transactions from transactions arising out of the earning and spending of the community's current income'. The second heading covers the census of production, the volume of wages paid, the volume of retail sales, the aggregate and the distribution of profits, the value of capital construction and the co-ordination of statistical work of different departments. On this last question the Report deplors the lack of industrial and regional comparability in the classifications used by different departments. It also recommends powers to exact certain information, not because most people would be unwilling to supply it but because 'the value of statistical returns can be greatly impaired by the recalcitrancy of a few of those from whom figures are required'.

If carried out, the measures advocated in the Macmillan Report would certainly have improved official statistics and helped to realize the programme outlined in the paragraph

I quoted above. Patinkin has suggested that at the time Keynes might have done more, especially with regard to national income statistics [46, p. 1115]. Given the degree of official and unofficial obstructionism, I am not sure that even Keynes would have succeeded had he only tried harder. Still it could be argued that he might have tried. I am rather doubtful about this for two reasons.

First, Keynes, then as always, had many irons in the fire and no one knew better than he that success in championing an unpopular cause involves tedious preparation and the using up of goodwill which might be otherwise employed. Even if he had wanted to, I do not believe he would have considered the time ripe for stepping up still further the fight for better statistics.

But apart from this, and despite what is said in the Macmillan Report about obtaining a complete inventory of economic life, I think it unlikely that in those days Keynes contemplated any grand scheme for the integration of economic statistics. There is little hint of it in *A Treatise on Money*. It begins to take shape, though it is not much in evidence, in *The General Theory*. It becomes fully apparent only in *How to Pay for the War*, where he writes:

The statistics from which to build up these estimates are very inadequate. Every government since the last war has been unscientific and obscurantist, and has regarded the collection of essential facts as a waste of money. There is no-one to-day, inside or outside government offices, who does not mainly depend on the brilliant private efforts of Mr. Colin Clark (in his *National Income and Outlay* [4], supplemented by later articles); but, in the absence of statistics which only a government can collect, he could often do no better than make a brave guess. [30, p. 13/381]

By this time Keynes fully accepted the need for a co-ordinated framework of economic statistics in his own work, as we shall now see.

## 5

Of all Keynes's manifold activities in the Second World War, the one most clearly relevant to my theme is his contribution to the solution of problems of war finance. In this we see him in action at all levels: devising and refining the general approach, obtaining through his assistants the necessary statistics, mounting in the press, on the wireless, by letter, interview, lecture and pamphlet a widespread campaign of persuasion and finally, when working inside the Treasury from the middle of 1940 as a member of the Chancellor's consultative council, playing a

leading part in crystallizing inside opinion preparatory to the Budget of 1941 and after.

In all this Keynes got off to an early start. In October 1939 he gave a talk to the Marshall Society in Cambridge on 'War potential and war finance'. In mid-November a revised version of this talk appeared as two articles in *The Times* [28] and these were followed by a paper entitled 'The income and fiscal potential of Great Britain' which appeared in *The Economic Journal* for December 1939 [29]. By the end of February 1940 all this material had been rewritten and was published as a pamphlet, *How to Pay for the War*.

The analysis here is set in a framework of national income and expenditure statistics. The estimates were prepared by Erwin Rothbarth, at that time assistant in statistical research at the faculty in Cambridge, and were obtained by bringing up to date Colin Clark's figures. It is sad to remember that at the end of 1944 Rothbarth was killed in action in Holland, having joined up after his release from internment as an 'enemy alien'.

An offshoot of the pamphlet was Keynes's note on the concept of national income which appeared in *The Economic Journal* for March 1940 [31]. It explains the two main totals he was using, namely taxable income, that is private income from productive activity and transfers, and national output, that is the net national product at market prices. He strongly defends the national output against Clark's gross national income, which is equal to the national output plus depreciation. Personally I am inclined to agree with Keynes though there are arguments on both sides. Nowadays I do not think that anyone would be confused but at the time they were. I am not giving these technical details here in order to air my views on them but because, as I explained in *Lessons of the British War Economy* [53], the initial motivation for publishing official estimates of national income and expenditure arose out of a misunderstanding about these very concepts. And this is when my personal connection with Keynes began.

In June 1940, shortly before Keynes went to the Treasury, James Meade joined the Central Economic Information Service of the War Cabinet Offices and started to work on financial aspects of the war economy, an undertaking which I believe had been requested by the Survey of Financial and Economic Plans. This committee was under the chairmanship of Stamp and was attached to the Cabinet Office over the period 1939-41. By August, Meade had completed a set of tables illustrating his

method of analysing the financial aspects of the war economy and he sent a copy to Keynes. The tables consisted of private and public income and outlay accounts, a foreign account, and an elaborate system of capital accounts. I was brought in at the end of August with the idea of helping with the statistics.

I do not know how far Keynes knew what we were up to. I have always attributed the initiative for this undertaking to Austin Robinson, so he must have known something; but we did not see him until December, by which time we had completed an initial set of estimates. These estimates did not achieve the detail proposed in Meade's tables, it would hardly have been possible to set out capital transactions in such detail for some years to come, but they did incorporate a national income and expenditure account. The period we covered was 1938 and then 1940 by quarters. Nothing was published on 1939 until the 1944 White Paper. This was rather a pity since Keynes's analysis of the sources of war finance related initially not to calendar years but to war years.

We sent our results to Keynes in December. He responded favourably and quickly. By early January 1941 he had circulated them within the Treasury and started to discuss the question of publication. At first he thought they might be published privately, and it was only in the course of February that the idea of an official publication caught on. The first proofs were circulated about 10 March. There followed the great Battle of the Commas between Keynes who disliked commas and Francis Hemming, my immediate boss in the Cabinet offices, who loved them. The battle raged over upwards of fifteen sets of proofs but in the end Keynes put his foot down and the White Paper Cmd. 6261 appeared on Budget Day, 7 April 1941 [66]. Part 1 contained Keynes's analysis of the sources of war finance; and part 2, our tables of national income and expenditure.

The reactions of economists who were consulted on the White Paper are interesting [44, pp. 325-53]. Henry Clay at the Bank of England was throughout in favour, writing to Keynes early in March: 'The proposed White Paper is a bit of a revolution. I welcome it; the public are much more likely to be co-operative if they know what is required of them.' Dennis Robertson, admittedly rather late in the day, made some suggestions for improving the paper which, given time and ignoring a number of decisions that had already been taken, could have been met; for instance, the original proof had quarterly figures for 1940

in tables B and C as well as in table A. However, he concluded: 'If the paper can be strengthened in these ways, I think the balance of argument is strongly in favour of its publication.' Hubert Henderson, I think to Keynes's disappointment, was the least happy and drafted an imaginary article to indicate 'the kind of criticism which I should be disposed to bring against the White Paper in its present form if I were reviewing it in an unfriendly spirit'. Nevertheless, subject to some revisions, aimed more against part 1 than part 2, he thought it would be desirable to publish it.

In his Budget speech [63] the Chancellor referred at an early stage to the White Paper with its estimates of national income and expenditure, 'the very valuable firstfruits of our new Central Statistical Office set up by the Prime Minister'. He explained why he was publishing this material and warned that the publication must not be looked upon as one of a new annual series. Keynes had prepared copious notes for the Chancellor [44, pp. 294-325] and the argument developed along the lines that he had been advocating. In the coming financial year the government's domestic expenditure was put at £3,700 million, of which it was expected that £1,636 million would be met by existing taxation and £1,600 million might be available from the savings of extra-Budgetary funds, depreciation and maintenance not made good, and private saving. This left an inflationary gap of about £500 million, towards which it was thought that some £200 or £300 million could be expected from increased private saving, leaving about £250 million to be raised in new taxation. The standard rate of income tax was raised by 1s. 6d. to 10s. in the pound, yielding £125 million in a full year, and the exemption limit and allowances were reduced to yield a similar sum. This last item was matched by postwar credits.

The budget was well received and the new White Paper, despite the Chancellor's warning, became a fixture.

At the end of 1940 the Office in which Meade and I had been working was divided into the Economic Section and the Central Statistical Office, both located in the Cabinet Offices. After the Budget, Meade was attached to the Economic Section and I was left with the national income. Actually, but for Keynes I would probably have found myself editing a digest of oil statistics. When I mentioned this rather bleak prospect, Keynes said: 'I'll soon stop that. I shall arrange for you to be appointed my assistant and you will take your orders from me.' This saved

the national income and in all respects was a very good arrangement since Keynes showed immense interest in the development of the work.

This interest took many forms. Naturally he was concerned with the arrangement and presentation of successive White Papers and the way they were used in the Budget speeches. He also gave much attention to conceptual problems and we spent a lot of time discussing them. As often as not perhaps the *mot juste* would be arguing heatedly about them, and the meeting would frequently end by Keynes telling me that he never wanted to see me again. This was rather worrying when it first happened but there was really nothing to worry about. If there was anything personal in his manner it was momentary and in a day or two I was round again and we were off on something else. As we often met late in the day he was probably tired, a possibility which I regret to say I too often forgot.

Another aspect of the work on which Keynes was active was the improvement of the data. Many departments, including the Bank of England, were press-ganged into this, although Keynes, with what seemed to me an encyclopaedic knowledge of everything that was going on, was in fact extremely good at spotting missing items himself. Often I would find on my desk a note saying, as it might be, 'What are you doing with the Government Tonnage Replacement Account?' Well, actually, I was not conscious of doing anything, as I had never heard of it. But I supposed it was tucked away among the extra-budgetary funds and it was not very difficult to find out about it and decide what to do. It was rather a point of honour to answer this kind of question the same day or the next, and this gave the work a continuity and liveliness which were very satisfying.

It is sometimes said that Keynes held strong views on orders of magnitude and was very difficult to shift from his preconceptions. This is to some extent true but it had its good side as well. He did more than anyone else I have known to break down the Cult of the Zeros, by which I mean the practice then common among statisticians of writing down zero when what they meant was that no reliable information was available. I shall give a typical example. At an early stage in a discussion on estimates of the balance of payments we were presented with a table containing the usual zeros against a number of items. 'Look,' said Keynes, 'you know as well as I do that the change in Commonwealth balances cannot have been zero last

year: what do you think it was?' 'We really don't know', was the reply, 'but probably between three and four hundred million.' 'Then put it down at £350 million' said Keynes 'and try to get some accurate information in the future, for by your own admission it is very important.'

Keynes continued to concern himself with the White Paper but in a sense that battle had been won and he had many more pressing preoccupations as the war years rolled on. I will mention therefore only one or two further incidents from this period which link up with some of his earlier interests.

At the time of the 1943 White Paper I put forward some suggestions for a set of alternative price index-numbers which I hoped might eventually be incorporated. At some stage in the discussion Keynes minuted: 'I find the whole thing fascinatingly interesting. It will be a great addition to the White Paper if this can be got out in sufficiently good shape to deserve formal publication.' Unfortunately it could not. But what a difference in attitude from the rather crabbed treatment of index-numbers in *A Treatise on Money* thirteen years earlier. I suspect it was often so: what Keynes wrote when he contemplated the literature as a non-participant and his reactions when he himself was on the breach were quite different.

About this time, or it may even have been a little earlier in connection with the Beveridge report, we attempted to estimate the probable size of the national income after the war. This got us into model-building; nothing very sophisticated, but the beginning of what came to be called national budgeting models which later were used for a time in the *Economic Surveys* [67]. As briefly described in a paper by E. F. Jackson, who worked with me on the national accounts and took over when I left the civil service at the end of the war, these models were essentially a tool for imposing national accounting constraints on the forward thinking of a number of committees concerned with economic projections [11]. Keynes, as far as I remember, did not play a very active role in this work but he thought it interesting and encouraged it.

Finally, I am glad that Moggridge has reminded us of a phrase that I often heard Keynes use in the later years of the war. Apropos of the White Paper on *Employment Policy* of 1944 [65], Moggridge quotes him as saying that 'theoretical economics has now reached a point where it is fit to be applied' and foreseeing 'a new era of "Joy through Statistics"' as the policy came into operation [43, p. 127].

## 6

Apart from the passages which I have already cited, Keynes's writings contain a number of items which fall clearly within the scope of this paper. Generally speaking I shall handle them chronologically, beginning with *A Treatise on Probability* [17] which only appeared in 1921 but was, as we have seen, first submitted in an initial version as a fellowship dissertation in 1908.

(a) *Statistical inference*. I shall not attempt to deal with the *Treatise* as a whole and shall restrict myself to part V, which is devoted to the foundations of statistical inference. This subject constitutes the principal means available to economists for bringing facts and theories together and is highly relevant to the interpretation of econometric results.

Keynes begins his discussion of the theory of statistics with a clear distinction between its descriptive and its inductive aspects. If, for instance, we calculate for a country with a good registration system the proportion of male births in a given year, we can regard the result as descriptive of that country and that year; but what can we infer about a region of that country or a different year? Keynes is anxious to warn the reader against two presumptions: first, that because an effect has been found on one occasion or in one sample, it will necessarily be found in another; and, second, that a general answer can be found by the application of some simple mathematical rule. Most people will agree with these warnings, but two points should perhaps be added. First, in the absence of further information it may be more sensible to assume that next year's birth-rate is the same as this year's than to pick a number off the ceiling or refuse to say anything about it. Second, in the matter of rules, Keynes, like Venn and many other writers before him, entirely rejects Laplace's rule of succession; this is not a rule that is very likely to be invoked by economists.

In developing his own ideas, Keynes follows what he terms the *inductive* method, which he associates with Lexis, rather than the *mathematical* method, which he associates with Laplace. The essence of the inductive method is an examination of the stability of the relative frequencies in various subsets of the data.

This is clearly a useful procedure but not without its difficulties. If the subsets are made very small, the results tend to become a prey to errors and mis-specifications. If we see the main danger as heterogeneity, the subsets tend to become intolerably numerous. It may be that the parameters in our model are not really constant and that what we need is a more

complicated model. Or it may be that they have been approximately constant in the past but that predictions go wrong because of an unexpected break in the structure of the system. Our ability to estimate parameters may be affected by correlated errors, simultaneous equations bias and a host of other considerations.

From an economist's point of view Keynes, it seems to me, did not get very far with a constructive approach; but then he was writing a general book on probability, without special reference to economics, at a time when econometrics could hardly be said to exist and at an age when, even if it had existed, he was too young to have had much practical experience of its real difficulties. Consequently he tends to rake over old arguments in a spirit very different from that which animated him when he was himself committed to the solution of a problem.

The book ends with a discussion of the use of probability theory in physics and biology and in this context Keynes is less negative: 'Here,' he says in his concluding paragraph, 'though I have complained sometimes of their want of logic, I am in fundamental sympathy with the deep underlying conceptions of the statistical theory of the day.'

(b) *The controversy over parental alcoholism.* In 1910 the Galton Laboratory produced *A First Study of the Influence of Parental Alcoholism on the Physique and Ability of the Offspring* by Ethel M. Elderton and Karl Pearson [7]. The study seeks to reach conclusions on this issue by comparing many characteristics of the children in two samples, taken in Manchester and Edinburgh, of what I shall call, for short, temperate and intemperate families. The paper is written in a calm style, pleads for the dispassionate study of social questions, and in the concluding paragraph contains the following passage: 'Yet the time is approaching when real knowledge must take the place of energetic but untrained philanthropy in dictating the lines of feasible social reform. We can only hope that this intrusion into the field of alcoholic inquiry will be recognised as an earnest attempt to measure the true influences of a grave social evil.' It comes, however, to a surprising conclusion: 'To sum up then, no *marked* relation has been found between the intelligence, physique or disease of the offspring and parental alcoholism in any of the categories investigated. On the whole the balance turns as often in favour of the alcoholic as of the non-alcoholic parentage.'

Keynes reviewed this memoir in the *Statistical Journal* almost as soon as it appeared [13]. I am not sure what led him to do this but it would clearly have been of great interest to him in connection with his work on probability and statistical inference. Furthermore, Udny Yule had recently reviewed an earlier paper in the memoir series and, while allowing it some value, doubted whether the material was anything like strong enough to bear the conclusions reached as regards the influence on intelligence of physique and of environment [69].

Keynes's review was exceedingly sharp but I think he had some good points of which perhaps the most important was the following. The Edinburgh sample, the only one which was publicly documented at the time, appears to have been drawn from a poor, slum district not representative of even the lower ranges of the working-class population. Keynes argued that 'the authors are comparing drunken stock with bad sub-normal sober stock, and find, naturally enough, that there is not much to choose between them.' The review ends with the remark: 'As a study in statistical method it is a salient example of the application of a needlessly complex mathematical apparatus to initial data, of which the true character is insufficiently explained, and which are in fact unsuited to the problem in hand.'

In the controversy which followed, the correspondence columns of *The Times* soon became full of letters from Pearson, Marshall and many others. The original memoir ran into a second edition and Pearson published a supplement to it in reply to 'the Cambridge economists' in which he attempted to defend himself, conceding virtually nothing to his critics [47]. Keynes replied to the new publication in a letter to the *Statistical Journal* for December 1910 and this was followed by letters to the same journal by Pearson in January and by Keynes in February 1911 [14, 48, 15]. By this time a great deal of heat had been generated without any appreciable change of position on either side. This aspect of the controversy faded out and Pearson turned his attention to his medical critics. In reading this exchange I was struck by the marvellous speed with which authors appeared in print seventy years ago.

(c) *Index-numbers*. In 1909 Keynes wrote a long essay on index-numbers which won him the Adam Smith Prize in that year. He returned to this subject later, briefly in section 11 of chapter XVII of *A Treatise on Probability* and at length in book II of *A Treatise on Money*, which appeared in 1930.

The essay begins with the theory of statistics divided into three main divisions which may be termed collection and presentation, measurement, and inference. Index-numbers belong to the second of these divisions. Keynes, relying mainly but not uncritically on the reports of the British Association's committee on index-numbers and the memoranda supplied to it by its secretary Edgeworth [2, 6], and on Walsh's *The Measurement of General Exchange Value* [68], limits his discussions to price indices, basically the measurement of general exchange value, and approaches the subject from two points of view: the price movements of a composite commodity representative of some interesting aggregate of transactions; and the common factor in price movements seen in terms of probability theory.

In his treatment of the first method Keynes discusses the type of aggregate of which the composite commodity is representative. He is mainly interested in two: the principal raw materials of industry, leading to what he calls the trade index-number; and the finished products actually consumed, leading to the consumption index-number. Nowadays this issue would invite a discussion of systems of index-numbers defined in terms of aggregates in the national accounts but this all came very much later and, not surprisingly, we do not find it in 1909 or even in 1930.

In constructing index-numbers Keynes lays great stress on the importance of weighting, on the grounds that in some of their writings both Edgeworth and Bowley had suggested that it was not of great importance. While excessive attention to weighting is unlikely to lead to much improvement, approximately correct weighting is essential. In most of the index-numbers of the time, weighting, if present at all, was extremely perfunctory; on the other hand, even assuming the data to be available, it would not always have been easy to see what weights were appropriate before the national accounts made possible a systematic approach to the relationships among index-numbers.

On the question of formulae, Keynes refers to the aggregates of Laspeyres and Paasche and the Marshall-Edgeworth intermediate version. He also gives strong support to the chain index proposed by Marshall in 1887 [41]. Although, in general, this index does not satisfy the circular test, it is in many ways an attractive formula; but it does require more data since it is necessary to know the quantities as well as the prices in each period, a point which Keynes seems to have overlooked. An

alternative which provides some of its advantages is to arrange for a regular change in the weights, say every five years, leaving them fixed within each quinquennium.

The second method considered by Keynes can be described as follows. Suppose that the variations of prices depend on a general factor, associated with money, and a number of specific factors, associated with supply and demand conditions in the markets for the different commodities. If we could isolate the general factor in a set of price series, we could adopt it as a measure of the price level. For instance, if money exercised a proportional effect on all prices, and market conditions contributed independently their specific components to the movements of individual prices, then we might take an unweighted geometric average of a sample of prices as the required measure.

Keynes was highly sceptical of this approach and gave many arguments for rejecting it. Yet, in another branch of the study of man, psychology, investigations had begun on a formally similar problem only a few years before Keynes wrote his essay. In 1904 Charles Spearman published his paper on "General intelligence" objectively determined and measured', a landmark which was the beginning of factor analysis [51]. If in this theory we replace tests by commodities, pupils by years and marks by price quotations we obtain a structure in many ways similar to the one with which Keynes was concerned. Indeed in 1960, in a paper entitled 'Best linear index numbers of prices and quantities', Henri Theil showed how to analyse matrices of series of prices and quantities by an extension of the method of principal components, thus making it possible to see how the two approaches are related to one another [56].

In the final chapter of his prize essay Keynes discusses some of the index-numbers that existed at that time. His criticisms are directed mainly at: (i) the small number of items included; (ii) the lack of, or extremely rudimentary nature of the weighting; and (iii) the remoteness of the base years. He does not go into problems associated, for instance, with quality, fashion or variety, or into questions of the best indicator to use when the available data are not ideal, which do or should plague the practising statistician.

The essay ends with an appendix which sets out to relate various types of average to the underlying distributions which would justify their use. A revised form of this appendix appeared in the *Statistical Journal* for February 1911 [16].

Keynes's published work on index-numbers formed book II of *A Treatise on Money* [19]. Compared with the essay, it is revised and rearranged and contains references to the literature of the intervening twenty years. Keynes still treats the making of index-numbers as essentially a statistical problem; and although he introduces the method of limits, he does not otherwise consider the economic theory of index-numbers based on indifference curves, utility functions and so on, which had been pioneered many years earlier by Pareto [45].

Keynes emphasizes at the outset that the purchasing power of money can be measured by the price of a composite commodity to be found *par excellence* in the basket of goods and services purchased by consumers. There are of course other baskets, which give rise to a plurality of secondary price levels and whose study is useful for particular purposes. He returns to the concept of the intrinsic value of money and the theory of probability as a basis for the construction of index-numbers, and now gives it as his view that 'such ideas . . . are root-and-branch erroneous' [19, vol. I, p. 85/76]. Since the tide of opinion was so clearly running in his favour, this all seems needlessly backward-looking and no justification for the derogatory comments on Cournot, Jevons and Edgeworth which accompany it.

The final part of book II is devoted to a discussion of methods of comparing purchasing power. Keynes considers a direct method consisting of asking for the general impressions of someone with experience of the two situations to be compared. This might be useful to a person wondering whether or not to take a job abroad, but it is hard to imagine how it could be made to yield general, numerical comparisons which were reasonably objective. He then goes on to what he terms indirect methods, that is those normally in use. He starts by dividing the two baskets into a common component and components specific to the two periods to be compared. As far as possible these specific components should be dealt with in terms of the substitution possibilities of the goods and services that they contain. A detailed discussion is needed at this point since some of the main practical problems of index-number construction arise from the changing nature of the goods and services bought and the changing tastes of their buyers; but Keynes does not provide this discussion and leaves the impression that not very much can be done. He concludes by: (i) considering the method of limits but in a way in which, as Staehle pointed out in 1935 [52],

limits are not needed; and (ii) describing the chain method which he now regards less favourably than he did in the essay.

Although he adopts a critical and at times almost defeatist tone, Keynes ends on a positive note by giving the reasons why 'in spite of so many practical and theoretical difficulties useful comparisons can often be made in actual practice'.

(d) *Stocks of staple commodities*. Keynes was among the principal originators of the London and Cambridge Economic Service, which began in 1923, and always maintained an active interest in its work. The Service provided a regular survey of current economic conditions, supplemented by special memoranda on a variety of subjects such as index-numbers of production, wages and share prices, statistics of investment in fixed capital, the monetary circulation and so on. From April 1923 Keynes, with the assistance of R. B. Lewis, J. W. F. Rowe and others, was responsible for a special memorandum on stocks of staple commodities which appeared until 1930 at roughly annual intervals [33]. This series brought together statistics, obtained from a variety of sources such as the US Department of Commerce, the London Metal Exchange and numerous private institutions and firms, on stocks and visible supplies of about a dozen major commodities in different parts of the world. This is a particularly difficult field because many of the commodities are supplied by different types of producer scattered all over the world and the information is incomplete and of varying reliability. A good deal of comment and interpretation is therefore needed if the figures are to be properly understood. The last in the series in which Keynes appeared as one of the authors was published in October 1930 and a final issue by Campion, Charles, Kahane and Rowe was published in November 1937.

(e) *The multiplier*. In 1929 Keynes and Hubert Henderson published a pamphlet entitled *Can Lloyd George Do It?* [32], an examination, or rather a defence, of the Liberal Pledge *We Can Conquer Unemployment* put out by Lloyd George for the forthcoming election campaign [37]. The pamphlet relates to the subject of public works and is concerned with a variety of issues such as: (i) that expenditure on these works gives employment to those who make and transport the materials used as well as to those directly engaged in their construction; (ii) that there are offsets to the gross cost, such as saving in unemployment benefits; and (iii) that there is no reason to think that public capital expenditure is in any way different from private capital expenditure in preventing other investment plans from being realized.

The fact that the purchasing power of the erstwhile unemployed is increased, thus giving rise to secondary as well as to direct and indirect primary employment, is recognized but not, at this stage, quantified.

The problem of how to calculate secondary employment was tackled by Richard Kahn in the late summer of 1930 in a path-breaking essay circulated at an early stage to the members of the Committee of Economists of the Economic Advisory Council and published in *The Economic Journal* for June 1931 under the title 'The relation of home investment to unemployment' [12]. In this paper Kahn shows how to calculate the ratio of secondary to primary employment and hence the multiplier. On the basis of statistical material provided by Colin Clark, he puts this ratio between  $\frac{1}{2}$  and 1, and suggests that  $\frac{3}{4}$  (giving a multiplier of  $1\frac{3}{4}$ ) might, if anything, err in the direction of understatement. These estimates are not restricted to the household sector but are intended to cover the whole community, including public as well as private institutions.

These ideas were used by Keynes in a series of articles which appeared in *The Times* in March 1933 [20] and subsequently in a pamphlet entitled *The Means to Prosperity* which was published in the same year [21]. Here he expresses the belief that in the circumstances of the time the multiplier was about 2, but to be on the safe side he takes a figure of  $1\frac{1}{2}$ . The figure of 2 is spelt out in greater detail in the American edition of the pamphlet. Keynes assumes that 'not less than 66 per cent of additional expenditure (whether on new capital works or on additional consumption) would become additional income in the hands of an Englishman, and that not less than 75 per cent of this additional income would be spent'; and this gives approximately  $1/(1 - \frac{2}{3} \cdot \frac{3}{4}) = 2$  for the multiplier. Incidentally, there appears to be a slip at the beginning of section III, since while the answer is correct the multiplicand and the multiplier are not. L. F. Giblin, who had had somewhat similar ideas a few years earlier, was troubled by this discrepancy and wrote to Keynes about it [8].

In this edition Keynes gives it as his opinion that the American multiplier is likely to be greater than 2 rather than less. In a lecture given to the American Political Economy Club in the middle of 1934 [22] he goes somewhat further, saying: 'I should be extremely surprised if the multiplier in the United States is less than 3, and it is probably appreciably higher.' One would expect the American figure to have been higher than the

British, partly because in America relief expenditure was on a less generous scale than in Britain and partly because America is less dependent on imports.

It is fairly clear that Keynes's figures were reached by Kahn's method of leakages but he does not go into their statistical basis. He is critical of the available data and says in his American lecture: 'It is, indeed, only by rather rash guesswork that one can arrive at the final figure of what the multiplier is likely to be. My own belief is, however, that whilst accuracy is unobtainable in the present state of our statistics, one can make a fairly reliable shot at the order of magnitude.' In *The General Theory* he again refers to the unsatisfactory state of the statistics but attempts an estimate for the United States based on time series: Simon Kuznets's data for capital formation [35] combined with some unspecified estimates of the national income. He does not form his estimate by applying regression analysis to the body of data as a whole but considers these data one or two years at a time, an approach which seems to hark back to Lexis and part V of *A Treatise on Probability*. He comments as follows:

If single years are taken in isolation, the results look rather wild. But if they are grouped in pairs, the multiplier seems to have been less than 3 and probably fairly stable in the neighbourhood of 2.5. This suggests a marginal propensity to consume not exceeding 60 to 70 per cent—a figure quite plausible for the boom, but surprisingly, and, in my judgment, improbably low for the slump. [23, p. 128/128]

Keynes returns to the matter at the end of a note in *The Economic Journal* for September 1936 on fluctuations in net investment in the United States [24]. He concludes:

If, however, as a very crude, preliminary test we take the Dept. of Commerce estimates of income (uncorrected for price changes), we find that during the large movements of the years from 1929 to 1932 the changes in money-incomes were from three to five times the changes in net investment shown above. In 1933 incomes and investment both increased slightly, but the movements were too narrow to allow the ratio of the one to the other to be calculated within a reasonable margin of error.

As far as I know this was the last occasion on which Keynes wrote about estimating the multiplier. It is a pity, because shortly afterwards longer time series became available and this might have led him to reconsider his methodology of looking at the observations one or two at a time. But we must remember that the following year, 1937, was the time of his heart attack and that many things had to be laid aside in the years which followed.

Others, however, carried on. In 1938 Clark estimated the British multiplier at 2.1 in a paper in *The Economic Journal* [5]. In the same year, in *The Review of Economic Studies* [55], my first wife and I estimated the marginal propensity to consume and the multiplier for a number of countries on the basis of three different kinds of information: (i) household budgets; (ii) Kahn's method of leakages; and (iii) time series. For the multiplier we gave: (a) for Britain, 1.8 from (ii) and 2.1 from (iii); (b) for the United States, 3.0 from (i) and 3.3 from (iii); and for pre-Nazi Germany, 3.7 from (i) and 3.6 from (iii). I do not propose to discuss the comparability of these figures with one another or with Keynes's; I give them simply as examples of the results that were being obtained at the time.

(f) *A blueprint for war finance*. As we have seen in section 5 above, Keynes brought together his ideas on this subject in *How to Pay for the War*, which set out Britain's problems in quantitative terms. Here I shall not go into numerical details but simply give a summary of the argument. This runs somewhat as follows.

A war can be financed partly out of capital and partly out of income. As regards capital, assets can be sold to and debts incurred with the rest of the world; further, domestic capital equipment need not be fully maintained and replaced and part of the provisions for these purposes can be lent to the government. As regards income, consumption can be reduced, thus leaving more to go to the government directly via taxation and indirectly via saving. As Keynes says, 'the size of the civilian's cake is fixed' and so, in the aggregate, consumers cannot succeed in improving their standard of living by spending more from their enlarged wartime incomes. If they do, what will happen in the first place is that prices will tend to rise and the excess purchasing power will get into the hands of business, whence most of it will find its way to the government via taxation or via loans by the wealthier members of the community. But, in the second place, the rising prices will lead to pressure for increased wages which, to the extent that it is successful and leads wage earners to spend more, will drive prices up again, and so on *ad infinitum*. Keynes's solution to this problem is partly economic and partly social. The economic aspect consists in fixing taxation at a level calculated, when the other factors are taken into account, to avoid an inflationary situation. The social aspect consists partly in attempting to distribute the tax burden fairly and partly in a scheme for deferred pay which

meant that a part of wartime taxation would be repayable after the war like any other loan.

Thus the 'radical plan' embodied in *How to Pay for the War* contains four main provisions. First, 'to determine a proportion of each man's earnings which must be deferred;—withdrawn, that is to say, from immediate consumption and only made available as a right to consume after the war is over'. Second, 'to provide for this deferred consumption without increasing the National Debt by a general capital levy after the war'. Keynes was in favour of having the capital levy as soon as possible after the war, especially if temporary boom conditions seemed imminent, or, alternatively, of collecting it in a series of instalments; but he thought that the repayment of deferred pay should be withheld until the onset of serious unemployment. Third, 'to protect from any reductions in current consumption those whose standard of life offers no sufficient margin. This is effected by an exempt minimum, a sharply progressive scale and a system of family allowances.' And finally, though this is not essential to the previous provisions, 'to link further changes in money-rates of wages, pensions and other allowances to changes in the cost of a limited range of rationed articles of consumption, an iron ration, as it has been called, which the authorities will endeavour to prevent, one way or another, from rising in price' [30, pp. 10–11/379–80].

These proposals were supported, as I have said, by a set of national income and expenditure estimates. The main product total used nowadays be called the net national product at market prices, which should be balanced by private and public consumption and investment at home and abroad; in fact the product account was made to balance without any reference to foreign investment, but this was a fairly small item in 1938–9. In addition, separate income and outlay accounts were given for the private and public sectors and an attempt was made to divide private income into broad ranges, a step which was needed to throw light on the incidence of some of the proposals. Though now out of date, the estimates in *How to Pay for the War* are still interesting and compare fairly well with those that would be made today.

## 7

Before concluding this paper I should like to say a few words about the institutions for the promotion of quantitative economics with which Keynes was associated from their inception,

namely the National Institute, the Cambridge Research Scheme and the Department of Applied Economics.

(a) *The National Institute of Economic and Social Research*. The National Institute was founded in 1938 and, at the invitation of Stamp, Keynes became an original member and governor and served from the outset as a member of the Council of Management.

At an early stage, in the spring of 1939, Keynes wrote to Noel Hall, the National Institute's first director, suggesting that the Institute should prepare 'a report for publication, and for pressing on the Government, as to the statistics which, in our opinion, it is vital that the Government should collect, and which have not been collected, or are not being collected adequately at present'. Hall was sympathetic to this suggestion and it came before the Institute's Standing Committee on Statistical Information, which agreed that the work was very necessary; but, not surprisingly, the proposal seems to have been caught up in events so that nothing was done at the time. However, the Institute did in effect take up Keynes's suggestion after the war with a project called 'An Examination of British Economic Statistics' which was set up late in 1950. Its object was 'the investigation of the statistical information (a) required for, or (b) actually used or available for, the formulation of economic policy in the United Kingdom'. The inquiry resulted in a report by C. F. Carter and A. D. Roy which was published in 1954 [3].

Another piece of work which Keynes actively supported was Charles Madge's inquiry into wartime saving and spending. This began in the spring of 1940 and led to a number of publications, two of them in *The Economic Journal* of that year [38, 39], and eventually to an occasional paper which was published by the Institute in 1942 [40]. As one of the originators of Mass-Observation, Madge was interested in studying consumers' attitudes by the methods developed by that organization and in answering such questions as what individuals would do if their incomes were increased or reduced by a given amount. The comments quoted by the Chancellor in his 1941 Budget speech on the scheme for deducting income tax from salaries and wages came from Madge's inquiry.

Josiah Stamp was president of the Institute until his death in 1941 and it was hoped that Keynes would succeed him. However, at that time Keynes did not feel he could take on anything more, since apart from all else he had recently agreed to become

chairman of the Council for the Encouragement of Music and the Arts: 'Lydia Lopokova's Husband Britain's New Art Chief', as one London newspaper put it.

(b) *The Cambridge Research Scheme.* One of the National Institute's original functions was to act as a channel between foundations which supplied finance for research, and individuals, universities and other institutions needing such support. An initiative sponsored by it was the 'Cambridge Research Scheme of the National Institute of Economic and Social Research'. The chairman of the Scheme was Keynes and the other members were Richard Kahn, Piero Sraffa, David Champernowne, Joan Robinson, Austin Robinson (secretary) and Michal Kalecki (statistician). In addition there were a number of assistants, research students and others associated with the group. It may interest those who have had the task of raising finance for economic research in recent years to know that the grant to the Cambridge Research Scheme for the academic year 1938-9 was £600, not all of which was spent.

The general subject of research was the process of economic change in the United Kingdom. One of the initial items in the programme was Kalecki's quantitative work (in which he was assisted by Brian Tew and Y. N. Hsu) on prices and prime costs; and it was intended that, when this was completed, Kalecki should undertake a detailed statistical study of the mechanism of interwar trade cycles in this country. 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, with the object of isolating some of the factors influencing the size of the multiplier and the limits imposed by international trade on the power of one country to act independently of others in dealing with a depression. Keynes himself intended to direct an investigation into the measurement of saving, whereby it was hoped to clear up some conceptual problems and to produce estimates which could be compared with estimates of investment based on industrial data; indeed, with the help of Piers Debenham, he made a start on this in the summer of 1939. Although I did not hold an academic post before the war, I had done a good deal of work, with the collaboration of my first wife, on current economic time-series, most of which appeared in the monthly *Industry Illustrated* in a feature entitled 'Trends' which had been started by Colin Clark [54]. We were in touch with the Cambridge

group and they kindly decided to help us. Among other things, we planned to study what we described as the problem of scarcity and the consequent changes in prices and outputs which would arise as a result of the rearmament boom which we believed to be following the pause of 1938.

Although some publications arose from this whole series of projects, little could be finished before it was all swept away by the war. However, enough had been going on to show that the programme had a strong econometric flavour. When we look at the date of these activities, around 1938–9, the Tinbergen episode seems even more bizarre. ‘The proof of the pudding is in the eating’, said Tinbergen in his reply to Keynes’s review. ‘Professor Tinbergen appeals to me several times to cook (or, should it be, eat?) more pudding myself before declaring it indigestible’, countered Keynes. Well, here he was in his Cambridge kitchen, apparently quite happy. Do we detect the traditional Englishman’s predilection for ‘good plain cooking’ and mistrust of *haute cuisine*?

(c) *The Department of Applied Economics.* From an administrative point of view the Cambridge Research Scheme was highly informal. The Faculty Board would not have been a suitable body to administer and supervise research because of its size and its many other responsibilities; and in practice the administration of the group devolved upon Austin Robinson. By way of providing a formal link between the group and the University, the Faculty Board, doubtless at Keynes’s instigation, proposed to the General Board 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 there was nothing that could be done at the time to implement the project which, accordingly, was put into cold storage for the duration of hostilities.

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 peace came. Keynes was Chairman of the Committee of Management. I was appointed the first director in mid-1945 and the work began of formulating a research programme, raising finance, finding accommodation and recruiting staff.

As it happened, we started with accommodation. Walking one day in Cambridge, I noticed removal vans outside a large terrace house in Trumpington Street. On inquiry I learnt that the house had been used by the London School of Economics

and that they were returning to London. This looked like a possibility and I reported it to Keynes. The reaction was unexpected: 'Dick,' he said, 'I don't know anything about your personal circumstances, but when I was a young don we should not have thought of starting off in quite such a grand house.' Much amused, I explained that I had thought of it not for myself but for the Department. 'Good idea', he said. 'It belongs to Peterhouse. I know the bursar well, I'll telephone him.' That afternoon we believed we had solved one of our problems, but matters turned out differently. Before the Department could move in, Keynes and I both went to America and when we got back early in 1946 we found that our house had been taken over by the town council for emergency housing. The University, who had done their best for us in our absence, eventually built us a warm and cosy hut on the Downing site. In the meantime, as staff began to arrive, the Department was housed in a large room above the Marshall Library, which in those days was in Downing Street.

But while our visits to America lost us a house, they gained us financial support. In 1945 the Rockefeller Foundation made a development grant for one year and this was replaced in January 1946 by a very generous grant of £23,500 for a 5½-year period.

Keynes's death on Easter Sunday 1946 came before the Department had really got into its stride. Had he lived longer, I do not think he would have been either surprised or disappointed by its activities: after four years of closely watching me at work in the Cabinet Offices, he could have few doubts as to which way the Department would go under my direction. So I think it can fairly be said that if econometrics struck firm roots in postwar Britain and has grown in spite of adverse winds into a sturdy tree, our thanks should go in large measure to Maynard Keynes.

## 8

This brings me to the end of my account. I realize that I have only touched on a small part of Keynes's contribution to the life of his times. Yet I think it is an interesting part given the growth of quantitative and econometric studies in the last generation, a movement of which Keynes saw only the early stages. Let me now try to sum up.

→ A striking feature of Keynes's personality, as it seems to me, is the difference in his attitude when he was a critical bystander

and when he felt committed to a course of action. In the first mood he seems to have often been guided by the traditionalist in him and to have preferred going over old arguments to tackling new ones. In the second mood he was authentically free and did not allow himself to be sidetracked by any force on earth. It is this which gives *The General Theory* its strength and which made him such an effective champion of political arithmetic in the Second World War.

His ambivalence on the role of theory and his hostility to the use of mathematics in economics were the outcome, I think, of his background and early experiences and reflect the critical bystander in him. With his rhetorical style there is no difficulty in finding overloaded, not to say outrageous, quotations. But they are representative of the young self; the actions of the old self belie them. Despite many hard words, there is no doubt in my mind that he should be counted among the benefactors of econometrics.

What he did for political arithmetic is beyond question. The charge that he should have done more for this cause or that cannot, in my opinion, be sustained. The truly remarkable thing about him is the amount he managed to accomplish. Compared with his achievements, any sins of omission seem venial and not to be remembered.

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