

IN PRAISE OF THE CLASSICS:
The Relevance of Classical Political Economy
for Development Policy and Research
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Abstract

This paper examines the relevance of the classical approach to the study of economic development in three areas: (a) the scope of the subject -- in particular the relevance of the "why" questions rather than the "how" questions which have been the major concern of theorizing on economic development; (b) the analytical framework for policy analysis, in which unlike the contemporary "public economics" school the polity is not subsumed into a committee of Platonic Guardians, and (c) an empirical research methodology which meets many of the epistemological objections against the currently fashionable positivist methodology based on Popperian falsificationism.

INTRODUCTION

Epigraph

As civilized human beings, we are the inheritors, neither of an enquiry about ourselves and the world, nor of an accumulating body of information, but of a conversation, begun in the primeval forests and made more articulate in the course of centuries. It is a conversation which goes on both in public and within each of ourselves. Of course there is argument and enquiry and information, but whereever these are profitable they are to be recognized as passages in this conversation, and perhaps they are not the most captivating of the passages. It is the ability to participate in this conversation, and not the ability to reason cogently, to make discoveries about the world, or to contrive a better world, which distinguishes the human being from the animal and the civilized man from the barbarian. Indeed, it seems not improbable that it was the engagement in this conversation (where talk is without a conclusion) that gave us our present appearance, man being descended from a race of apes who sat in talk so long and so late that they wore out their tails.

M. Oakeshott: The Voice of Poetry in the Conversation of Mankind, Bowes and Bowes, London, 1959, p. 11.

Over the last seven years Hal Myint and I have been co-directing a massive multi-country comparative study on "The Political Economy of Poverty, Equity and Growth" for the World Bank. When we came to write the draft synthesis volume of the findings of the project, it became apparent to me that our work was in the 19th century tradition of political economy, however refurbished by modern artifacts. It also provided an analytical and empirical validation of the classical policy prescriptions. That this approach is antithetical to the sensibilities of many of our contemporary peers was brought home to me by an anonymous reader of the manuscript who fulminated against its archaic style! In this paper, therefore, I would like to provide a defense of the fruitfulness of the classical tradition of political economy for the study of economic development. As the quotation from Oakeshott which is the epigraph of this paper pinpoints, a good education requires us to continue age old conversations. With the current "scientistic" pretensions of technocratic economics, there is an increasing

danger that many of our younger brethren will come to believe that the only conversations worthy of their notice are those taking place in the latest issues of well-known journals. The past masters having been superseded by their modern descendants, the former's views are then considered to be of only antiquarian interest for students of the history of economic thought. This is a view that Hal Myint has and would rightly condemn not merely for being uncivilized (pace Oakeshott) but also, as he has shown from his earliest work, because it fails to take account of the policy and analytical relevance of classical thought for the economics of developing countries.¹ In this context we need only note his development of the classical "vent for surplus" model of international trade (Myint (1958)), his demonstration of its usefulness in explaining the economic histories of the land abundant peasant economy of S.E. Asia (Myint (1972)), and his most recent attempts to incorporate the importance of organizational factors in the effectuation of comparative advantage (Myint (1985)). They all show the continuing relevance of the classical tradition. Together with his LSE colleague, Peter Bauer, he has been one of the few leading scholars of economic development who while making common cause with much of the policy prescriptions of the recent "neoclassical resurgence" in development economics (Little (1982) Lal (1983)), have sought to base them on a perspective and methodology which is that of the classics. In the case of Peter Bauer, this has been explicitly noted by Michael Lipton, who wrote:

Lord Bauer is a classical economist. Enterprise, trade, enlargement of markets: these are the engines of development. Bauer makes no neo-classical claim that all businessmen act like profit-maximisers, or would maximise welfare if they did. For Bauer it is the move from subsistence to even larger markets that counts. (Lipton (1984), p.45)

But anyone reading Myint's first book, based on his LSE doctoral thesis Theories of Economic Welfare cannot fail to be impressed that he too finds

classical political economy most useful in thinking about public policy issues in the Third world. As a recent convert to this view, I am delighted to pay my homage to Hal Myint in this paper in his honor, but not in the usual way of converts, I hope, by overstating the case for the continuing relevance of classical concerns and methods!

If I can thereby persuade my colleagues to re-engage in a conversation which seems to have been muted if not stifled by the rise of technocracy, my purpose and I believe Hal Myint's life long one will have been well served.

In this paper, therefore, I will briefly outline three aspects of the classical tradition which (no doubt, refurbished by modern techniques) need to be revived in the study of economic development. The first concerns the scope of the subject, the second the framework for policy analysis and the third, research methodology.

I. Classical Concerns

In his Chichele lectures for 1966, Lord Robbins notes that:

The theory of economic development addresses itself to two types of questions. It asks what are the fundamental causes or conditions of economic development and it asks what path development will take, given any particular configuration of these factors or fundamental conditions. If you like to put things this way, we can say it asks why development takes place and how it happens.

(Robbins (1968) p. 1)

He then goes on to show that the classics by and large were concerned with the why questions. By contrast the modern neoclassical theory of growth and its offshoots such as the theory of the dual economy, which has been developed as particularly relevant for developing countries, are concerned with the "how" questions. The basic answer provided to the "why" question by the classics was that economic development was dependent upon thrift, productivity and entrepreneurship. Government consumption was regarded as unproductive, and (in modern terminology) could crowd out investment. Eltis

(1984) in a masterly translation of the classical theory of economic growth into modern language (including formalism) notes:

there was far less to prevent growth in The Wealth of Nations than in the Physiocratic models. Only excess government expenditure which absorbed more than the surplus of the productive sector, and so rendered accumulation impossible, or government regulation, or a failure to sustain property rights could prevent the market from solving one or more of the many problems it was required to solve. Smith's simple statement that, all that was profitable was productive and that too much government expenditure or interference could prevent the conversion of the potential surpluses of the productive into the extra capital that was necessary for growth, were comprehensible to all, and sufficiently plausible to the economically literate to remain utterly orthodox until 1936.

(p.319)

Bacon and Eltis (1976) in their influential study of the British economy based it on a distinction between the "productive" surplus producing (agriculture, industry and the entire private sector) and the "unproductive" surplus using sector (provision of unmarketed government services). This as Eltis notes was clearly an echo of classical growth theory. As was the model that van Wijnbergen and I produced of the global crowding out of investment by the activities of national public sectors, increasingly linked through a globally integrated capital market (Lal & van Wijnbergen (1985)). However, many of the other aspects of the classical theory of development seem to be obsolete. These include the assumption that wages were too low to be taxed or to provide an investible surplus, which must then come for profits and rents. Nor does the Malthusian analysis of population appear persuasive any longer (see Birdsall (1988) for a review), nor the belief in aggregate diminishing returns to capital accumulation that have been taken to be a hallmark of the classics. But as Eltis notes, these were later glosses on Smith's model of development, in which increasing returns from the division of labor and the widening of the market are central. These were supposed to characterize the new "industrial" sector. By contrast in

agriculture Smith assumed constant costs (Eltis, p. 100). It would go too far beyond the remit of this paper to outline the mechanics of the resulting model of growth. The interested reader is referred to Eltis, particularly how he shows that Smith arrived at the conclusion that despite increasing returns to industry, it would be misguided to help industry at the expense of agriculture (Eltis, p. 104).

The diminishing returns assumption which underlay classical economics has not been borne out by the subsequent economic history of the developed or developing world (see Scott (1989)). The classics believed that technical progress could hold the stationary state at bay, and in our day this avenue of escape has become the "deus ex machina" in the neo-classical theory of economic growth -- on which more below.

However, as Wrigley (1988) argues in a brilliant book, the classics were concerned in explaining development in the economic conditions they believe prevailed at that time. Though these were changing with the rolling industrial revolution, the classics still saw their concern as the development of what had been for centuries an organic economy, which Wrigley defines as "an economy bounded by the productivity of land" (p.5). In such an economy -- and historically this has been the dominant type -- there is a universal dependence on organic raw materials for food, clothing, housing and fuel. Their supply is in the long run inevitably constrained by the fixed factor -- land. This was also true of traditional industry and transport. Most metal working industries were dependent upon the supply of charcoal (a vegetable substance) for providing the heat to smelt and work the crude ores.

Again, since many branches of industry, most mining ventures and almost all forms of transport, as well as agriculture, made extensive use of animal muscle as a source of mechanical energy, the productivity of the land was crucial to the supply of power as well as heat. Woodland and pasture were as necessary to English industry as her arable land was to the family table. A recogni-

tion of the significance of the productivity of land to the whole range of the productive activities of society is both implicit and explicit in the writings of the classical economists, and the leverage which the application of the principle of declining marginal returns thus exacted was very powerful.

(Wrigley, op cit. p. 18-19)

The industrial revolution according to Wrigley essentially led to the substitution of this organic economy by a mineral based energy economy. But this fundamental change was only becoming apparent towards the mid-19th century in England, and it was not till Marx was writing that it had become manifest. This new economic regime "escaped from the problem of the fixed supply of land and of its organic products by using mineral raw materials". Moreover, the:

organic economy was necessarily severely inhibited by its energy budget. Just as raw materials were all organic, both heat and mechanical energy were obtained from organic sources, the heat energy from burning wood (or its derivative charcoal); the mechanical energy from human or animal muscle.... Moreover, since animals need the same "fuel" as men, they compare with men for the same scarce resource, fertile land. When, therefore, a mineral source, coal, began to supply more and more of the heat energy needed by industry and later, following the development of an effective device for turning heat into mechanical energy in the form of the steam engine, also provided a solution to the problem of securing a virtually unlimited supply of such power, the prospects for growth both in aggregate output and in output per head were entirely transformed from those which had always previously obtained.

(Wrigley, p. 5-6)

Thus the industrial revolution in England was based on two forms of "capitalism", one institutional, namely that defended and described by Adam Smith -- because of its productivity enhancing effects, even in an organic economy -- and the other physical: the capital stock of stored energy represented by the fossil fuels which allowed mankind to create a

world that no longer follows the rhythm of the sun and the seasons; a world in which the fortunes of man depend largely upon how he himself regulates the economy and not upon the vagaries of weather and harvest; a world in which poverty has become an optional state rather than a reflection of the necessary limitations of human productive powers.

(op cit., p. 6)

The classical insights therefore which still remain valid for the contemporary process of economic development concern the institutional aspects, their mechanics of growth (though logically consistent) have been overtaken by a historical change in mankind's economic base. Though those who believe in the limits to growth imposed by a finite stock of fossil fuels would argue that, the final denouement -- the stationary state -- has merely been postponed!

It is time to turn to the answers to the "how" question. The growth theories developed in the 1950s and 1960s were the descendants of the Harrod-Domar (HD) model. In particular the Solow-Swan model of steady state (equilibrium) growth repaired the worrying knife-edge problems of the HD model by substituting a neoclassical production function for the fixed coefficients technology implicit in the famous HD growth equation. But the relevance of the subsequent neoclassical equilibrium growth theory for answering the "how" question of economic development has been questioned (for instance by Hicks (1965), p. 3). Nevertheless, as Arthur Lewis (1955) in his more directly relevant and famous work The Theory of Economic Growth, accepted, the proximate determinants of growth remain technical progress and the per capita investment rate -- the basic building blocks of the Solow-Swan model. Moreover, even though the steady state features of the model may not be of direct relevance, developing country growth outcomes might still be explicable in terms of the so-called "traverse" in the model; that is the movement of an economy from one steady state equilibrium to the next (as, say, the rate of investment is increased or the exogenously determined rate of technical progress alters). However, one prediction of the model is that in the long run, growth is entirely exogenous -- dependent only on the rate of labor-augmenting technical progress and the rate of growth of the

labor force, the so-called natural rate of growth.

This exogeneity of growth within the neoclassical framework has troubled theorists. Attempts by Kaldor (1957) and Arrow (1962) to repair this lacunae by essentially making technical progress endogenous were unsuccessful. After a lapse of nearly two decades there is a recent flurry of interest in developing endogenous equilibrium growth models. The pioneers of this "new" growth theory are Romer (1986, 1989) and Lucas (1988). However, as for instance Stern (1991) has shown, these models which build on the earlier models by Arrow and Uzawa (1965), make growth endogenous by essentially making arbitrary assumptions -- the value of a key parameter in Romer's model,² and by an unexplained externality to human capital formation in Lucas. Like so much of modern growth theory their relevance in explaining developing country growth outcomes is limited (see Stern op. cit., and Lal and Myint (1991)).

A more fruitful approach to the "how" question which yields endogenous steady state growth is a model recently developed by Scott (1989). It is relevant for our purposes because it harks back to an approach to growth -- what Harry Johnson (1964) labelled the "generalized capital-theoretic" approach, stemming from Irving Fisher (1930). On this approach, investment is defined broadly to include all forms of investment (consumption foregone) which yields a stream of income (and consumption) over time. This wider notion of investment includes

such diverse activities as adding to material capital, increasing the health, discipline, skill and education of the human population, moving labor into more productive occupations and locations and applying existing knowledge or discovering and applying new knowledge to increase the efficiency of the productive processes.

(Harry Johnson in MEIER (1989), p. 461)

This notion of investment as the prime mover of the growth process also makes growth endogenous (as is explicitly shown by Scott) and "facilitates

the discussion of problems of growth policy by emphasizing the relative returns from alternative investments of currently available resources" (Johnson, op. cit., p. 462). Technical progress is not an independent determinant of growth. It is subsumed in investment.

Moreover, as Scott shows, the relevant notion of investment is gross investment,³ whose rate, together with the growth of the quality adjusted labor force are the only sources of growth in the Scott model. In the World Bank comparative study referred to above, Myint and I found that this model also provided a good statistical explanation for the proximate causes of growth in the 21 countries studied over the 1950-85 period. Moreover, the qualitative evidence (on whose status more below) from the country studies, which were the building blocks for the Lal-Myint synthesis, also supported Scott's model.

Thus unlike the Solow-Swan model, which says "no technical change no growth," Scott's model asserts (as did Fisher) the importance of conventional economic factors as the proximate causes of growth. "The determinants of the volume and efficiency of investment are restored to the centre of attention and both have long been the concern of economists" (Scott (1989), p. 100).

How long? This takes us back to the classics. For as Robbins emphasizes, and as Fisher recognized explicitly in the dedication of his great book The Rate of Interest, there was a classical precursor to his "capital-theoretic approach" to growth and development. This was John Rae, whose New Principles of Political Economy (1834), though outside the main classical tradition dealing with the "why" question, did provide an answer to the "how" question in terms of the interaction of productivity and thrift (or as Fisher called it, "opportunity" and "impatience") in determining per

capita growth rates of income and consumption. This, as the above highly-condensed summary of subsequent developments shows, is still, in my judgment, the only sensible way to examine the proximate causes of growth. But as has become increasingly obvious, this only leads us to dig deeper into the reasons for differences amongst nations in these factors, by in effect asking the classical "why" questions. For the volume of investment (savings) depends upon preferences and its efficiency upon policy. Both depend also upon "entrepreneurship".

Differences in preferences ultimately require explanations in terms of what can broadly be called "culture" and its determinants -- a sorely neglected subject to this date, despite popular assertions in the media about such things as the culturally determined high savings rates of the Confucian societies, e.g., Japan! The policy determinants of the efficiency of investment requires explanations in terms of what the classicals called political economy. It is these differences in public policy which the evidence from the Lal-Myint country studies show are crucial in explaining the inter-country growth and poverty alleviation outcomes. The classical perspective on political economy (along with differences in factor endowments, within a three factor, multiproduct, open economy model due to Krueger (1977) and extended by Leamer (1987)) was found most useful in both explaining these differences as well as in providing prescriptions for the future. We therefore turn to these public policy aspects of economic development in the next section.

II. Political Economy

As Robbins (1976) notes, classical

political economy, in the sense of the contents of [Adam] Smith's book, not only described how the economic system actually worked, or could work, but also how, according to the assumptions of the

author, it ought to be made, or allowed to work. This usage was followed in general by the majority of classical economists. Thus description and prescription enjoyed a common title. (p. 3)

But Robbins prefers to limit the term explicitly

to a discussion of principles of public policy in the economic field: and while it makes appeal to the finding of economic science, it also involves assumptions which, in the nature of things, lie outside positive science and which are essentially normative in character. (p. 3, ibid.)

The ethical objective was the greatest happiness, where each man's capacity for happiness was to be counted as equal. But this "individualist utilitarianism" derived from Hume did not (emphasizes Robbins) imply any belief in interpersonal comparisons of utility or adherence to a quantitative felicific calculus. It was for Bentham "nothing more than a working rule of legislation" (Robbins (1952, p. 180). Robbins notes:

There is much talk in the Benthamite literature of a felicific calculus; and the term naturally suggests a most pretentious apparatus of measurement and computation. But, in fact, this is all show window -- the use of the felicific calculus lay in quite another direction -- in rough judgments of the expediency of particular items of the penal law, in general estimates of the suitability of existing institutions or the desirability of other institutions to take their place -- there can be no doubt that their practice was in the sphere of broad appraisals rather than quantitative computations. (Robbins (1952), p. 187, ibid)

Moreover, the classics did not assume that the state consisted of a committee of Platonic Guardians. Following Hume, they were aware of the divergence between the interests of public agents and the general public they were meant to serve. This of course is a theme which has been revived by the "new" political economy.

The greatest happiness principle was best served according to classical liberalism by Adam Smith's system of natural liberty. "This liberty in the field of individual choice and liberty in the organization of production became the innovating doctrines of the classical political economy" (Robbins (1976), p. 6). The resulting system of free enterprise was not seen by the

classics as Myint in his first book emphasized "as desirable merely for the static advantages of allocative efficiency" (Myint (1948) p. 52).

It is only when free competition is further interpreted as an auxiliary instrument of dynamic economic progress that it fits in with the fundamental classical outlook on the economic problem ... Thus a free economy was thought to encourage thrift, enterprise and initiative which promote a greater division of labor, expansion of markets, accumulation of capital ... (ibid.)

As he notes, the "allocative" interpretation of free competition became current with the development of the Walrasian theory of perfect competition. The subsequent incorporation of an explicit utilitarian social welfare function with an implicit egalitarian bias, then led on to the famous development of what is today called public economics (see Atkinson and Stiglitz (1980) for a coherent treatment and Newberry and Stern (1987) for applications to developing countries). This has become the modern "welfare economics" school of public policy analysis. The danger with this as Myint noted in his Pioneers lecture is that: "welfare economics with its emphasis on market failures, externalities, and the divergences between social and private costs, has for many decades been a powerful intellectual force behind interventionist policies" (Myint (1987), p. 108). This dirigisme fostered by modern technocracy leads to a new form of mercantilism (see Lal (1987), (1990)), and then to its demise because of the "disorder" it engenders.

The basic argument against this form of enlightened dirigisme flowing from the "market failure" literature, is that the public agents charged with its implementation cannot be expected to have the requisite information (as emphasized by both the classics (see Robbins (1952)) and neo-Austrians (see Hayek (1948)), nor character (they need to be "economic eunuchs" in Buchanan's apt phrase), as emphasized by the classics and the new political economists (see Lal, 1987a).

The classical policy prescriptions have been caricatured "by Carlisle's phrase "anarchy plus the constable", or by Lasalle's simile of the night-watchman" (Robbins (1952), p. 37). As Myint (1948) and Robbins (1952) amongst others have noted "to identify such doctrines with the declared and easily accessible views of the classical economists is a sure sign of ignorance or malice" (Robbins (1952), p. 37). As Robbins (*ibid.*) notes, Adam Smith's famous statement of the three functions of the state,⁴ is almost identical with Keynes' famous formulation in The End of Laissez-Faire. The ensuing principles of economic liberalism were clearly set out in Mills Principles, and their clearest modern reformulation is in Hayek's (1960) great work The Constitution of Liberty. But the most succinct restatement is by former colleague David Henderson (1986):

The objections to economic liberalism and the market economy centre round the role of governments and states, both nationally and internationally. For many people liberalism goes with laissez-faire, which in turn is viewed as outdated, negative, unconcerned with what happens to weaker members of society and de facto favouring the stronger, and uncompromisingly negative in its attitude to the state. This rests on a double misconception. First, it distorts the message of laissez-faire. Second, it wrongly identifies belief in a market economy with an extreme interpretation of the laissez faire principle.

As to the first point, laissez-faire gets an undeservedly bad press. The message it conveys is not that governments should be inert or indifferent. Its emphasis is a positive one. It is concerned with economic freedom, including the freedom of individuals and enterprises to enter industries or occupations, to choose their place of residence or operation within a country, and to decide their own products, processes and markets. There is nothing outdated about these principles, nor do they operate against the weak. To the contrary, they enable opportunities to be opened up more widely, and thus operate against special privileges within an economic system. It is no accident that outside the Communist world the economy which most conspicuously departs from laissez-faire is that of the Republic of South Africa.

In any case, liberalism is not to be identified with hostility to the state, nor with a doctrinaire presumption that governments have only a minor role in economic life. On the contrary, the liberal view of the role of the state, both internal and external, is strongly positive. (pp. 98-9)

At a time of widespread neo-mercantilist predatoriness, engendered in part by the contemporary "welfare economics" school of public policy, the importance and validity of the prescriptions of classical liberalism remain as important as ever for economic development in all three worlds.

III. The Classical Method

1. Introduction

In the study of developing countries, one of the most fruitful methods has been the use of comparative historical analysis or what I prefer to call the method of analytical economic history. The famous Little-Scitovsky-Scott (1970) study of Industry and Trade in Some Developing Countries, the NBER studies of Krueger (1978) and Bhagwati (1979) on trade liberalization, by Krueger (1983) on employment and trade, and the more recent comparative studies sponsored by the World Bank⁵ are all examples -- as is the well-known study by Sargent (1982) of five big inflations. This type of analysis has a distinguished pedigree in social science, though more technocratic economists have usually sniffed at it for not meeting current standards of rigorous testing. J.S. Mill sets out the logic of the method explicitly in his A System of Logic (esp. Book III Ch. VIII), and this comparative method was applied with great skill by de Tocqueville in his Democracy in America, and by Marc Bloch in Feudal Society.

The method is particularly suited to the empirical study of problems of growth and development as J.N. Keynes noted at the turn of the century:

There are in fact few departments of political or social science in which the a priori method avails less than in economic development ... In more general problems relating to economic growth and progress the part played by abstract reasoning is reduced to a minimum, and the economist's dependence upon historical generalizations is at a maximum ... For only by the direct comparison of successive stages of society can we reasonably hope to discover the laws, in accordance with which economic states tend to succeed one another or to become changed in character. (J.N. Keynes:

The Scope and Method of Political Economy, 1890, pp. 283-4)

It is increasingly being realized (see the Sept. 1989 issue of World Development for instance) that the evolution of institutions is likely to be central in explanations on differences in growth performance. For the present decisions of economic agents which impinge on the development process will be constrained in part by their past -- through various cultural and ideological norms and organizational structures. We need to understand the different "rules of the game" and the different incentive structures relating to alternative organizational forms, and how they evolve. Also we need to know how people react to changes in the set of choices facing them when either the informational constraints or "cultural" norms or the more purposive ones embodied in particular organizational structures alter. In short, we need to understand dynamic institutional change if we are to ultimately find deeper answers to the question of why some countries have grown and developed and others have not. However, as there is no rigorous model defining dynamic institutional change, and even if there were, it is unlikely to be amenable to econometric analysis, our approach must be eclectic. It must also connect the present with the past. This is one major aim of the historical comparative studies method.

A major difference between the comparative study method and statistical (econometric) analysis can be seen in terms of the two forms of induction distinguished by J.N. Keynes, namely quantitative induction and "qualitative induction" (p.334). Quantitative induction is based purely on statistical analysis. Qualitative on the historical or comparative method. It is the latter which concerns us.

The logical status of this experimental method was clearly set out by Mill. He distinguished between what he called the "Method of Agreement" and

the "Method of Difference", (as two of his five methods of experimental inquiry). The method of difference is the normal method used in the experimental sciences. By this method "one can try to establish that several cases have in common a set of causal factors, although they vary in other ways that might have seemed causally relevant" (Skocpol, (1979) p.36). The method of agreement by contrast deals with "cases in which the phenomenon to be explained and the hypothesized cause are both present to other cases in which the phenomenon and the causes are both absent, but which are otherwise as similar as possible to the positive cases" (Skocpol, *ibid.*).⁶ Mill emphasized that the method of difference is by itself a more powerful method for establishing valid causal connections. But that

on those subjects where artificial experimentation is impossible ... our only records of a directly productive nature [is the method of Agreement], while in the phenomena which we can produce at pleasure, the method of difference generally affords a more efficacious process which will ascertain causes as well as mere laws.
(Mill, *op. cit.* p. 219)

In practice, on the comparative historical method, it is possible to combine both types of comparative methods. "This is done by using at once several positive cases along with suitable negative cases as contrasts", (Skocpol, p. 36).

The method like any other method of multivariate analysis must depend upon some prior theoretical structure. It also has the usual problem concerning the selection of the relevant sample, the appropriate ceteris paribus conditions used in the identification of relevant causal links, as well as in drawing robust inferences about the relative strength of various "causes" leading to the "effects" which are of interest. If statistical analysis is broadly defined as the arrangement of quantitative data into patterns, together with the attachment of some measures of the confidence that can be attached to the "correctness" of the pattern that is inferred, it

is clear that statistical analysis, whenever it is feasible, is not only complementary but can form an important part of the comparative method. However, as Keynes emphasized, this comparative method is wider than the purely statistical inquiry by which "social science and political economy are spoken of as branches or departments of the science of statistics, a science which studies social and economic phenomena in the only satisfactory way, namely, by the accumulation of facts and generalizations from them" (op cit. p. 334). He rightly argues that, this view is too narrow, as the economic methods used in practice by economists, embody not only this form of quantitative induction, but also qualitative induction and, of course, deduction.

The comparative studies method is thus by and large the classical method of economic analysis applied to comparative history. As such it is best termed "analytical economic history", which combines the deductive analysis of economic theory with both the quantitative inductive analysis of statistics (to the extent this is possible) with the qualitative inductive analysis of comparative history.

2. Three Schools of Thought on Method

This method of "analytical economic history" can be looked upon as part of what Stewart (1979)⁷ calls the "analytical school" and Hausman (1989)⁸ the "deductivist" school in economics. This is to be distinguished from those economists who subscribe to either an "aprioristic method" (notably von Mises and some other members of the Austrian school) or to some form of positivism (which at its most cogent, is based on Popperian Falsificationism).

As the major practitioners of the "analytical" or "deductivist" school were the classical English economists (particularly Mill) and their modern day successors (particularly Keynes and Robbins), we prefer to refer to this method as the classical English method in economics. As both Stewart and

Hausman emphasize, economist's actual practice is based on, and most of the existing body of economic knowledge has been obtained through this method. Though the official methodological rhetoric is positivist.⁹ Thus there are 3 broad schools of thought, one a priori, and two empirical -- the classical and positivist.

The epistemological difference between the "apriorism" of the Austrian school and the empiricism of the classical English and positivist schools is best seen in terms of Hume's distinction between analytic (a priori, deductive) and synthetic (factual, inductive) statements.¹⁰

It is usually held that deduction leads to logical truths, induction to material truths. But some philosophers have argued that there may be some propositions which are materially true a priori. Amongst these are the Austrians. von Mises (1962) argued that all the deductions of economics followed from an axiom concerning human agents calculus of choice, which was materially true a priori. For it was impossible to make sense of a world in which this axiom -- whereby human agents sought to substitute a "better" for a "worst" situation -- was not true. All the theorems of economics followed from this and any "testing" of economic theories was superfluous. We need not go into the cogency of this viewpoint. Suffice it to say that it has not been accepted by most economists.¹¹

The classical view was refurbished and put in modern garb by Robbins. Caldwell summarizes this as follows:

The fundamental generalizations of economics are self-evident propositions about reality: ends are multiple and can be ordered; means and time are limited and capable of alternative application; knowledge of present and future opportunities may be incomplete or uncertain, so that expectations are important. To handle this last difficulty, the expository devices of rationality (consistency in choice) and perfect foresight are usually invoked as simplifying assumptions which are first approximations to reality. Finally, these basic postulates are combined with subsidiary postulates which reflect the actual conditions of the world to

yield the applications of economic theory. Empirical studies are used to suggest plausible subsidiary postulates, and to check on the applicability of the theoretical framework to given situations. The collection of data to predict future constellations of valuations on the basis of past valuations may be of limited use in the short run, but it should not be imagined that such efforts will ever yield empirical "laws" which share the necessity of the basic postulates. (Caldwell (1982) p. 103)

The positivist views are better known. There are 3 broad positivist methodological stances, in "scientific" economics. The first two have been called "confirmationism" and "instrumentalism". The former associated with early Hutchinson and Samuelson;¹² the latter with Friedman¹³ and the Chicago school. The third is "falsificationism" associated with Karl Popper¹⁴ and whose most thorough going recent proponents have been Blaug and Hutchinson.^{15,16}

Most economists have come to believe that a form of positivist methodology is the only valid form of scientific activity. This is ironical when the philosophers have resoundingly rejected the naive positivism espoused by most economists as the hallmark of the scientific method.¹⁷ The Appendix outlines how the classical method relates to the current positivist methodological prescriptions for conducting scientific economics, namely, that it should be concerned with the construction and testing of theoretical models.

Moreover, as both Stewart and Hausman (and others e.g., McCloskey) argue, in fact most of our existing corpus of economic knowledge was developed by some variant of the classical method (production theory, consumption theory, Keynesian theory, etc.) rather than that recommended by positivists whether of the confirmationist, instrumentalist or Popperian variety.¹⁸

3. Forensic Story Telling

The comparative studies method which is largely based on the classical method can also be looked upon as a form of story telling. Moreover, as a story teller tries to tell a story which is both interesting and persuasive, the method is attuned to the multifaceted aspects of persuasion.¹⁹ These concern the selection of facts, the crafting of the story, and choosing from amongst a number of competing stories, the one which fits the 'facts' better than another.²⁰

In our stories we are attempting to establish plausible casual links. Two different forms of causation are distinguished by philosophers. One is based on statistical regularities, the other on counterfactual analysis. It is the latter form of causal analysis which underlies analytical economic history.

This analysis of causation in terms of counterfactuals has recently been most ably outlined and defended by Sir John Hicks.²¹ He distinguishes between 3 types of causality in relationship to time: "Sequential (in which cause precedes effect), contemporaneous (in which both relate to the same period) and static (in which both are permanencies)" (Hicks, 1979). Much economic analysis is concerned with contemporaneous and static causation. While this is useful and illuminating in many areas -- demand analysis, welfare economics, international trade theory -- it is not much use in answering the type of questions concerning the processes of development which are concerned with sequential causation.

As Hicks notes:²²

The more characteristic economic problems are problems of change, of growth and retrogression, and of fluctuation. The extent to which these can be reduced into scientific terms is rather limited; for at every stage in an economic process new things are happening, things which have not happened before -- at the most they are rather like what has happened before. We need a theory

that will help us with these problems; but it is impossible to believe that it can ever be a complete theory. It is bound, by its nature, to be fragmentary. It is commonly called "dynamic" in contrast to "static"; but that is a name which now seems to me to be better avoided. For "dynamics", in its original sense, is a branch of mechanics; and the problem to which the economic counterpart (if it is a counterpart) refers, are not mechanical. As economics pushes on beyond "static" it becomes less like science, and more like history.

(Hicks (1979) p. XI (emphasis added))

But the resulting "historical" method is different from that conventionally used by historians, largely because in economics it must include the application of the theoretical apparatus (the "tool kit", in Joan Robinson's felicitous phrase) of the economist.²³ As Hicks²⁴ notes: "When theory is applied it is being used as a means of explanation: we ask not merely what happened, but why it happened. That is causation; exhibiting the story, so far as we can, as a logical process."

The resulting method of analytical economic history is then a composite of the characteristics of two different types of economic history. According to Hicks (1965):²⁵

One of the standard ways of writing economic history (much practiced by political historians in their economic chapters) survey the state of the economy under consideration, as it was in various historical periods comparing one state with another. This is comparative statics. It is when the economic historian tries to throw his work into the form of a narrative that it becomes in our sense, dynamic. (p.11)

Moreover, the method is essentially forensic. As Hicks notes:²⁶

Take the field which is common to economics and history; the study of the past, with the object of finding out, not only what happened, but why it happened. That is causality; if the study is successful, it should enable us to state a cause; we should be able to say that A caused B ... There is clearly an analogy with the proceedings of a court of law. Someone was murdered; who was the murderer?

The resulting answer will not be infallible, but should still meet the requirement that the jury arrives at a conclusion that is "beyond reasonable doubt". But, of course, there will always be debate about what is

"reasonable doubt", and in that sense it is unlikely that any important empirical issue in economics will ever be finally settled. This applies to the method of analytical economic history as to any of the other methods of empirical social science.

IV. Conclusion

In summary, the classical method of analytical economic history involves using theoretical constructs to order whatever evidence is available to tell as plausible a "story" as the facts will bear, using Mill's principles of agreement and difference in a comparative analysis of different countries historical experience. The theoretical constructs required to order the "facts" are Joan Robinson's economist's "box of tools". They are

economic theory in its verbal and mathematical forms, statistical theory and practice, familiarity with certain accounting conventions, statistical sources, and a background of stylized historical fact and worldly experience. The use of such tools to fashion sturdy little arguments is the metier of the economist, the economists' "method". (McCloskey, op. cit. p.24)

Finally, the classical comparative studies method is essentially forensic. We need to persuade the jury of our professional peers. And, because the method recognizes the importance of persuading a sceptical jury, if successfully applied, it is persuasive!

APPENDIX

The Epistemology of Two Empirical Methods: The Classical and Positivist

To contrast and evaluate the views of the two empiricist schools who accept the validity of Hume's Golden Fork, it is useful to briefly set out the hypothetical syllogism which underlies induction in both forms of empiricism in economics. This also shows why the notion of "testing theories" on positivist lines in economics cannot have the same validity as in the experimental sciences. Consider the famous hypothetical syllogism which allows us to predict that the sun will rise tomorrow.

1. I have observed that every day in the past the sun has risen [minor premise].
2. What the sun has done in the past it will continue to do in the future [major premise -- proposition of regularity].
3. Therefore the sun will rise tomorrow.

The major premise in all inductive reasoning must be the proposition of regularity. The individual observations which are part of (1) [the minor premise] can never provide evidence in themselves for the proposition of regularity. In the statistical context, too, statistical analysis cannot provide evidence to support a proposition of regularity relating to the data. No philosopher has been able to provide a satisfactory a priori justification for this assumption either. However, the experimental physical sciences are based on this assumption. Why is this valid, and why might not it be in economics? The reason is that in the experimental sciences this assumption can be looked upon as a hypothesis which has been shown to be true in the deterministic sense.²⁷ The hypothesis is "that if a pattern of past events is established by inductive observation, then this

pattern can be expected to persist into the future, within limits of variability set by statistical analysis" (Stewart, p.95).²⁸

As the falsificationists (like Popper) are right to note, this hypothesis like any other hypothesis cannot be proved to be true by observation, but it can be proved to be untrue (falsified), by one counter example in the deterministic version of the hypothesis. "In the experimental sciences no such contrary observation has ever been made. In other words, in experimental science we never come across a situation where statistical patterns established in the past do not persist into the future." By contrast in economics we know that a deterministic version of the hypothesis of statistical regularity is untrue. Past statistical patterns in the economy do not always persist into the future. This is of course the epistemological foundation of the so-called Lucas critique. The most famous example of this lack of regularity, in a past statistical pattern, is the Phillips Curve. Nor given irreducible uncertainty (ignorance) about the future can we attach any probabilities on the likelihood of any past statistical regularities persisting into the future. This implies that contrary to positivist ideology, testing economic theories by predictionism or falsification is not cogent.²⁹

What about the classical English approach?³⁰ An outline is best presented in terms of the language of the philosophy of science. We can distinguish the world of theory or the domain of constructs (the C-domain) from the real world, the domain of protocols (or p-domain). The propositions making up the theory in a scientific field should be linked by deductive reasoning and form part of the c-domain. The highest level statements in the theory are those from which all the other statements in the theory can be deduced. The lowest level statements in the c-domain are

the least general of all, and are usually the theoretical statements compared directly with the p-domain (i.e., observations). But the confrontation of theory with facts need not be confined to the lowest level statements in the c-domain. It may be possible to compare fairly high level statements with observations in the real world. The notion of "cause" and the "proposition of regularity" are part of the c-domain. In any scientific area there will also be "rules of correspondence" about how investigations confronting "facts with theory" should be conducted. These are neither immutable nor given from outside the scientific area. They are "intersubjectively agreed procedures" (Stewart, p.75).

In this framework, the classical English method asserts that the higher level statements of the c-domain "are capable of being set against factual observations -- but that they are so obvious to common sense that they do not need to be "tested" by inductive-statistical means".³¹

Common sense, everyday observations and introspection are all means of investigating the applicability of higher level statements. Given this applicability, the implications can be deduced to provide a practical guide to decision-making "in situations to which that set of starting statements are applicable... If these recommendations work -- not just once or twice, but consistently -- then one can take it the analysis has indeed been a correct expression of the real world situation as the times and places concerned". This can be described "as the method of testing by repeated usefulness". If the recommendation of the analysis fail to work, "this does not mean that the higher level statements of the reasoning used are 'falsified', just that they have turned out not to be applicable to that particular situation" (p.125). "The whole notion of 'testing theory', as recounted by philosophers of science, hinges on the idea that there are

permanent (or at least very stable) relationships in the p-domain, which can be brought into contact with constructs in the c-domain" (Stewart, p.141). But we do not as yet have these stable relationships in economics and given "ignorance" about the future (irreducible uncertainty) probably never will.

Moreover, there is a distinction between a "theory" and "models" which is relevant in comparing the positivist and classical methods. "A theory always includes statements specifying exactly what part of the p-domain the theory applies to; a model does not include this set of statements, or only incompletely" (Stewart, p.145).

In economics, the c-domain is not occupied by a theory but by a multiplicity of models. As the conditions for the applicability of models are not fully specified, a model cannot be verified by correct or falsified by false predictions. But from statistical procedures we can have some reason to believe that it was applicable in the situation to which it was applied. But in judging its future applicability we face the problem of the irreducible uncertainty (ignorance) about the future. For even if the model's applicability were specified, it would remain impossible to convert "ignorance" over a model's future applicability into uncertainty by means of inductive observations, because in the economic p-domain:

no stable constants have been found, you have no means of knowing in probability terms whether your model will be applicable to the new situation, no matter how much inductive testing you do of its applicability to past situations. Indeed, as writers like Robbins have pointed out, attempts to use inductive testing in this way ... may be positively undesirable, in that they may give the economist a false sense of security about the applicability of a particular model ... In short ... the analytical [classical] school's denial of inductive - statistical testing as a correspondence rule is not merely wilful abandonment of scientific rigour. On the contrary, it is a purposeful response to the premise that in the real world of economics, there are no long-standing stable relationships.

(Stewart, pp.149-50)

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ENDNOTES

¹Thus, as Hicks (1975) notes, unlike the natural sciences there are no real scientific revolutions in economics. One cannot then say that modern theory has superseded the old. Most revolutions in economic thought are more in the nature of a "change of attention". Economic theories underpinning beliefs about the workings of an economy, which at particular times are fairly appropriate, are subsequently rejected in the light of changing circumstances. Whereas in the natural sciences "the facts" to be explained are in a sense immutable, in economics "the facts" are thrown up by a constantly changing world:

A theory which illumines the right things at one time may illumine the wrong things at another. This may happen because of changes in the world (the things neglected may have gained in importance relatively to the things considered) or because of changes in ourselves (the things in which we are interested may have changed).
(Hicks, 1975, p. 320)

²This can be seen most simply as follows (See Stern (1991)): The Arrow-Kaldor type model is as follows: suppose there are N firms. The representative firm's production function is:

$$y = F(k, A\ell) \tag{1}$$

where y = output; k = capital; ℓ = labor; and A is the level of knowledge and dependent on the economy's capital stock K so that:

$$A = K^a \tag{2}$$

where $K = Nk$, and $a < 1$.

So labor productivity (y/ℓ) depends upon past investment in the whole economy with elasticity a . There are increasing returns to scale, as a doubling of K and L , ($= N\ell$) not only doubles output directly, but

because of its indirect effects in raising A , leads to a further increase in output. Each firm believes A is fixed and behaves as if its production function has constant returns to scale, and so perfect competition will prevail. However, steady state growth will still not be endogenous. For if the growth rate of labor supply is n , the growth rate of output g , and of capital g_k , then from the aggregate production function

$$Y = F(K, K^a, L) \quad (1a)$$

we have

$$g = e_1 g_k + e_2 [a \cdot g_k + n] \quad (3)$$

where e_1 and e_2 are the elasticities of F with respect to K and AL from (1a). They are also the imputed factor shares, which under perfect competition add to unity, so

$$e_1 + e_2 = 1 \quad (4)$$

In a steady state, with a constant capital-output ratio: $g = g_k$, hence from (3)-(4)

$$\begin{aligned} g &= e_1 g + e_2 [ag + n] \\ g &= [n / (1 - a)] \end{aligned} \quad (5)$$

Growth will be determined by the exogenous natural rate of growth n .

Romer's "trick" in creating endogenous growth is to set the value of the parameter a in (2) which Arrow assumed to be less than one, at unity. Then from (1) assuming there is no population growth, L can be normalized at unity. With $a = 1$, the firm level production function becomes

$$y = F(k, K) \quad (1c)$$

Now doubling capital alone leads to the doubling of output. Moreover if $a > 1$, then output will grow without bound. As N is constant, and assuming a Cobb-Douglas production function for (1a), this yields:

$$Y = N^{1-b} K \quad (1d)$$

(as $a = 1$; and $\ell = 1$; $L = N$). The social marginal product of capital from (1d) is

$$r = (\delta y / \delta k)_s \quad (6)$$

But as firms take A in (1) to be constant, the private marginal product of labor $(\delta y / \delta k)_p$ is lower and

$$i = (\delta y / \delta k)_p = bN^{1-b} \quad (7)$$

On the optimal growth path the social marginal productivity of capital from (6) must equal the rate of fall of the marginal utility of consumption (d).

For a constant elasticity utility function:

$$V(C) = C^{1-e} / 1-e, \quad (8)$$

where C is per capita consumption.

$$d = -\dot{V}'' / \dot{V}', \quad (9)$$

which from (8) yields

$$(1+d) = (1+g_c)^e$$

where g_c is the growth rate of consumption. On the steady state path $g_c = g$, and so

$$(1+d) = (1+g)^e \quad (10)$$

As on the optimal path $r = d$, we have from (6) and (10) that, on the optimal steady state growth path

$$g = (N^{1-b}) / d \quad (11)$$

There is thus long run steady state growth without population growth or exogenous technical progress. Moreover, as the private marginal product of capital is lower than the social, (from (6) and (7)) the actual growth rate of a private laissez-faire economy will be below this optimal rate. If profits are taxed, growth will be still lower. So public policy can influence the growth rate. But this endogeneity of growth has been derived purely by changing the parameter a 's value from less than one to unity!

However, as Stern rightly concludes: "That such important conclusions turn on such a fine distinction (which is unlikely to be settled empirically) should make us uneasy about relying on the Romer model as a basis for explaining the role of policy in determining the rate of growth " (p. 127).

³This echoes an old debate about the measurement of capital, and the validity of the neoclassical production function. The latter is concerned with the change in the net stock of capital -- gross investment minus depreciation. This depreciation has either been taken to be due to a physical process or as a result of scrapping. Scott says neither notion of depreciation makes sense, as machines which are well-maintained can go on producing the rated output well past the date they disappear from the production function, while scrapping is an economic process which depends on the date they cease adding to net output. When machines which are profitless are scrapped no productive capital is lost. Hence, gross investment is the best measure of the change in the capital stock. But there is no way in which a measure of this stock can be obtained, say by summing up all past gross investments. So the idea of the production function that links the level of output to the level of capital has to be abandoned, and all one can do is explain changes in output by changes in capital -- that is gross investment. The difficulty contemporary theorists bred on the neoclassical growth theory have in swallowing these commonsensical ideas is shown by the exchanges between Scott and eminent American economists in the Journal of Economic Literature (Sept. 1990) and Oxford Economic Papers (1991).

⁴For Adam Smith (Canan edition, vol. ii, pgs. 184-85) these were (i) to protect society from foreign invaders, and (ii) every member, as far as

feasible from oppression and injustice, by other members of society and (iii) provide and maintain various public works and public institutions which provided public goods. For Keynes: "The important thing for government is not to do things which individuals are doing already; but to do these things which at present are not done at all" (Keynes (1926), pgs. 46-47).

⁵Besides the one on the political economy of poverty equity and growth co-directed by Hal Mynt and myself, there are 3 others on liberalizing foreign trade regimes co-directed by D. Papageorgiou, M. Michaely, and A. Choksi; on the political economy of agricultural pricing co-directed by Anne Krueger, A. Valdez and M. Schiff, and on macroeconomic adjustment co-directed by I.M.D. Little, W.M. Corden, R. Cooper, and S. Rajapatirana.

⁶Skopcol (1979) provides a good account and justification of this method.

⁷Stewart (1979). This book provides the best available account of the epistemological bases of different viewpoints on economic methodology.

⁸D.M. Hausman (1989). Also see Hausman (ed.) (1984); Redman (1991); and DeMarchi (ed.) (1988).

⁹As Caldwell (1984) puts it:

It may reasonably be conjectured that a majority of economists would consider the construction of theoretical models which are capable of generating testable predictions to be the hallmark of scientific activity. And it seems that the converse is also a widely held sentiment; a proposed theory which is not expressed in testable (preferable, falsifiable) form is not scientific. (p.124)

¹⁰Thus Hume stated:

All the objects of human reason or enquiry may naturally be divided into two kinds, to wit, Relations of Ideas, and Matters of Fact. Of the first kind are the sciences of Geometry, Algebra and Arithmetic; and, in short, every affirmation which is either intuitively or demonstratively certain ... Propositions of this kind are discoverable by the mere operation of thought, without dependence on what is anywhere existent in the universe ...

Matters of fact, which are the second objects of human reason, are not ascertained in the same manner; nor is our evidence of their truth, however great, of like nature with the foregoing. The contrary of every matter of fact is still possible; because it can never imply a contradiction, and is conceived by the mind with the same facility and distinctness, as if ever so conformable to reality. That the sun will not rise tomorrow is no less intelligible a proposition, and implies no more contradiction than the affirmation, that it will rise.

David Hume, (1748), An Enquiry concerning Human Understanding, Sec. IV, pt. I, pp. 25-6, Clarendon Press, Oxford, 1975.

He goes on to formulate what has come to be called Hume's Golden Fork:

When we run over libraries persuaded of these principles, what havoc must we make. If we take in our hand any volume; of divinity or school metaphysics, for instance, let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact? No. Commit it then to the flames, for it can contain nothing but sophistry and illusion. (ibid., last page)

¹¹For a critique of Mises' "apriorism" see Stewart op. cit. pp. 118-121. Also see Caldwell, pp. 103-351.

¹²See T. Hutchinson (1938). P. Samuelson: (1983), (1952) and (1963). Critiques of these views are in F. Machlup (1982) and B. Caldwell, op. cit.

¹³M. Friedman (1953).

¹⁴K. Popper (1959).

¹⁵M. Blaug (1980) and Hutchinson, op. cit. and also Hutchinson (1977), (1978).

¹⁶Caldwell offers a succinct summary of the first two views. He writes:

In the construction and evaluation of their theories, most economists adhere to some variant of confirmationism or instrumentalism. Both of these approaches emphasize the testing of theories by their predictions. They differ in that instrumentalists consider the most highly confirmed theory the most useful instrument, whereas, confirmationists consider the most highly confirmed theory the most probable; that is, confirmationists consider the most highly confirmed theory the most probable; that is, confirmationists do and instrumentalists do not associate strength of confirmation with some notion of truth value.

(p.231) See his p. 238 and following for a critique of these views.

The third view (falsificationism) implies:

Scientists should not only empirically test their hypotheses, they should construct hypotheses which make bold predictions, and they should try to refute hypotheses in their tests. Equally important, scientists should tentatively accept only confirmed hypotheses, and reject those which have been disconfirmed.

(Caldwell, op. cit. p. 125)

Falsificationism, is logically the most cogent of the positivist methods. For Popper's falsificationist principle is based on the realization of the logical fallacy in the verificationist version of the syllogism for hypothetico-deductive "testing of theories". This takes the form: "1. If A is true, then B is true. 2. B is true. 3. Therefore A is true." This is logically fallacious as it affirms the consequent. [Note that the major premise in 1 can be split up into the antecedent (A is true) and the consequent (B is true). In the above syllogism, affirmation of the consequent cannot establish the truth of the antecedent.] But the negative form of the hypothetical syllogism leads to the logically correct "falsificationism". The syllogism now becomes: "1. If A is true, then B is true. 2. B is not true. 3. Therefore A is not true." The null hypothesis in econometric methodology applies this principle. However, as

Stewart, op cit (pp. 54-56) has shown, once we recognize the probabilistic nature of our data and "tests" then the conclusions from the two versions of the two syllogisms are: for the logically correct "falsification"... A is perhaps not true", and for the logically incorrect verificationist version that "A is perhaps perhaps true." Stewart concludes that though the former is more satisfactory from a logician's point of view "as far as the working scientist is concerned the difference does not really matter much" (p.56).

¹⁷ See for instance the devastating critique of Popper by the Harvard philosopher Hilary Putnam (1981), and by Ian Hacking of that other member of the LSE Popperian group, Imre Lakatos. See Hacking (1981). For another devastating critique in terms of the history of ideas, of the whole Popperian enterprise see D.C. Stove (1985). Also see the essays in P. Achinstein (ed.) (1983), E. Sosa (ed.) (1975), B. Caldwell (1982) and D.N. McClosky (1985), J. Elster (1983), A. Ryan (ed.) (1973) and Antony Flew (1985).

¹⁸ The rise and fall of "testing" in economics, has been valuably surveyed by De Marchi (1988), in terms of the revolt against the leading contemporary proponent of the classical viewpoint -- both its methodology and prescriptions -- Lionel Robbins, which was led at the L.S.E. by the group of Young Turks surrounding Richard Lipsey. Robbins and Keynes took the view that "in economics the parameters are themselves in the most important cases quickly changing variables" (Robbins (1938)), so that they were both sceptical about quantification. The above quotation was also cited by Popper (1957, p. 143) in his "nearly" exempting economics as a subject requiring testability.

But as De Marchi notes, amongst Robbins' younger colleagues there was

growing dissatisfaction that: "There must be something more to economic science than (as Steuer has nicely put it...) 'the discovery of irresistible truth through logical manipulation of a few self-evident postulates'" (de Marchi (1988) p. 144). De Marchi surveys their subsequent discovery of Popper and the twists and turns in their final disenchantment with Popperian falsificationism, epitomised by Lipsey's statement in the 2nd edition of his famous textbook: "I have abandoned the Popperian notion of refutation" (Lipsey (1966), p. XX).

Robert Solow (1985) has also recently questioned the belief amongst "the best and brightest in the profession ... [that] economics is the physics of society" (p. 330). He argues that, "the attempt to construct economics as an axiomatically based hard science is doomed to fail ... [as] the classical hard science devices for discriminating between competing hypotheses are closed to us. The main alternative device is the statistical analysis of historical time series. But then another difficulty arises". In order to distinguish between complex and subtle competing hypotheses, many of which "are capable of fitting the data in a gross sort of way ... we need long time-series observed under stationary conditions ... [But] much of what we observe cannot be treated as the realization of a stationary stochastic process without straining credulity" (p. 328).

He then "recommends" marrying the tools of the economist with "the ability [of the economic historian] to imagine how things might have been before they became as they now are" (ibid. p. 331). This is pretty close to the classical method of analytical economic history as I have dubbed it.

¹⁹ Robert Clower has put the point nicely.

Contrary to popular opinion and the pretensions of some scientists, the bulk of all knowledge commonly regarded as "scientific" is expressed in terms of stories that differ little

from stories told by writers of serious novels. The resemblance is not accidental. The aim of the novelist is to persuade us that his story might almost be true, while that of the scientist is to persuade us that outwardly chaotic sense data fall into meaningful patterns. We might argue that the two situations differ in that the scientist doesn't invent his facts (at least, he is not supposed to) whereas the novelist is not so constrained. On further reflection, however, the two cases seem to be indistinguishable. Although the scientist does not invent his facts, he does choose them. More precisely, he selects from an infinity of possible facts collections in which (for reasons best known to him) he is able to "recognize" interesting patterns. In exactly the same manner, the novelist chooses from an infinity of possible characters and situations just that combination about which he thinks a good story can be told. In both cases, therefore, it is strictly true to say that the artist "invents" his story. We need not be surprised, therefore, to find "order" in economic or social phenomena any more than we are surprised to find "order" in natural phenomena -- or in any good novel. Scientists would not bother to write about "nature" or "society" any more than novelists would bother to write about "life" unless they were first convinced that what they had to say made a story that was worth telling. (R.W. Clower: "The Ideas of Economists," Sixth Monash Economics Lecture, Monash University, 1972, reprinted in A. Kramer, D.M. McCloskey and R.M. Solow (eds.), The Consequences of Economic Rhetoric, Cambridge, 1988, p. 87.

²⁰ Blaug's doubts on such an enterprise may be noted:

Story-telling makes use of the method of what histories call colligation, the binding together of facts, low-level generalization, high-level theories, and value judgments in a coherent narrative, held together by a glue of an implicit set of beliefs and attitudes that the author shares with his readers. In able hands, it can be extremely persuasive, and yet it is never easy to explain afterwards why it has persuaded.

How does one validate a particular piece of story-telling? One asks, of course, if the facts are correctly stated; if other facts are omitted; if the lower-level generalizations are subject to counter examples; and if we can find competing stories that will fit the facts. In short, we go through a process that is identical to the one that we regularly employ to validate the hypothetic deductive explanations of orthodox economics. However, because story-telling lacks rigour, lacks a definite logical structure, it is all too easy to verify and virtually impossible to falsify. It is or can be persuasive precisely because it never runs the risk of being wrong.

M. Blaug (1980), p. 127

²¹ "Hicks (1979, Chp. 2) rests his whole analysis of causation on the

purported counterfactual implications", Elster op. cit., n.23, p.240.

²²Ibid. p.XI.

²³Also Hayek has emphasized

if our historical fact is such a complex as a language or a market, a social system or a method of land cultivation, what we call a fact is either a recurrent process or a complex pattern of persistent relationships which is not "given" to our observation but which we can only laboriously reconstruct -- and which we can reconstruct only because the parts (the relations from which we build up the structure) are familiar and intelligible to us. To put it paradoxically, what we call historical facts are really theories which, in a methodological sense, are of precisely the same character as the more abstract or general models which the theoretical sciences of society construct. The situation is not that we first study the "given" historical facts and then perhaps can generalize about them. We rather use a theory when we select from the knowledge we have about a period certain parts as intelligibly connected and forming part of the same historical fact. F.H. Hayek: "The Facts of Social Science," in his Individualism and Economic Order, Routledge, 1949, p. 71.

²⁴Ibid., p. IX.

²⁵J.R. Hicks (1965).

²⁶Hicks, Causality, op. cit. p.5. Also see McCloskey: "The Consequences of Rhetoric," in Klammer, McCloskey, Solow (eds.) The Consequences of Economic Rhetoric, op. cit., p.289. In this McCloskey also deals with the fear that many have of this forensic approach. Thus he notes:

The positivist philosopher will claim that using such a rhetorical forensic approach to science would not have standards. But he is wrong. On the contrary, the standards of "consistent theory" or "good prediction" presently in use are low, to the point of scientific fraud ... Consider this. Is it more difficult for a Chicago economist to produce still another regression consistent with the hypotheses of peasant rationality or, on the other hand, to produce a set of arguments, drawn from all the evidence he can find and his audience thinks relevant, that can actually persuade an economist from Yale? (p.289)

²⁷A deterministic hypothesis makes a statement about all the members of the relevant group. It is to be contrasted with a statistical hypothesis which only applies to some of the group.

²⁸These statistical limits of variability refer to the so-called "law of large numbers", whereby individual deviations from the observed pattern in the future tend to cancel out over a large number of observations, as in the past.

²⁹There is the further problem with verificationism and falsificationism as a scientific method, which has been known since it was noted by the physicist Pierre Duhem. It is well known that in the hypothetico-deductive method, if H is the hypothesis, P the deduced consequence, and $A_1 \dots A_n$ various auxiliary statements and hypotheses, then the first statement in the hypothetical syllogism becomes: "if (H and $A_1, A_2, A_3 \dots A_n$) are true, then P is true." Suppose following the falsificationist procedures we find P is not true. That does not falsify H , for instead of H , any of the A 's may have been false. That is why in science, accepted theories are rarely questioned. As Putnam (op. cit. ps. 71 & following) shows, the schema for scientific problems in "normal" science is usually of the form.

Schema 1

Theory

?? _____

Fact to be explained

(where the auxiliary statements (AS) are missing and the problem is to find these AS which are true, and when conjoined with the theory provide an explanation of the fact) rather than the schema proposed by falsification

(where the AS and the theory are given)

Schema 2

Theory

Auxiliary Statements

Prediction -- True or False?

Normal science and economists for that matter, will question the "truth" of the auxiliary statements rather than the theory. As Putnam shows the Law of Universal Gravitation was not strongly falsifiable. As he concludes:

(1) theories do not imply predictions. It is only the conjunction of a theory with certain "auxiliary statements" (AS) that in general implies a prediction. (2) The AS are frequently suppositions about boundary conditions (including initial conditions...) and highly risky suppositions at that. (3) Since we are very unsure of the AS, we cannot regard a false prediction as definitely falsifying a theory; theories are not strongly falsifiable. (p.67-8)

Or as McCloskey has put this point:

The main hypothesis is insulated from crucial test by the auxiliary hypotheses necessary to bringing it to any test. The test may be worth doing... But... it is not a certitude, not the crucial experiment, not the only Real Test. This insulation from crucial test, is the substance of most scientific disagreement. Economists and other scientists will complain to their fellows, "Your experiment was not properly controlled". "You have not solved the identification problems, you have used an equilibrium model when a disequilibrium model is relevant."

(McCloskey, op. cit. p.??)

³⁰The following is based on Stewart op. cit. This version can be contrasted with the highly confused and confusing account of this method in Blaug op. cit., who seems to subsume it as a variant of "verificationism".

³¹Stewart op. cit. Robbins provides a clear statement of this view:

The propositions of economic theory, like all scientific theory are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience relating to the way in which the scarcity of goods which is the subject matter of our science actually shows itself in the world of reality. The

main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realized. We do not need controlled experiment to establish their validity; they are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious. (Robbins (1935) op. cit. p.78-9)