ECONOMETRICS: A VIEW FROM THE TOOLROOM

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I made my first visit to the city of Birmingham and this University almost two years ago. On that occasion, I had the pleasure of speaking to a small gathering of economists from the Faculty of Commerce and Social Science. I won't trouble you now with the title of that seminar. But, as I was swept out of New Street Station in a taxi, around Smallbrook Ringway and Holloway Circus and on towards the University I was myself troubled that the subject I had chosen was a little technical. Tonight, I find that I am holding down a similar thought; and it may be that I am not alone in having such misgivings. Let me start, therefore, with an attempt to break their spell. I hope that those of you who are not economists or mathematicians will set your own minds at rest. I am not about to launch a display of handwaving mathematics or shower the blackboard with a sea of technical symbols. My colleagues, I know, have all sat through or themselves delivered, many hours of live econometrics in the past; and my students already understand the role of the blackboard, the chalk and the eraser in an econometrics lecture. They all know that, if indeed this were the real thing, then we might even by now be cleaning the blackboard for its second helping.

My task this evening, if I do not misunderstand it, is to bring my own specialism alive to a general audience. I can be guided by the fact that all of us know of the passion and scent for adventure which drive man to explore the unknown. It is the same passion and scent for adventure which can take man into specialised study. Specialists in other fields will themselves know the thrill of discovery, the excitement of the moment a difficult proof is unlocked, the adventure in tackling a problem neglected by other researchers or set aside by them as too demanding. And in all this the character of a man's work can vary greatly. The Cambridge economist Arthur Pigou once distinguished between tool-makers and tool-users in economics, while allowing that certain men or women may well play both parts. If you will permit me, I should like to quote a famous passage from his essay.[1]

> "During some thirty years until their recent deaths in honoured age, the two outstanding names in English economics were Marshall at Cambridge and Edgeworth here at Oxford. Their contrasted methods illustrate what I am saying very clearly. Edgeworth, the tool-maker, gloried in his tools. He would make them for the sake of making them; and he would, I suspect, have cherished more dearly a well-fashioned probe, for which no use whatever could be found, than a serviceable but clumsy blade. Marshall, on the other hand, had what almost amounted to an obsession for hiding his tools away. He would work out a problem, for example, by a mathematical technique, and would then spend endless trouble in burying the technique in language designed to sound as platitudinous as he could make it. In Edgeworth's

work the student knew what he was up against; he might not be able to understand it, but, if he did not, there was little danger of his imagining that he did. In Marshall's work, on the other hand, there was great danger of this. . . . Sometimes, being myself addicted to high places, I have been tempted to think of these two men as contrasted types of mountaineers. I have imagined myself meeting them on the summit of a difficult Alpine peak. Edgeworth is there, festooned around with rubber shoes, crampons and all the paraphernalia of his craft, his hands torn and bleeding, his clothes in rags: Marshall is seated by his side dressed in an elegant summer suiting and . . . pretending with all his might to have stepped from a passing balloon! But a careful survey of the horizon shows that it was not in that manner that he arrived! No: These great twin brethren have both ascended by ice-slope and chimney and slab; but the one is more interested in the process, the other in the goal! To both types of mind economic science owes much". (pp. 3–4)

This fundamental distinction that Arthur Pigou made in 1929 has grown to be of great significance in modern economics. Indeed, it forms the battleground of an important current debate over the practical relevance of economics. And, to this, I hope to return later. Arthur Pigou believed that progress in economics was made through the inspired use of a simple tool kit. The tools themselves were not great structures of human intellectual achievement that were of any value for their own sake, despite the fact that their invention often presented the concentrated power of a great mind, like that of Edgeworth. The tools of economic analysis were of value only to the extent that they helped to explain the sort of phenomena and problems we observe in the everyday economic world.

Almost half a century has elapsed since Pigou wrote those words, and in that time, sweeping changes have occurred in the range and depth of abstract mathematical technique used in economics. In the first place, these changes have been central to the development of modern economic theory along rigorous aximomatic lines and have led to a body of knowledge that we describe, perhaps rather loosely, as mathematical economics. In the second place, these changes have played a critical role in the growth of statistical methods designed to measure the relationship among economic variables that are implied by theory. The body of theoretical and empirical studies connected with the latter come under the name of econometrics.

In the following minutes, I hope to describe to you some small part of the work that has, in more recent years, engaged the attention of researchers in this field and tell you something about the tools they have fashioned to aid them in their task. Much of this work is technical; but this need not necessarily lead to conflict between Pigou's position on the role of tool-makers and the status of their tools on the one hand, and the underlying objectives of economic analysis on the other.

II

The immediate problem that faces an econometrician speaking to a general audience is the extent to which he must explain to his listeners the nature and scope of his subject, the role it has to play in economics and the features which distinguish it from its closer sister, mathematical economics. But, in this respect, I am very fortunate. For, the University of Birmingham has itself the distinction of a long history in econometrics. And this means, I am sure, that for many of you I need not embark on a tiring expository lecture.

All the same, some guidelines may be useful to us in our following discussion. And the guidelines I have selected to use, for the simple reason that I cannot improve on them, date back to a seminal discussion in 1944 by Trygve Haavelmo[2], who did much to initiate the modern era of econometrics. Haavelmo and other eminent econometricians[3] succeeding him have found it convenient to speak of econometrics in terms of the activities it includes: First of all, the formulation of econometric models; then the estimation and statistical testing of these models with observed data; and, finally the use of these models for prediction and policy purposes.

To clarify the concept of an econometric model we need to recall the precise meaning of the term model itself. One definition is just 'a simplified representation of a real world process'. The emphasis here is on 'simplified'. This is why the use of models in any science implies humility on the part of the investigator. Nowhere is this more true than in the social sciences and, in particular, economics. For, in the study of economics, experimentation such as that in the physical sciences is virtually impossible. An investigator in economics is frequently confronted with a series of observations on a number of economic variables. These observations are, in statistical terms, the realisation of a random process; but this process cannot be taken as exclusively economic in nature for it is conditioned by our social organisation and political institutions.

Despite all this, there is usually some permanent features of the phenomena that merit consideration, some systematic forces at work behind the data we observe which makes it possible to model such a process. A primary task in econometrics is to reframe the models which are developed, in abstract, in economic theory into a form sufficiently precise to be estimated from a series of observations on the relevant variables. Once a model is in the form it becomes known as an econometric model.

Such an introduction to the notion of model building in econometrics is bound to underestimate the complexity of the task. But no econometrician worth his salt makes light of this activity. That is why our major journal *Econometrica* now offers a prize[4] for the most outstanding piece of empirical work to be published in its pages every two years.

The battle, to which I referred earlier, over the relevance of modern economic theory and the methods of econometrics will be fought and won on this very field. A number of celebrated economists have already sounded the battle trumpet and met with an enthusiastic army of supporters in the ranks of our profession. Indeed, as I joined the staff of the University of Birmingham in the Summer Term of 1976 I found that we were ourselves engaged here in a series of lunch time discussions on this fashionable reappraisal of economics. In one way or another, this sort of debate has been smouldering away for many years.

In 1939, Jan Tinbergen, who now has the honour of sharing the first nobel Prize in economics, published an account of the statistical method of multiple correlation and its use as a tool for analysing statistical relationships in economics. Tinbergen's study was exposed to a celebrated attack by John Maynard Keynes. And in a concluding paragraph, Keynes added this comment:

> "No one could be more frank, more painstaking, more free of subjective bias or *parti pris* than Professor Tinbergen. This is no one, therefor, so far as human qualities go, whom it would be safer to trust with black magic. That there is anyone I would trust with it at the present stage or that this brand of statistical alchemy is ripe to become a branch of science, I am not yet persuaded. But Newton, Boyle and Locke all played with alchemy. So let him continue". (p. 156)

Keynes was writing in the *Economic Journal* in 1940.[5] Some thirty years later, the American economist Wassily Leontieff crystallised the current debate in a spirited presidential address to the American Economic Association.[6] He struck out mercilessly at the superstructure of modern mathematical economic theory that now wobbles on such a weak empirical base. And turning to econometrics, he characterised work in this field as

> "an attempt to compensate for the glaring weakness of the data base available to us by the widest possible use of more and more sophisticated statistical techniques. Alongside the mounting pile of elaborate theoretical models we see a fast growing stock of equally intricate statistical tools. These are intended to stretch to the limit the meager supply of facts". (pp. 2–3)

Of the many others who have joined Leontieff in the front line of his attack on our subject, I quote here only Mr. G. D. N. Worswick, the Director of our National Institute of Economic and Social research. Mr. Worswick argues that[7]

> ". . .there exists among some econometricians and some editors a doctrine, of econometric escalation whereby if one believes that a particular piece of econometric work is unsatisfactory, the rules of the

game require that even heavier econometric weapons have to be mobilised. The employment of lower levels of weaponry, such as a critical analysis using only the English Language, is considered off-side". (p. 83)

Worswick is disillusioned with what he calls the "pretend-tools" of econometrics —the tools which, and I quote, "would perform wonders if ever a set of facts would turn up in the right form".

But it is not unfair, I think, to ask in return how confident we would be in our critical analyses of economic policy if we were to rely on literary skills alone. I say economic policy here because the disillusionment I have just described is most often apparent in the statements and writings of those leading members of our profession who serve in one capacity or another as economic advisers to governments. I do not wish to underrate the enormity of their task. Quite the reverse. But I cannot share their wholesale despair over the present state of quantitative economics. After two decades of professional endeavour in the field of economy-wide models designed as tools of policy it is perhaps too easy to forget the more severe shortcomings of economic analysis without quantification that Marshall and Pigou recognised long ago. Writing in 1908, Pigou[8] made it quite clear that quantitative information on practical issues of economic policy was the most urgent need of economic science at that time; and he appealed to the coming generation of economists to take up the torch he had kindled. This is not to understate the importance of and insights that can be gained by critical economic analysis in the absence of quantitative precision. But, I do not think we should take lightly now the appeal Pigou made in 1908 or turn tail because we cannot flatter ourselves with a decade or more of scorching success in policy recommendations.

The point is this: that advising policy makers about the central problems of macroeconomic policy is a weighty and responsible task. And those who engage in it know well the great intellectual leap they make from the body of limited theoretical knowledge and, often inconclusive empirical evidence that surrounds the particular questions they face. Back in 1968, Terence Hutchison[9] called this intellectual leap the predictability gap, when he published a sobering account of the appalling record of advice volunteered by University and other economists in Britain on this country's post-war economic difficulties. I don't doubt that if his chronicle were to be updated today the picture would be just as grotesque.

But in one important respect the situation may have changed. Whereas in the 1960s many economists got by perfectly well without acknowledging the predictability gap, the striking failure of forecasters to predict and theorists to adequately explain the inflation of the 1970s has brought the gap full into view. As one commentator put it recently[10] "we were caught with our parameters down".

And as we struggle, shame-faced and in full public view, to refit these parameters, I am brought full circle, back to the humility that is implied by the very notion of model building. The time has surely come for us to admit openly our inadequate understanding of the complex, dynamic linkages that underlie macroeconomic activity, to confess that public pronouncements on economic policy are often made with little or no scientific basis. Only when we have done so can we settle down, honestly and with the degree of humility that befits serious scientific work, to the hard intellectual challenge of shaping our theories and our statistical tools so that they may together advance our understanding of economic phenomena.[11]

Some twenty years ago Tjalling Koopmans[12] published three penetrating essays on the state of economic science. The first contains a brilliant exposition of the logical structure of the general model of competitive equilibrium, the workings of the price system in a competitive environment that includes consumers, producers and holders of natural resources. This essay is essential and compelling reading for students of economic theory. The other two essays in the book are probably seldom read by students and are seldom referenced in the professional literature. Nevertheless, these later essays provide us with substantial insight into the role of logical reasoning and factual observation in the foundation of economic knowledge on the one hand and the need for sharper technical tools in the analysis of economic problems on the other. Koopmans suggested that we view economic theory as a sequence of models.

> ". . . that seek to express in simplified form different aspects of an always more complicated reality. At first, these aspects are formalised as much as feasible in isolation, then in combinations of increasing realism. . . . The study of simpler models is protected from the reproach of unreality by the consideration that these models may be prototypes of more realistic but also more complicated subsequent models. The card file of successfully completed pieces of reasoning represented by these models can then be looked upon as the logical core of economics, as the depository of available economic theory". (pp. 142–143)

The premises on which each member of this sequence of models rest involve approximations to reality and often comprise what seem to be rather crude simplifications of the objectives behind the behaviour of various economic agents such as consumers and producers in an actual economy. The challenge which the student of economic theory must accept entails the conception of premises which provide a closer approximation to reality and, further, the construction of models on the basis of such premises whose implications can be fully explored. If, in this process of reasoning, the implications of a model appear rich enough to accord with certain observed economic phenomena of interest (such as the simultaneous occurrence of high rates of inflation and levels of unemployment) then the model may provide a suitable basis for empirical work in those areas.

When the careful student of econometrics comes to shape the empirical version of such a model he is mindful of the limitations of the premises that underlie it and other problems such as the aggregation over individuals and commodities that may be implicit in the equations he writes down. These considerations do not make his task any easier but sharpen his awareness of the dangers of over-reaching in his conclusions and encourage him to accept his model as an approximation which, at best, will prove useful in explaining observed data. If it is useful in this way, his work provides an important feedback and stimulus to the process of guiding the development of Koopmans' model sequence.

A difficulty which emerges from this very natural process, however, is that as the theoretical models grow in complexity and attempt to incorporate more features of reality they also present new problems in statistical inference for the econometrician. Thus, the econometrician finds himself with two rather different jobs of work: assisting the theorist in the refinement of models and designing tools of statistical inference appropriate to the models he intends to use. For the remainder of this lecture I have selected two areas of study in the current phase of economics, which may serve to illustrate these different tasks.

III

The first area probably attracts about as much interest amongst economists and general commentators in economics as any other, certainly in the United States. It also bears in an important way on our earlier discussion. This is the work on economy-wide models of economic activity.

In the beginning, these models were modest tools, designed to capture the essence of the national income determination process popularised in Paul Samuelson's "Economics" and to see how well this theory accorded with the observation of the inter-war and early post-war years of a number of industrialised economies. The models were also characterised by an attempt to wrestle with monetary phenomena and trace the feedback mechanism from the monetary to the real sector. For this reason, they were regarded as essentially Keynesian in structure. The exploratory study of the United States economy by Lawrence Klein and Arthur Goldberger,[13] which was published in the mid-1950s, can be taken as the starting point in practical investigations of this kind. It was received with much interest at the time and has been the subject of continuing investigations of one sort or another ever since. So it is still far from being a museum piece. The model had some success, forecasting aggregates and turning points in economic activity rather better than did some naive alternative methods set up as bench marks, and capturing the downturn of the 1930s well enough to be accepted at least initially as capable of generating an interesting business cycle.

Since the problem of modelling business cycle behaviour had fascinating investigators in economics for many years, this feature of the Klein-Goldberger model quickly attracted attention. From the technical point of view the model incorporated adjustment lags in its behavioural equations so that, for example investment would not react instantaneously to changes in interest rates and a change in import prices would influence the flow of imports over a period of years not just one year alone. We call these models dynamic and in simple cases, we can discover quite a lot about the time paths or trajectories they generate by methods of analysis which are well established in applied mathematics. I say in simple cases, because, when the lags in the model are long and the model is large, considerable computing resources are required for a detailed analysis of a model's dynamic properties. In the case of the Klein-Goldberger model, which consists of some twenty or so equations, there is the additional difficulty of non-linearity amongst the variables. It is hard to escape from this particular difficulty in econometric work, where we regularly encounter such simple non-linearities as the equation of expenditure to the product of price and quantity sold.

These mathematical difficulties can, in part, be circumvented by the use of simulation. We simulate a model by taking an assumed time path for the model's exogenous variables—these are the variables the model does not set out to explain; we then solve numerically by a computer algorithm for the implied values of the model's endogenous variables—these are the variables the model does set out to explain. We can analyse the trajectories that result from this exercise and determine whether or not they reproduce business cycles as they have actually occurred. In this analysis, we can be guided by considerations such as the length and amplitude of the historical cycles as well as the structure of leads and lags between various series that have been observed in the past.

In assembling exercises of this sort model proprietors have great scope. It is, as they say, the nearest we come in economics to experimental situations. Simulations can be made random, or, as we say, stochastic by the introduction of synthetic shocks into equations and the properties of the synthetic shocks can be made as elaborate as we please. The time paths assumed for the exogenous variables can be made oscillatory and can also be subjected to random shocks of a predetermined type. Equations representing different government policies on taxation expenditure and the supply of money can be included and their various effects analysed. Structural changes can be made in the model itself simply by altering the magnitude of a key parameter or constant. Such changes can then be used to help isolate the role of certain linkages that we suspect are important in governing the behaviour of the variables themselves.

If there is now great scope in the design of these simulation exercises then there have also been great developments in the tools that can be brought to bear in the analysis of the simulation results. One of the most noticeable developments over the last decade has been the introduction of methods that are currently used by statisticians and others in the analysis of multiple time series. I am thinking here, particularly of the range of techniques that come under the general head of spectral methods. These methods now figure as an increasingly useful tool in econometric work.[14] Let me give an example. Suppose we consider a series of observations taken at quarterly intervals of a country's gross national product and graph these observations against time. As we study the graph, we notice that cycles of various lengths play an important role in determining its shape. We observe regular movements in the series in relation to the seasons of the year and there are periods of expansion and periods of recession at more or less regular intervals, corresponding to what we call the business or trade cycle. Spectral methods can often help us to make these ideas more precise. They have become, as a result, a popular tool for analysing the simulations of an econometric model. Used in this way they are largely a descriptive tool, helping us to describe those features of phenomena (whether simulated or observed) that interest us most.

But, like many statistical tools, spectral methods have important direct uses in the field of inference as well as description. And, while I am on the subject of these methods, it would be a travesty not to mention the spectacular contributions that have been made in this area over the last ten years principally by Edward Hannan at the Australian National University. The power of spectral techniques as a tool of inference lies in the generality they allow us in the treatment of the random disturbances or shocks which drive our equation systems. These disturbances reflect the outcome of political, social and other events not directly incorporated into our equations; and econometrians have been troubled for many years by the fact that such disturbances may have effects more prolonged than the single time period of a month, a quarter or a year that forms the basis of our model. There are arguments why we should attempt to take proper account of this unfortunate but often realistic feature of disturbances in fitting our equations to data and, moreover, in using these fitted equations for purposes of prediction. Attempts to do so have led many econometricians to construct small scale models for the disturbances themselves. But, only in rare instances do we have any guidance concerning the form these models should take, which makes a scientific approach along these lines rather difficult. Spectral methods, on the other hand, have wide applicability in this context. They free us from the need to make very detailed assumptions about the disturbances and they enable us to concentrate our attention on the type of movements in our series that attract our special interest.

For all these reasons, spectral methods are now appealing to research workers in econometrics. The statistical theory that surrounds the rigorous treatment of these methods is quite properly in the domain of mathematical statistics; but the

the use of these methods in econometrics has thrown up new problems; and the statistical theory that deals with the application of spectral methods in this context then occupies a twilight zone between mathematical statistics and econometrics. Researchers with central interests in both these areas have been contributing to the emerging body of theory and in doing so they help to sustain the traffic of information and technique between these two areas which has so far played such an important role in the history of our subject.

Before I stray too far into this territory let me return to my chosen path, in the area of economy-wide models. My earlier discussion concentrated on some of the more specialised activities that occupy the proprietors of these models. I should like to take up the discussion again with some more general observations. The most striking external feature of these investigations is the scale on which they are now carried out. The size of these models has grown from the twenty or so equations of the Klein-Goldberger model to systems that sometimes stretch to several hundred equations. The construction and maintenance of such large scale systems normally requires the effort of a whole team of investigators. These research teams regularly publish their results, reporting both forecasts and policy analyses. Their results then come under the scrutiny of competing research teams and the profession at large. In recent years, symposia have been held on a regular basis in the United States with the explicit purpose of facilitating comparisons between these models. The papers and proceedings of these symposia reflect a lively undercurrent of debate surrounding not only the differences in the structural specifications of the models and the statistical techniques used in their estimation, but also the scientific basis on which the investigations are being carried out.[15] This debate itself reflects more general disquiet in the profession about the large scale models. The widespread use of subjective adjustments to forecasts, the large number of variables taken to be exogenous in these models and the statistical difficulties of estimation and testing in this context are all the cause of some considerable concern. On forecasting performance, there is still a good deal to be desired;[16] and, in policy simulations, many of the U.S. models have been found to disagree so completely that we can gather little reliable guidance from this source concerning the quantitative effects of monetary and fiscal policies by government.[17]

In assessing some of this recent evidence it is hard to resist the view that, because so many of these models have been designed with a policy orientation in mind, their present use seriously over-reaches the real range of their applicability. Some support for this view comes from the observation that whereas models have grown enormously in size the underpinnings they derive from economic theory have often changed little in the process. This weakens the theoretical content in the equations of the model; and the limitations of the model that should be clarified by the underlying theory, are, as a result, less apparent. Inevitably, the danger of overestimating the applicability of the model increases as this process continues.

But the finger of dissatisfaction can be pointed with some conviction at the bare bones of the theory as well as the large scale empirical models themselves. And a group of economists inspired by the work of Don Patinkin and Robert Clower in the mid-1960s have recently engaged in this task with some vigour. They advocate a new class of models with rather different characteristics, designed to capture features of economic activity largely ignored by the older generation of models. An example which carries particular force is the treatment of disequilibrium. Disequilibrium is a state of affairs where goods are traded at prices that do not equate supply and demand. Recall the market for the Jaguar XJ12 in this country a few years ago. These cars were being sold at a list price for which demand outstripped supply. Trading then took place out of equilibrium on what economists call the short side of the market—this time the supply side. The manufacturers could not supply enough vehicles to match demand at the list price. Hardly an unusual situation to find British Leyland in. Now turn to the large scale models I have been describing and ask on what basis they deal with this sort of disequilibrium trading in commodities. The answer in every case is that these models have no theoretical foundation for commodity trading that occurs out of equilibrium.

On the other hand, the new class of models I mentioned are characterised by a serious attempt to allow for market disequilibrium. Strong research schools have grown up recently, notably in France and the United States, who are guiding the theoretical development of these models.[18] My own interest springs primarily from the models which have been constructed with a view of empirical applications. The pioneering work in this field has been done by Rex Bergstrom[19] in this country and Ray Fair[20] in the United States. Their models are highly original in conception, both allow for quite general market disequilibrium and both place great reliance on economic theory in their specification. They have highly geared dynamic structures which present formidable problems of statistical analysis and call for extensive new computer software. But, in spite of these difficulties, the models seem to offer great promise and can be expected to give the older generation of models stiff competition in the years ahead.

IV

This seems a natural point to close our discussion of economy wide models. The second area of study I have selected is rather different. It concerns a problem of central importance to the subject on which, I am afraid, we know a good deal less than we would like. This is the problem of drawing inferences about the characteristics of a model when we can marshall only a small quantity of statistical data. A problem which has for long been a major preoccupation of econometrics and has distinguished it from the various applications of mathematical statistics in the natural sciences. The natural scientist can usually generate data by recording the results of an experiment. He can then marshall a very large quantity of data by continued experimentation and this greatly facilitates the process of statistical inference in his work.

Consider for a moment the measurement of some physical constant such as the velocity of light. In spite of the high precision of his instruments, the investigator knows that there still remain many sources of error. He then takes several readings and uses the average value as his estimate of the true velocity. As the number of readings increases, the estimate becomes more reliable. But, there is still some uncertainty about the estimate. For, if the whole experiment of taking a number of recordings and averaging the results were repeated, we would then almost certainly end up with a different estimate. However, the fact that the investigator can take a very large number of separate recordings before averaging is of great significance and for two reasons. First, the average value will usually produce a very reliable estimate because the errors which enter individual recordings often tend to cancel each other out when the average is taken. Provided, of course, there is no systematic bias in the instrument. And, second, the area of uncertainty about the estimate that does remain can itself be accurately measured by a well defined mathematical curve, which we call the normal distribution. These properties hinge on the availability of a large quantity of data. They provide us with a large sample theory; and they can be justified, under certain qualifying but essentially weak conditions, by rigorous mathematical arguments. These arguments belong in the realm of the theory of probability and they have a long and illustrious history in mathematics.

The investigator who can marshall only a small quantity of data is, by comparison in a much less favourable position. On the one hand, his estimate is less precise and, on the other hand, the area of uncertainty about the estimate is less well described by the normal curve. The problem is particularly acute in econometric work. The fact that we are often confronted in econometrics with only a small quantity of data must itself be viewed in the light to two further limiting factors. First, our interest in the linkages among many different economic variables has led us to the study of rather complicated systems that involve a large number of unknown parameters. It is now not unusual for an econometrician to have some forty or fifty quarterly observations of each variable in his model. But, the size of the model may well mean that there are as many as thirty unknown parameters to be estimated; sometimes, even more.[21] And, in this context, we can expect the large sample theory I mentioned earlier to have much less relevance. The second factor which sharpens the effect of the small sample problem in econometrics is that there are often undetermined and possibly large inaccuracies or errors of measurement in the observations themselves. In a celebrated study published by the National Bureau of Economic Research, Simon Kuznets once estimated the average margin of error in the national income statistics of the United States to be around ten per cent.[22] This type of inaccuracy is to be contrasted with the precision of measurement that is implied by many of the statistical methods currently used in econometrics. When we come to evaluate these methods in accordance with their various statistical properties we should also take care to note the demand for accuracy in the underlying economic data that is implied by the choice of one procedure over another. This is a point which has been forcefully advocated by the American econometrician Robert Basmann.[23] Basmann argues that, if we are to adequately assess this demand for accuracy, then we need more precise information about the small sample properties of various statistical procedures.

The different strands of this argument lead us therefore, to the same conclusion. In the conditions which typically prevail in econometric work, an investigator cannot dispose of a large sample of data; he cannot expect large sample theory to provide an accurate measure of the area of uncertainty in his estimates; and yet he needs such a measure if he is to be properly aware of the limitations of his work. In statistical terms he needs a small sample theory to measure the uncertainty in his estimates. The problem can be simply stated but the mathematical difficulties are very great. And it is only in the last fifteen years that the mathematical study of the small sample problem in econometric models has begun in earnest. Here, as elsewhere, we have borrowed heavily in our methodology and in our technical tools from mathematical statistics.

The problem of developing a small sample theory can be approached in two different ways. We can concentrate our efforts on discovering the exact form of a small sample distribution or we can attempt to construct a useful approximation to the exact distribution. Mathematical statisticians have made very substantial contributions to both these areas. The methods they have used have inspired a good deal of work in econometrics; and it is heartening to note that in the last few years there has been some useful feedback from econometric research to mainline statistical theory in this area.[24]

Let me turn to the two ways we can approach a solution. An exact theory is possible only when we have specified precisely the distribution of our underlying data and have negotiated the difficult mathematical passage to the distribution of the parameter estimate or statistic in which we are interested. In principle, this passage can be bridged by numerical calculations on a computer; but, in practice, our computers are not large enough or fast enough to support this bridge in most cases of interest. To obtain a clear mathematical solution is very demanding and in spite of some remarkable results we have such a clear solution only in a small number of cases. The results we do have have themselves been subject to criticism.[25] For they are in no way simple. They involve some of the special functions of applied mathematics in complicated formulae which take up many lines on a printed page. They are a nightmare for our secretaries and a warhead in the salvos our critics fire against us. Nevertheless, these complicated formulae have been useful to us. They can be programmed on a computer and they have given us valuable insight into the small sample properties of various statistical procedures. Our critics are right, however, in suggesting, that these methods have not yet been able to help us in the bread and butter work of statistical inference in econometrics.

I mentioned a second way we can approach a solution to the small sample problem: by approximation. Contributions to this approach over the last couple of years *are* designed to help us in the bread and butter work of inference. They will, I suspect, play a significant role in the future development of the subject. There are many different ideas behind the approach, which have led, naturally enough to many different types of approximations. Let me concentrate briefly on one of those ideas, which has a particularly long history dating back to the work of the Russian scholar Tchebycheff in the mid-nineteenth century.

When the sample of data is large, we can use the normal distribution as an approximation to the real law that governs the error in our estimates. When the sample of data is not so large, we may be able to improve on the normal approximation by making some modification to that law. This idea of modifying the normal distribution as a law of the frequency of error was intriguing not only to mathematicians. In 1905 a long paper on the subject was published by the English economist Francis Edgeworth[26], whom have have already had occasion to mention. In this paper, Edgeworth devised a new approximation to the law of errors that was based on the normal distribution itself and a special combination of its derivatives. The full implications of his work only become clear much later. We now know that Edgeworth's approximation is, in a well defined mathematical sense, more accurate a measure of the law of errors than the large sample normal distribution. The result now takes the form of an elegant limit theorem in the theory of probability.[27] Edgeworth's achievement in the 1905 paper was a simple advance in technique, a new method, but one of those real range of application we are still exploring today. We remember how Arthur Pigou had described Edgeworth: a tool maker, a man who gloried in his tools, a mountaineer excited by the technique of ascent, much less by the summit itself. From this type of mind are born the tools which so often allow us to take the first step forward in a new direction of research. As always, it is this first step that counts.

I said that we were still exploring the range of application of Edgeworth's method. But our interest is a recent one and the source of our knowledge is the work that a few econometricians have done in the last five or six years. They have been inspired by the new ideas and the mathematical skills of the man who has led developments in this area of research and who is, indeed, the central pillar of the econometrics community in Britain: John Denis Sargan of the London School of Economics. From Sargan's work we have results of great generality and formulae that can be used for a wide range of different estimates and test statistics.[28] These formulae can be programmed on a computer for direct use in statistical inference;

and we can take advantage of the fact that modern computers can be programmed to carry out a good deal of routine alegebra and calculus for us.[29] We have found that in a number of cases the methods works very well; but not so well in others. And in those other cases we still have a lot of work to do in developing new methods which give better results.

V

I began this lecture with a number of quotations. Some of these made the inadequacy of the present state of our knowledge in economics and the surrounding disquiet in the profession painfully apparent. The examples I have just given of research work in econometrics have aspects which no doubt reinforce this view. But I have also suggested some promising new developments which, I think, send a flicker of light along the path to useful quantitative knowledge in our subject. If we are to travel further along this path we need men and women eager to dedicate themselves to the careful refinement of our models and design of our statistical tools. The toolroom in which they are apprenticed and in which they conduct their research may lack the glamour of economics as a practical art in government or business, but it is every bit as important. For, the tools they fashion provide the key to improvements in our quantitative information concerning matters of economic policy. And it is on such information that economic advisers to government and business are now so frequently basing their practical judgements.

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FOOTNOTES

1. Pigou (1929).
2. Haavelmo (1944).
3. Bergstrom (1966).
4. The Ragnar Frisch Memorial Medal.
5. I first encountered the attack by Keynes on Tinbergen's study when I read Richard Stone's spirited defence of econometrics, *"The Role of Measurement in Economics"*, published in 1951. The passage from the Keynes-Tinbergen exchange that I have quoted was quoted by Stone and has recently been referenced by other writers (c.f. Fisher (1973) and Patinkin (1976)).
6. Leontieff (1971).
7. Worswick (1972).
8. Pigou (1908), pp. 29-32.
9. Hutchison (1968).
10. Reported in a recent presidential address by Heller (1975).
11. The sentiments I have expressed in this paragraph were advanced some time ago by Colin Simkin (1969) in a Presidential Address to the Association of New Zealand and Australian Economists.
12. Koopmans (1957).
13. Klein and Goldberger (1955).
14. A very useful "tutorial" paper on the nature of spectral methods and their use in economics has recently been published by Engle (1976).
15. Ample testimony is contained in the volumes edited by Hickman (1972) and the recent symposium on "Econometric Model Performance" in the *International Economic Review,* 1974/75. The reader may also profitably refer to the sharp attack by Basmann (1972a) on the quantitative foundations of the Brookings model, the reply be Fromm and Klein (1972) and the subsequent rejoinder by Basmann (1972b).
16. See, in particular, Cooper (1972) and Nelson (1972); but note also the rather more encouraging results given by Christ (1975).
17. See Christ (1975). But note also the more favourable conclusions of Fromm and Klein (1976) who, while admitting that there are discrepancies among the values of dynamic multipliers across models, still argue that there is "a fair amount of agreement".
18. Grandmont and Laroque (1976) and Benassy (1976) are examples of the French school and Barro and Grossman (1976) of the American School.
19. Bergstrom and Wymer (1976).
20. Fair (1974) and (1976).
21. For instance, the model by Bergstrom and Wymer (1976) comprises ten stochastic equations and contains thirty-five unknown parameters; it was estimated from forty-eight quarterly observations. In the model by Fair (1976) there are twenty-six stochastic equations and one hundred and sixty-

six unknown parameters; the model was estimated from eighty-two quarterly observations.

22. Kuznets (1941) pp. 527–528.
23. Basmann (1974).
24. In this respect the paper by Anderson (1976) is illuminating.
25. See, for example, some of the remarks made by Kmenta (1974) and Maddala (1974) in their comments on Basmann (1974).
26. Edgeworth (1905). Note that this long paper appeared in two parts. As far as I can gather, it has never been mentioned that Edgeworth considered, inter alia, in Part II of this paper approximations for bivariate distributions and the distribution of a function of sample moments.
27. The result was first proved by Cramer (1928). A modern treatment is given by Feller (1971) Chapter XVI.
28. The two path breaking papers are Sargan (1975) and (1976).
29. It is only fairly recently that facilities have been developed for computers to execute elementary algebra and calculus. A very helpful review of current developments in this area is given by Barton and Fitch (1972).

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