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Chapter 2

"The ghosts I called I can't get rid of now": The Keynes-Tinbergen-Friedman-Phillips Critique of Keynesian Macroeconometrics

2.1 Introduction

2.1.1 This chapter offers a fresh perspective on the much publicised dispute between those followers of Keynes who presented econometric evidence in favour of a Phillips curve trade-off, and those monetarists who presented counter econometric evidence. Contrary to common perceptions, the collapse of the Keynesian Phillips curve was a vindication of a common critique of macroeconomic practices, which was jointly authored by John Maynard Keynes, Jan Tinbergen, Milton Friedman and A.W.H. 'Bill' Phillips. This analysis is informed by the usual sources, plus two sources which had been thought to be no longer in existence (Phillips' private papers and the London School of Economics (LSE) Methodology, Measurement and Testing (M²T) Staff Seminar records), plus two essays by Keynes (1938a, 1938b) which have been overlooked in this context.

Keynes' critique of econometrics can be disaggregated into three distinct categories: namely, technical issues that could be overcome by further research; criticisms that were directed at macroeconomics, but not necessarily at microeconomics; plus concerns about the possibility that econometrics, in the wrong hands, would become a hazard for the economics profession. Econometricians have long been aware that Keynes' detailed technical criticism were sometimes ill-founded (Robert Gordon 1949, 53, n4). Since this first category of Keynes' critique is not germane to the theme of this chapter, these can be safely relegated to a footnote.¹

This framework is used to marshal evidence in favour of four propositions. The first (section 2.3) is that Tinbergen acknowledged the validity of the central thrust of Keynes' critique of the more mechanical aspects of the econometric practices that were developing

in the late 1930s; practices that had been unwittingly stimulated by the *General Theory* (Stone 1978, 62; Tinbergen 1947). The second proposition (section 2.4) is that there is a considerable overlap between the views of Keynes and Friedman with respect to econometrics .

The third proposition (section 2.4) is that Friedman (who during World War II, was Deputy Director of the Statistical Research Group, and who must be regarded as one of the most statistically literate economists of all time) was implicitly *predicting* that macroeconomic disputation could only end inconclusively. In the late 1950s, there was a changing of the Keynesian guard in Cambridge, Massachusetts, with Alvin Hansen, who was sceptical about econometrics, making way for younger Keynesians who apparently did not share these doubts. 1958 was a pivotal year in macroeconomic history: it was the year of Phillips' seminal empirical paper; and around this time, the Senator from Massachusetts began to mobilise his "academic Kennedy gang" at Cambridge (Halberstam 1972, 157; Leeson 1997a, 1997b). It was also the year that H.S. Houthakker, on study leave at Harvard, chose to engage Friedman (1957) in an econometric dispute over the relative merits of rival consumption functions. This may also explain why the anti-Keynesian counter-revolution, when it came, was monetarist and not Austrian (section 2.4).

The fourth proposition (section 2.5) is that Phillips was an insightful early critic of Keynesian Phillips Curve econometrics. This section is based, in part, on the recently discovered - and complete - seminar records of the LSE Staff Seminar on Methodology, Measurement and Testing (M²T). This section, therefore, supplements Neil de Marchi's (1988) fascinating discussion, which was based on the best - but incomplete - records then available. Some concluding remarks are provided in section 2.6. A brief historical introduction is provided in section 2.2.

2.1.2 Econometrics has had some success stories; it has also had some less impressive episodes. More importantly, it has become an ambiguous, but high status language, engaging a large share of professional effort. According to George Stigler (1962a, 1), the

statistical evaluation of economic relationships is the only distinctive trait of modern economics; but in 1912, Fisher had been unable to find enough interested people (apart from W.C. Mitchell and a few others) to establish an Econometric Society. This Society was ultimately founded by only sixteen people, at a meeting at the Statler Hotel, Cleveland, Ohio, in December 1930; the first European meeting of the Society, in Lausanne in 1931, attracted about twenty people; and the first edition of *Econometrica* had a circulation of less than 300 (Cowles 1960, 173-4; Frisch 1970, 152; Christ 1952, 5; Bjerkholt 1995, 755). But today, econometrics occupies a large proportion of the pages of the professional journals, and according to Darnell and Evans (1990, ix) some see econometrics as an "umbrella discipline for economics".

Some econometricians have made an impressive theoretical contribution to statistical analysis; but doubts remain about the value of the 'average economic regression'. In the pre-econometric age, the average academic economist could aspire to become an authority on some aspect of the economy; now, it seems, many economists find that professional advancement is more easily facilitated by applying (or misapplying) estimating techniques to data - the quality or relevance of which often remains unexamined - in spite of Keynes' warnings.

The econometric pioneers had great hopes that they were uncovering a 'rock' upon which to base reliable policy advice (Frisch 1970). But later econometricians (Pagan 1984, 103) have been scathing about the research strategy which underpinned the econometric models of the 1960s, which appeared to suggest that inflation would reduce unemployment. Econometricians (Laidler 1985) have also been concerned that the unemployment cost of reducing inflation - the dominant policy derived from 'natural rate' econometric models from the 1970s onwards - was much greater than anticipated by those econometric models. An investigation into the origins of this unwarranted confidence in macroeconometric models is also, simultaneously, an investigation into the origins of these policy failures.

Despite Schumpeter's (1933, 5) protestations, many econometricians have neglected, if not "belittle[d]", a superbly rich data source - the serial dependent history of their own

subject. If the history of econometrics stood in equal status along side other sub-disciplines within economics, this might tend to alleviate some of the rather disturbing problems with regard to both graduate education and professional incentives: "something is terribly wrong in the economics profession and in the incentives that economists perceive ... in economics normal science has run amok. The invisible hand of truth has lost its guiding influence" (Colander 1989, 31, 34-5; Colander and Klammer 1988).

2.2 Historical Background

The econometrics movement was moulded, to a large extent, by the desire to understand and tame the inter-war business cycle. In April 1928, Charles F. Roos, Ragnar Frisch and Irving Fisher met, at Fisher's home in New Haven, to set in motion the ball that would lead to the Econometric Society. In 1931, Alfred Cowles discontinued his forecasting service, explaining to his clients that he was insufficiently informed about the nature of business and stock market fluctuations. In October 1931, Roos - who initially thought that he had received a crank letter - met with Cowles and Fisher to discuss Cowles' offer to fund econometric research. Roos subsequently became Research Director of Roosevelt's National Recovery Administration, the first Director of Research at the Cowles Commission, and, from 1937, the head of The Econometric Institute, a private forecasting agency (Christ 1952, 3-17; Rima 1988, 17; Cowles 1960, 173-4). Roos' (1955, 394-5) monumental Survey Article on forecasting techniques expressed enormous confidence in econometric techniques, it also offered the prospect of rebutting Keynes' proposition that investment demand was beyond the reach of forecasters.

Keynesian and Marxian economics are modern versions of the 'endogenous instability of capitalism' thesis. In 1929, the Dutch Statistician's Office, under Tinbergen's editorship, began a statistical business cycle journal. The Great Depression gave an added dimension to these controversies. In the 1930s, many observers were concerned about the long run viability of capitalism and of the apparently infeasible combination of political liberty and economic freedom (Desai 1981, 41). Marschak initiated one of the earlier debates on the viability of socialism as an economic system, involving Pareto, Barone, von Mises, Schumpeter, von Hayek, Lange and Lerner. The 1940 Cowles Commission Report stated

that unemployment was the primary economic problem to be tackled (Malinvaud 1988, 191; Christ 1952, 22). Many economists lost their faith in the ability of markets to solve the problem of unemployment, and many embraced the new faith of economic planning. Tinbergen (1984, 315), for example, retrained as an economist under the influence of the onset of World Depression; he regarded his econometric work, and in particular his estimation of parameters, as providing the tools to effect socialist intervention in the economy in order to minimise cyclical fluctuations and poverty (Tinbergen, in Magnus and Morgan 1987, 118-9; Knoester and Wellink 1993, 19). The within-sample 'explanatory' power of Tinbergen's equations were high; generally his R^2 exceeded 0.98 (Epstein 1987, 33, 48).

Lawrence Klein shared this approach to econometrics; later he would be persecuted because of his socialist convictions. Harold Hotelling also favoured market socialism (Arrow 1990, 107). Oscar Lange was a "proclaimed socialist", and later a member of the Polish Communist government (Friedman 1974a, 15). Ragnar Frisch also had socialist leanings, according to Tinbergen (conversation with Arie Kapteyn, 15 November, 1994; Blaug 1985, 67). Frisch came to believe that uncovering the underlying structure of the economy - the structural parameters - would enable the business cycle to be tamed (Epstein 1987, 41). Part of this optimism may reflect the initial training in physics which Tinbergen, Frisch, Koopmans, and others had been exposed to (Kol and De Wolff 1993, 29; Mirowski 1989; Tinbergen 1984, 315; Craver and Leijonhufvud 1987, 175; but see Andvig 1985).

These ideological undercurrents were present in many of the business cycle research institutes which were established all over Europe and the United States in the 1920s. The New School for Social Research opened in 1919, attracting refugees from Europe, and from Columbia University, whose President, Nicholas Butler, had pledged the University to stand firm against the "rule of the literary and academic Bolsheviki" (cited by Bender 1987, 299). In Russia in 1917, there had been a political victory for those who believed in the endogenous instability of capitalism thesis. In 1920, the Konjunktüre Institute of Moscow was founded, with Kondratieff as Director. The controversy over business

cycles was more than idle speculation and involved political passions of "venomous ferocity" (Schumpeter 1954, 1158, n.8). In 1928 the Konjunktur Institute was closed down and Kondratieff was sent to Siberia and subsequently shot, whilst his long wave theory of the business cycle was labelled "wrong and reactionary" (Garvy 1943, 204; Solzhenitsyn 1973, 50; Nove 1992; Morgan 1990, 66-7).

Interest in the business cycle and its control were not, of course, an exclusively socialist preoccupation. In the nineteenth century, Marx used the business cycle as his fundamental unit of analysis; so too did W.S. Jevons (Schumpeter 1954, 742; Morgan 1990, 16). Marx built an endogenous instability of capitalism thesis; Jevons sought to locate the origins of commercial crises in *exogenous* forces, namely a 10.45 year sunspot cycle. As late as 1923, Henry Ludwell Moore outlined a similar causal sequence extending back to movements in the planet Venus (Stigler 1962a, 11). But Gottfried Haberler, whose League of Nations study, *Prosperity and Depression* led directly to Tinbergen's econometric work, concluded that non-economic explanations of the origins of the business cycle were, by the inter-war period, rare if not eccentric (1939, 9-10; but see Keynes 1936a, 329-332).

After the war came the *methodenstreit* between the economic statisticians represented by Burns, Mitchell, Friedman and Vining at the NBER, and the econometricians, represented by Koopmans and the Cowles Commission. The context, and hence the emphatic nature of this *methodenstreit*, was the quite spectacular failure of Keynesian models in the immediate post-war period. After the U.S. Full Employment Bill, the Cowles Commission Paper No. 23 stated that this *methodenstreit* was between "the nonstatistical economist" and "The Use[rs] of Econometric Models as a Guide to Economic Policy"; concluding that "the latter is better equipped" (Klein 1947, 112). Paul Samuelson (1944a, 1944b, 1988, 63-4) predicted "Unemployment Ahead"; and even though U.S. unemployment in 1946 turned out to be 3 million, rather the 8 million predicted in late 1945, the choice was now between tackling the forecasting problem "with renewed vigour", or discarding econometrics and "relax[ing] again into armchair comments ... the line of least resistance ... We cannot be very hopeful about solving our economic

problems if we have to rely on such methods [of the pre-war guessers] in the future ... Econometric methods could not have been worse than any other methods that were used" (Klein 1946, 302-6).

2.3 Tinbergen and Keynes

In the last decade, econometrics has begun to attract the attention of increasing numbers of historians of thought, much of it focused on the Keynes-Tinbergen exchange. Most commentators have adopted one (or sometimes two) attitudes with respect to Keynes' critique. For some, it was a lamentable performance on Keynes part (Klein 1951, 450-1), traceable to his ill-health, technical rustiness and tactical predilections (Stone 1978, 62-3). For some, Keynes simply misunderstood what Tinbergen was attempting to do (Klant 1985), or "he did not really have the necessary technical knowledge to understand what he was criticising" (Samuelson 1946, 197, n11). For others, Keynes was, in a qualified way, more sympathetic to econometrics than had hitherto been supposed (Bateman 1990). Keynes' reference to "alchemy", it has been argued, might have been intended as a gesture of encouragement, suggesting that Tinbergen might ultimately succeed in creating the foundations of an econometric science (Rima 1988, 16). Some have even speculated that had he lived longer, Keynes might have become a computer-based modeller at the centre of a "high-tech 'circus'" (Bodkin, Klein and Marwah 1988, 9, n10, 10-11, n15). Alternatively, others have argued that Keynes' critique is still relevant to modern econometrics (Patinkin 1976; Hendry 1980). Indeed, the co-founder of the New Classical anti-Keynesian counter-revolution and the author of a devastating critique of econometric policy evaluation (constructed with Keynesian macroeconomics in mind) recognised an irony: "In referring to those who built in part on Tinbergen's work as 'Keynesian' I am, then, contributing to the continuation of an historical injustice" (Lucas 1977, 10, n5).

Too much of the history of macroeconomics has been bedevilled by attempts to label (and sometimes libel) the author of the macroeconomic Old Testament. Surprisingly, while there has been much discussion of the Keynes-Tinbergen debate, both at a general and at a specific level, there has previously been no attempt to dissect Keynes' critique into operational categories. By treating the Keynes critique *en bloc*, we are in danger of concluding that his suspicions about econometrics were "invalid" (Malinvaud 1991, 636);

or "venial and not to be remembered" (Stone 1978, 88, last sentence); or not worthy of mention (Stone 1980, section III). A disaggregated approach allows much greater light to be shone on those aspects of Keynes' critique which have relevance to contemporary econometric practices. It also reveals that Tinbergen finally acknowledged the potency of *parts* of Keynes' critique, *as Keynes predicted he would* (1939, 568, last paragraph). The words from Goethe's *Zauberlehrling*, which forms part of the title of this chapter, were prophetically cited by Tinbergen on the occasion of his Nobel Lecture (Tinbergen 1969, 43; for similar sentiments see Klein 1971a, 416).

Ragnar Frisch (1970, 164), Tinbergen's co-recipient of the first Nobel Prize in Economic Science, also bemoaned "the cascade of papers of the playometric kind"; and had long been sceptical about some of the directions of applied econometrics (Arrow 1960, 183). Tinbergen (1967, 272) criticised economists for being averse to time-consuming factual and statistical research which was required for quality empirical research. Tinbergen (1956, 149-85) also developed parts of what became known as the Lucas Critique (Lucas 1976, 20); and he assumed that "expectations are 'rational' i.e. are consistent with the economic relationships" ([1932], cited by Keuzenkamp 1991, 1247). Half a century after the publication of his *Econometric Approach To Business Cycle Problems*, Magnus and Morgan (1987, 136) asked Tinbergen, "How do you feel about the way econometrics has developed over the last twenty years or so? In 1952 you feared that techniques could take over from attention to human needs and problems in the field of economics. Do you feel this fear was justified?" Tinbergen replied: "I'm afraid, yes". Again, a vindication of *parts* of the Keynes Critique.

Keynes and Tinbergen are usually characterised as having incompatible views on econometrics. Yet, Tinbergen's Nobel Lecture can be viewed as the opening volley of the "orgy of self-criticism" (Blaug 1980, 253) which descended on the economics profession when the predictive power of many of the macroeconometric models of the period were found to be less than impressive (Leontief 1971, 3; Worswick 1972, 79; Phelps Brown 1972, 6). Given that *some aspects* of Keynes' warnings still retain their validity (Samuelson 1992, 243-4; Ormerod 1994, 92-112), it is instructive to disaggregate his

concerns about econometrics.

Keynes appreciated the qualities of "a real trained statistician", and was "in fundamental sympathy with the deep underlying conceptions of the statistical theory of the day". The subject of his second known letter to a newspaper was the interpretation of statistics; and one of his earliest academic disputes was with Karl Pearson over the appropriate statistical methods of studying the effects of parental alcoholism on offspring; a dispute which illustrated "the pitfalls of statistical inference" (Harrod 1951, 154). His final posthumously published article was "solely concerned with the available statistics". One of the themes of his career was the analysis of "the logical basis of statistical modes of argument", and the search for "the principles of sound induction" which might constitute "a good scientific argument ...". Keynes had planned to specialise in Logic and Statistical Theory, and *A Treatise on Probability* attempted to "cover the whole field of empirical thinking ... it would be difficult to find a parallel for a comprehensive attack of this kind since the days of Aristotle" (Harrod 1951, 126, 133-4). The final section of his Fellowship Dissertation was entitled 'The Foundations of Statistical Inference', which concluded with an 'Outline of a Constructive Theory'. The union of descriptive and inferential statistics was "the occasion of a great deal of confusion. The statistician who is mainly interested in the technical methods of his science, is less concerned to discover the precise conditions in which a description can be legitimately extended by induction. He slips somewhat easily from one to the other, and having found a complete and satisfactory mode of description he may take less pains over the transitional argument ... [but] he must pay attention to a new class of considerations and must display a different kind of capacity ... He is faced, in fact, with the normal problems of inductive science ... [involving material which] will be necessarily incapable of exact, numerical, or statistical treatment ... Generally speaking, therefore, I think that the business of statistical technique ought to be strictly limited to preparing the numerical aspects of our material in an intelligent form, so as to be ready for the application of the usual inductive methods". Most of the mathematical methods applied to statistical inference were invalid, and could "only lead to error and to delusion" (JMK XV [1908], 12; [1909], 20-1; VIII [1921], 359-60, 419, 427, 428, 468; XXVII [1946], 428, 430).

Keynes informed the Macmillan Committee that although "the empirical method is not by any means successful for the diagnosis [of the Depression] it is not by any means valueless for seeking the cure" (JMK XX [1930], 99). He concluded Volume II of his *Treatise on Money* (1930, 408) with a plea for greater quantitative knowledge: "statistics are of fundamental importance to suggest theories, to test them and make them convincing ... [and] to eliminate impressionism". He opened *The General Theory* with a call for a statistical examination of the relationship between changes in money wages and changes in real wages. It was on statistical grounds that he asserted that the wage units could "only be reduced amidst the decay and dissolution of economic society" (1936a, 9-10, 40-41, 102-4, 340, n1). Keynes was particularly opposed to the statistical method underpinning the American Keynesian 'Phillips curve' trade-off (JMK XXIII [1941], 181-93). Chapter 6 of the unwritten *Footnotes to the General Theory*, was entitled 'Statistical notes' (JMK XIV [1936], 134). He told Austin Robinson (1972, 535) that "all his best ideas came from messing around with figures and seeing what they must mean". But throughout his career he opposed the use of mathematical methods in both statistics and economics. When it came to questions of inference, experimental methods were often to be preferred to statistical methods (JMK XI [1911], 216). Certain methods of statistical analysis led to invalid results. Investigations of samples, but not complete populations, were also suspect. For statistics to be decisive, they had to extend over a period long enough to eliminate other influences (1936a, 104; JMK XIX.I [1923], 122).

Keynes was suspicious of all numbers derived by formulae from non-experimental data, especially when the original data had been suppressed. To "enable the reader to form some sort of independent judgement ... the real character of the evidence" must be displayed; not just the products derived from applying "mathematical machinery" (JMK XI [1910], 191). Graphs were highly suitable for "publicity or propaganda purposes", as Florence Nightingale discovered; but Keynes warned of the "horrid examples of the evils of the graphical method unsupported by tables of figures. Both for accurate understanding and particularly to facilitate the use of the same material by other people it is essential that graphs should not be published by themselves but only when supported

by the tables which will lead up to them. It would be an exceedingly good rule to forbid in any scientific periodical the publication of graphs unsupported by tables" (JMK XI [1938], 234). But he was unstinting in his support for the statistician Udny Yule in his quest for a lectureship in Cambridge (Skidelsky 1983, 222). Appropriately, Yule (1926) went on to produce some classic work on "nonsense correlations".

In 1923, Keynes helped launch the regular London and Cambridge Economic Series barometric survey of business conditions, and he repeatedly campaigned for improved economic statistics, not to be used for regression analysis, but to offer intuitive insights into reality (Stone 1978, 64-72; Skidelsky 1992, 106, 414, 270; JMK XXVII [1944], 371). He was very supportive of James Meade, Richard Stone and the new Central Statistical Department, and he thought that it was "most dangerous for too wide a gap to develop between inside and outside statistical information" (JMK XXII [1941], 329, 331). His first concern was whether the data were "good enough to stand the strain which has been placed upon them". The accuracy of statistics whose "sole purpose is to satisfy the ... troublesome and often trifling curiosity of the academic statistician" could not be relied upon (JMK XI [1929], 229; XV [1909], 36; XVIII [1923], 152; XXII [1939], 82); "The suspicion of quackery has not yet disappeared [from statistics] ... There is still about it for scientists a smack of astrology, of alchemy" (JMK VIII [1921], 367). But he looked forward to a systematic theory of statistics and the continued quantification of economics: "Whether the uniformity of economic settings is sufficient to enable the economist to make full use of this kind of work, time will show" (JMK XI [1909], 50-51, [1929], 226). The "excellently complete statistics now available in the United States" were available to illustrate aspects of the theory of the Trade Cycle (1936a, 332). If Tinbergen simply examined "statistically particular cases, regarding them as particular cases, and no more ... I am entirely in favour of him" (JMK XIV [1938], 302).

Keynes was a leading exponent of the 1930s revolt against non-quantitative economics: "it was [Keynes'] natural inclination to approach any problem from the angle of measurement of the phenomena ... But just as he was sceptical of ideas that could not be verified by measurement, so he was sceptical also of the adventures of the statisticians

into the world of correlations built on insufficient logical foundations" (Robinson 1947, 44; 1992, 211). It was one of the principals of statistical method that "elaborate calculations ... confuse, though they might also impress, all readers outside a very restricted class". It was "the nature of valid argument which is in dispute ... Professor Pearson may cover up by elaboration of method ... [but] it is difficult to know how properly to characterise the work of a statistician who uses in controversy a table of this description with complete dogmatic assurance and without making plain to the reader the principles of its construction" (JMK XXIII [1910], 191-2, 199, 205).

With respect to Tinbergen, the problem of multicollinearity between variables exposed econometricians to "the extraordinarily difficult and deceptive complications of 'spurious' correlations". Yule's discovery "sprang a mine under the contraptions of optimistic statisticians ... It becomes like those puzzles for children where you write down your age, multiply, add this and that, subtract something else, and eventually end up with the number of the Beast in Revelation" (JMK XIV [1938], 309-10; see also Tinbergen 1992, 278). It was essential to investigate whether correlation coefficients were stable across sub-series. Such was the excessive emphasis on "the mathematical complications, that many statistical students hazily float between from defining the correlation coefficient as a statistical description to employing it as a measure of the probability of a statistical generalisation between quantitative variations" (JMK VIII [1921], 428, 464). The coefficients derived from the method of applying multiple correlations to "unanalysed economic material, which we know to be non-homogenous through time ... are not constant. There is no reason at all why they should not be different every year ... How are these coefficients arrived at? ... One gets the impression that it is a process of fitting a linear equation through trial and error". Thus when Tinbergen and co-workers econometrically 'confirmed' Keynes' (JMK II [1919]) intuition that the price elasticity of demand for a country's exports was -2, Keynes declined to interpret these econometric results as compelling: "how nice for you to have found the correct figure!" he replied to Tinbergen (1979, 342).

Tinbergen believed that he had tested for the constancy of his coefficients (JMK XIV [1938], 286-7, 292). Keynes, however, was absolutely correct. Pesaran and Smith (1985,

144) noted the complete absence of parameter stability when they re-estimated Tinbergen's investment equations: "The estimated coefficients move all over the place". Tinbergen (1969, 43) acknowledged that he and his fellow researchers found it safer "to ask industrialists for their investment programs rather than rely on an econometric explanation"; and the econometric modelling of investment remains a notoriously unsuccessful areas of applied econometrics.

Keynes objected to econometrics for the same reason that he criticised the 'classical' economists: "*Progress* in economics consists almost entirely in a progressive improvement in the choice of models. The grave fault of the later classical school, exemplified in Pigou, has been to overwork a too simple or out-of-date model ... But it is the essence of a model that one does *not* fill in real values for the variable functions. To do so would make it useless as a model" [emphases in text]. Economics was a method of thinking: "The object of our analysis is, not to provide a machine, or method of blind manipulation, which will furnish an infallible answer, but to provide ourselves with an organised and orderly method of thinking out particular problems; and, after we have reached a provisional conclusion by isolating the complicating factors one by one, we then have to go back on ourselves and allow, as well as we can, for the probable interactions of the factors amongst themselves. This is the nature of economic thinking. Any other way of applying our formal principles of thought (without which, however, we shall be lost in the woods) will lead us into error" (1936a, 297; JMK XIV [1938], 296; for Friedman's approving echo of this Marshallian theme, see 1953, 7).

Keynes was not "content with the sort of broad general impression of how things worked that contents so many macroeconomists" (Robinson 1972, 534). He warned his students that "the stuff of economics was not sharp or precise, and it was too easy to distort it and create for it the impression of an exactitude that it really lacked, and by subjecting it to mathematical manipulation also to wind up with a seriously distorted picture of the economy" (Tarshis 1977, 73). He was concerned about "the appalling state of scholasticism into which the minds of so many economists have got which allows them to take leave of their intuitions altogether. Yet in writing economics one is not writing

either a mathematical proof or a legal document" (JMK XXIX [1935], 150); "The real tool is thought, and [equations] are not a substitute for it, but at most a guide or embodiment" (cited by Young 1987, 13). Almost identical concerns were echoed a generation later by Frisch and Koopmans.

Like Friedman (1967, 88; 1974), Keynes had a corresponding nosological concern about the economics profession. Keynes had "a very poor opinion of Marschak", and described Colin Clark as "almost the only economic statistician I have ever met who seems to me quite first class" (JMK XXIX, 57, n11; O'Donnell 1992, 16). He had long-held opinions concerning the fruitlessness of *certain* statistical rather than experimental methods of analysis; of the impossibility of reducing human conduct to a set of equations; and of using "the collection of facts for the prediction of future frequencies and associations" (JMK VIII [1921], 368). There was "great danger in quantitative forecasts which are based exclusively on statistics relating to conditions which are by no means parallel" (JMK XXIII [1941], 192).

He was concerned that the statisticians' occupational disease should not become the economists' occupation. As he wrote to Harrod (in reference to Tinbergen's work), "I think it most important, for example, to investigate statistically the order of magnitude of the multiplier ...[but] to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought ... by filling in figures, which one can be quite sure will not apply next time, so far from increasing the value of his instrument, he has destroyed it". Most of the claims derived from statistical inference, he argued, were inadmissible from the perspective of logic (of which economics was a branch), and were evidence of "mathematical charlatanry" (cited by Skidelsky 1983, 223). Keynes, like Harrod, held Tinbergen in the highest regard, yet Tinbergen's econometric work, he wrote in a note to Richard Kahn, was "all hocus" and simply a "mess of unintelligible figurings". The influences on investment were variables, and, therefore, "it is logically impossible to discover by Tinbergen's method the comparative dependence on profit lagged ... I complain that this sort of logical point is not first discussed - or even mentioned. Until it is, the whole thing is charlatanism in spite of Tinbergen's admirable

candour" (JMK XIV [1938], 301, 304, 289, 299, 332, 305).

In *The General Theory*, Keynes located one of the origins of macroeconomic instability in the "animal spirits" of those undertaking investment, which depended upon "the nerves and hysteria and even the digestion and reaction to the weather of those upon whose spontaneous activity it largely depends". Crucial variables, such as the rate of interest and the marginal efficiency of capital "are particularly concerned with the indefinite character of actual expectations; they sum up the effect in men's market decisions of all sorts of vague doubts and fluctuating states of confidence and courage. They belong, that is to say, to a stage of our theory where we are no longer assuming a definite and calculable future ... Our precision will be mock precision if we try to use such partly vague and non-quantitative concepts as the basis of a quantitative analysis". Statistical comparison could be useful, "depending on some broad element of judgement rather than strict calculation" (1936a, 161-2, 39-40; 1937, 151).

Multiple correlation analysis was "too elaborate and adds little or nothing" ([1940], cited by Epstein 1987, 143). This type of analysis requires that a *complete* set of relevant variables are included, and are accurately measurable (for Friedman's elaboration of this point, see 1953, 32; 49). There will be a "serious misrepresentation of the causal process, if in fact some significant factors have been omitted". As a piece of "historical curve fitting and description ... it is not a very lucid way of describing the past" (1939, 566; Carabelli 1988, 291, n10). To derive inductive generalisations from statistical descriptions is a hazardous operation which requires that environmental conditions remain homogenous and uniform in future time periods (JMK VIII [1921], 359-470). The material to which economic models are applied is, "in too many respects, not homogenous through time" (JMK XIV [1938], 296; for similar sentiments, see Alfred Marshall, approvingly cited by Friedman 1953 [1949], 90). This implies that econometrics is inappropriate in cases when "political, social and physiological factors, including such things as government policy, the progress of invention, and the state of expectations may be significant. In particular, it is inapplicable to the problems of the Business Cycle" (1939, 561). In a letter to Gerald Shove, Keynes wrote that "as soon as one is dealing with the influence of expectations and of transitory experience, one is, in

the nature of things outside of the realm of the formally exact". Keynes concluded that "one feels a suspicion that the choice of factors is influenced (as is indeed only natural) by what statistics are available, and that many vital factors are ignored because they are statistically intractable or unprocurable". On a visit to the United States, he cautioned younger economists such as Gilbert, Humphrey and Salant, against neglecting important theoretical considerations "in the interests of simplifying their statistical task" (JMK XIV [1936], 2; [1938], 287; XXIII [1941], 192).

Prophetically, as Friedman (1991, 36) pointed out, Keynes (1939, 568, 559) predicted that econometrics had acquired a momentum of its own that would tend to make its practitioners resistant to criticism. Tinbergen will probably "engage another ten computers and drown his sorrows in arithmetic ... The worst of him is that he is much more interested in getting on with the job than in spending time in deciding whether the job is worth getting on with. He so clearly prefers the mazes of arithmetic to the mazes of logic". Keynes was clearly not opposed to statistical analysis, but "he hated stupidity, not only with aesthetic but also with a moral hatred: stupidity prevented the accomplishment of what was best for the world" (Robinson 1947, 29). He was primarily concerned that mechanical econometric practices might become a tangled web for the economics profession. He conducted a "ferocious campaign to discredit the activities of Tinbergen and later Kalecki ... Keynes' opposition to [multiple correlation analysis] was extraordinarily unyielding" (Epstein 1987, 142-3). Skidelsky (1992, 618) reported that "Keynes attacked Tinbergen's efforts with an astonishingly fierce barrage of arguments". According to Pesaran and Smith (1985, 147) "It was the unjustifiable inductive pretensions that provoked his venom". Keynes also referred, perhaps mockingly, to "nefarious econometrics" (Stone 1978, 63), and in 1946 he told Jacob Viner (1964, 265) that he "disowned any responsibility for their [his disciples] reliance on restricted and mechanical manipulations of a few statistical series, rather than making a broad survey of the significant factors and using judgement in assaying their importance and the nature of their impacts".

W.C. Mitchell provided the momentum that led to the establishment of the Oxford

Institute of Statistics (Young and Lee 1993, 119-20; Harrod 1949). Keynes was determined to establish a Cambridge department of what he called "statistical realistic economics" in opposition to Tinbergen's macroeconometrics, and as a rival, perhaps, to the Oxford Institute of Statistics, which had appointed Marschak (who had fled both Lenin and Hitler) as Director, and where Klein would seek refuge during the McCarthyite period. Keynes favoured the use of balance sheets and survey data (which elicited preferences) in the investigation of quantitative policy issues (Epstein 1987, 142-3). Richard Stone (1978, 83-7) became the first Director of the Cambridge Department of Applied Economics in April 1946, the month of Keynes' death. In one sense, Stone and his co-workers acknowledged Keynes' critique by focusing their research efforts on the econometric analysis of modern demand theory, which is widely regarded as an econometric success story, in contrast to the rather disappointing performance of the large macroeconomic models (Gilbert 1991).

Finally, and perhaps most importantly for Keynes, was the question of the likelihood of self-deception, and of the integrity and biases of the econometrician - "the spirit with which the subject is tackled", as Hendry (1980, 403) called it. Richard Feynman argued that the first principal of scientific integrity is that "you must not fool yourself, and you are the easiest person to fool" (cited by Warsh 1988, 251). For Keynes, "the more complicated and technical the preliminary statistical investigations become, the more prone inquirers are to mistake the statistical description for an inductive generalisation". In particular, *ad hoc* specifications of time-lags introduces the possibility that the econometrician will fidget "about until he finds a time-lag which does not fit in too badly with the theory he is testing". With respect to the assumption of linearity, Keynes warned that "it would certainly seem that quite easy manipulation on these lines would make it possible to fit any explanation to any facts" (JMK VIII [1921], 361; 1939, 565, 563-4; 1940, 155; Klein 1992, 184).

With respect to Tinbergen: "There is no-one, therefore, 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". It might be fruitful to use these methods to investigate more elementary cases, such as the estimation of the various influences on the net investment in railway rolling-stock (JMK XIV [1940], 320; [1938], 287-9, 295, 317). Keynes (1938a) spoke highly of forecasts derived from statistical analysis - when applied to cases such as the international corn trade. But regression analysis could not legitimately be applied to macroeconomic problems such as the "problem of imports as a whole" ([1939], cited by Carabelli 1988, 291, n10).

2.4 Friedman and Keynes

2.4.1 Econometric Disputation

Keynes (1936a, 33, vii-viii) and Friedman (1953, 30) are both associated with the idea that predictive failure is damaging to scientific status; both doubted the existence of "conclusive" tests or evidence in economics. Both perceived themselves to be heirs to an "oral tradition" in monetary theory (JMK XI [1911], 375; Friedman 1956; Patinkin 1969; Leeson 1998; Leeson forthcoming). But methodologically, *The General Theory* is a tract on the importance of examining the realism and relevance of assumptions (see, for example, 1936a, 276); and Milton Friedman (correspondence, April 18, 1995) has confirmed that his own methodology of positive economics was constructed in opposition to this tendency.

When it came to the "scientific problems" associated with data analysis, for over half a century Friedman (1957, ix) has elaborated and echoed many of the themes discussed by Keynes. In his seven page Centenary article for the *Economic Journal* (1991, 36-8), entitled appropriately "Old Wine in New Bottles," Friedman humorously refers to some of his own regressions as a "clear case of GIGO" (Garbage In, Garbage Out); but on a more serious note he concluded that the capacity to put data through the computer-based "econometric wringer" has "induced economists to carry reliance on mathematics and econometrics beyond the point of vanishing returns. I generate multiple regressions these days at a rate that I never would have contemplated three or four decades ago - and many more than I would have if I followed my own prescription for proper research procedures".

Friedman (1991, 36-8) also displayed an appreciation of the way in which "the Keynesian revolution changed the language and tools with which economists analysed the aggregate economy". He noted that the structure of professional incentives - "the tendency to count rather than to evaluate publications" - had created an inbuilt bias toward generating low quality econometric research, derived from data mining: "there is wide agreement that GIGO ... is a real problem". His training as a statistician had made him acutely aware that all statisticians "like to use our fancy techniques to see what the data show" (1963a, 8).

These themes occur throughout his career. His famous and influential methodology of positive economics (1953 [1947], 301-319; 1953, 3-43), had been formulated in the context of some highly misleading Keynesian macroeconometric forecasts: "errors in forecasting may have nothing to do with the validity of many of the underlying theories... these [other] more accurate predictions do not prove that their methods are superior to those that failed" (Klein 1946, 289). For Friedman, in contrast, the chief obstacle to the attainment of positive status was the difficulty of testing the validity of tentative hypotheses. Economic data were difficult to interpret: "this hindered greatly the permanent weeding out of unsuccessful hypotheses. They are always cropping up again". This led to an emphasis on the realism of assumptions, "the battle cry of institutionalists and the closely related emphasis on extensive statistical studies of economic phenomena which constituted an easier test of hypotheses ... Alfred Marshall's emphasis on the construction of an 'engine for the discovery of concrete truth' has tended to be submerged under the urge for descriptive realism". Friedman approved of the Marshallian method used by Keynes to explore the theory of employment; but disapproved of the Walrasian method employed by some Keynesians (1952, 456-7; 1953 [1949], 56-7, 92). However, from the mid-1930s, the formalist general equilibrium revolution began to supplant the Marshallian 'engineers'. Marshall's ambiguities, it was claimed, had "paralysed the best brains in the Anglo Saxon branch of our profession for three decades" (Samuelson 1967a, 109).

Lucas and Sargent (1978, 50) noted that the "Keynesian revolution was, in the form in which it succeeded in the United States, a revolution in *method*" [emphasis in text]. Friedman (1953 [1944], 277-300) led the "Methodological Criticism" of Oscar Lange's 1944 Cowles Monograph, *Price Flexibility and Employment*. An economist, he argued, who is concerned about economic reality "is not likely to stay within the bounds of a method of analysis that denies him the knowledge he seeks. He will escape the shackles of formalism, even if he has to resort to illogical devices and specious reasoning to do so". For almost a decade, Frank Knight and Friedman led the "fairly intense struggle" against the Cowles Commission at Chicago (Reder 1982, 10). As part of his critique of the Cowles Commission approach to econometrics, Friedman (1951, 107) noted that "we have fallen into the habit of not trying to test the validity of many hypotheses even when we can do so ... After all most experiments are destined to be unsuccessful; the tragic thing is that in economics we so seldom find out that they are".

Friedman took over much of the Keynes Critique and made it his own. Yet the evaluation of econometric evidence became the 'space-time' *arena* of the disputes between Keynesians and monetarists - who began to resemble electrons with opposite spin, in the same orbit. Paradigmatic challengers have to fight on grounds chosen by the dominant orthodoxy. Both sides confidently concluded that the evidence supported their *a priori*s, and was "so strikingly one-sided" (Friedman 1963a, 8; Stein 1982, 209; Desai 1981, 203). On the Keynesian side in particular, there was a belief that precision econometric modelling would eliminate the "ambiguous use of language - [the] Marshallian legacy shamelessly indulged in by all sides" (Desai 1981, 64). This episode of intellectual history revealed that econometrics was not powerful enough to unambiguously discriminate between alternative *weltanschauungs*.

There is a paradox here. Keynesian macroeconometrics, at least initially, retained a faith in structural estimation as a tool for discriminating between the 'true' and the 'false' model, and also for effecting the type of government policies which would mitigate, if not eliminate, the business cycle. The losing side (in terms of policy-influence from the mid-1970s) suffered a double defeat. The winning side scored a double victory: monetary

targeting (based, in part, on the results derived from monetarist macroeconometrics) temporarily replaced Phillips curve targeting; Friedman's (and Keynes') suspicions about macroeconometrics also appeared to have been partly vindicated.

Harry Johnson, in his remarkable Richard T. Ely Lecture on 'The Keynesian Revolution and the Monetarist Counter - Revolution', wrote that one of the reasons for the success of the Keynesian revolution was that "*The General Theory* offered an important empirical relationship for the emerging tribe of econometricians to measure". Likewise, monetarism advanced "a new and important empirical relationship, suitable for determined estimation by the budding econometrician. That relationship was found in the demand function for money". The methodology of positive economics "offered liberation to the small-scale intellectual, since it freed his mind from dependence on the large-scale research teams and the large and expensive computer program". The monetarist counter-revolution would "peter out" because "monetarism is seriously inadequate as an approach to monetary theory"; with its "abnegation of responsibility for explaining the division of the effects of monetary changes between price and quantity movements ... one should not be too fastidious in condemnation of the techniques of scholarly chicanery to promote a revolution or a counter-revolution in economic theory" (Johnson and Johnson 1978 [1971], 189, 196-8).

Milton Friedman (correspondence April 18, 1995) recalls that "Harry Johnson was an extremely subtle and sophisticated person ... Harry was originally a very strong Keynesian who was converted to monetarism. He remained something of a Keynesian whenever he was in Chicago and was a strong monetarist whenever he was in London". Johnson was clearly fuelled by a variety of motives and inputs; and jealousy of his Chicago colleague may have been one of them. But Friedman's genius (like Keynes') extends to an understanding of the sociology of knowledge in the economics profession (see, for example, 1955a, 902).

Keynes (1936a, 21, 81) highlighted the power of the "optical illusion" of Say's Law. In his defence of Mitchell, Friedman (1950, 470, 467) drew the contrast between Mitchell's work and "the shoddy work that passes for scientific". He also bemoaned the ability of

the Cowles Commission econometricians to successfully cultivate the "illusion that Mitchell was antitheoretical". He noted that "worthless" Keynesian national income models, which misrepresented the underlying macroeconomic structure, could nevertheless become hegemonic on the back of a "Statistical Illusion"- when accompanied by an analytical system which "once mastered, appeared highly mechanical and capable of yielding far-reaching and important conclusions with a minimum of input" (Friedman and Becker 1957, 68, 73; Friedman 1970, 207, n6). His early sophisticated theoretical work on stabilisation policy (1948a; 1953 [1951], 117-132) had not noticeably undermined Keynesian confidence; nor had it stimulated much further research (see, for example, Neff 1949a,b); despite his assertion that "the question is empirical" (1949, 954). The Cowles-NBER methodological dispute had produced only "desultory skirmishing" (1951, 114). His theoretical work left him feeling "as if I were preaching in the wilderness and belaboring the obvious". Even "distressingly obvious" conclusions could be "widely neglected" (1953 [1951], 131; for almost identical words, see Keynes 1936a, viii). Likewise, his plea for a Marshallian redirection of economics - Walras' "divorce of form from substance" had led to some "nonsense" - failed to be persuasive (1955a, 980-9; for almost identical words, see Keynes, cited by Skidelsky 1992, 615). Friedman (1955b, 402) found it "fantastic" that his empirical estimates of the effect of unions on the wage structure should lead to only a rather unproductive theoretical rebuttal: "I guess the farther grass looked greener to both of us".

A Theory of the Consumption Function (1957) - which was labelled, in part, "The Friedman Effect" - was perceived to have contributed towards putting "trade cycle theory on what one might call 'a fully expectational footing'" (Farrell 1959, sections VII-VIII, 694). It also appears to occupy a transitional position with respect to Friedman's ability to engage his opponents in a statistical dispute. In his assault on one of Keynes' (1936a, 95) central propositions regarding the stability of the consumption function, Friedman (1957, 86, 231) argued that Keynesians such as Lawrence Klein had presented, as supporting evidence, regression results "revealing a high degree of sophistication and ingenuity in statistical techniques and economic analysis", that were, nonetheless, "almost worthless ... an illusion attributable to the method of analysis ... The consumption analyst as it were,

has been priding himself on his success in adding yet more epicycles".

Friedman's (1957, ix) book is notable also for the "almost complete absence of statistical tests of significance"; but is widely regarded as "one of the masterpieces of modern econometrics" (Blaug 1985, 63). It also provoked an intense statistical exchange between Hendrik Houthakker (1958a, 1958b) and Robert Eisner (1958a) - who simultaneously (Eisner 1958b) was defending Harrod-Domar-Hicks growth models against the "Neo-Classical Resurgence", which was led by Tobin and Solow. Friedman (1958a, 991) thought he had been addressing the "statistically sophisticated reader". Houthakker, somewhat on the back foot, thought that "Friedman had strained the statistical sophistication of his readers to the limit". Part of the debate centred around "alleged correlation[s]", and Houthakker (1958b, 991, 993) concluded by stating that "the process of testing the hypothesis has only just begun".

Houthakker's article was "the first full frontal statistical assault on my work" (correspondence from Friedman, April 18, 1995). Friedman, and a growing body of associates and students, were venturing "into almost virgin territory" which they expected would "provoke controversy ... What the calculations of our critics do is to establish a presumption that further research along similar lines may be more rewarding than we thought was likely" (Friedman and Meiselman 1965, 753, 784). What followed was the contest between the radio stations, FM versus AM (Ando and Modigliano). Friedman had found in econometric disputation, the soft underbelly of the Keynesian system.

Schumpeter (1946, 196) wrote, in a now almost forgotten article on 'Keynes and Statistics', that "Throwing discretion to the wind, they [the orthodox Keynesians] have attempted to rush trenches that are stronger than they looked to them. Econometricians behaved like the inexperienced armies of 1914-18, and with exactly analogous results ... Keynes did not order these attacks". It seems that those who neglect the study of history may be condemned to repeat it; the first time as tragedy, the second time as farce.

2.4.2 The Friedman Critique of Econometrics

Monetarism was projected, and interpreted, as a belief in the existence of a stable, empirically identifiable relationship between the rate of growth of the stock of money and the corresponding rate of inflation. However, although he is happy to describe himself as "an empiricist", a perennial theme of Friedman's writings is a suspicion about the reliability of empirical results. It was the "impact of experience" of inflation which led to the "rediscovery of money", not the "serried masses of statistics massaged through modern computers". What Koopmans in the 1940s called the "Friedman critique", involved in part, an 'assault on structure', and in part was concerned to pour cold water on the post-war enthusiasm about the possibility of deriving causal relations from data. The "exaggerated claims" of "scientific magic" could not disguise the fact that "every attempt... to forecast economic activity has to date met with failure". In particular, it was "a pure act of faith to assert that [Klein's] econometric model can predict the effect of policy changes, and there is no reason to share this faith until some evidence for it is presented" (Friedman 1981, 30; 1975a, 176; 1948b, 140-1; 1951, 111). Robert Lucas (1976, 20) also found in Friedman's *A Theory of the Consumption Function* a forerunner of his critique of econometric policy evaluation.

A few citations from Friedman will illustrate this theme: "Tinbergen's results are simple tautological reformulations of *selected* economic data ... The methods used by Tinbergen do not and cannot provide an empirically tested explanation of business cycle movements. As W.C. Mitchell put it some years ago 'a competent statistician with sufficient clerical assistance and time at his command, can take almost any pair of time series for a given period and work them into a form which will yield coefficients of correlation exceeding $\pm.9$ '" [emphasis in text] (1940, 659). High *t* statistics and correlation coefficients are "a test primarily of the skill and patience of the analyst" (1951, 108). Statistical evidence could be "extremely misleading" (1962, 170), and was only available to confirm "general reasoning" and to offer a guide to what is "reasonable" (1953 [1951], 231; 1953 [1947], 312, n8); "In view of the record of forecasters, it hardly needs to be argued that it would be better to shun forecasting and rely instead on as prompt an evaluation of the current situation as possible" (1948a, 253).

The opening words of *A Monetary History* were from Alfred Marshall: "Experience in controversies such as these brings out the impossibility of learning anything from facts till they are examined and interpreted by reason; and teaches that the most reckless and treacherous of all theorists is he who professes to let facts and figures speak for themselves, who keeps in the background the part he has played, perhaps unconsciously, in selecting and grouping them, and in suggesting the argument *post hoc ergo propter hoc*" (1963); "... facts by themselves are silent ... The economist must be suspicious of any direct light that the past is said to throw on the problems of the present. He must stand fast by the more laborious plan of interrogating facts" (Marshall, cited by Friedman 1953 [1949], 90; 1950, 465; 1957, ix). The interpretation of evidence "cast up by experience, as opposed to controlled experiments, generally requires subtle analysis and involved chains of reasoning, which seldom carry real conviction" (1953, 10-11).

Friedman's (1957, 149-50; 1991, 36) emphasis on spurious correlation, and on the corresponding suspicion regarding statistics such as a high R^2 , echoed Keynes' (1939, 561) sentiments and also foreshadowed later work by Tobin (1970), Granger and Newbold (1974) and Cooley and LeRoy (1981). It is consistent with Hendry's (1980) demonstration that cumulative rainfall outperforms the money stock in price equations, with R^2 approaching unity. Indeed, the problem of "nonsense correlations" was commonly acknowledged in the inter-war period (Yule 1926); and Friedman was expressing a widely-held view. The implication of Friedman's cynicism is that the 'shoot out at high noon' approach of the econometrics movement could only end inconclusively, at least at the level of conventional statistical criteria. (As it turned out this was highly accurate as a *prediction* of the forthcoming bouts between monetarists and Keynesians). The 'winner' would have to emerge on grounds other than those of conventional levels of statistical significance. Yet Friedman and Meiselman (1963, 166) were interpreted as having taken their stand in favour of the monetarist macroeconomic model on the grounds of superior econometric performance, as measured by the size of the correlation coefficient. Rather late in the day, econometricians came to realise "the futility of the R^2 game" (Poole and Kornblith 1973, 916; Brainard and Cooper 1975, 169-70; Samuelson 1973, 389); that is, the validity of parts of Friedman's critique of econometrics. A quarter

of a century too late, Friedman's major empirical adversary recognised that with respect to the FM-AM dispute: "I must acknowledge that the difference in parameters is partly the result of prior belief or ideology ... There is obviously an ideological bias in assessing the value of parameters ... we end up with somewhat different estimates of the same thing" (Modigliani 1989, 578).

There were "sharp differences of judgement" between members of the Cowles Commission (during its sojourn at Chicago) and economists at the University of Chicago (Hildreth 1986, 5). In 1946-8, Friedman was a frequent participator at the Cowles Commission seminars. His relentless criticism prompted Koopmans to ask "But what if the investigator is honest?" (cited by Epstein 1987, 107). Friedman predicted that the Cowles Commission macroeconometric models would be revealed to be unsuccessful: "the construction of a model for the economy as a whole is bound to be almost a complete groping in the dark. The probability that such a process will yield a meaningful result seems to be almost negligible". Structural estimation was a "blind alley for empirical research"; "Despairing of their abilities to reach quantitative answers by a direct analysis of these complex interrelationships, most investigators have sought refuge in empiricism and have based their estimations on historical relationships that have appeared fairly stable". Like Keynes, he argued that prejudices or the "psychological needs of particular investigators" would tend to predetermine the outcome; "the background of the scientist is not irrelevant to the judgements they reach". Friedman drew an analogy with Heisenberg's indeterminacy principle and "the interaction between the observer and the process observed that is so prominent a feature of the social sciences ... both have a counterpart in pure logic in Godel's theorem, asserting the impossibility of a comprehensive self-contained logic" (1943, 114; 1951, 113; 1953, 12, n11, 30, 5, n3).

In his contribution to *The Lives of the Laureates*, Friedman concluded that "I've been very sceptical of the economic forecasts that people like myself and others make by using multiple regression analysis" (1988a, 88); "I have long been sceptical of placing major emphasis on purely statistical tests, whether *t*-values, Durbin-Watson statistics, or any others. They are no doubt useful in guiding research, but they cannot be the major basis

for judging the economic significance or reliability of the results and cannot be a substitute for a thorough examination of the quality of the data used" (1988b, 232, n11); "low standard errors of estimates, high t -values and the like are often attributes to the ingenuity and tenacity of the statistician rather than reliable evidence of the ability of the regression to predict data not used in constructing it ... In the course of decades [my] scepticism has been justified time and time again" (Friedman and Schwartz, 1991, 49). These judgements were not particularly original to Friedman; it would be equally appropriate to describe them as an elaboration of the Keynes Critique, or, indeed, as part of the Keynes-Tinbergen-Friedman-Phillips Critique.

2.4.3 The Keynes-Friedman Critique

Keynes noted that "the inductive verification of the adherents of the [quantity] theory have been, I think, nearly as fallacious as those of its opponents". Tinbergen's inclusion of a trend term was close to being "a method for correcting imperfect results and obscuring the fact that the explanation given is the wrong one". Superimposed on all of these problems is the "frightful inadequacy of most of the statistics employed". Keynes also highlighted what would later be called the model selection problem (JMK XII [1912], 765; 1939, 567; JMK XIV [1938], 287; 1940, 155-6). Statistical tests can neither prove a theory to be correct nor incorrect, since the latter requires that the proponents of the theory accept that all the auxiliary conditions of the test are neutral with respect to the refutation. The "fiction" that econometricians can test the relationships provided by economic theory is retained only for the consumption of undergraduates (Pesaran and Smith 1985, 145, 148, 139).

According to Roy Weintraub (1983, 18), in the 1930s there were two 'centres' of formalist work in the United States: the Cowles Commission and Paul Samuelson (later joined by Robert Solow). Samuelson (1976a, 25) observed that "By 1935 economics entered into a mathematical epoch. It became easier for a camel to pass through the eye of a needle than for a non-mathematical genius to enter into the pantheon of original theorist. A kind of Gresham's Law operated as those of us who benefited from it know only too well". Keynes - with his "tremendous capacity for mastery of detail" (Robinson 1972, 534) - was

concerned, in this context, that economists might "lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols". He cautioned against the "pitfalls of a pseudo-mathematic method" (1936a, 298, 305, 275); he warned Sidney Alexander "against the insidious disease of mathematics" (Samuelson 1977, 73); and he wrote mockingly about "those who feel a special confidence in a proposition which is expressed algebraically" (JMK XI [1911], 380-1). Samuelson (1946, 197) traced this animosity back to *A Treatise on Probability*. Economists were already too prone to "specious precision" (Keynes cited by Skidelsky 1992, 540). Attempts to turn economics into a "pseudo-natural science" would be counter-productive with respect to the training of economists: "the pseudo-analogy with the physical sciences leads directly counter to the habit of much which is most important for an economist proper to acquire". In his obituary of Marshall, Keynes (1924) emphasised 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, and philosopher - in some degree. He must understand symbols and speak in words". W. S. Jevons brooded over his charts "to discover their secrets. It is remarkable, looking back, how few followers and imitators he had in the black arts of inductive economics in the fifty years after 1862. But today he can certainly claim an unnumbered progeny, though the scientific flair which can safely read the shifting sands of economic statistics is no more common than it was".

This was the essence of Keynes' concern about the practices and habits likely to be acquired through the mechanical practice of econometrics: "The question to be answered, however, is whether the complicated method ... employed [by Tinbergen] does not result in a false precision beyond what either the method or the statistics actually available can support. It may be that a more rough and ready method which preserves the original data in a more recognisable form may be safer"; "The truth is that sensible investigators only employ the correlation coefficient to test or confirm conclusions at which they have arrived on other grounds. But that does not validate the crude way in which the argument is sometimes presented, or prevent it from misleading the wary, - since not all investigators are sensible" (1924, 321; 1936b, 524; JMK XIV [1938], 296-7, 300; [1939],

289; JMK VIII [1921], 466). Thus, mechanical econometric procedures, Keynes thought, would "displace insight and intuition and confine the scope of economics" (Pesaran and Smith 1985, 146). Unlike intuition they could not offer a privileged description of economic reality (Keynes [1924] cited by Robinson 1972, 536; Keynes [1942] cited by Dyson 1979, 56-57).

Like Keynes, Friedman highlighted the objections to the Cowles Commission approach, based on "the choice of 'model' in their terminology ... the choice of a 'structure' ... [and] the so-called 'identification' problem". Like Keynes (JMK XIV [1938] 287), he also discussed "trial and error" specifications searches and how they invalidate classical statistical inference procedures: "Tinbergen's results cannot be judged by ordinary tests of statistical significance [his variables] have been selected after an extensive process of trial and error *because* [emphasis in text] they yield high coefficients of correlation". Like Keynes, he objected to the use of trend terms, which were "highly questionable on statistical grounds". Like Keynes, he demonstrated that Tinbergen's coefficients were highly specific to the data that had been examined, and did not agree with other data. Like Keynes, he was alarmed by the "excessively crude" data. Like Keynes, he questioned the validity of drawing meaningful interpretations from Tinbergen's results: "The methods used by Tinbergen do not and cannot provide an empirically tested explanation of business cycle movements". Like Keynes, he cautioned against economic theory becoming a species of "disguised mathematics ... a retreat into purely formal or tautological analysis" (1940, 659-60; 1953, 11-12, n11; 1953 [1949], 77-8, n37; 1991).

2.4.4 The Market for Influence

The third perennial theme of Friedman's writing (in addition to confidence about monetarism and doubts about econometrics) is the Smithian case for competition as an irresistible force undermining the market power of producer groups (Friedman and Kuznets 1945; Friedman 1962b). Large scale structural macroeconometric modelling was erecting (for non-Keynesians) a considerable barrier to entry into the policy marketplace (Walters 1977, 834; Friedman cited by Frazer 1988, 707). Friedman had been preoccupied with monumental scholarly work; but from the mid-1950s, he began to

address a wider audience (see, for example, Friedman 1962b, his *Newsweek* column, his appearances before Congressional committees; plus his association with U.S. Presidential candidate Barry Goldwater). As if to demonstrate the "fertility of the market" and the "generally unstable" and "brief" (1962b, 158, 131) nature of these barriers to entry and other anti-competitive forces, he began to engage at this time in intense competition with Keynesian macroeconometrics: "we were then trying to meet an argument on its own ground. I would never have been comfortable with the conclusions reached if the only basis for them had been the statistical correlations we were presenting. However, by 1963 the bulk of the *Monetary History* book had been written. I felt very confident in the evidence from history independently of the evidence from the statistical correlations, and hence regarded these as confirmatory rather than decisive evidence" (correspondence from Friedman, 2 November, 1993).

Friedman is widely regarded as the most persuasive debater in the economics profession (Blaug 1985, 62; Stigler, cited by Rose Friedman 1977, 26; Galbraith 1987, 271). This stems, in part, from his conviction that "You cannot be sure that you are right unless you understand the arguments against your views better than your opponents do" (1974, 16). His initial lack of influence has been attributed to "his early habit of extreme aggressiveness in debate" (Breit and Ransom 1971, 256, n57). Indeed, in an article entitled "Libertarians at Bay", Lincoln Gordon (1949, 976-8) from Harvard University, argued that "There has emerged in recent years a new fashion of egregious rudeness among self-styled libertarians ... the Hayek-Mises-Jewkes-Graham manner ... One can hardly escape the conclusion that Mr Graham's swimming suffers from a failure to understand which way is down". But later, when the Keynesian tide turned, it was those who were losing policy-influence who displayed a "bitterness beyond reason" (McCloskey 1986, 184). A fair-minded observer noted that "modern econometricians may well look askance at some of [Friedman's and Schwartz's] econometric methodology" (Goodhart 1982, 1542). But the primary structural failure was not Friedman's lapse from best-practice structural estimation, but the failure of the econometrics fraternity to develop a suitably trained historical sub-discipline.

2.5 Phillips and Phillips Curve Econometrics:

Fresh Textual Evidence from "The One and Only True and Complete Set of the

Bones of The Saints''

Jacob Marschak had been Minister of Labor in the short-lived Menshevik government of the Terek Republic in the North Caucasus. He had an early encounter with perspicacious forecasts when a colleague warned him that this paedocracy (a government of children) would fall "when the corn has grown high enough to conceal a man on horseback" (Koopmans 1978, xii). He became Director (1943-48) of the Cowles Commission at Chicago, initially believing that structural estimation possessed a unique epistemological status: it was "the Gospel ... I hope we can become 'social engineers'" (cited by Epstein 1987, 69, 61, 67; Hildreth, 1986, 3-8; Malinvaud 1988, 194; Klein 1978, 326, Arrow, 1978). Frisch's description of the first European meeting of the Econometric Society at Lausanne in 1931 captured this heady enthusiasm. Klein (1947, 111) believed that econometric models "eventually should lead all investigators to the same conclusion"; Tinbergen believed that "differences of opinion can, in principle, be localised" (1937, 73; Samuelson 1992, 243).

At the risk of oversimplification we can describe the 'econometrics movement' as an attempt to locate The General Theory of Macroeconomic Structure; the quest for a "single 'final' equation" (Schumpeter 1954, 1168, n20), or series of equations, with reliable estimated coefficients; a form of econometric fundamentalism. Combined with this pioneering confidence was a willingness to directly confront as many theoretical problems as their critics could muster; plus a deep understanding of the unsatisfactory nature of economic data. This sense of integrity gradually eroded such pioneering confidence, and contributed to the 'retreat from structure'. In many ways, it was Klein (1992, 184) who symbolised the ongoing faith in large scale macroeconometric models, in opposition to the principle of parsimony.

The third 'wave' of macroeconometric enthusiasm - primarily associated with the construction of Keynesian Phillips curves - was the 'wave' most damaging to the prestige and scientific credibility of the economics profession. The opposition to the first 'wave' of macroeconometric enthusiasm (associated with Tinbergen's work in the 1930s) was led primarily by Keynes. The opposition to the second 'wave' (associated with the Cowles

Commission) was led by Friedman. This enthusiasm did not survive what Koopmans called the "Friedman critique" (Epstein 1987, 111). Disappointing empirical results led to the 'retreat from structure' after 1947. Yet large scale Keynesian macroeconomic models continued, often with *ad hoc* monetary sectors, and following Klein and Goldberger (1955, 1), an ongoing "constant adjustment". Keynesian macroeconometricians abandoned their optimism concerning the revealing nature of structural estimation, and had come to rest, in part, on the *judgement* of the researcher (Tinbergen 1969, 44; Zarnowitz 1968, 427; Klein 1971b, 48; Hildreth 1986, 60; see Desai 1981, 154 for the 'endogenise a bit more' approach to the pursuit of structure). The Keynesian Phillips curve macroeconomic models which collapsed in the 1970s were, together with their underlying method of research, effectively orphaned thrice: disowned by Keynes, abandoned by most of the Cowles Commission workers, and antithetical to both the spirit and the detail of Phillips' work.

Nevertheless, the third 'wave' of econometric optimism occurred simultaneously with the increased availability of computing power in the 1960s. Phillips was one of the most insightful critics of the Keynesian Phillips curve estimation industry. Like H.L. Moore (Stigler 1962a), Phillips avoided controversy, but it is clear that in many important respects his work does not belong in the same category as most of the macroeconomic exercises of the 1960s. First, he pioneered the role of inflationary expectations in this type of macroeconomics. Secondly, many of these models did not adequately deal with money; but Phillips' model and his famous Machine were based on monetary dynamics. Thirdly, Phillips was opposed to the idea of trading-off higher rates of inflation for supposed benefits with respect to unemployment. Fourthly, a decade before Clower and Leijonhufvud, Phillips was teaching Keynesian macroeconomics as a disequilibrium phenomenon (Lipsey 1981, 547). Phillips' dynamic stabilisation exercise was concerned to minimise the deviations of the business cycle 'pendulum'; not to attempt to locate the macroeconomy at a point of other than 'rest'. Phillips provided the theoretical explanation behind Christopher Dow's (1967) subsequent empirical analysis of the destabilising effects of fine tuning. His curve, however, came to be interpreted as a proposition that ongoing inflation would reduce the rate of unemployment, which Phillips had specifically

cautioned against.

The complete, recently rediscovered seminar records of the LSE Staff Seminar on Methodology, Measurement and Testing (M²T) capture the flavour of Phillips' influence. (The records of the M²T Seminar series were titled by the words which head this section). Richard Lipsey and his colleagues attempted to reconstruct economics as a series of empirically testable propositions. Arnold Harberger - who had been closely associated with the Cowles Commission during the 1950s (Hildreth 1986, 64) and who shared Friedman's views of econometrics (correspondence from Friedman, April 18, 1995) - attempted to persuade the M²T economists that their project was flawed because of the problem of the "back door alibi ... Testing is subjective ... infinite number of possible H [hypotheses] to explain anything ... Trouble in econ[omics] - people will agree neither on which H are the most plausible nor on what experiment would be crucial Can't convince man who won't be convinced. Have to depend on what 'seems sensible' ... Scientist builds up picture of world. The more open to surprise the better. When surprised he amends picture. World complicated, need intuition". Richard Lipsey explained that "we want to be formal because we associate with people whose intuition we don't like and Harberger doesn't. Why does Harberger wince whenever Archibald says 'rule'?". Harberger replied: "Because these matters always subjective among the inquirers" (M²T Seminar notes, February and March 1958).

Phillips presented a paper on *The Problem of Refutation* on 27 April 1960, and 18 May 1960. He argued that static theory could not be tested from time-series data. Because of unstated maintained hypotheses, categorical statistical refutation was impossible. Autocorrelated time-series were treacherous data. 'Testing' was, in reality, little more than "measurement plus" (M²T Seminar notes). It seems that Phillips was influential in effecting the retreat from the "Popperian notion of refutation" which Lipsey (1966, xx) drew attention to in the second edition of his *Introduction to Positive Economics*.

Rowley and Wilton (1973, 385, 387) re-estimated various Phillips curves using Generalised Least Squares, and concluded that the "pseudo" *t*-values had been inflated by

at least 100 per cent in most cases: "one can only speculate whether the various authors would have advanced the Phillips curve model had they been faced with the GLS estimates rather than the OLS estimates". Most of these models from the 1960s and early 1970s had been plagued by the unacknowledged problem of autocorrelation. Yet in the discussion following his paper on *The Problem of Refutation*, Phillips emphasised that with respect to data analysis he was only "happy if not autocorrelated" (M²T Seminar notes, May 1960). It is, therefore, appropriate to label these doubts about the macroeconometric practices that culminated in the Keynesian Phillips curve, as the Keynes-Tinbergen-Friedman-Phillips Critique.

2.6 Conclusion

The history of econometrics is worthy of more attention amongst practicing econometricians than has hitherto been the case; it is a subject that should stand in equal status with other sub-disciplines within econometrics. A disaggregated approach to Keynes' critique of econometrics reveals the nature of his objections to the underlying logic, and pretensions, of this relatively new approach to the analysis of economic data. His contemporaries were in no doubt as to the intensity of his hostility (Klein 1951, 450-1). Keynes did not soften his position; in fact, Tinbergen came increasingly to recognise the validity of some of Keynes' criticisms.

There is a large degree of similarity between Keynes' position on econometrics and that of Friedman. The econometric disputes between Keynesians and monetarists which raged in (and perhaps disfigured) the profession, were, from a methodological perspective, a sometimes ill-tempered conversation between Keynesians and the modern representative of Keynes. Keynes' opposition to macroeconometrics was based on the suspicion that results derived from this method of analysis would come (illegitimately) to be regarded as decisive evidence. This was also Friedman's suspicion. Likewise, Phillips deserves to be credited with a good deal of insight into the fundamental weaknesses of the Phillips curve estimation industry.

Gardiner Ackley (1961, 109) argued that historical misrepresentations (with respect to the

myth of the 'Klassical' whipping boys) could be analytically fruitful. But analytically, Keynesian macroeconometrics left behind some "jerry built structures" (Lucas 1977) - most notably the trade-off interpretation of the Phillips curve - although Phillips (1968) developed parts of the critique which was subsequently named after Robert Lucas (Court 1999; Peter Phillips 1999). Friedman, Marschak and Tinbergen have also been credited with a similar approach to econometric policy evaluation (Lucas 1976, 20; Pagan 1987, 20). Thus, the collapse of the Keynesian Phillips curves in the 1970s was a vindication of both the Keynes-Tinbergen-Friedman-Phillips and the Friedman-Marschak-Tinbergen-Phillips Critiques.

In his final posthumously published article, Keynes (1946, 177) bemoaned how much "modernist stuff, gone wrong and turned sour and silly is circulating". Many aspects of his critique of econometrics retain their validity with respect to contemporary practices. Yet the tradition of "fancier econometric footwork" (Lucas 1976, 257) continues, often oblivious to some of the issues that alarmed Keynes, Friedman, Tinbergen, Phillips, *et al.*

Stigler (1963a, 63) suggested that "methodological controversy has never had a marginal product (of scientific progress) above zero"; and this seems to capture Friedman's sentiments exactly. Friedman echoed Marshall's description of theory as "a 'language', designed to promote 'systematic and organised methods of reasoning' ". Mitchell's style of research had fallen out of favour, in part, for reasons of "language rather than substance and he uses no mathematics [but] his theoretical discussion can readily be translated into current jargon" (Friedman 1953, 7; 1950, 489). Max Weber noted the tendency for intellectual opponents to avoid "the other's terminology as though it were his toothbrush" (cited by Haberler 1961, 40). Samuelson's formalist work, *Foundations of Economic Analysis* (1947), included on its title page, the statement "Mathematics is a Language." Keynesian methods overran NBER statistical business cycle research; but Friedman's polemical genius led him to use Keynesian language (IS-LM, income-expenditure, money demand and econometrics) to effect a remarkable, if temporary, counter-revolution. Clearly, econometrics has a subterranean history which too many econometricians are unaware of.

I do not wish to be misunderstood. As David Hendry (1980, 395) put it, "some editors can be persuaded to publish on the basis of econometric fools-gold: *caveat emptor*, but do not denigrate the whole project". Econometric evidence has illuminated many debates, and clarified some issues. It was, for example, careful empirical work which revealed that the consumption-income ratio appeared to be constant over long periods (Kuznets 1942, Goldsmith 1955), in contrast to the simple linear Keynesian consumption function (Keynes 1936a, chapter 8; Davis 1952). Yet the large macroeconometric forecasting models have not drastically improved in predictive accuracy in the last three decades (Hendry 1980, 388; Leamer 1983, 42; Pagan 1987, 3-4; Epstein, 1987, 4; Rivlin 1987, 2; Zarnowitz 1992; Ormerod 1994, 3). Neither can new classical macroeconometrics claim a greater degree of academic respectability than the Phillips curve equations which preoccupied applied econometricians in the 1960s. Monetary targeting, often based on applied econometric research, was also a disappointment. In addition, Friedman's forecasts of a surge in U.S. inflation, beginning in mid-1984, and of a growth in real GNP of just 1 per cent for 1984:Q1, both proved to be inaccurate (Gordon 1987, 441). He also made an unfortunate prediction that "The world crude oil price cannot stay at 10\$ a barrel; it will drop drastically within the next six or nine months ..." (1974a, 12). Likewise, Granger-Sims style tests of exogeneity of the money supply have yielded mixed results (Cagan 1989).

Macroeconometric modelling, however, remains a lucrative business (Tobin 1977, 760; Galbraith 1987, 261-2). Ragnar Frisch (1970, 152) spoke of the "service to the econometrics fraternity by being critical and outspoken". Clive Granger (1981, 124) has also appealed to model builders to pay more attention to econometric theory: "One wonders what has been the purpose of the work of the majority of theoretical econometricians for the last twenty years, or of a third of the pages of *Econometrica*". David Hendry (1980, 396) stated in his inaugural lecture that Keynes' critique should be "compulsory reading" for econometricians. If applied econometricians paid as much attention to the history of their subject as they do to running regressions, this might improve the quality and reliability of the empirical side of our profession.

NOTES

i. The question of how variables were measured, and in what units, left open the possibility of "devastating inconsistencies". Secondly, the assumption of linearity with respect to *all* economic forces was, he thought, "ridiculous". Thirdly, the arbitrary choice of the first and last year of a series for which a time trend is calculated, "looks to be a disastrous procedure" (Keynes 1939, 563-6; 1940, 155; see also Klein 1992, 184).

ii. A similar fate befell the Harvard Economic Society, whose econometric forecasts misread the downturn in 1929 (Galbraith 1987, 262).

ii. Inter-war business cycle research was dominated by the empirical projects of W.C. Mitchell and Warren M. Persons (Morgan 1990, 40). During his period at the New School (1919-1922), Mitchell organised the NBER, which devoted its resources to the statistical investigation of the business cycle (Dorfman 1949, 456). Mitchell distrusted correlation techniques, and was concerned that invalid causal implications could be derived from them. He was alarmed by the possibility that professional ethical standards could become corrupted in this enlarged world of business cycle statistics, forecasts and funding: "[Economists] do not always decline the over-flattering invitation with the firmness which befits a scientific conscience" (cited by Dorfman 1959, 210). In its first research project, the NBER became engaged in an early econometric dispute about the validity of Pareto's 'law' of income distribution (Dorfman 1959, 204-5); and it was as Secretary to the NBER Conference on Research in Income and Wealth (1937-1940) that Milton Friedman became more of an economist than a statistician.

ii. Most students who have passed through the various econometrics courses offered by our profession would be forgiven for thinking that this subject lacked a systematically analysed history: "It is a minor scandal that there is no comprehensive history of either the rise of econometrics or the mathematisation of economics" (Weintraub 1985, 140). In recent years we have benefited from some excellent research into the history of econometrics from Darnell (1984), Christ (1985), Epstein (1987), Morgan (1990), Darnell and Evans (1990), Keuzenkamp (1991), Dharmapala (1993), and others; plus the contributors to the special issue of *Oxford Economic Papers* (1989), edited by de Marchi and Gilbert.

ii. Tinbergen (1969, 43) wrote "Returning to models, I am sometimes wondering whether, upon looking at some recent work by planners, I should not repeat the famous words by Goethe's Zauberlehrling ... 'The ghosts I called I can't get rid of now'. Sometimes indeed some of our followers *overdo* model building" [emphasis in original].

Tinbergen (1979, 342-3) knew what Keynes meant: "Sometimes, indeed, intuition constitutes a basis for new scientific results. It should be the intuition of a genius, however. For simpler souls, intuition may be less reliable".

ii. Frisch (1970, 165) quoted approvingly Norbert Weiner's remark about the economists' habit of "dressing up their rather imprecise ideas in the language of the infinitesimal

calculus", which was analogous to the vague feelings that "these magic rites and vestments will at once put them abreast of modern culture and technique". Frisch concluded that "I am sorry to say that some econometricians have often been liable to forget these basic principles in practice and, therefore, have not been critical enough when they apply their techniques and mathematical analyses. This remark is particular important when it is a question of drawing conclusions about *the economic policy to be followed in a concrete situation*" [emphasis in text]. Koopmans (1949, 70) stated that "The econometric approach is not a substitute for theory, but one of the servants of theory"; but later (1957, 170, 198) reflected that "in principle, tools have a servant's status ... if we look with a historian's interest at the development of a science, however, we find that tools also have a life of their own. They may even come to dominate an entire period or school of thought ... Our servants may thus become our guides, for better or for worse ... It should be kept in mind that the sharpness and power of these remarkable tools of inductive reasoning are bought by willingness to adopt a specification of the universe in a form suitable for mathematical analysis".

ii. Friedman knows more about the use and abuse of statistics than most economists: he studied at Colombia University under the mathematical statistician Harold Hotelling (1933-4); he was a statistical assistant to Henry Schultz at Chicago (1934-5); he worked with economic measurement and data analysis at the NBER (1937-40); he was Statistical Director of the Wisconsin Income Study (1940-1). Friedman's early career either combined, or alternated between, mathematical statistics and economics. For most of the war years, at least, he was exclusively concerned with mathematical statistics (Rose Friedman 1976, 22; Friedman 1988a, 83-6; Wallis 1980, 322).

ii. Critics argued that they did not test the restrictions imposed, and they were accused of a misspecification, which influenced the outcome of the race, and were "setting up two strawmen and crowning one of them" (Desai 1981, 112, 104-6; Ando and Modigliani 1965; de Prano and Mayer 1965; Hester 1964).

ii. Koopman's rejoinder (1947) indicated that the pioneer econometricians did not regard the Keynes-Friedman critique as fatal to their project. This brilliant group of scholars (including nine future Nobel Laureates, Simon, Debreu, Becker, Arrow, Tobin, Koopmans, Modigliani, Markowitz and Klein) proceeded to lay the theoretical foundations of econometrics. Eleven of the thirty three research associates (1939-1955) were elected to membership of the National Academy of Sciences, and twenty-two became presidents of major professional associations (Hildreth 1986, 111; Klein 1978, 326). Frisch and Tinbergen shared the first Nobel Prize in Economics. This econometric work was in stark contrast to the 'statistical economics' of Burns and Mitchell at the NBER, who, Koopmans believed, studied business cycles "as if they were the eruptions of a mysterious volcano" (cited by Epstein 1987, 64).

ii. "We the Lausanne people, were indeed so enthusiastic all of us about the new venture and eager to give and take, that we had hardly time to eat when we sat together at lunch or at dinner with our notes floating around on the table to the despair of the waiters" (Frisch 1970, 152). It was what Martin Beckman called "the heroic age of econometrics" (cited by Craver and Leijonhufvud 1987, 181).

ii. In the post-war period the "economics miracle" really took off: it was as if economics became the 'language' of government, and there was a great demand for those who spoke the language (Stein 1986). Samuelson described the period from 1932-75 for economists as "the great wave of a Kondratieff expansion [for economists]. The New Deal and Welfare State created a vast new market for economists in government ... Then came the post-war boom in education" (1988, 60-1; Desai 1981, 55; Pesaran and Smith 1985, 148). The 1940s and 1950s were the decades of enthusiasm and optimism for government planning, and this created a massive demand for advice from economists, often of a technical nature. The entire economies of Japan and West Germany were available for experimentation. The price mechanism had, it was believed, failed in the 1930s and had subsequently played little role in allocating resources during the war (at least at the governmental level). Foreign aid and the Marshall Plan, would it was believed, restructure the non-communist world. A new sub-discipline, development economics, emerged, much influenced by the structuralist approach to planning (Little 1982, 76-85; Meier and Seers 1984). The emerging welfare state required a broad tax base to fund it, and taxes came to be perceived primarily as a technical tool to be manipulated by policy makers in order to contain inflation. A flavour of the confidence of the time can be sampled by reference to the papers by Marschak, Klein and Edward Teller, and Marschak and Klein (the latter delivered to the Econometrics Society meeting in 1946), which advocated an expenditure of 20 billion dollars per year over fifteen years to relocate all inhabitants of cities with over 50,000 inhabitants to ribbon cities or underground cities in order to minimise the effects of an atomic assault on the U.S. (Epstein 1987, 81, 95, n8).

ii. The pioneering optimism of this second wave was almost immediately confronted with scepticism. The Institute for Mathematical Statistics meeting in Ithaca New York, in August 1946 concluded that the Cowles group approach was unlikely to result in meaningful estimated parameters. Little confidence was held out for future developments: "Data as bad as economic data" was incapable of accurately discriminating between alternative models (Tukey, cited by Epstein 1987, 100). The attempt to derive an exact model of the capitalist system was derided by Schumpeter; Irving Fischer concluded that he had seen "a lot of people burn their fingers over discoveries of cycles. The discoverer 'sees things' almost as bizarre as drunkards" (cited by Epstein 1987, 103). The most persistent critic was Milton Friedman who presented to the 1947 Econometrics Society meeting a manifesto entitled 'A Monetary and Fiscal Framework for Economic Stability' (1948a), which offered an alternative to the short-run stabilisation perspective of the Cowles workers. Koopmans asked "can we meet the Friedman critique: that Christ's experiments have shown that the information contained in the data so far processed have been insufficient for good forecasting" (cited by Epstein 1987, 111). Marschak retreated from his previous position with respect to the NBER survey research method. The Friedman critique, plus reviews by Arrow, Orcutt, Solow, Samuelson, Leontieff, Wold and others, plus disappointing empirical results, and an increasing awareness of the paucity of reliable data, effected a 'retreat from structure' from 1947: "the empirical work was an exhausting disappointment both for the tedium of computation and the lack of professional acceptance" (Epstein 1987, 110); "The econometric approach of the Cowles Commission seems to be petering out rapidly or not getting anywhere beyond extensive methodological discussions" (Haberler 1949, 84).

ii. The Cowles econometricians were perceived to be seeking a "social analogue for Newtonian mechanics ... Tycho and Kepler are becoming fairly regular attenders of economic discussions these days" (Vining 1949, 80, 77). This analogy was also prevalent among the LSE econometricians. In September 1608, a trader at the annual Frankfurt Fair offered for sale a telescope which could magnify seven times. In March, 1610, Galileo published his first booklet, a short but dramatic work, called *Sidereus Nuncius*, or *Messenger from the Stars*. The Universe would never appear to be the same again. The walled-in Aristotelian universe, with its immutable social order, would be destroyed by a seventeenth century retreat to the heliocentric perceptions of Aristarchus (Koestler, 1959, 43-65; Butterfield, 1957, 55-76). Exactly three-and-a-half centuries later, Phillips (November, 1958) and Lipsey (February, 1960) turned the newly refined econometric telescope onto the problem of the behaviour of money wages during the course of the business cycle. With pioneering optimism the M²T economists sought to use this telescope to turn economics into a fully empirical science. There was also an Aristotelian authority to be vanquished. In the first edition of his textbook, Lipsey cited Robbins on empirical analysis: "But is it not desirable to transcend such limitations? Ought we not to be in a position to give numerical values to the scales of valuation, to establish qualitative laws of supply and demand? ... No doubt such knowledge would be useful. But a moment's reflection should make it plain that we are here entering into a field of investigation *where there is no reason to suppose that uniformities are to be discovered* ... Is it possibly reasonable to suppose that coefficients derived from the observations of a particular herring market at a particular time and place have any *permanent* significance - save as Economic History" [emphasis in text]. Lipsey (1966, 219, 218, n1) bemoaned that these views were "still held by economists". In a seminar on "Refutation and Comparison", Kurt Klappholz also mentioned Robbins. Chris Archibald retorted: "Robbins Aristotelian, not relevant" (M²T Seminar notes, 7 March 1958). Further evidence of the importance of this historical analogy, is provided by the opening extract from Beveridge, and the final sentence, in Lipsey's (1966, xi-xii, 860-1) best selling textbook. In seeking to rigorously scrutinise economic data they were aspiring to the highest standards of science. They hoped to resolve conflicts over perceptions and policies, and to effect a Newtonian-style revolution in economics.

ii. Econometric agnosticism, or at least reservations about policy relevance remained a minority taste in the 1960s, with potentially explosive critiques such as Phillips' (1968) being almost entirely ignored. The exchange between Basmann and Klein and associates (in Brunner 1972) reflected the determination of practitioners to press on almost regardless. Unorthodox and problematic ideas tended to be ignored because they "would have inconveniently impeded the progress of econometrics at the time of its most rapid growth" (Desai 1981, 116-7, 120). Applied econometrics in the age of the computer, and in the presence of an increasing demand for financially lucrative expert consultancy from government agencies, acquired an ad hoc character which was often cut adrift from professional disquiet. The statistical economists believed that potential regularities and relationships could be revealed by an interaction with the data. Econometricians believed that the data would 'speak' when a model had been imposed upon it. Tinbergen was also very knowledgeable about his data and was concerned about its quality. Post-pioneering econometrics gradually entered a less creative, more mechanical phase, where concerns about the quality of the data were less prominent. Frisch and his co-workers were aware of the

possibilities of deriving "fictitious" results from econometric analysis (Epstein 1987, 91). Koopmans persistently, if vainly, emphasised the need to report all results, not just the preferred set. Coal face enthusiasm for model estimation appeared to be largely oblivious to the scepticism and concern expressed by some about the lack of model evaluation.

ii. Don Patinkin (1976, 1095) found it "somewhat depressing to see how many of [Keynes' criticisms] are, in practice, still of relevance today". Maurice Allais (1992, 35), a theoretical physicist, in addition to being the recipient of the 1988 Nobel Prize in economic science, bemoaned "the crop of pseudo-theories based on the mechanical application, devoid of any real intelligence, of econometrics and statistical techniques ... *pseudo-models*, accompanied by a mathematical-statistical panoply of *untamed*, totally unjustified economics which seem to the naive to be scientific theories, whereas they are generally just empty shells" [emphasis in original].