The history of econometric ideas

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CAMBRIDGE
UNIVERSITY PRESS

Published by the Press Syndicate of the University of Cambridge
The Pitt Building, Trumpington Street, Cambridge CB2 1RP
40 West 20th Street, New York, NY 10011–4211, USA
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

First published 1990
First paperback edition 1992
Reprinted 1995, 1996

British Library cataloguing in publication data

Morgan, Mary S.
The history of econometric ideas – (Historical perspectives on modern economics).
1. Econometrics, history.
I. Title. II. Series.
330'.028

Library of Congress cataloguing in publication data

Morgan, Mary S.
The history of econometric ideas / Mary S. Morgan.
p. cm. – (Historical perspectives on modern economics)
Bibliography.
Includes index.
ISBN 0 521 37398 0 hbk ISBN 0 521 42465 8 pbk
1. Econometrics. I. Title. II. Series.
HB139.M66 1990
330'.01'5195–dc20 89–9790 CIP

ISBN 0 521 37398 0 hbk
ISBN 0 521 42465 8 pbk

Transferred to digital printing 2003

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Introduction

Econometrics was regarded by its first practitioners as a creative synthesis of theory and evidence, with which almost anything and everything could, it seems, be achieved: new economic laws might be discovered and new economic theories developed, as well as old laws measured and existing theories put to the test. This optimism was based on an extraordinary faith in quantitative techniques and the belief that econometrics bore the hallmarks of a genuinely scientific form of applied economics.[1] In the first place, the econometric approach was not primarily an empirical one: econometricians firmly believed that economic theory played an essential part in finding out about the world. But to see how the world really worked, theory had to be applied; and their statistical evidence boasted all the right scientific credentials: the data were numerous, numerical and as near as possible objective. Finally, econometricians depended on an analytical method based on the latest advances in statistical techniques. These new statistical methods were particularly important for they gave economists of the early twentieth century ways of finding out about the world which had been unavailable to their nineteenth-century forebears, ways which, in themselves, seemed to guarantee scientific respectability for econometrics.

So, when econometrics emerged as a distinct activity at the beginning of the twentieth century, its use of statistical methods and data to measure and reveal the relationships of economic theory offered a form of investigation strikingly different from those of nineteenth-century economics, which ranged from the personal introspection and casual observation of classical economics to the detailed empiricism of historical economics. The applied economics of the twentieth century was to be based on a more modern technology: its tools were to be statistical methods, mathematical models and even mechanical calculators. With

[1] Moore (1911) (particularly pp. 4–6) provides an excellent example of the optimistic programme laid down in econometrics and its associated technology.

the appeal of this new technology, econometrics was to become firmly established by the 1940s, and the dominant form of applied economic science thereafter.

Those readers who know something of present-day econometrics may already suspect that the econometrics programme has changed in some respects from its original conception and practices. The most salient difference between early and modern econometrics is that the early econometricians consciously *conjoined* mathematical economics and statistical economics. Indeed the Econometric Society, founded in 1931, had as its main aim:

> to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences. Any activity which promises ultimately to further such unification of theoretical and factual studies in economics shall be within the sphere of interest of the Society.
>
> (Frisch (1933b), p.1)

For the econometricians of the first half of the twentieth century, the union of mathematics and statistics with economics was the ideal way of practising scientific economics.

Exactly how the econometric ideal, the union of mathematical and statistical economics, emerged in the early twentieth century is an interesting question. The obvious supposition is that the roots of econometrics lie in the mathematical and statistical economics of the nineteenth century. Yet, in the contemporary view of that time (as in economics today), mathematics and statistics were believed to operate in different spheres. Mathematics was thought essential to the further advancement of economics as a deductive scientific discipline.[2] It was believed that the use of mathematics, that is calculus and algebra, would lead to clarity and conciseness in the expression of theories (and of the assumptions these involved) and that the process of mathematical reasoning would make economic arguments more rigorous. The many virtues claimed for the mathematical method helped it to gain acceptance amongst a small group of analytical economists in the nineteenth century, with the result that mathematical economics was at the cutting edge of theoretical work from the 1880s. But, if one takes into account the full range of styles and methods in economics,

[2] See for example Schabas' papers (1984) on Jevons, and (1989) on Marshall. Of course, the desire for a scientific economics as opposed to a theological or historical economics was not shared by all nineteenth-century economists. Arguments about the validity of using mathematics and statistics in economics both pre-date and post-date the development of econometrics.

mathematical reasoning was used by only a handful of economists, albeit an influential one, in the last quarter of the nineteenth century. The method only really came into more common, though still not widespread, usage in the period after 1930. Numerical examples and geometric illustrations were sometimes introduced to help explain economic ideas before algebra and differential calculus were used in theory development, but even these were still comparatively rare before 1900.[3]

By comparison, the use of statistical data in economics has a long history going back to the political arithmetic school of Petty and Graunt in the late seventeenth century.[4] But there was no continuing tradition, not that is until the general rise of statistical thinking in the nineteenth century which was particularly associated with the emergent social sciences.[5] It was believed then that the application of statistical analysis would improve the inductive side of economics, and as the nineteenth century wore on, the casual and often indiscriminate use of statistical data in economics did begin to give way to a more careful usage. Statistical data proved helpful in establishing economic regularities, in presenting economic arguments and most effectively in taking measurements of economic variables (of which easily the most sophisticated was the construction of index numbers).

Jevons even hoped that statistics could be used to obtain the numerically precise (or 'concrete') laws thought to be typical of good physical science.

> I do not hesitate to say, too, that Political Economy might be gradually erected into an exact science, if only commercial statistics were far more complete and accurate than they are at present, so that the formulae could be endowed with exact meaning by the aid of numerical data.
>
> (Jevons (1871), p. 25)

Perhaps he had in mind the example of astronomy, generally regarded as the most perfect and advanced of the sciences, which had by the mid-nineteenth century already invoked the aid of both mathematics and statistics. Astronomers faced with several different measures of the path of a planet (believed to be of an exact mathematical form)

3 The post 1930s timing is suggested by G.J. Stigler (1965) who analysed the proportion of journal articles using mathematical techniques of all sorts. The early uses of graphs in economics are surveyed by J. L. Klein (1987a).

4 The political arithmetic school of late seventeenth-century Britain and the physiocrats of eighteenth-century France are covered at some length in Schumpeter's (1954) discussion of examples of statistics in economics. In passing he also mentions other economists who depended on statistical information in Spain (sixteenth century) and in Germany and Italy (eighteenth century).

5 See the essays in the fourth section: 'Society' in Krüger *et al.* (1987, I), and Porter (1986), which I discuss later in this introduction.

extracted the equation of the path, discarding the residuals as errors of measurement. Economists could look also to the field of psychology, where statistical techniques were first used in the 1860s to measure stimulus–response relationships in experimental circumstances (the Weber–Fechner law). These 'psychophysicists' adopted both the mathematical model and statistical methods of early nineteenth-century astronomy, thus effectively safeguarding both determinism in theories and objectivity in applied work, in order to gain scientific respectability for psychology.[6] It does not seem too far-fetched to imagine a polymath such as Jevons experimenting on himself to try and measure increments of utility, rather as Fechner measured his own ability to discriminate between differently weighted objects. Note, though, that the possibility of experiment was not a necessary prerequisite to the use of statistics to measure economic relationships; after all, astronomers could not manipulate the planets and stars.

But these are speculations, for econometrics did not emerge in the nineteenth century. With hindsight, one can see a number of obstacles which precluded a unified scientific programme of mathematical and statistical economics of the sort which flowered in the first half of the twentieth century. One obvious problem was the lack of relevant economic data: not all theoretical variables had measurements or even observational counterparts (utility being only one example). Equally pertinent, statistical methods (discussed in more detail later in this introduction) were not sufficiently advanced to be able to assign numerical values to the complex causal laws of behaviour which featured in economic theories. Even in the late nineteenth century statistical methods could still do little more for the social sciences than establish descriptive regularities in single variables, like suicide, or provide comparisons of regularities in two variables. Even had data and appropriate methods been available, mathematical economists rarely framed their theories with an eye to making them amenable to statistical treatment. With some prescience, J. N Keynes (1891) noted that the geometric representations favoured by mathematical economists 'lend themselves naturally to the registering of statistics', but geometric representations as I have noted already were still unusual. Last but not least, nineteenth-century economists believed that mathematics and statistics worked in different ways: mathematics as a tool of deduction and statistics as a tool of induction. Jevons, who pioneered the use of both mathematics and statistics in his work,

[6] See S. M. Stigler (1986) for a recent account of the nineteenth-century use of statistics in astronomy and psychophysics. Gigerenzer relates how the tactics of psychophysicists were followed by later generations of psychologists in essay 1 in Krüger *et al.* (1987, II).

expressed both the status quo of those aiming at a scientific economics and his own vision of econometrics when he wrote:

> The deductive science of Economy must be verified and rendered useful by the purely inductive science of Statistics. Theory must be invested with the reality and life of fact. But the difficulties of this union are immensely great. (Jevons (1871), p. 26)

It might be argued that we should look not to the history of economic methods but to the history of the people, the economists themselves, in order to understand where econometrics came from. Here, too, we can suggest possibilities rather than find definitive answers. In one of the few systematic descriptions of the rise of quantification in nineteenth- and early twentieth-century economics, Spengler (1961) discusses both national styles and schools of thought. He suggests that quantification flourished in the neoclassical school, and that Marshall and Edgeworth were particularly responsible for its success, for both believed in the complementarity of the deductive and inductive methods in economics. Yet Marshall did not have much time for statistical economics (and his strong influence on English economics perhaps accounts for the poor representation of Britain in early econometric work) and Edgeworth's peculiar brand of probability and economics did not turn into econometrics. That other great mathematical and neoclassical pioneer, Walras, like his compatriot Cournot, had no time for statistics (see Ménard (1980)). Jevons alone seems to fit the bill and the surprise is rather that despite his pronouncements he avoided the complementary use of mathematical models and statistical techniques in his applied work. Further, the most important pioneer American econometrician, Moore, embraced econometrics partly because he became disenchanted with the ideas and methods of the neoclassical school. Although econometricians of the 1930s were to refer to their tradition as that of Cournot, Walras and Marshall, any simple derivation of econometrics from the neoclassical programme is highly dubious, not least because it ignores the important inputs from empirical economics.

Late nineteenth-century economists of the historical and institutionalist schools tended to welcome the growing wealth of statistical facts without necessarily adopting statistical explanations. In Germany, the historical school of political economists formed the nucleus of a wider intellectual circle which warmly embraced statistical thinking.[7] This circle provided intimate links between statistical thinkers such as Lexis and Engel and historical economists such as Schmoller. Craver and

[7] See the three overlapping accounts by Porter, Hacking and Wise (essays 17–19) in Krüger *et al.* (1987, I).

Leijonhufvud (1987) trace both how this empirical statistical strand entered the American institutionalist school in the late nineteenth century (many of whose members had trained in Germany with economists of the historical school) and how this statistical approach was re-exported back to Europe in the 1920s. But the American economists who accepted statistical evidence were often those who most strongly rejected mathematical methods and models in economics. The USA apart,[8] the most fertile environments for the practical development of econometrics in the early twentieth century proved to be the Netherlands and Scandinavia, yet Spengler suggests that nineteenth-century Dutch economists had no particular strengths in mathematics or statistics and the Scandinavians were good in mathematics but had little in the way of statistical background. So, despite many interesting comments on mathematical and statistical economics separately, Spengler does not help us to gain a clearer idea about the personal process by which econometrics emerged.

This search for a pattern reaching back into nineteenth-century economics may well be doomed, for the econometricians of the early twentieth-century period were not wedded to any particular tradition. They were a determinedly international bunch, of diverse intellectual backgrounds, and eclectic in their economic beliefs.[9] National style and theoretical allegiance seemed to matter less than the enthusiasm econometricians generated for their common methodological programme. Frisch's memory of the First European Meeting of the Econometric Society in 1931 in Lausanne was vividly recalled in 1970:

> We, the Lausanne people, were indeed so enthusiastic all of us about the new venture, and so eager to give and take, that we had hardly time to eat when we sat together at lunch or at dinner with all our notes floating around on the table to the despair of the waiters. (Frisch (1970), p. 152)

Tracing out the personal roots of this enthusiasm, and the links between econometricians and the wider scientific community of the twentieth century, remain important tasks for the future. The most fruitful place to start such a search might well prove to be amongst the statistical thinkers, for the content and evolution of the econometric programme in its formative years was much influenced by developments in statistics. And in the longer run, mathematical economics and

[8] Craver and Leijonhufvud (1987) also stress the importance of the intellectual migration from Europe in the later period (1930s and 1940s) to the econometric work done in the USA during the 1940s.

[9] That the econometricians believed in freedom of economic theory is borne out by a glance at the original Advisory Editorial Board of *Econometrica* which included economists from many different schools of thought.

statistical economics divided again, leaving econometrics firmly on the statistical side of the fence.[10]

When we look at the history of statistics we find that econometrics was far from an isolated development, for statistical methods and probabilistic thinking were widely introduced into nineteenth- and early twentieth-century science. Few scientific fields remained unaffected, although statistical thinking and probability did not always enter at the same level in each discipline, as I have helped to argue elsewhere.[11] The econometrics movement was paralleled in particular by biometrics in biology and psychometrics in psychology. As their names suggest, all three were concerned with measurement and inference, in particular with the use of statistical methods to reveal and to substantiate the laws of their subject matters; and all three developed at roughly the same time.

Two recent books on the history of statistics prior to 1900 set the scene for this explosion of statistical activity (of which econometrics was a small part) in the early twentieth century. T. M. Porter's (1986) excellent book illuminates the difficult terrain between statistical thinking (ways of analysing masses of events) and probabilistic thinking (where chance and randomness hold sway). This account is complemented by S. M. Stigler's (1986) equally interesting history of the techniques of statistical inference. Both authors stress the importance in the early nineteenth century of Quetelet's statistical characterisation of human behaviour: individuals behave in an unpredictable way, but taken together these apparently disorganised individuals obey the law of errors in deviating from the ideal 'average man'. This observation of statistical regularity in human affairs proved enormously influential. For example, Porter shows how such social statistics led physicists, by analogy, to statistical mechanics. For Stigler, on the other hand, Quetelet's notions created barriers to the transfer of

[10] Although the econometricians aimed to synthesise mathematical and statistical economics, it is not clear whether mathematical models were necessary for the successful application of statistics to economics. The material in the first two Parts of this book suggests only that mathematical economics was an important prerequisite for certain applications of statistics, but not all (see also Morgan (1988)). That there is no absolute necessity for the two methods to go together is evident when we remember that Galton required no mathematical law of inheritance to see the statistical regression relation in his data on the heights of fathers and sons. In psychometrics, we find mathematical models developed out of the statistical work rather than being an essential part of its development.

[11] The question of levels of entry is discussed in the introduction to Krüger *et al.* (1987, II), and the papers in that volume provide case studies of the use of statistics and probability in the sciences. These show that in some sciences, probability and statistics helped in measurement and data description; in others, the focus was on inference; and in others still, such ideas entered the theories themselves.

inference techniques from astronomy and geodesy to the emergent social sciences.

Quetelet's influence is reflected in the typical nineteenth-century interpretation of statistical regularity to be found in Mill's (1872) classic discussion of methods of scientific inference.[12] Thus: taking many observations together has the effect of eliminating those circumstances due to chance (i.e. the variable or individual causes), and the statistical regularity that emerges from the mass of data verifies that a causal law (or constant cause) is at work. This idea of individuals as subject to one constant cause and many small variable causes (obeying the law of errors) left the constant cause operating at, or through, the medium of 'society'. But statistical inference procedures were not adept at uncovering such a constant cause of the average man's tendency to suicide, or to murder, or whatever. A plurality of causes acting at the individual level might be more credible to our minds, but would be equally difficult to uncover, for, as Stigler points out, there were no natural or external (theoretically imposed) ways of categorising such social observations into the homogeneous groups necessary for the existing statistical analysis, and as yet no statistical methods to deal with the plurality of causes.

The resolution of these difficulties began in the late nineteenth century with the reinterpretation of people's behaviour not in terms of errors, but as natural or real variation due to the complex causes of human and moral affairs. Porter identifies this as the second breakthrough in statistical thinking, when the 'law of errors' became the 'normal distribution'. This led directly to the development of statistical laws of inheritance in genetics, namely regression (1877–86) and correlation (1889–96).[13] These law-like relations are important because they are, according to Hacking (1983a), the first genuinely 'autonomous statistical laws': laws which offer an effective explanation for phenomena without the need to refer back to previous causes. In Porter's account, the adoption of such statistical models freed the social and natural sciences from the determinism of nineteenth-century physical science, and led both to a flowering of statistical work in diverse applied fields and to the development of the theoretical field of

[12] The original edition of Mill's treatise was 1843. My account reflects the situation in the eighth edition (1872) (the last in his lifetime), by which time Mill had become acquainted with Quetelet's ideas on statistics via Buckle's popularising efforts. From the point of view of the interpretation of statistical regularities, the situation changed little between these editions, or until the end of the century, as is evident from Durkheim's (1895) treatise on sociological method.

[13] The span of these dates reflects the main development of the concepts and their mathematical expression, based on S. M. Stigler's (1986) and Porter's (1986) accounts.

mathematical statistics. For Stigler, it is Yule's (1897) contribution linking these new statistical laws to the old astronomical method of least squares which was the crucial step in making least squares regression a general tool for statistical analysis. This transformation opened up the development of multivariate analysis in which there was the possibility of statistically controlling (or categorising) one variable while investigating the behaviour of the remaining variables.

An account which aims at a more specialised history of the conceptual foundations of econometrics, from late seventeenth-century political arithmetic to 1920, is offered by J. L. Klein (1986). Although she draws on the same areas of applied statistics as both Porter and Stigler, she tells yet another story. Her argument is that both biometrics and econometrics were founded on the need to make temporal variation amenable to analysis. Thus, logical (or non-temporal) variation in human phenomena were given a statistical identity in terms of Quetelet's 'average man' and Pearson's normal distribution. In order to provide for the temporal variation between generations, biometricians developed the relationships of regression and correlation. Econometricians adopted these new biometric tools, along with some notions of stochastic processes from astronomy, to provide statistical ways of characterising economic relationships over time.

These three accounts of the history of statistics by Porter, Stigler and Klein are all very suggestive, but why should economists want to adopt the new statistical methods? What was it about the new methods that justified early twentieth-century econometricians' optimism about their approach? The answer lies in the ability of the new statistical methods to provide a substitute for the experimental method. The idea of a scientific or controlled experiment is to reproduce the conditions required by a theory and then to manipulate the relevant variables in order to take measurements of a particular scientific parameter or to test the theory. When the data are not collected under controlled conditions or are not from repeatable experiments, then the relationship between the data and the theoretical laws is likely to be neither direct nor clear-cut. This problem was not, of course, unique to economics; it arose in other social sciences and in natural sciences where controlled experiments were not possible. In order to see how statistics comes in here, we need to return just once more to the nineteenth century, when economists seeking a more scientific profile for economics regularly bewailed the fact that the experimental method available to the physical sciences was inapplicable to economics.

A statistical substitute for scientific experiment in the nineteenth century relied on the statistical method's ability to extract a regularity, or repeated pattern, or constant relationship, from a mass of data (instead of taking one observation from an individually manipulated or varied event). As Porter's account emphasises, the ability of statistics to discern order out of chaos was one of the crucial innovations in nineteenth-century social statistics. And, I have referred already to the example of astronomy, the paradigmatic example of nineteenth-century use of statistical methods in which an exact relationship was extracted from several varying measurements of the relationship. But such nineteenth-century statistical ideas and methods did not necessarily provide ready-made solutions to twentieth-century problems. Yule expressed this most neatly:

> The investigation of causal relations between economic phenomena presents many problems of peculiar difficulty, and offers many opportunities for fallacious conclusions. Since the statistician can seldom or never make experiments for himself, he has to accept the data of daily experience, and discuss as best he can the relations of a whole group of changes; he cannot, like the physicist, narrow down the issue to the effect of one variation at a time. The problems of statistics are in this sense far more complex than the problems of physics. (Yule (1897), p. 812.)

The highly complex causal laws of the social and biological sciences required measurement methods designed to neutralise or allow for the effects of the variable (i.e. uncontrolled) circumstances under which data have been collected in place of the full control which defines the ideal case of the scientific experiment. These are precisely the characteristics which Stigler seizes upon in his account of the new statistical methods emanating from the biometric school; measurement methods which were to enable the twentieth-century scientist some degree of control over non-experimentally obtained data and which allowed them to deal with the plurality of causes.[14]

It does seem that the conditions necessary for the successful prosecution of a statistical economics were fulfilled in the early twentieth century, but we should beware, for there is no necessity about any field's adoption of new ways of statistical thinking as the variety of case studies in Krüger *et al.* (1987, II) makes clear. In addition, both Porter

[14] As support for my argument here, it is appropriate to note that many twentieth-century philosophers of science take it for granted that scientists use these statistical methods as a substitute for experiments they cannot conduct. Whether statistical methods are efficacious in uncovering causal laws continues, as in the nineteenth century, to be open to question. Of several recent contributions, Cartwright's (1989) treatment is particularly relevant for she discusses the case of econometrics. She argues that the statistical methods used in econometrics could in principle give knowledge of causal relationships, though very often the presuppositions of the method are not met, so that the causal inferences can not be made (see her Chapters 1 and 2).

and Stigler stress the difficulties of adapting statistical tools and ideas designed in one scientific arena for use in another. This problem did not simply disappear in 1900, as evidenced by the fact that biometrics, psychometrics and econometrics forged their own different versions of the new tools. In biometrics, R. A. Fisher designed techniques to randomise (and thus neutralise) the effects of non-controllable factors in agricultural experiments. Psychometricians such as Thurstone developed Spearman's factor analysis method to extract measurements of the apparently unobservable 'vectors of the mind' from data on observable characteristics in order to solve their own data-theory gap.[15] Neither technique was directly appropriate for economics. Provided with general tools, econometricians still had to develop their own statistical solutions to the problems of bridging the gap between conditions demanded by theory and the conditions under which data were collected. It would be wrong to suggest that, in doing so, econometricians were consciously making their own substitute for experiments, rather they responded to the particular problems of measurement and control, the particular mismatch between theory and data, which occurred in their applied work. Only later did they begin theoretical discussions and seek general solutions to their practical problems.

The structure of this book reflects the importance of applied work in the development of econometric ideas: applications formed the catalyst necessary for econometricians both to recognise their difficulties and to search for solutions. Parts I and II of the book trace the evolution of econometrics through the practical work on business cycles and on market demand analysis, which together constituted most of the applied econometrics of the period up to about 1950. The exploration of these fields reveals not only advances in understanding, but also confusion and dead ends. Part III of the book reconstructs the history of formal econometric models of the data-theory relationship. This final part draws on the applied econometric work discussed in the earlier sections and on the theoretical econometrics which began to develop during the 1930s. By the 1940s, theoretical discussions and applied practice had crystallised into a programme which is recognisable as modern econometrics.

[15] The different ways in which the new statistical methods were taken up by American psychologists are discussed by Danziger and Gigerenzer (essays 2 and 3 in Krüger *et al.* (1987, II)) who give considerable insight into the way data-theory problems were overcome in that field.