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BOLTON, Craig Jay, 1949-
THE BRITISH HISTORICAL SCHOOL IN
POLITICAL ECONOMY: ITS HISTORY AND
SIGNIFICANCE (VOLUMES I AND II).

Texas A&M University, Ph.D., 1976
Economics, general

Xerox University Microfilms, Ann Arbor, Michigan 48106

THE BRITISH HISTORICAL SCHOOL IN POLITICAL ECONOMY
ITS HISTORY AND SIGNIFICANCE
VOLUME I

A Dissertation

by

CRAIG JAY BOLTON

Submitted to the Graduate College of
Texas A&M University
in partial fulfillment of the requirement for the degree of
DOCTOR OF PHILOSOPHY

December 1976

Major Subject: Economics

THE BRITISH HISTORICAL SCHOOL IN POLITICAL ECONOMY
ITS HISTORY AND SIGNIFICANCE
VOLUME I

A Dissertation

by

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ABSTRACT

The British Historical School in Political Economy

Its History and Significance. (December 1976)

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This dissertation summarizes the methodological views of each of the major participants in the Nineteenth Century British Methodenstreit in Political Economy and isolates those currents in Nineteenth Century economic methodology which have persisted into the Twentieth Century. Among those involved in the British Methodenstreit I have examined the relevant writings of Walter Bagehot, John E. Cairnes, J. K. Ingram, Richard Jones, T. E. C. Leslie, Alfred Marshall, David Symes, and William Whewell.

Three major conclusions arise from this study. First, each of the writers considered possessed a somewhat idiosyncratic conception of the scope and procedures appropriate to economic inquiry. In this respect, then, it is misleading to speak simply of Historical and Orthodox "schools," since these labels have frequently been interpreted as denoting homogeneous points of view.

Second, those fundamental characteristics which were shared in common by writers within each of the two methodological traditions are not the characteristics which have frequently received the attention of the intellectual historian. The Historical School, for example, has often been associated with its German counterpart and portrayed as a reaction against all economic theorizing. Instead of

disposing of economic theory, however, the typical British Historicist of the period prior to the 1890's was interested in tying the existing theory to specific institutional contexts, thus integrating into economic analysis some important behavioral constraints. So far as this attempt was successful it resulted in economic theories yielding definite predictions and testable consequences, as opposed to a theory which was nebulous enough to explain everything but which predicted only ex post.

Third, the Historical and Orthodox orientations toward the meaning of and justification for economic studies have persisted, in somewhat mutated forms, to the present day. Frank Knight, Fritz Machlup and, to a lesser extent, Milton Friedman have emphasized the role of economics as a way of viewing the world and organizing our perception of social events. While prediction is granted a role in most versions of neo-Orthodoxy, the certainty of the theory is still guaranteed by our introspective inspection of our own motives and by the intuitive appeal of economic reasoning. Prediction on the basis of economic analysis is still limited by the reputed inaccessibility of controlled experimentation in social science and by the "partial" character of economic motives in the direction of human action. Opposed to the neo-Orthodox tradition have been writers such as T. W. Hutchison, Eugene Rotwein and, to some degree, Paul Samuelson. These neo-Historicists have demanded that theories be clearly specified, tested by comparison with existing data sources, and either modified or rejected if found to be contradicted by test results. Although many neo-Historicists, like their Nineteenth

Century counterparts, have been more concerned with generalized consideration of what is to be done, rather than with the mechanics and experimental techniques required to carry through their proffered research programs, they do represent a recognizable and distinct alternative to the neo-Orthodox methodology.

The Nineteenth Century conflict between Historical and Orthodox economic methodologists is thus found to have a close parallel in recent economic discussions. The case of the British Methodenstreit is instructive not only as a premature and abortive "scientific revolution," but also as the historical background for concerns of more immediate interest to modern economists.

ACKNOWLEDGEMENTS

Footnotes may indicate the source of a particular idea, but they seldom reveal either the source of the original inspiration or the continuing guidance which have gone into the construction of a completed project. For demonstrating that the history of economic thought is not only interesting but also rewarding and engrossing I wish to thank both Professors Robert B. Ekelund, Jr. and Alfred Chalk of Texas A&M University. To Professor Ekelund must also be extended my deepest gratitude for originally suggesting the importance of the British Historical School and for his patient aid and encouragement throughout the subsequent research. Without Dr. Ekelund's experienced guidance I would have never finished the project.

The idea of viewing the British Historicists as methodological reformers rather than revolutionary nihilists was suggested to me by the researches of Professor Erik Furubotn of Texas A&M. His studies of the impact of alternative property structures and the empirical enrichment of production functions were clearly just the sort of research toward which the British Historicists were developing.

Professor Robert L. Basmann of Texas A&M is responsible for stimulating my interest in the methods and difficulties involved in the testing of economic theories. Where I have discovered faults with the British Historicist program, my criticisms have been primarily grounded upon the basis of considerations he had suggested during course work at A&M. I only regret that my consideration of many questions concerned with empirical economics was restricted by

the comparatively primitive level of methodological controversy during the Nineteenth and early Twentieth Centuries.

Finally I must express my debt to my teachers in philosophy of science, Professor Richard Byerly of the University of Arizona and Professor Hugh McCann of Texas A&M. They are primarily responsible for introducing me to a realm for which I have the greatest enthusiasm, but concerning which I am yet little better than a neophyte.

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CHAPTER I

AN INTRODUCTION TO THE DISSERTATION AND A REVIEW OF
PREVIOUS RESEARCH CONCERNING THE
BRITISH HISTORICAL SCHOOL

The development of Nineteenth Century economics in Britain has been documented by scores of books and articles, and it has long been believed that the central issues and major figures connected with the period were well-known. Only a few accounts of the development of economics during the last two hundred years have, however, concerned themselves with an issue which virtually dominated the discipline during the later decades of the Nineteenth Century: The Methodenstreit between "Orthodox" and "Historical" economists.¹ Those few sources which have sought to examine and explain the clash of methodologies have portrayed it as a passing fit of professional infighting, of little real importance to the "pure theory" of economics, or as a debate involving issues long since settled in the modern age.

The traditional limitations placed on the history of economics, though, have to a great extent reinforced this neglect of the British Methodenstreit, as have the prohibitions against extensive methodological discussions popularized by Marshall. The "proper" study of the history of economics has been variously interpreted as a history of economic analysis, as a history of political economy or as a history of the filiations between economic and "non-economic" ideas. All major

The citations in this dissertation follow the style and format of The American Economist.

forms of past historical studies have, however, ignored the scientific goals and explicit methodological statements of previous economic writers as matters of little concern to the present day. The research methods advocated and practiced by the Classics and early Neoclassicals, the questions which they posed concerning the scope and significance of economic inquiry, and the changes which they suggested in the overall reorientation of economic problem solving have been viewed as issues of little historical importance. Yet the admittedly erroneous views of past authors concerning matters of "theory" or their discussions of policy issues long since extinct have been the subject of meticulous probing and detailed evaluation.

The inherent traditionalism of many historians of economic thought has, of course, had its good side. Their endorsement of the theorist's view of methodological controversies as an unproductive and devisive pursuit² has undoubtedly aided in the rapid and reasonably unfettered development of economic theory, especially neoclassical micro-theory, along pre-established paths. This same surrender to the "mainstream of economic thought" has, however, distorted the historian's own perspective on past events and caused him to ignore many rich sources of "original" and important ideas.

While a perpetual search for "anticipations" of contemporary theories could not fail to flatter the theorist's ego and thus raise his estimate of the "Study of Dead Men," the eventual consequences of limiting intellectual history to such a pandering approach are less than pleasing. There are only a limited number of "anticipations" to be found in the writings of truly scholarly social thinkers, no matter

how strained the interpretation of sources may become. The historian of economics who limits himself to "anticipations" may thus run short of relevant material or be forced to exhume progressively less sophisticated writings. In order to gain any long-run worth, the history of economic thought must make contributions of its own to the continuing development and improvement of the discipline; and in order to accomplish that task the historian must become something more than a perennial sycophant.

It is only to the extent that the intellectual historian concentrates his efforts and attention on the "heterodox" notions of past writers and on the uniqueness of their suggestions for the improvement of both theory and policy that he is able to provide a fresh perspective on present controversies as an aid to the development of new patterns for economic speculation. This is especially true in matters concerning economic methodology and in those other areas in which the present practices and forms of economic research and of "economic explanation" have yet to be fully developed into universally agreed-upon and readily defensible modes. This dissertation is devoted to just such a re-examination of certain key methodological issues, which were considered at length in the writings of the British Historical economists and the writings of their "Orthodox" antagonists. While I believe that the study of these authors supplies important background for a consideration of more contemporary contributions to the literature of economic methodology (e.g., the methodological writings of Knight, Friedman, Machlup, Coats and Hutchison)³, and that this study constitutes a rather different interpretation of the significance of

previously neglected authors, the lesson which I have hoped most of all to illustrate in the following pages is the importance of rereading past writers for their own insights into the process of economic analysis, rather than viewing them as mere forerunners of contemporary "innovations" in our theories.

The Scope of This Inquiry

In this study I have not and could not attempt an exhaustive interpretation of the world-wide methodological debate which ravaged economic inquiry during the Nineteenth Century. A task of that magnitude would run to several volumes and would be many years in completion. I have, however, examined the major contributions of the central figures in the British Methodenstreit and traced the effects of their methodological views to the present day. Germany and France each had their own methodological controversies and conflicts, but the insularity of British economics during much of the later Nineteenth and early Twentieth Centuries provides a reasonable justification for the separate consideration of authors in that environment. Although many of the early Historical writers in Britain had contact with foreign sources, their views and the issues they debated were mainly indigenous to their native lands. It was only in the immediate pre-Marshallian period that influences from France (Comte) and Germany (Roscher and Schmoller) became recognizable as the source inspiring British methodological controversy. Even during this period, however, the issues considered were not those so hotly debated on the Continent.

The Methods of this Study

There are two major alternative methods which may be pursued in a study of intellectual history. The historian may choose to consider past writers as evolutionary steps in a process culminating in the views of his own period, or he may attempt to probe an author's beliefs and theories from "the inside," making sense of that which is obscure and searching for the common thread which binds together the separate pieces of an author's work.

The "Whig theory" of intellectual history views the development of a discipline as a steady progression from ignorance to knowledge, with each worker in a "tradition" building upon the foundations left by his predecessor. The "Revisionist" historian, on the other hand, asks the question "What went wrong, and how may the damage be repaired?" While Whig historians are interested in their subjects only as cogs in a developmental machine, Revisionists consider them as creative and original forces, both acting upon and being acted upon by the social context. Although these methodological perspectives on historical research are neither proscriptive nor definitive, they do provide a means for defining extreme points in the spectrum of possible approaches. As such they act as an aid in the formation of more definite judgments concerning the methodology appropriate to any particular historical study.

In actuality, of course, few studies could be cited which are paradigms of either of these approaches. Historians who choose to emphasize the "history of economic analysis," and who are thus primarily Whigs in their historical methodology, have frequently concerned them-

selves with certain anomalous ideas or individuals, and have even been driven to comment upon the "tone of the times" and its impact upon the discipline. Alternatively, historians with a more Revisionist orientation have sometimes chosen to emphasize those doctrines which would later gain professional acclaim or to criticize the "errors of the past" from the perspective of the present. Although the distinction between the Whig and Revisionist approaches is a valid one, it is perhaps more applicable to the aspects (or component questions) composing any historical study than it is to the study as a whole. It is with this in mind that we turn to a description of the methods of this study.

Positions on economic methodology are nearly as varied today as they were in the Nineteenth Century, even though they now occupy a less central place in economic discussions. This lack of a stable consensus concerning the types of questions with which we are primarily interested in this dissertation seems to decisively rule out a purely Whig interpretation of the British Methodenstreit and of the echoes and secondary impacts of it on Twentieth Century economic writings. There are, however, certain requirements which should be met in order to establish the continuity of influences from the past on the present, and in this respect something like the Whig interpretation of historical causality is inescapable. As a compromise, then, I have considered each of the major authors in both the early Historical camps as original if not autonomous thinkers, often aware of the contributions or blunders of their immediate contemporaries but just as frequently striking out on new and unexplored paths. I have

also, at the same time, made use of the concluding chapter of the dissertation to summarize and organize the views of the opposing Schools, to criticize both of their basic methodological positions from the standpoint of more modern research and to indicate how many of the basic issues they debated and the basic stances which they assumed on these issues are mirrored in "modern" writings on economic methodology. The "smooth-flow" of an issue-oriented approach to the Nineteenth Century Methodenstreit would surely have improved the literary qualities of this study, but the more accurate and appropriate way to approach the methodological views of such diverse authors as Symes and Whewell is surely a case-by-case consideration of their own meta-economic writings.

The unity and overall development of the "Historical School" in England are matters discussed in more detail in the subsequent chapters, but the somewhat related question of the temporal and philosophic scope of this inquiry requires additional clarification at the outset. By the early 1880's the meta-economic orientation of the early British Historical School had been virtually forgotten, although pseudo-methodological controversies would continue to disturb the peace of British economics until well into the next century. The efforts of Jones, Whewell, Bagehot, Symes and Leslie which had aimed at the reconstruction of economics along institutional and empirical lines were gradually and almost imperceptibly supplanted by three divergent trends operating under the "Historical" label: the growth of economic history in the writings of Ashley, Cunningham, Rodgers and Toynbee, the evolutionary and biological analogies of the Comtists

and English organicists, and the reduction of economics to moral philosophy and "common sense" at the hands of social philosophers and rabid ideologues. Despite the initial burst of enthusiasm which had greeted the extension of biological and scientific methods to economic inquiry, none of these later trends represented a viable alternative to the prevailing orthodoxy. By the "Nineties" or the early decades of the Twentieth Century these pseudo-alternatives to Marshallian neoclassicism had either collapsed under the weight of their own rhetoric or had been absorbed into the "mainstream" of the discipline as specialized fields of somewhat dubious worth. Through the criticisms of Sigwick, J. N. Keynes, and the more subtle asides of Alfred Marshall, the term "Historicist" gradually came to refer solely to the works of the later British historians and social organicists as well as to the writings of the later German Historical School of Gustav Schmoller. The concerns of the early British Historicists were soon to be forgotten in muddled debates concerning methodological positions which they themselves had repeatedly disavowed.

The distinctive and most important contributions of British Historicism were exclusively the property of the early Historicists, and the bulk of this study is, therefore, devoted to an examination of their works and ideas. Ingram and Marshall have been considered at some length in order to illustrate the transition between the early and later Historical views and the reasons for the reinterpretation of the Historicists' original concerns, but these two authors could just as well have been omitted from this study if an appreciation for the different periods in the British Historical movement had previously

been developed.

Although the interests of this study have been historical as well as methodological, the methodological side of the inquiry has tended to predominate. The following pages are thus concerned more with a detailed examination of the Historicists' and Orthodox economists' meta-economic doctrines than they are with a consideration of their life histories or their intellectual attainments in other fields.

A Review of the Existing Literature

Despite the vast number of texts and articles concerned with the development of economics in Britain, there are only a handful of secondary sources which consider the history of the British Methodenstreit. Among these the most frequently cited are T. W. Hutchison's A Review of Economic Doctrines, 1870-1929⁴ and A. W. Coats' "The Historicist Reaction In English Political Economy, 1870-1890."⁵ Despite their reputation as authoritative works, however, both Hutchison's and Coats' accounts of the development and character of the Historical School are deficient if not blatantly inaccurate.

Hutchison's discussion of the methodological debate in Britain is limited by his virtual exclusion of all events occurring before the mid-1870's and by his concentration upon the policy aspects of the debate. Although he explicitly cites each of the major participants in the methodological controversies of the period (including David Symes), his overall consideration of strictly methodological questions is limited to a scant four pages.⁶ Hutchison's development of any organized statement concerning the goals and procedures of economic

historians along with the economists of the Historical School and by his rather indecisive separation of the two groups. Although Hutchison's Review of Economic Doctrines must be acknowledged as one of the most subtle and suggestive histories of economic thought, its treatment of British Historicism is only slightly more revealing than the single footnote usually accorded the School in other, more standard, accounts of the period.

Coats' 1954 Economica article, "The Historical Reaction In English Political Economy, 1870-1890," builds upon the foundations laid by Hutchison and suffers from many of the same defects. Coats explains the popularity of British Historicism by reference to the downturn in the British economy after the 1850's, and he finds the roots of the movement in the philosophy of August Comte, the German Historical School of Roscher and the British historians of social development (i.e., Morgan and Sir Henry Maine).⁷ Coats states that Jones' influence on the development of the School and on economic theory as a whole was overrated by past historians,⁸ a claim which is undoubtedly true but which was to the detriment of the discipline rather than to its advantage. He was further remiss in omitting entirely any reference to the writings of either David Symes or William Whewell.

Like Hutchison, Coats' entire consideration of the "dispute over method" is limited to only a few pages, and like Hutchison he includes the later minor Historicists and economic historians (i.e., Fawcett, Cunningham and Sidgwick) on an equal footing with more major writers like Leslie and Ingram. It is clear from his account that the Historical economists in Britain were disturbed about something, but

whether they had legitimate grievances against the Classics' methodology or were merely intellectual imperialists, eager to conquer all social inquiry for the disciplines of Sociology and History, was a question which remained an unsolved mystery in his analysis of the movement.

Two lesser known, but superior, treatments of British Historicism are to be found in William Scott's The Development of Economics⁹ and Robert B. Ekelund's "A British Rejection of Economic Orthodoxy."¹⁰ Ekelund in his 1966 article outlines the history of the Historical School in British economics, placing it within the context of the intellectual trends of the day and against the background of similar movements in other nations. He quite properly objects that: "the role of the British (Historicists) in this 'historical revolution' has been greatly neglected, and their substantive and important contributions have been jaded by attention to the German School,"¹¹ and he correctly identifies the British Historicist's primary target as the "abstract a prioriism" and speculative methodology of the Orthodox economists.¹² Although Ekelund fully recognized the indigenous status of the Historical movement in Britain, he laid somewhat more emphasis on the influence of French (Comte) and British (Spencer) evolutionists than would be appropriate in the present more limited consideration of early Historical writers. While his consideration of some of the later Historical economists (i.e., Ingram and Toynbee) heightens our appreciation for the type of Historicism encountered by J. N. Keynes and Alfred Marshall and strengthened his case against considering the British Historicists as "poor relations" of Germans,

those parts of his discussion are outside the scope of this dissertation.

Scott's survey of particular writers in the British Historical tradition is less interpretative and more descriptive than Ekelund's, but it still possesses many excellent features. Scott's section on the School contains concise and informative summaries of the main doctrines professed by Leslie, Ingram and Ashley, although he omits any consideration of Bagehot, Jones or Symes, and he misinterprets the methodological views of Alfred Marshall. While carefully constructed and largely accurate, Scott's treatment of British Historicism is still too brief to serve as more than a rough guide to any of the following sections. Although Scott has accomplished an admirable summary of the School within the context of a general text on the history of economic thought, his research in no way fulfills the role of an authoritative study, or even a brief but comprehensive outline of the issues debated by and the methodological roots of British Historicism. The works of Ekelund and Scott considered together do, however, provide a firm foundation for a more detailed history of the methodological controversies of the Nineteenth Century. They have often been referred to in structuring the research which has gone into the present history, and they promise to provide a basis for an even more in-depth study of other, later, currents in the British Methodenstreit.

Other References to the British Historical School

Passing comments restricted to the consideration of individuals within the British Historical tradition are not uncommon in the more popular and "up-to-date" histories of economic thought or in some of the older references to the development of the discipline. L. H. Haney devotes some thirteen pages to the topic under the heading of "Concrete Historical Criticism in Great Britain,"¹³ Eric Roll comments briefly on Richard Jones in his History of Economic Thought,¹⁴ and Joseph Schumpeter cites the works of Leslie, Jones and Ingram in his History of Economic Analysis.¹⁵ None of these works, however, extends much beyond a mere recitation of the basic fact of the movement's existence and its opposition to the Ricardian tradition. In short, none of them are substantial enough to warrant further consideration outside of those chapters to which they most directly relate.

A Brief Introduction to British Historicism

The roots of the Historical movement in Britain are as varied as the many writers who contributed to its development and are sometimes identifiable only through conjectures based upon the broader trends in European thought. Francis Bacon, Adam Smith, Sir Henry Maine and Auguste Comte were each mentioned in reverent tones by one or another of the Historicist writers. Yet the more probable source for their common inspiration was the inter-related complex of views, represented in the traditions of nominalism, associationist psychology and empiricism, that had dominated British thought since before the Fourteenth Century.

Unifying Factors in British Historicism

The British Historicists were in an odd position, historically speaking, and they were well aware of the anomaly represented in their intellectual environment. Economics was the only field in British science so completely dominated by the rationalistic and speculative methods of the Continent, and its unique status as a "metaphysical" study seemed to demand an explanation. Jones, Bagehot, Symes, Leslie and Ingram each examined this question, and to a man they reached something like the same conclusion: the speculative and "metaphysical" character of economics in the Nineteenth Century was primarily due to the influence of Ricardo. Although most of the Historicists were willing to concede that pre-Ricardian writings, as exemplified by Adam Smith's The Wealth of Nations, were both primitive and unstructured when compared to the Orthodox treatises of the Nineteenth Century, they recognized in these earlier writings something like their own interest in and concern for "concrete" empirical inquiries and their own belief in the importance of determining "the facts of the case" before formulating theories to explain it. The Ricardians, however, were charged with the use of "vicious a priorisms" in formulating economic hypotheses, and they were held to be doubly guilty for applying their counter-factual speculations to the policy issues faced by the British nation. Leslie, among others, was so incensed by the Ricardians' use of the "absolute principles" of economics as justifications for their own political biases that he was driven to quip that "Instead of a science of wealth they have given us a science

for wealth."¹⁶ However, neither Leslie nor any of the other early British Historicists ever questioned the basic social importance of a science of economic relationships nor faltered in their hopes for its indefinite improvement.

The Decentralized Character of the "Historical School"

Despite basic agreements concerning a common enemy and a general course along which economic inquiry should be redirected, each of the British Historical writers remained largely ideosyncratic in his own methodological views and the justifications which he offered for these views. The critical and empirical approach which the Historicists had adopted in their investigation of economic and social phenomena left no basis for the creation of a scientific "paradigm", or, more properly a scientific dogma. There were no ground rules for delimiting and proscribing the types of questions which it was "legitimate" to pose in an Historical investigation or the types of answers which were acceptable in response to these questions. For, in fact, there was no such thing as a well-organized and proscriptive "Historical School."

In this sense, but only in this sense, were J. N. Keynes and Alfred Marshall correct in their identification of the Historical movement with a rejection of "theory." The theory which the Historicists rejected was not, however, a theory which attempted to examine the economic and political constraints which structured human action, which attempted to critically approach social problem-solving through the tools of empirical research. The "theory" rejected by the His-

toricists was, rather, the theory which composed the "core" of Classical and Neoclassical economics, i.e., the methodological ground-rules which conditioned and limited the thoughts and explanations for social action proffered by "Orthodox" economists. The British Historicists clearly recognized that a critical and reflective methodology was required in social and economic investigations even more than in the researches of the physical scientist, and they were enthusiastic in their endorsement and defense of those types of procedures without much regard for the content of the theories being proffered for testing. As Bagehot once remarked in a caveat to those orthodox economists who were excessively hasty in proclaiming the absolute truth and infallibility of their doctrines, as well as their universal applicability to any social structure:

...the cultivators of an abstract science are always in great danger of forgetting its abstract nature; they rush and act on it at once. In the abstract physical sciences there is an effectual penalty--a person who acted on abstract dynamics would soon break his head; but in mental and ... (social) ... sciences, unhappily, there are no instant tests of failure,--whatever happens a man can always argue that he was right.¹⁷

A Note to the Following Chapters and Appendices

In the chapters that follow I have traced the ideas and achievements of the major figures in the British Historical tradition and of their opponents in the Orthodox School during the period of roughly 1830-1880. Although each of these "methodological sketches" is largely self-contained, the overall emphasis of the various branches of the Historical tradition are discovered to be related through a

"family resemblance," and that relationship is summarized in the concluding chapter. In addition, a number of important, if somewhat tangential, issues and some clarification of the terminology used in this study have found a home in the appendices attached at the end of this dissertation. While the body of the work is intelligible without reference to these supplementary materials, the meaning and development of the School are more readily comprehended if careful attention has first been devoted to them. Of special importance are the appendices on "J. S. Mill's Methodology," "On The Terminology Used In This Investigation" and "On The Methodology of William Whewell." These three sections serve to fill certain rather glaring gaps in the continuity or interpretation of the School, and they act to clarify certain issues which might otherwise remain obscure.

Footnotes to Chapter I

1. Of the standard secondary sources concerned with the history of economic thought, Joseph Schumpeter, in his massive History of Economic Analysis (New York: Oxford University Press, 1954), devotes only seventeen pages to the Methodenstreits of the Nineteenth Century. Two-thirds of this already abbreviated discussion is limited to a consideration of German and Continental writers, and Schumpeter further confounds the issues of the day by failing to distinguish between Historicism, historicism and the "extravagant claims advanced in favor of economic history" (historicism). Eric Blaug in his Economic Theory In Retrospect, Revised Edition (Homewood, Illinois: Richard D. Irwin, 1968) found the English Historical movement to be worthy of less than a paragraph, of which the most significant, and significantly wrong, line reads: "The English Methodenstreit was put to rest by John Neville Keynes' Scope and Method of Political Economy (1890) and by Marshall's conciliatory attitude in the Principles (1890)...," p.305. Wesley Clair Mitchell, last of all, takes only two pages in the second volume of his Types of Economic Theory (New York: Augustus Kelley, 1969), pp. 36-38, to thoroughly misinterpret the views of Leslie, Ingram and a host of lesser known British Historicists.

2. For an especially vituperative discussion of the deleterious effects of methodological controversy see Frank Knight's "What Is Truth In Economics?," Journal of Political Economy, Vol. 47 (February, 1940), pp. 1-23; especially pp. 1, 12 and 15.

3. The views of these authors are examined against the background of Historical economics in the concluding chapter of this dissertation.

4. T. W. Hutchison, A Review of Economic Doctrines, 1870-1929 (London: Clarendon Press, 1953).

5. A. W. Coats, "The Historicist Reaction In Political Economy, 1870-1890," Economica, N. S., XXI (May, 1954) pp. 529-537.

6. Hutchison, op. cit., pp. 18-22.

7. Coats, op. cit., pp. 144-145.

8. Ibid., p. 145 fn.

9. William Scott, The Development of Economics (London: D. Appleton-Century, 1933), see especially, pp. 510-519.

10. Robert B. Ekelund, Jr., "A British Rejection of Economic Orthodoxy," Southwestern Social Science Quarterly (September, 1966), pp. 172-180.

11. Ekelund, op. cit., p. 174.

12. Ibid., pp. 177, 174.
13. L. H. Haney, History of Economic Thought, 4th edition (New York: Macmillan, 1949), pp. 523-536.
14. Eric Roll, History of Economic Thought, 3rd edition (Englewood Cliffs, N. J.: Prentice-Hall, 1964), pp. 311-318.
15. Schumpeter, op. cit., pp. 539, 822-823.
16. Haney, op. cit., p. 531.
17. Walter Bagehot, Economic Studies (Stanford, California: Academic Reprints, 1953), p. 87.

CHAPTER II

THE FORMAL BEGINNINGS OF BRITISH HISTORICISM:

THE REVEREND RICHARD JONES

Richard Jones (1790-1855) was of Welsh extraction, the son of a prosperous British solicitor who originally had planned for him to follow in the family profession. As fate would have it, however, Jones' poor health thwarted his father's ambitions, and he was, instead, sent to Caius College, Cambridge to pursue a less strenuous course of study leading to the ministry.¹ It was during his college years that Jones' mind took on the mold which it would retain during the remainder of his life, being formed in discussions held with a small and closely knit group of fellow students. The Cambridge Study Group, as they were later known, were mostly acolytes of Francis Bacon, and had joined together with the primary intention of studying and debating his philosophic works.² From their number would arise some of the greater minds of the following decades: John Herschel, the author of the influential Discourse on the Study of Natural Philosophy; John Babbage, father of the modern computer and founder of the British Society for The Advancement of Mathematics; and William Whewell, Jones' lifelong friend and the author of such definitive studies as A History of the Inductive Sciences and The Philosophy of the Inductive Sciences. (See Appendix D at the end of this dissertation for a more complete account of Whewell's life and scientific contributions.)³

While Jones' own scholastic career was not so distinguished as

those of his college acquaintances' it was far from uneventful. In 1833 he was elected to the Chair of Political Economy at Kings' College⁴ and in 1835 he was appointed as Professor of Political Economy and History at East India College, Haileyburg (thus filling the position left vacant by the death of his acquaintance and correspondent, T. R. Malthus).⁵ Although Jones was frequently drawn away from academic duties and economic research by the tide of public affairs, Haileyburg remained as a refuge for him throughout the remaining twenty years of his life. In the brief periods between his multitudinous political crusades he would return to his cherished position at Haileyburg to partake of those activities which brought him the highest enjoyment and self-satisfaction: his continuing studies into economic anthropology and into the "Political Economy of Nations".⁶

In 1836, after only one year of exclusively academic pursuits, Jones added to his other positions membership on the newly created Parliamentary Tithe Commission. Although he believed that this appointment was a service to the clergy, and thus a duty owed his office, he found his time increasingly absorbed in the details of everyday decision-making and increasingly diverted from the systematic pursuit of his intellectual goals.⁷ When the Tithe Commission was reorganized in 1851, Jones at first believed that he would be allowed to return to his studies and lectures, but the House of Lords, at the instigation of the clerical faction, reappointed him to serve as Secretary of the Capitular Commission and later as Charity Commissioner for England and Wales. So it remained until his death in 1855: Jones' duties in defense of the prerogatives of the Church

continually overwhelmed his own interest in economic research.⁸

Despite the weight of his extra-academic obligations, however, Jones' merits as a scholar and as a teacher did not go unnoticed. He is said to have been acclaimed by both students and colleagues for his vast knowledge of nations and institutions, both of Europe and the Far East, and for a gentle and persuasive classroom manner. He was a founder of the London Statistical Society (later renamed the Royal Statistical Society)⁹ and was the author of several books and articles on the theory and application of political economy.

In 1831 Jones published An Essay on the Distribution of Wealth and on the Sources of Taxation, Part I: Rent which was intended as the first part of a three part work on rent, wages and profits. Although the completed work never saw the light of day, several articles and pamphlets, which may have been intended as fragments of future volumes, followed over the next twenty years. These included A Short Tract on Political Economy, Primitive Political Economy,¹⁰ and, in 1852, the book-length collection, Textbook of Lectures on the Political Economy of Nations. Despite J. S. Mill's references to Jones' work in his Principles, remarks which Whewell would rightly characterize as "very disparaging praise," and despite Mill's own eventual adoption of Jones' system for the classification of peasant land rents,¹¹ Jones' works received little favorable notice outside of his circles at Cambridge and Haileyburg. At his death, in 1855, Jones was a recognized force in clerical politics, but a virtual unknown in his chosen field of political economy.

The situation of Jones' academic reputation improved somewhat in

1859 when there appeared a posthumous collection of his previously published works and unpublished papers, edited and with a "Prefatory Note" by William Whewell. It was Whewell's "Note," of over thirty pages, which gave a definite form and coherent unity to Jones' fragmentary and often rambling presentations.¹² And it was probably due to the guidance and influence of this Note that Jones' writings slowly gained a modicum of fame among the members of the economics profession.

In the following pages I have not consistently distinguished the clarifications to be found in Whewell's Note from Jones' own contributions to meta-economics. This procedure seemed justified on the basis of Jones' long and intimate friendship with Whewell, and Whewell's enthusiastic endorsement of his economic methodology.¹³ It should be mentioned, however, that Jones and Whewell were not always in complete agreement.¹⁴ Because of this and because of Whewell's own role in the development of British Historicism, a brief appendix (Appendix D) has been added which deals with his economic and philosophic views.

Jones and the Historians

The reputation of Richard Jones as an original and important thinker has fluctuated wildly both over time and between authors. J. K. Ingram found Jones' works to be "akin to the labors of Cliffe-Leslie," the highest praise that he could bestow on a pre-Comtian author; and Marshall, writing in 1897, stated that Jones' influence "largely dominated the minds of those Englishmen who came to a serious study of economics after his work had been published by Dr. Whewell in

1859."¹⁵ Marshall had also confessed, within an earlier writing, that Jones "gave direction to a good deal of my subsequent thinking"; and he had alternatively criticized Jones' meta-economics, from the standpoint of logical rigor, while praising it as an example of the best procedures which could be followed in economic research.¹⁶

In the entry on Jones in Palgrave's Dictionary the reader is informed that "The role of Jones in political economy was like that of Bacon in physical science: to preach the importance of experience, and the danger of hasty generalization." And Jones, we are told, undoubtedly "...deserves to be regarded as the founder of the English Historical School."¹⁷ Yet, less than eleven years later, Marian Bowley, in her Nassau Senior and Classical Economics, would label Jones as "an isolated representative of the historical methods in the 'thirties',"¹⁸ and Schumpeter would add, in 1954, that Jones was "... no more than a forerunner ..." of British Historicism, that he could not be considered as a "... root-and-branch objector ..." ¹⁹ to the traditional methods of the Classics.

Most recent historians of economic thought have apparently concurred in Bowley's and Schumpeter's opinions. Jones has generally been omitted from textbook treatments of the period or classed with a heterogeneous group of "early objectors to Ricardo."²⁰ There have been, however, several exceptions to that rule, and over the years a small but informative literature has grown up in appreciation and appraisal of Jones' views.

Eric Roll in his History of Economic Thought (1938) and Henri Grossman in his JPE article, "The Evolutionist Revolt Against Classi-

cal Economics,"²¹ both devoted substantial space to a treatment of Jones. Unfortunately, both of these authors patterned their analyses of Jones upon Marx's earlier critiques of his writings in the Theories of Surplus Value (a work unavailable in English at the time of their publications).²² Neither had apparently expended the time necessary to reconsider original sources. Consequently, both Roll's and Grossman's treatment of Jones suffered from the same defect: as Marx had criticized all previous thinkers for incompletely comprehending his own system for analyzing the process of social evolution, so Roll and Grossman saw all thinkers as forerunners of Marx.

In Grossman's paper, for instance, Jones became an advocate of evolutionary economics, whose primary interest and efforts were directed toward the construction of a model explaining "the sequence through which every nation must pass, though at different tempos" (emphasis in original).²³ His property rights theory was, in Grossman's interpretation, a demonstration that "... different property relations correspond to different stages in the development of productive power,"²⁴ and his inductive methods were traceable to Sir James Steuart (who Jones, in fact, never mentioned),²⁵ rather than to Sir Francis Bacon, whom he idolized.

Roll's discussion of Jones' writings is much more restrictive than Grossman's. Although it touches on many of the same points it is more oriented toward the technical details of Jones' rent theory, rather than toward his methodology, strictly speaking. Roll's assessment is, therefore, of less interest for the purposes of this dissertation than other, more philosophical, critiques. While Roll did

realize that "Jones urged economists to pay greater attention to the historical differences between economic institutions ... (and) ... also stresses the relativity of economic laws,"²⁶ his subsequent discussion of these doctrines indicates that the emphasis in this sentence is meant to fall upon the word "historical," i.e., evolutionary, rather than upon the phrase "differences between economic institutions." The remainder of Roll's evaluation is little more than an attempt to (unjustly) interpret Jones as a proponent of Marxian theories of class conflict and capitalistic accumulation.

After years of complete obscurity, interest was again aroused in Jones' writings by the centenary anniversary of the Royal Statistical Society (which he had helped to found). In celebrations of the Centenary a paper by L. G. Johnson, concerning Jones' achievements, was circulated to select members of the Society. In his still unpublished contribution to the literature on Jones, Johnson suggested that his (Jones') proper claim to "economic fame" was that "he was a founder of the English Historical School."²⁷ This suggestion was adopted and built upon by R. Glendy in a note appearing in the 1956 Journal of the Royal Statistical Society. In addition to offering valuable and insightful observations on Jones' academic career and the character of his economic research, Glendy wrote that he "... was not simply a 'forerunner' (of the British Historical School)--as has been so frequently alleged--but (was) one of the progenitors of the inductive approach to economic problems in the Nineteenth Century."²⁸ The issue of induction vs. deduction, which had confused many previous historians, was further clarified in Glendy's note. He

rightly interpreted the "deductive" position as one of a prioristic certainty and the "inductive" position as one implying a due regard for "the facts." As he stated the matter: "Jones and his supporters in the inductive school found themselves opposed by those--the great majority in those days--who believed with Whateley, Drummond, professor of political economy at Oxford, that the 'principles of action are known by consciousness and do not require detailed observation.'"²⁹

While Glendy's note may rightly be faulted for its excessive brevity it has added more to our understanding of Jones than have longer appeals to pseudo-sophistication and professional snobbery, of which one prime example is William L. Miller's "Richard Jones: A Case Study In Methodology."³⁰ The central theme of Miller's discussion is a defense of his own, oddly interpreted, version of Ricardian rent theory³¹ against what purports to be an accurate summary of Jones' theory of peasant rents. Miller, of course, ignores Jones' major claims concerning the opposing paradigm: that there either was no empirical theory in Ricardo's writings (only an a prioristic theory describing situations which were inapplicable to the existing world), or that Ricardo's speculations were, at best, a special case of his own, more comprehensive, view of rental returns as a function of the institutional framework. Yet he (Miller) obviously believes that he has decisively refuted Jones' "inductivism" by invoking a vulgar form of Friedman's methodology of the "as if."³²

An Aside on the Vulgar Interpretation of Friedman's Methodology

Although Friedman's paper concerning "The Methodology of Positive Economics" is dealt with more fully in Chapter 9 of this dissertation, a few summary considerations may be of aid in assessing Miller's arguments. If, as Miller says, "there can only be one use of 'induction', in scientific analysis, that of testing hypotheses"³³ then existing facts about the world would have nothing to do with the content of scientific hypotheses. That is, in Miller's view, the theories of economics need assert nothing about conditions prevailing in the world, nor can they ever refer to any observable conditions. If this is the case, however, how is it that economic theories can have "testable consequences"? The alternative to Miller's methodological rationalism is obvious. If scientific theories do refer to the existing world then they must specify accurately (if incompletely) the particular situations (or types of situations) to which they do or do not apply. That is, they must be based on "inductions," in Jones' sense of the term, for it is only through "inductions" that we can decide whether the conditions for the application of a theory are or are not present. The simple-minded rejection of "induction" by amateur economic methodologists thus leads to either an accompanying rejection of that which they also wished to retain, i.e., the testable consequences of their theories, or to the very position which they are attempting to refute.

Other Sources Dealing With Jones' Methodology

By far the best appraisal of Jones' work is to be found in an article, yet unpublished: "Richard Jones, William Whewell and Induction In Political Economy" by Professor Salim Rashid of Dartmouth College.³⁴ Professor Rashid does an admirable job of presenting his readers with a compact picture of the "intellectual milieu"³⁵ in which Jones lived and wrote, and in illustrating Jones' empirical spirit and impatience with the speculative approach of abstract Ricardianism. Rashid's familiarity with the journals and opinions of Jones' day is unmatched for its breadth and thoroughness.³⁶ Yet it must be admitted that the degree of philosophic sophistication displayed in his paper leaves something to be desired.³⁷ Rashid's article has been frequently referred to in the following discussion for its perceptive analysis of the Jones-Whewell attack on the "abstractness" or "universality" of Classical theory, their objections to the Classics' methods for constructing and using economic terms, and their critique of the "doctrine of tendencies." Yet Rashid's examination of more central concerns in Jones' writings, regarding the uses of deduction and hypothesis, are not so original as he apparently believed. Many of these same points were previously raised and examined in a 1973 paper by Mardis and Sturges.³⁸

There are other more serious defects with Rashid's paper than occasional lapses into redundancy, however. He, for instance, oversteps the available evidence in claiming that "Jones certainly did not espouse the naive belief that facts could arrange themselves in theoretical patterns if only one collected enough of them,"³⁹ Jones,

unfortunately, did believe in exactly that doctrine, and his differences with Whewell, which Rashid himself acknowledged, were over that very issue.⁴⁰ Rashid must also be taken to task for failing to distinguish between the generic and spatial-temporal universality of theories,⁴¹ and for failing to recognize that Jones was an early precursor of the theory of anthropological types as well as a "social economist."⁴² Overall, however, it is difficult to rate Rashid's contributions to the literature concerning Jones as anything less than a brilliant study. When complemented by the supplementary materials to be found in these pages it probably is rightfully considered as a definitive evaluation of Jones' economic works.

Plan and Purpose of this Chapter

One of the striking features of the British Historical movement is the mixture of scientific and "metaphysical" suggestions which were proffered by Historical economists for the improvement of their subject. These heterodox rebels would often, justly and devastatingly, critique Orthodox economists for their anti-empirical and self-justifying procedures, while, at the same time, advocating goals and procedures as unattainable or unoperationable as anything conceived by the "Orthodox School". Jones, as "the recognized founder" of British Historical economics, was as much at fault in this regard as any of his successors. Yet he also had much of worth to offer to the field of meta-economics. In order to distinguish the good and the ill in Jones' writings this chapter has been divided into three parts. The first deals with topics regarding which Jones' and Whewell's

advice would have been well taken by the economists of their day, the second with unclear or mistaken procedures or goals advanced by Jones and the third with an overall evaluation of his work. The purpose of this chapter is, thus, to identify many of those questions in which Historical economists would have a persistent interest in the form in which they first arose, to categorize these various concerns as fruitful or unfruitful, and, finally, to remedy the defects and omissions in the past accounts of Jones' methodological views.

The Contributions of Richard Jones

Jones' emphasis upon the description and investigation of economic institutions and social relations which actually existed in various parts of the world acted as a healthy antidote to the purely a prioristic speculations of most Orthodox economists.⁴³ It was this emphasis which led him to criticize numerous points in the Classical system (some still present in an altered form in Neoclassical views) and to offer positive suggestions for the improvement of economic methods.

Jones specifically attacked Orthodox economists for playing ideological word games, in which economic terms were used in a technical and highly restrictive sense during the construction of a theoretical system, only to be used in a quite different sense when policy claims were advanced on the basis of the theory.⁴⁴ Jones believed that the terms used in economic discussions should be flexible enough to accommodate (or refer to) new or different situations which might be encountered in the course of empirical research.⁴⁵ Yet he remained

adamant in his opposition to the use of terms which could not be tied, directly or indirectly, to observable phenomena. Thus the term "rent" was better restricted to actual payments made by tenants to their landlords, rather than referring to some component of this payment (i.e., Ricardian fertility rent) which could never be empirically distinguished from the rest.⁴⁶

There is, however, one rather murky aspect of Jones' discussion concerning the "proper" use of economic terms and their "proper" definition. That is, he clearly believed that there was no connotative sense which could properly be given to any economic term prior to an empirical study of the subject area (or problem) to which it was intended to apply. As he himself stated this position: "...where syllogistic reasoning is out of the question, and we are traveling towards and not from general conclusions, words are to be used to indicate, not to limit our subject, and, of course, are not meant to be used as the foundation of the general propositions we are searching for ..."⁴⁷ This was, almost certainly an expression of Jones' "inductive view" in the mistaken, Baconian, sense of that term. It indicates a view even more extreme than that adopted by German Historicism, that it is a mistake to have any prior conceptions, or hypotheses, concerning the subject of one's investigations (i.e., it indicates a rather absolute belief in the neutrality of the scientific observer vis-a-vis "the facts", and a total disregard for the necessity of formulating the problem of an inquiry in a clear and answerable fashion).⁴⁸

To be more generous to Jones, his suggestion might be alter-

natively interpreted as an assertion that some definitions will eventually prove more fruitful than others in organizing the concepts which we are using to describe and predict social phenomena. That interpretation is, however, incomplete, or too fully generous, when viewed against the backdrop of Jones' philosophic underpinnings and methodological writings.⁴⁹

The Restrictiveness of the Classics' Postulates

Tying in closely with Jones' discussion of the a prioristic character of Classical terminology was his criticism of the Classics' overly-restrictive theoretical "postulates." In extreme moments Jones had declared that the Ricardian system was useless for any explanation of, or predictions about, the world, for it assumed a fairy-land of perfectly mobile capital, homogeneous labor and unfettered free markets.⁵⁰ In more generous moods, however, Jones was forced to concede that the Orthodox system of political economy was, at least, somewhat applicable to "the peculiar form and structure of society existing in Great Britain."⁵¹ This latter suggestion foreshadowed, of course, the precise pattern of attack upon the Classics' postulates, and the same admission of a singular exception, which Walter Bagehot would popularize in his Fortnightly Review article of 1876. While we have no evidence to tie Bagehot's speculations to the influence of Jones, and, in fact, no evidence to suggest that Bagehot was even aware of Jones' writings, the similarities between the meta-economic views of these two authors are sometimes striking.⁵²

Further Errors of the Orthodox School and Suggested Alternatives

In his "Prefatory Note" to the Literary Remains of Richard Jones William Whewell extended Jones' criticisms of Orthodox economics to the frequently abused notions of an economic "tendency."⁵³ He noted the obscure and ambiguous ways in which the Classics had used this term in their apologias in defense of Orthodox methods, and he also suggested conditions under which the term could be properly applied. As Whewell noted: to state that there is a tendency for some type of event to occur, without qualification, is to commit the absolutist error of leaving unspecified those initial conditions on which any hypothetical prediction must necessarily rest. The assertion of a single tendency may also ignore other possible forces which could diminish or "swamp" the impact of the first. In examining the Ricardian theory of differential rents Whewell commented as follows:

The doctrine of a universal tendency in the social world to reduce rents to the form of the Ricardian definition, we may perhaps be allowed to illustrate by saying that it is, as if a mathematical speculator concerning the physical world should teach, as an important proposition, that all things tend to assume a form determined by the force of gravity ... To which the reply would be, that these tendencies are counteracted by opposite tendencies of the same order, and thus have only a small share in shaping the earth's surface ... and the doctrine that the earth's surface tends to a level, is of small value and limited use in physical geography. (emphasis in original)⁵⁴

Jones and Whewell did not merely criticize the narrowness of a Classical theory confined to those highly restrictive cases where the Classical's postulates were approximately true, however. They also suggested procedures for building a more general economics. Since it

seemed obvious that differences in the degree of combination of "moral or physical temperament ... climate, soil, religion, education and government" could have a bearing on the construction of empirically justifiable economic theories and on the accuracy of the predictions yielded by these theories,⁵⁵ Jones recommended extensive observation of the particular class of phenomena to which any given theory was intended to apply. Although he expected little regularity in the behavior displayed by singular individuals, even when these individuals lived "under similar conditions," he did believe that the behavior of "bodies of men" was predictable so long as the various groups being compared had "similar backgrounds" and were in "similar situations."⁵⁶

For Jones, then, a universal economics, or the set of different economic theories describing different types of societies, could only rest upon an economic anthropology (or a study of "economic types"). This economic anthropology would, in turn, be responsible for providing a schema of the major categories of social-economic systems the institutions commonly associated with each of these categories and the relevant behavioral constraints imposed by each of the respective sets of institutions. At one point in his investigations Jones considered the possibility that the racial traits (or "national traits," in the old sense of that term) of populations were as important in determining their economic behavior as the institutional and customary constraints which were dominant in these various societies. To this doctrine which is surely historicist in the sense in which Popper uses that term, he responded that: "I will not venture to say that there is nothing in this, though I believe there is very

little."⁵⁷

Jones' own theoretical approach to the problems of economic methodology was quite different than that of either the a prioristic economists or those who professed a belief in an historical fatalism. Instead of dealing with one type of wage payment or one type of rent he distinguished three categories in wage payments to laborers and four categories in payments for the use of land.⁵⁸ His theory of national development was constructed around the particular institutional structure which corresponded to the different paradigms of social organization and the different ways in which income was distributed in each. He always sought to uniquely determine the expected rate and direction of national growth and development as a function of this multi-dimensional system for the analysis of social institutions.⁵⁹

Although Jones was vitally interested in the distribution of wealth between the different functional classes in society his approach to economics remained in the aggregative-developmental tradition of Ricardo and Smith. Authors such as T.E.C. Leslie would later consider the question of how changes in the economic environment affected the acting individual, and how peculiarities in the institutional structure of individual countries affected the details of the composition and structure of enterprises within those countries. But Jones consistently dealt with "the mass" and the process of national growth and national development.

"Facts" and the Construction of Economic Theories

The program which Jones had outlined for the economic community--that of examining all "important" aspects of an economy before formulating theories about it--was certain to require a massive expenditure of time and effort, even in those few instances where information was readily accessible. Yet Jones consistently insisted that there was no other alternative open to the future advancement of economic research.

The attempt of past economists to discover "The principles which determine the position and progress and govern the conduct of large bodies of the human race, placed under different circumstances ... (from a) ... mere effort of consciousness, by consulting, [their] own views, feelings and motives, and the narrow sphere of his observations and reasoning a priori ..." would be absurd.⁶⁰ No truly "general principles" could possibly be constructed except from a "comprehensive view of facts." And any attempt to short-cut that procedure would result in "general principles which will be found to have no generality" and which would then have to be supported through numerous ad hoc hypotheses.⁶¹

The False Paths Within Jones' Meta-Economic Views

Introduction

While Jones and Whewell were responsible for contributing many valuable insights to the budding tradition of British Historicism,

they must also share at least a portion of the responsibility, along with the Comtists, the Social Darwinists and sundry melioristic reformers, for the more unproductive turns taken by the movement. A consideration of those aspects of their writings which were adopted by many authors in the Historical tradition, but which led, ultimately, to wasted effort and discarded pathways, is therefore in order.

The most popular and prevalent of the several errors propagated by Jones and Whewell was the notion that economic investigations should properly adopt the Baconian version of empiricism, with its accompanying stress upon "induction." Jones' investigations into scientific matters were clearly inspired by his early contact with Bacon's Novum Organon, and he continued to pay an almost religious devotion to the Baconian view of scientific method throughout the remainder of his life. As Whewell commented in his Prefatory Note to Jones' Literary Remains:

Having noticed the inductive nature of Mr. Jones' social and political philosophy as its special and distinctive character, perhaps I may be allowed to say that the disposition to take such a course in his speculations belonged to him from an early period. It existed at the time of his Cambridge undergraduateship, and was nourished by the sympathy of some of the companions of his college days. The Novum Organon was one of their favorite subjects of discussion.⁶²

Whewell, who was, himself, a companion of Jones' college days, and a participant in the frequent discussions held concerning "the father of induction," would later write that the method of Bacon, "that general process of induction," was the means "by which the most substantial truths which man possesses (except only mathematical truths)

have been obtained."⁶³ Yet Jones would come to view certain passages in Whewell's Philosophy of the Inductive Sciences as dogmatically unsound, and their relations over many methodological issues outside of economics were, for a time, somewhat strained.⁶⁴

The division which momentarily threatened to tear asunder the budding inductivist movement in British economics was not, however, especially surprising. The issues involved in an "inductivist" position are difficult, even when the term "induction" is clearly limited to one well-defined meaning. They become substantially more numerous and more nearly insoluble when that term is used indiscriminately in several different senses; and that, unfortunately, was Jones' standard practice.

Perhaps the primary way in which Jones and his Cambridge fellows used the term "induction" was simply to indicate their desire for an increased accumulation of basic facts (i.e., a set of accepted observation statements which could be used in the formulation and testing of scientific hypotheses). While there is much merit to this enterprise, if it is meant to supplement and correct the construction of a body of scientific theories, it can, and has, been carried to extremes. The idea that conjectures about the connections between observed phenomena should wait until "the facts" are "complete" is one example of the absurdities to which an improperly interpreted inductivist program can lead the unwary, and it is an example which has a substantial degree of application to the meta-economics of Richard Jones. Although rightly anxious that "we determine to know as much as we can of the world as it has been, and of the world as it

is, before we lay down general laws,"⁶⁵ Jones seems to have determined that the acquisition of this requisite amount of knowledge would require many decades, if not many generations:

the history of other branches of knowledge teaches ... both the necessity and the rewards of patience and continuous labor, when great and wide truths are to be approached. In astronomy, the most perfect of the sciences, predictions ... are assisted by observations which are the results of the successive labor of many generations ... A philosophical union of humility and hopefulness will lead men to mistrust the importance ... of the results of their individual observations, and to rely ... for the discovery of general laws on the gradually increasing power of the united efforts of our race, extended through large intervals of time and space.⁶⁶

The actual situation, in fact, may be far different than the picture of scientific development which Jones painted. At least some philosophers of science today believe that the most fruitful speculations in many areas of the physical sciences have been those least connected with "established facts" or established paradigms of those various fields. As Popper has suggested, bold and daring speculations have the greatest potential for fostering new and fecund areas of scientific research precisely because they seem to be so readily susceptible to falsification. This is not to say that ordered research is not the predominant form of scientific activity, nor would one want to assert that it is not a very useful form of scientific inquiry. The significant advances in the "pure theory" of a science are, however, almost always the result of investigations which are directed along new and previously unimagined pathways.

In any case, Jones' successor, Walter Bagehot, decisively laid

to rest attempts to construct a full or complete history of economic events, noting that (1) the data on every past economic event was simply not available and (2) the data on all future economic events would be as difficult to compile as "a complete history of human conversation."⁶⁷ While it is at least questionable that Jones himself had ever believed otherwise, the impression that he endorsed an all-encompassing economic history continued to haunt many of the later accounts of his writings, and would eventually become an infectious source of methodological error in the period of the later British Historical School.

We have not quite exhausted the controversy over "induction", however, for "the inductive view" seems to have been sometimes interpreted by Jones as a belief that the uniquely correct hypothesis for describing and explaining the causal links which governed a given class of phenomena is derivable from an examination of "facts" about the phenomena.⁶⁸ This contention is to some extent "fore-shadowed" by a belief in the importance of facts to the formulation of correct hypotheses, but it is certainly not necessitated by that belief.

It is a simple matter, however, to refute an "inductivist" position which claims to infer general (or universal) laws from a collection of particular facts. It is quite clear that no finite number of particulars can imply a universal unless the universe of discourse is itself finite. This is merely another way of stating Popper's original and most fundamental assertion concerning scientific hypotheses, i.e., that an hypothesis can conceivably be falsified, but

can never be "confirmed" or proven true.⁶⁹

Social Evolution

The second major defect in Jones program for constructing a "Political Economy of Nations" was his inclusion of a "dynamic" theory of social evolution along with his static theory of property structures. To a certain extent, this feature of Jones' views was excusable, since Classical economics was itself a system for explaining the causes of and impediments to national economic development. Thus when Jones wrote of "social evolution" he frequently combined with it a consideration of those factors leading to "economic progress" or economic stagnation, factors which were primarily connected with matters of income distribution:

In entering on the subject of the Distribution of Wealth, we have opening before us some of the widest departments of political economy. It is the distribution of its wealth which determines always the social, and most often the political, relations of human society; and until we have analyzed it, we cannot understand their internal mechanism. This is obvious enough, if we regard nations only at one point of time, and seek to understand their actual condition. But the vital and lasting importance of our knowledge of the causes which determine that condition, becomes fully apparent only when we contemplate human societies as capable of progress and scrutinize the laws which govern their advance, stagnation, or decay.⁷⁰

Jones' version of evolutionism was thus, in the main, cyclical rather than linear. He viewed societies as institutional and cultural structures capable of health or decay, and was only tangentially concerned with the conception of an ever developing Weltgeist.⁷¹

One can also exempt Jones, in large part, from the methodological error most frequently associated with evolutionary philosophies--the claim that social events are historically unique. Although anxious that the institutional framework of economic action be specified in some detail,⁷² Jones was certain that there were economic and social regularities common to all people living under similar circumstances. It is unfortunate that the cyclic character of Jones' evolutionism and his rejection of historicism, in Popper's sense, were not more clearly discerned by the later British Historicists and by Alfred Marshall. Had Marshall and the later Historicists fully comprehended the limited character of Jones' science of social development, they might have been somewhat dissuaded from their own wholehearted endorsement of Continental evolutionary philosophies (e.g., Comtian and Hegelian social philosophies).

Unified Social Science

A final, and much less serious, error in Jones' methodological writings was his insistence upon the unified nature of social inquiry. From Jones' perspective it was simply inappropriate to engage in anything like an analysis of social phenomena, which isolated out certain of the factors influencing the decisions of groups or individuals, while impounding all other factors in ceteris paribus. He himself expressed this point in a lengthy passage contained in his Textbook of Lectures on the Political Economy of Nations:

It has been said with superfluous modesty ..., that ... changes in social organization, and the subjects

they lead us in sight of, are not the proper objects of economical science, which is wealth and wealth alone.

Economical science can never, however, be successfully pursued, if such subjects be wholly eschewed by its promoters. There is a close connection between the economical and social organization of nations and their powers of production ...

If we were even erroneously to admit, out of complaisance to some of those who have adopted a narrowed view of the province of political economy, that all which bears directly on the social structure, morals, and happiness of nations lies beyond that province, still we should not be turned for a moment from our own selected course of investigation. Beyond political economy, strictly so called, but still closely and indissolubly connected with the truths it taught, would then lie those applications of it by which alone it could be made to assist in unfolding the shifting political and social influences which accompany the march of nations from rudeness and feebleness to power and civilization. This application of the science would ever be, to the best order of minds, that which makes its results valuable, and the labor of approaching them tolerable.⁷³

The error of insisting upon a social science which is indissolubly unified is thus found to rest upon two principles, one sound and one faulty. This doctrine was, on the one hand, merely a reflection of Jones' desire for a testable, or "applied" social science, while, on the other hand, it was an extension of his excessive attachment to the Baconian "know-everything" view of science and his inability to conceive of a science which was "hypothetical" in the sense of Marshall's partial equilibrium analysis.

Jones Historical Impact: An Assessment

While Jones may have served as a reasonable antidote to the overly rationalistic outlook of the later Ricardians, his errors--

derived primarily from his early interest in Bacon--would too frequently influence the path taken by later British Historical writers. While his interest in inter-cultural applications and tests of economic theories undoubtedly inspired the excellent empirical studies of Leslie and the methodological polemics of Bagehot, his comments on induction, social evolution and the unified nature of social investigations would often be misinterpreted and misused as a justification for points of view which he never imagined.

In the grand synthesis of economics carried out by Marshall the less desirable elements of Jones' outlook were resurrected and again injected into the mainstream of economic thought. Unfortunately, the positive elements of his writings, represented by his exhortations to an increased emphasis upon the study of property structures, were submerged for an indefinite period to come. What was worthwhile in Jones' writings was thus either overlooked or discarded while that which was questionable or vague was elevated in importance.

Footnotes to Chapter II

1. William Whewell (ed.), The Literary Remains of the Late Rev. Richard Jones, Consisting of Lectures and Tracts on Political Economy (1859) (New York: Augustus Kelley, Publisher, 1964), p. xx (Hereafter cited as Literary Remains).
2. Ibid., pp. xix-xx.
3. For an account of the membership of the Cambridge Study Group see N. B. De Marchi and R. P. Sturges, "Malthus and Ricardo's Inductivist Critics: Four Letters to William Whewell," Economica, N. S., Vol. 40, 1973, p. 380, and Literary Remains, op. cit., pp. xx-xxi.
4. Literary Remains, op. cit., p. xxii.
5. Ibid., pp. xxv-xxvi.
6. Ibid., pp. xxxvii.
7. For a summary of Jones' work on the Tithes Commission see Literary Remains, op. cit., pp. xxix-xxxiv, and for Whewell's appraisal of the detrimental influence exercised by Jones' non-academic duties on his scholarly research see Ibid., p. xxxix.
8. Literary Remains, op. cit., pp. xxxv-xxxvi.
9. For an account of Jones' activities in the formation of the London Statistical Society, and his hopes that one day social science might become more statistical, see R. Glenday's "Richard Jones: A Reappraisal," Journal of the Royal Statistical Society, Series A, Vol. 18 (1953), pp. 192-193, and De Marchi and Sturges, op. cit., p. 571.
10. The dates of these intermediate works are c. 1844 (unpublished until 1859) and 1847, respectively.
11. Whewell, with some justice, referred to Mill's comments concerning Jones as "very disparaging praise," noting that "whether he means it so or not, (this) is the way in which people speak of books, when they want to deny their originality and philosophical value." (Contained in a letter from Whewell to Jones dated April 30, 1848, and reprinted in Issac Todhunter (ed.), William Whewell: An Account of His Writings with Selections from His Literary and Scientific Correspondence, Volume II (London: Macmillan, 1876), p. 345. Page 353 of the same volume contains further remarks from Whewell to Jones regarding Mill's ingratitude in not acknowledging Jones as the source of his system for the classification of peasant rents.

12. The lack of "literary symmetry" and the "confused arrangement" of Jones' writings was even noted by his usually uncritical friend, William Whewell, in his "Prefatory Note" to the Literary Remains, op. cit., p. xxxix.
13. For one example of Whewell's enthusiastic response to Jones' meta-economic views see the passage quoted in Professor Salim Rashid's unpublished paper, "Richard Jones, William Whewell and Induction In Political Economy" (Dartmouth College), 1975, p. 22.
14. For some details of, and rather extensive references to, the Jones-Whewell disagreements over the proper sense of "induction" see De Marchi and Sturges, op. cit., p. 381 fn.
15. J. K. Ingram, A History of Political Economy (New York: Augustus Kelley, 1967) p. 141. Marshall's remark is quoted in T. S. Hutchison's A Review of Economic Doctrines, 1870-1929 (Oxford: Clarendon Press, 1953), p. 66.
16. A. C. Pigou (ed.), Memorials of Alfred Marshall (New York: Augustus Kelley, 1956), p. 296.
17. Henry Higgs (ed.), Palgrave's Dictionary of Political Economy, Vol II (London: Macmillan, 1926), p. 490.
18. Quoted in Eric Roll A History of Economic Thought, Third Edition, (Englewood Cliffs, New Jersey: Prentice-Hall, 1964), p. 311.
19. Joseph Schumpeter, History of Economic Analysis (New York: Oxford University Press, 1954). See p. 539 for Schumpeter's evaluation of Jones as "not a root-and-branch objector" and pp. 544 and 822 for his evaluation of him as no more than a forerunner of British Historicism.
20. See, for instance, John Fred Bell's A History of Economic Thought, Second Edition (New York: The Ronald Press Company, 1967), pp. 345-346. Exceptions to the widespread misinterpretation of Jones' writings are L. H. Haney's History of Economic Thought, Fourth Edition (New York: Macmillan, 1949), pp. 525-527, and William A. Scott's The Development of Economics (New York: D. Appleton-Century, 1933), pp. 133-137. Scott's treatment is especially well-constructed, but is more concerned with the technical details of Jones' critique of Ricardian rent theory, than with his meta-economic doctrines.
21. See Roll, op. cit. The complete citation to Grossman's article is: Henri Grossman, "The Evolutionist Revolt Against Classical Economics," Journal of Political Economy, Vol. 51 (1943), Pt. 1, pp. 381-396; Pt. 2, pp. 506-522.

22. Marx's Theories of Surplus Value is now available in a complete, three volume, English translation. For his comments on Richard Jones see Karl Marx, Theories of Surplus Value, Volume III (Moscow: Progress Publishers, 1971), pp. 399-452.
23. Grossman, op. cit., p. 511.
24. Ibid., p. 512.
25. Grossman, op. cit., p. 387.
26. Roll, op. cit., p. 311.
27. Glenday, op. cit., p. 192. Rashid, op. cit., Note 2, criticizes Johnson for the "well-meaning effort ... to remove Jones from the list of forerunners of Historical Economics or Institutionalism and make him one of the founders of a Statistical Society ...". But it is apparent that Johnson's intent was to re-emphasize Jones' role as a founder of the British Historical School, rather than a mere forerunner. It is also apparent that Historical Economics and Institutional economics have little in common, at least if one is referring to the Historical Economics of Jones, Whewell, Bagehot, Symes, and Leslie, and finally, it should be noted that Jones' role in the formation of the London Statistical Society was very much in line with his beliefs in an Historical Economics.
28. Glenday, op. cit., p. 192.
29. Ibid.
30. William L. Miller, "Richard Jones: A Case Study in Methodology," History of Political Economy, Vol. 3, No. 1 (Spring, 1971), pp. 198-207.
31. For Miller's interpretation of Ricardo see Ibid., pp. 204-206.
32. Miller, op. cit., p. 201. As representative of "modern views" concerning induction Miller cites a 1958 volume by Hansen and Pierces and a collection of journal articles, mostly from the Nineteen 'Twenties.
33. Miller, op. cit., p. 206.
34. Rashid, op. cit., Note 13.
35. Rashid, op. cit., pp. 13-16.
36. Ibid., pp. 18-22.

37. It is notable that Rashid never directly examined the question of what is meant by "induction" in Jones' writings, although he did do an admirable job of fending off undeserved criticisms of Jones' methodological views (e.g., his comments on "abstraction" on p. 9 of his article). He also, however, committed occasional errors, as, for instance, his misinterpretation of Whewell's comment to Jones (p. 4 of his paper) which he mistakenly assumes was by Jones and favored the construction of hypotheses, when, in fact, it was by Whewell, and was intended as reprimand to Jones for his excessive "look-and-see" attitude.

38. N. B. Marchi and R. P. Sturges, op. cit., pp. 379-393, see especially p. 380 for their comments on the issue of the role of hypotheses in scientific investigations. Marchi and Sturges' paper is by far the best research and best constructed of the papers dealing with Jones, Whewell and their contemporaries. Unfortunately, it was written with one specific purpose in mind, to focus upon Malthus' correspondence with Whewell. The other issues it considers are handled as tangential to this purpose.

39. Rashid, op. cit., p. 3.

40. See Rashid, op. cit., p. 13 for mention of the feud, Todhunter, op. cit., p. 61 for an exchange between Jones and Whewell concerning it, and De Marchi and Sturges, op. cit., pp. 389-391 for further references and commentary upon it.

41. Rashid, op. cit., pp. 8-9.

42. Rashid, op. cit., p. 1.

43. For an examination of the methodology of the "Orthodox School" see the appendix "On Mill's Methodology" to the Introductory Chapter of this dissertation and Chapter 3, on J. E. Cairnes.

44. In illustration of this point Whewell notes, in his "Prefatory Note" to Jones' Literary Remains, that although Ricardo had redefined rent so as to refer to something quite different than the ordinary use of that term, "It is certain that he did not ... really confine his assertions concerning rent" to that (technical) sense of the term. Literary Remains, op. cit., pp. xii-xiii. This same issue is examined at greater length in Rashid, op. cit., p. 7.

45. Literary Remains, op. cit., pp. 598-599.

46. As Whewell clearly notes, in this regard:

...the object of Mr. Jones was to give an account of the laws by which rent, "in the ordinary sense of the word," is regulated. He tried to ascertain

the progress and consequences of "what is commonly called rent." And the reader might be left to decide for himself which subject of inquiry may be the better worth his notice,--the rents that are actually paid in every country, or the Ricardian rents, which are not those actually paid in any country. (Emphasis in original)

Literary Remains, pp. xii-xiii.

Jones himself stated in clarifying this same point that:

Suppose, for instance, rent were defined to be the payment made to the landlord for the original powers of the soil: the fact is, that when outlay is so mixed up with the land that it cannot be again moved, the return to that capital is influenced by the laws which govern rent and not those which govern profits; and to separate the payment made for such a spot of land into rent and profits is only perplexing the subject by a definition, not making it more easy.

Literary Remains, op. cit., p. 599.

There is, incidentally, no direct evidence that Jones consistently favored the adoption of the common usage of a term as its technical meaning within economics, despite Whewell's implicit assertion to the contrary. He always demanded, however, that if a term was used in two distinct senses that these senses must be kept clearly separate. Jones was so adamant in this stance that he even spoke disrespectfully of the writings of his deceased friend, T. R. Malthus, for committing this very error (Literary Remains, op. cit., p. 95.) (Malthus, incidentally, had foreseen this turn of events and wrote to Whewell, shortly before his death, that he believed that Jones was going too far in his empirical attitudes. See De Marchi and Sturges, op. cit., pp. 388 and 390.)

47. Literary Remains, p. 600.

48. Jones' views of "proper definition" were never perfectly clear-cut, but we can locate some key passages from his literary fragments:

I have been reproached with giving no regular definition of rent. The omission was not accidental. To begin, or indeed to end, an inquiry into the nature of any subject, a circumstance existing before us, by a definition, is to shew how little we know how to set about our task--how little of the

inductive spirit is within us ...

... when we wish to establish general facts or principles relating to things as they actually exist, if we begin by a definition, it is to suppose our task finished before it is begun; and as man's art can rarely exhaust the subtlety or guess at the extent of nature, therefore to end with a definition is seldom a much wiser attempt than to begin with one ...

Men have too often on this, as on many other subjects, instead of using definitions to assist their reasonings, treated them as the foundation of their conclusions; and there cannot be a greater mistake ...

It is obvious that, in inquiring into principles and laws relating to things as they exist in the world, words may be used to indicate the subject of the research but not to supersede them.

Literary Remains, op. cit., pp. 598, 599,600.

49. I am referring, of course, to Jones' lifelong attachment to the views of Sir Francis Bacon, and to the already mentioned feud with Whewell over this very perspective.

50. Jones' own analysis was based on an examination of the different categories of labor and capital which existed in various countries and the different institutional structures developed for the remuneration of these factors; see, for instance, Literary Remains, op. cit., pp. 12-14, 48-66, and 185-225 for Jones' examination of some of the possible categories.

51. In his "Prefatory Note" to Jones' Literary Remains, Whewell expands on this qualification, noting that: "... there can be no doubt in England, and in countries circumstanced like England, it [the Ricardian theory of rents] is a very happy and striking generalization of the conditions of the problem ..." (Literary Remains, op. cit., p. xiv.) And Jones himself expands on this same theme by introducing his Lectures with the remark that: "The general principles of Political Economy have hitherto been laid down by English writers with an especial and exclusive view to the peculiar form and structure of society existing in Great Britain ... I shall endeavor to avoid this error." (Literary Remains, p. 1. The original passage quoted in the text of this chapter is to be found on p. 338 of the Literary Remains.)

52. Compare with the passages cited in the last Note, Walter Bagehot's Economic Studies (Stanford: Academic Reprints, 1969), p. 19.

53. Counter to the use of the term "tendency" as it was employed by the defenders of Ricardo's theory of rents, Whewell replied that:

Those who ... cling to the Ricardian formulation respecting rent, while they allow the wide extent of the exceptions to its applicability pointed out by Mr. Jones, say sometimes that there is everywhere ... a tendency to conform to the formulas though this tendency may be overmastered by the peculiar circumstances of the various countries ... Now to this the reply is, that it is not the obstacles to the tendency which are the exceptional case, but the tendency itself. The tendency of rents to the formula (the excess of good soils over the bad) results entirely from the hypothesis of the accessibility of land to the farmer, and the mobility of the farmer's capital ... But this hypothesis ... is very rarely verified.

Literary Remains, op. cit., pp. xiv-xv.

54. Literary Remains, op. cit., pp. xiv-xvi.

55. Literary Remains, op. cit., p. 189. The methodological doctrine which recommends that all obvious features of a situation should be included in any theory describing or explaining the situation is called "verbal realism." There is no more sound foundation for believing in verbal realism than there is for believing in a priorism and intuitive certainty, although it was common among the British Historicists to endorse this position. The issue is discussed further in the concluding chapter of this dissertation.

56. Literary Remains, op. cit., pp. 178, 187-188.

57. Literary Remains, op. cit., p. 574. See also Remains, p. 410.

58. For a detailed presentation of Jones' system for classifying wage earners, see his "Lectures on Labor and Capital," Literary Remains, op. cit., pp. 4-20 and his "Textbook of Lectures," op. cit., pp. 414-418. Rental payments on land and terms of land tenancy are dealt with in his "Short Tract on Political Economy," Literary Remains, op. cit., pp. 197-219.

59. As Whewell expressed Jones' opinions concerning national development:

... the original structure of nations, their early history, customs, and habits determine the tenure of land, and the relation of the cultivator to the classes above him, (they

have a social impact) in a degree indefinitely greater than the mobility of capital and the consequent changes of tenure. Over a large portion of the earth's surface, and during a large portion of the history of every nation, the former causes do almost everything, the latter, almost nothing.

Literary Remains, op. cit., p. xvi.

60. Literary Remains, op. cit., pp. 188-189.
61. Ibid., pp. xxiv-ssv, 562.
62. Ibid., p. xix.
63. Ibid., p. xii.
64. Todhunter, op. cit., pp. 115-116.
65. Literary Remains, op. cit., p. 570.
66. Ibid., p. 180.
67. Walter Bagehot, Economic Studies, op. cit., pp. 16-17.
68. Literary Remains, op. cit., pp. 472, 556, 559.
69. Karl Popper, The Logic of Scientific Discovery (New York: Harper and Row, 1967). See pp. 278-279 for Popper's critique of Bacon's views.
70. Literary Remains, op. cit., pp. 74-75.
71. There is no real objection to a cyclical theory of social development as long as (1) the forces leading to social growth and social decay are clearly defined rather than being replaced by some variety of a biological analogy to aging, (2) "social growth" and "social decay" are, themselves, clearly defined, and (3) there is no assertion of historical uniqueness, i.e., no assertion that mere differences in temporal, spatial, racial or other singular differences between cases will significantly affect the applicability of the theory. The same cannot, however, be said of a linear theory because of the singular character of its predictions.
72. See Literary Remains, op. cit., pp. 346, 445, and previous notes to this chapter.
73. Ibid., pp. 405-406.

CHAPTER III

J. E. CAIRNES AND THE ESTABLISHMENT OF ORTHODOX METHODOLOGY

John Elliot Cairnes was undoubtedly the most vocal critic of the Historical movement in British economics and the staunchest defender of the "deductive" or a prioristic method of economic inquiry. Cairnes' role in the development of economic methodology was, however, quite different than he himself believed. Although Cairnes regarded himself as no more than a defender of the meta-economic tradition which had developed linearly and without essential modification from Adam Smith through John Stuart Mill, in fact, he was much too modest concerning his own originality. While his extensive references to past writers did demonstrate the continuity of an Orthodox tradition dating from the time of Adam Smith,¹ Cairnes' own methodological views were both more and less than a summary of this tradition. His observations concerning economic method were certainly more systematically developed than those of any previous Orthodox authors. Furthermore, they dealt with several doctrines not considered or only superficially considered within even the copious methodological writings of J. S. Mill. Cairnes' meta-economic writings also differed from those of earlier methodologists by being far less "impure" in their reliance upon a prioristic foundations. Cairnes relied hardly at all on empirical facts, but rested his case almost exclusively on the "intuitions" which he believed were common to all competent economists.

This section is intended to throw some light on the meta-economic thought of the Nineteenth Century, and thus upon the intellectual environment to which the Historical economists were reacting. In the process of examining Cairnes as the paradigm of Nineteenth Century orthodoxy after Mill, I have also attempted to highlight a few of his views which have maintained their popularity, albeit in somewhat altered form, to the present day. It is only through an understanding of the essential features of "the deductive view" (as embodied in the works of economists such as Senior, Mill, Whately² and Cairnes) that we can fully appreciate the merits of the British Historical economists both in their own age and within the methodological context of modern neoclassicism.

Previous Research into Cairnes' Methodology

J. E. Cairnes has received the attention of many historians for his Some Leading Principles of Political Economy Newly Expounded,³ sometimes characterized as the dying gasp of the Classical School, and for his participation in the wages-fund controversy, initiated by Mill's 1874 "recantation" in the Fortnightly Review.⁴ He was equally well-known among his contemporaries, however, for his popular political treatise, The Slave Power,⁵ and for his influential text on The Character and Logical Method of Political Economy.⁶

Although Joseph Schumpeter once referred to The Character and Logical Method of Political Economy as "... a landmark in the history of methodology,"⁷ the professional literature explicitly

concerned with Cairnes' methodological writings has been almost non-existent. In the two key papers dealing with Cairnes' relations with his fellow economists J. S. Mill and W. S. Jevons,⁸ only passing mention is made of his methodological views. And the treatment accorded him in many of the standard histories of economic thought does not even extend to an acknowledgement of his interest in meta-economic questions.

The only published source⁹ to attempt an evaluation of Cairnes' methodology is Ekelund and Olsen's "Comte, Mill and Cairnes: The Positivist-Empiricist Interlude in Late Classical Economics."¹⁰ There are, however, points of emphasis and completeness, even in this generally excellent study, which require addition and correction for our purposes in this dissertation. Ekelund and Olsen were primarily concerned with the Cairnes-Comtist debate over the scope of economic theory and the proper relation between economic inquiry and the other social sciences. While they consider issues such as the research procedures proposed by both Cairnes and the Comtists and the role of empirical evidence within these alternative methodologies, they never enter into these matters in much depth. In addition, the way in which Ekelund and Olsen interpret Cairnes' position on what were to them subsidiary issues often does not square with his acknowledged role as a standard-bearer of methodological orthodoxy. I have discussed the ambiguities embodied in some of Cairnes' meta-economic doctrines and the consequent difficulty in arriving at an interpretation of them in the appropriate sections below.

An even more comprehensive description of the Cairnes system of economic methodology, one which touches upon most of the central issues in his perspective, is to be found in Emilie Olsen's unpublished thesis on the Comte-Cairnes controversy.¹¹ Although Olsen has done an admirable job of summarizing Cairnes' meta-economic views, she tended to present his doctrines in much too terse a manner, without sufficient supplementary commentary. Many times she also seemed unaware of the full implications of Cairnes' methodology for the path taken by economists after the 1890's, and occasionally she did not seem to fully appreciate how certain positions of Cairnes' meta-economic views related to the remainder of his system. Although Olsen's thesis is an excellent reference on the Comte-Cairnes debate for those already familiar with the issues, it is much too complex for the uninitiated. Her preoccupation with Comte tended to obscure Cairnes' quite respectable roots in the tradition of British economics and made him appear as somewhat of an isolated crank.

Cairnes on the Goal of Science and its Taxonomy

Cairnes' discussion of economic methodology was, of course, grounded in his views regarding the nature of science in general. That topic serves as a necessary prolegomena to any of his more specific views.

Cairnes equated science, any science, with what today would be referred to as "pure science" or, perhaps, "pure theory." He continually reiterated his conviction: that scientific studies

should not be pursued for any immediate practical purposes, but solely for the abstract knowledge of "cause and effect" which was to be gained from them. The scientist, according to Cairnes, should pursue his subject only for the "intellectual satisfactions" arising from his investigations,¹² as opposed to the artisan, who should seek after immediate applications for his skills.

Practical by-products of scientific activity, although "accidental" to its true purposes, were not entirely neglected by Cairnes, however.¹³ Despite a high-minded adherence to the pursuit of "pure knowledge," he was no more above an appeal to the practical achievements of science, as a justification for that enterprise, than are clergymen above citing the social conduct of the religious as a merit of faith.¹⁴

Even though all sciences shared the common goal of "establishing" those cause and effect relationships which prevailed in their particular fields of study, they were differentiated by much more than just the character of their subject matter. Cairnes introduced (or systematized) three distinct categories to be used in the classification and division of the sciences.

First, the various sciences were separable according to the character of the phenomena with which they dealt. There were the sciences of physical objects, such as chemistry, mechanics and physics, the science of mental objects or thoughts (psychology) and the social sciences of politics, economics and sociology.¹⁵ The social sciences were distinct from both the physical and mental sciences in that their subject phenomena were neither physical

objects nor thought but, rather, the appraisal of physical objects. They dealt with, in Cairnes' own terms, "valued matter."¹⁶

A second means for distinguishing the sciences was found in the distinction between those disciplines in which induction was a central investigative tool and those which could proceed only through non-inductive techniques. In Cairnes' writings the term "induction" was usually used to refer to Mill's "rules of inductive inference" and the accompanying conditions for their application. He thus resolved the distinction between inductive and deductive studies into a distinction between those fields in which controlled experiments could be carried out and those fields which were barred from the use of experimental techniques.¹⁷ The non-physical sciences were, hence, almost entirely "deductive" or, at least, non-inductive.

The modern characterization of science as composed of hypothetical-deductive systems of conditional statements (of laws and theorems) would probably fit most closely into Cairnes' third distinction between "hypothetical" and "positive" sciences. Positive studies were those which were concerned solely with the discovery of generalized facts (or "empirical generalizations"). Although this was considered in Cairnes' time as a perfectly legitimate and fully autonomous branch of scientific inquiry, we would today recognize that it is no more than a part of the procedure for testing of present or future hypotheses (i.e., that part in which "important" or "significant" facts are isolated from those which are "insignificant"). Hypothetical studies, on the other hand, were

defined by Cairnes as those in which either the premises were "arbitrary," or the conclusions of the science were derived by deduction and were true only "on the hypothesis that the premises include all causes affecting the results."¹⁸

It is to Cairnes' credit that he recognized the distinction between positive and hypothetical "science" many years before it became popular in the discipline at large. Later controversies over the role of historical research in economic studies and over issues raised by the later English and German Historical Schools could have been more easily resolved had most economists been aware of the possibility of hypothetical (deductive) inquiry and the role of positive research within that framework. The arguments of the later German Historical School were, in fact, little more than a contention that "positive science," in Cairnes' sense, should comprise the whole of the economist's endeavors.

Science as a Study of Tendencies

In a discussion colosely related to his distinction between hypothetical and positive studies, Cairnes considered the nature of the results to be expected from any scientific investigation and the procedures to be followed in scientific research. In Cairnes' view, a science did not predict classes of events, but merely the tendency for an event of a particular class to occur.¹⁹ The fact that the laws of science were limited to the prediction of tendencies was itself a consequence of the procedures available to analyze human institutions and relationships. The multi-dimensional and "complex"

character of social phenomena led to the specialization of social inquiry into various subfields: i.e., politics, which dealt with governmental organization and power; sociology, which dealt with societal customs and such informal institutions as the family; economics, which dealt with the production and distribution of wealth; and ethics, which dealt with sanctions against certain forms of action. Each field of social science thus proceeded to abstract out its own aspect of study from complex reality and thus to dissolve the reality into its elemental components. Once complete, the separate analyses of the different components of human action could be synthesized into an overall explanation of the actions customarily observed in everyday life. Although this explanation could never become predictive in character, for the relative weightings to be attached to the different types of human motives could never be determined before the fact, it could, at least, eventually become complete.²⁰ (That is, it could become satisfying to the social scientist.)

Cairnes believed that any more direct or more unified approach to the analysis of social phenomena was doomed to failure, and he attacked the Comtists for proposing such a grossly unspecialized program of social research. The sheer difficulty of performing a satisfactory analysis on even the relatively simple components into which most social questions were divided precluded, for him, a frontal attack on the significantly more complex phenomena of which these components were the parts. Further, the duration of the educational program which a social scientist was required to undertake in order

to become competent in even a single field of social study effectively eliminated the Comtist dream of a unified science of society.²¹ As we shall see below, Cairnes' arguments for the hypothetical or incomplete character of economic investigations were not, however, purely "abstract" or "philosophic." They provided him with a useful tool to be used in the defense of orthodox economics against the attacks leveled by its historicist critics.

The Character and Classification of Economic Science

In terms of the foregoing classification schema, Cairnes considered economics as: (1) a social science whose object was neither strictly mental nor strictly physical, but, rather, a combination of both;²² (2) a science in which controlled experiments could not be performed, and thus, one in which induction would play no important role;²³ (3) a science which described only one aspect of human action, the economic, leaving the merits of conduct to ethics, the "rules of thought" to psychology and the religious motive to dogmatic theology;²⁴ and, finally, as (4) a study which was hypothetical, in the sense that its conclusions were derived by deduction and were, in the language of the economist, "true only in the absence of disturbing causes," yet also a science which was positive, in the sense that its premises were representative of the facts of the world rather than being the result of arbitrary choice.²⁵

Wertfrei Science and the Formulation of Public Policy

Cairnes was especially vehement in denying any valuative char-

acter to the pronouncements of economic science. In his influential essay on "Political Economy and Laissez-Faire," he attempted to undo the damage inflicted on the reputation of economics by its past association with the increasingly unpopular doctrines of laissez-faire, and to thus preserve its status as a respectable field of Wertfrei investigation.²⁶ Despite the fact that Cairnes' critique of normative economics was more thorough and exacting than similar discourses penned by his predecessors, he, however, like these predecessors, fell back into the role of political philosopher.²⁷

It was Schumpeter's opinion that Cairnes wished to reduce all of economics to "pure economics" or "pure theory,"²⁸ and, as we have seen, there is some justification for that opinion. Yet, Cairnes' complete view on the topic of the applied or normative significance of economic inquiry was not as simple or as consistent as it might at first appear. Ekelund and Olsen have noted that Cairnes believed that:

... the extinction of trade corporations, the abolition of usury laws, the more or less extensive adoption by the leading nations of Europe of the principle of free trade, English colonial policy, English financial, monetary, and poor-law reforms [were] achievements which it will scarcely be denied, may be fairly credited to Political Economy.²⁹

and it is well-known that Cairnes was not at all hesitant about taking stands on issues such as unionization and free-trade.³⁰ Passages in his "Political Economy and Laissez-Faire" seem even to contradict his primary stress on a value-free approach to economics, as he turns from a critique of the pro-laissez-faire

pronouncements of past economists to a critique of laissez-faire itself.³¹ If Cairnes had really wished to break with the long-standing tradition among economists of insinuating their own values into the conclusions of their theoretical analyses he surely would have abided by his own prohibitions against the application of a "purely hypothetical" theory to "concrete" cases. He would have been more cautious about the use of economic analysis as a justification for personally preferred public policies rather than invoking its authority in support of his own positions on virtually all major policy issues of his day. As it was, Cairnes gained both the animosity of the Historical economists, for his repeated appeal to an untestable theory, and the contempt of the man he most respected, J. S. Mill,³² for his often dogmatic defense of policy views.

The Boundaries and Subject Matter of Economic Inquiry

The problem of the appropriate boundaries for economic inquiry was much less perplexing to Cairnes than it was to generations of economists before or since his time. Whenever an "economic fact" could be causally (viz., "deductively") traced to either a "mental principle" or a "physical law" then the problem "so far as the science of wealth is concerned" was to be considered as closed.³³ The business of the economist was concisely summarized and tightly circumscribed in the following quote from Cairnes' Character and Logical Method of Political Economy:

It is for the economist to prove, first, that the premises (of his theories) are true in fact (we will discuss the method of the "proof" below;

and secondly, that they account for the phenomena; ... when this is done his business is ended. He does not attempt to explain the physical laws ... and no more does he undertake to analyze the nature of those feelings of self interest ... He regards them both as facts, not to be analyzed and explained, but to be ascertained and taken account of; not as the subject-matter, but as the basis of his reasonings. If further information be desired, recourse must be had to other sciences; the physical facts he hands over to the chemist or the physiologist; the mental to the psychological scholar. ³⁴

Perhaps the critical point to note in Cairnes' treatment of the scope of economic inquiry was his delineation of the boundaries of the various social sciences in terms of the type of phenomena with which they were concerned. Later authors have frequently chosen to ignore the distinction between "economic" and "non-economic" "motives" for action by defining economics as the social science which uses the model of constrained maximizing behavior (whether the variables being maximized are "economic" or "non-economic"). They have handled the problem of deciding which type of motives dominate in particular situations by making economics responsible only for the prediction of changes in the values of dependent variables rather than for the determination of the total values of these variables.³⁵ Cairnes, however, knew nothing of these distinctions. His naive acceptance of the traditional division between those motives which were properly the concern of the political economist and those about which the economist could say nothing was to lead him into further varieties of meta-economic error.

The doctrine of the "hypothetical" or "incomplete" character of

economic hypotheses combined with the restriction of economics to the consideration of "economic variables" inexorably led to Cairnes' reinterpretation of economics as a tautological system. If human actions were seldom the result of unmixed motives, but economists could only be concerned with those motives which flowed from the desire for wealth, then it followed that the science could never be anything more than a study of tendencies. The hypotheses of a science of tendencies can, by definition, never be refuted by "facts" based upon any observable events, however. Any "fact" contradicting explanations deduced from proffered economic hypotheses can always be explained as an instance where "non-economic motives" dominated (or overwhelmed) the proffered "economic motives." Facts could help to "confirm" theories, but they could never really contradict the hypotheses of a "science of economic tendencies." (The contradiction in this last statement is apparent, but it was never really recognized by Orthodox methodologists of Classical Economics.)

Intuition, Experimentation and the Role of Social Facts

Cairnes was very much in the mainstream of Nineteenth Century thought when he endorsed intuition and introspection as methods appropriate to the social sciences. Like Marshall, the later Symes and Ingram he readily accepted the notion that social scientists had open to them a special class of data, composed of mental impressions, which were denied to the physical scientist in his investigations. The type of "mental facts" upon which social inquiry was properly based was derived from peoples' secret worlds of thought and moti-

vation. Only the individual could know what he was really thinking or what he was really feeling, although he could report both his thoughts and his emotions to others.³⁶

While the procedure of introspection was widely endorsed in Nineteenth Century social science, Cairnes was definitely in a minority in his expressed willingness to exclude any other types of investigative methods within economics. Although he sometimes hedged concerning this "extremist" stance, it clearly occurs in several places in his methodological writings, the following comprising one exceptionally clear example:

For what purpose is hypothesis used in physical research? Always as a means of arriving at ultimate causes and laws. Such causes and laws not being susceptible of direct proof, through an appeal to the consciousness or senses, ... the physicist frames an hypothesis as to the nature of ... the causes and laws, and having done so, proceeds to bring together conditions fitted to test the correctness of his guesses ... Such a course would be obviously unsuitable in the analogous case in economic investigation. No one thinks of framing an hypothesis as to the motives which induce men to engage in industry, to prefer remuneration to unremuneration... Conjectures here would be manifestly out of place, inasmuch as we possess in our consciousness and in the testimony of our senses ... direct and easy proof of that which we desire to know.³⁷

Controlled experiments, which were and are essential to investigations in the physical sciences, had been denied a role in the social sciences by J. S. Mill; and Mill's authority, for Cairnes, precluded any further consideration of this issue.³⁸ Although Cairnes believed that the procedures of controlled experimentation were "powerful instruments" as opposed to the "inferior substitutes"

available to the social scientist,³⁹ he expressed much less regard for the virtues of statistical tools when applied to the phenomena of human action. Any attempt to determine the existing social conditions in the world he considered to be futile since "the economist starts with a knowledge of ultimate causes." That is, we, as economists, possess " ... direct knowledge of these causes (of human action) in our minds, and in the information which our senses convey... "⁴⁰ It is further clear, from Cairnes' subsequent remarks, that " ... the information which our senses convey ... " was a reference not to " ... those refined inductive processes by which the ultimate truths of physical science are established ..." but rather to "... the direct proof of our senses" (emphasis added). That is, it constituted an "anticipation" of what Marshall would later describe as "casual observation."⁴¹

Ultimately, however, Cairnes did waver in some slight degree concerning the usefulness of observational methods and other non-introspective techniques. As already noted he had maintained that it was important for the premises of economic theory to be based on "... the existing facts of nature," although offering nothing approximating formal observation rules for determining how such "facts" were to be arrived at. Cairnes also admitted that "... observation and experience ..." could "... furnish sufficient corroboration to the processes of deductive reasoning to justify a high degree of confidence in the conclusions thus obtained ..." and that empirical tools could be useful in isolating "... disturbing causes ..." (and thus in furthering the increased "perfection"

or "completeness" of economic hypotheses).⁴² None of this, of course, had any direct bearing on the possibility of testing economic theories with a view to their possible falsification.⁴³

Statistical Evidence and the "Validity" of Economic Theories

We have already observed that Cairnes' general attitude toward the use of statistics in economics was one of neglect, if not of outright hostility. Yet Ekelund and Olsen have noted that Cairnes was not above the use of statistical data in support of his own analyses. In an 1877 pamphlet entitled "The Gold Question"⁴⁴ he quoted extensively from the available data sources. Cairnes' true views concerning empirical techniques are further obscured by the fact that W. S. Jevons, the popularizer of statistical studies in British economics, "always had a high regard for Cairnes' work and capabilities" and had used some of Cairnes' writings on empirical subjects to support the conclusion of his own research.⁴⁵

The key to these seeming paradoxes, I believe, must lie in Cairnes' psychological attitudes toward the essential nature of economic theory. The "Laws" of Classical economics were, for Cairnes, the object of an almost religious veneration. Had he been more familiar with Kantian philosophy Cairnes might even have stated his methodological position in a form similar to that adopted by Ludwig von Mises⁴⁶ many years later, i.e., "the basic propositions of economic science are expressive of fundamental categories of human thought." Although both Cairnes and Mises would admit that empirical evidence could be used to bolster psychological assurance

in an economic relationship, and that such evidence might be psychologically suggestive of embellishments to be added to the basic theoretic structure, neither would assent to the notion of falsifying economic theories by reference to such evidence. As Cairnes stated in his major methodological work:

From this conception of an economic law, as expressing a hypothetical, not a positive, truth ... we can have no difficulty in perceiving the kind of proof on which such a law rests, and the kind of arguments ... by which alone, if questioned it can be refuted.

Not being an assertion respecting the order of economic phenomena, it can neither be established nor refuted by an appeal to the records of such phenomena--that is to say, by statistical or documentary evidence ...

We also discover, at another point in Cairnes' writings, that he means by the term "fact," in this context, not an observation carried out according to some well defined observation procedure, but rather "some mental or physical law."⁴⁷

The tendency to transform social investigation into a secular faith was, unfortunately, very prevalent in the intellectual traditions of the Nineteenth Century. It was a spirit which captured and inspired such diverse thinkers as Comte, Marx and Cairnes, and which eventually provided the tone of Marshall's reconstruction of economic analysis. We will see in the closing sections of this dissertation how the attitude of worshipful devotion to the theoretical structure existing in a particular field of social science has been preserved, even today, in the meta-economic writings of several prominent economists.

Mathematics and Economics

A rather unfortunate aspect of Cairnes' methodological views was his distinct hostility toward the use of mathematics in economic problem solving. Although Cairnes had read and reviewed Jevons' Theory of Political Economy,⁴⁸ which contained a lucid statement of the adaptability of mathematical techniques to ordinal rankings,⁴⁹ he persisted in rejecting mathematical economics on the grounds that economic relationships were qualitative rather than quantitative.⁵⁰ He later weakened this original position somewhat but still opposed the extensive use of mathematics in economics because it added nothing to the subject not already known and was a mode of stating economic theorems which was unfamiliar to many, otherwise competent, thinkers. Cairnes' examples in demonstration of the inappropriateness of a mathematized economics do, it is true, add some superficial plausibility to his case against an overuse of mathematics in those areas of economics which are not yet well developed theoretically, i.e., in those areas where basic problems have not yet been well established. Yet in many instances Cairnes seems to have confused the issue of empirical vs. theoretical economics with the issue of mathematical vs. verbal economics.⁵¹

Relativism and the Influence of Popular Culture

A final point of some interest in Cairnes' meta-economic discussions is found in his views concerning the issue since described as "relativism vs. absolutism," a long-debated question in the

history of economic thought. The debate, summarized and simplified, is concerned with the determinants for the particular path of development taken by economic theory, the factors causing certain specialities within the discipline to flourish while others wither, and certain theoretical systems (or "paradigms") to prosper while others are ignored.

The basic relativist position is that the particular historical course followed in the development of economics has been a consequence of the history of those social problems for which economists were expected to provide solutions. Thus the primitive systems of development economics, which constituted Mercantile and classical theories, arose as a result of a demand by Western European nations for programs which would hasten the rate of their commercial and industrial growth. Keynesian macro-economics was called into being by the economic upheaval of the Great Depression, and the study of large scale production with elements of high fixed costs was a product of the early programs to regulate the railways "in the public interest."

A more extreme and logically unrelated form of relativism claims that both questions that economists pose for analysis and the responses they offer to these questions have been pre-determined by the social milieu. Although this position has been most popular among the less sophisticated Marxists, it has also found a home in the writings of less doctrinaire authors such as Leo Rogin.⁵²

The basic position of absolutism, as presented by George Stigler in his 1960 "The Influence of Events and Policies on Economic

Theory,"⁵³ is that the path along which economics has progressed has been determined by the inner "dialectic" of the theory, functioning according to "internal values and pressures of the discipline." This theme was further refined and modified in the later contributions of Spengler, Eagly and Fetter.⁵⁴ Eagly has noted that the development of economics has become more self-directed as the discipline has itself become more professionalized and insulated (or isolated) from the thoughts and goals of those outside of the academy. Spengler introduced into "absolutism" a distinction between the "core" of "pure theory" in economics, which he believed had developed according to the absolutist's conception of intellectual progress, and the "shell" of "economic doctrines" (i.e., matters connected with policy or issues of "applied economics") which changed in response to influences exogenous to the discipline. Finally, Fetter noted that "The more closely one associates economic thought with technical analysis ... the greater is one likely to consider the effect of economic thought on history, and the less the effect of history on thought."⁵⁵

Cairnes' own position presents an interesting contrast to these views. If Schumpeter is correct in believing that Cairnes was concerned with pure theory to the virtual exclusion of applied or policy economics, then it might well be expected that he (Cairnes) would be an absolutist. His emphasis on the speculative nature of economic inquiry (as opposed to the applied craft of statemanship) and his polemics against a reliance on statistical methods in economic studies would further reinforce this expectation. In fact

however, Cairnes was clearly a relativist and declared himself as such (although, of course, not in those terms) at several points in his writings. The following is probably the clearest such passage:

The economic conditions of patriarchal life, of Greek or Roman life, of feudal life, are not the economic conditions of modern commercial life; and had Political Economy been cultivated in those primitive, ancient or mediaeval times, it would doubtless have contained some expositions which we do not now find in it.⁵⁶

"Relativism" and "Absolutism": A Digression

The relativist position is not without its justification, however, and we need not fault Cairnes for adopting it. Eagly, Spengler and Fetter may, in fact, have conceded so much to their relativist opponents that there is little basis remaining for a distinct absolutist stance.

A simple profit-maximizing analysis of the pursuits in which economists engage would indicate that the more professionalized the discipline becomes the more relativist it will also become. Despite the idealization of the scientific enterprise presented by Cairnes, one would expect that a significant factor affecting the choices made by economists between alternative types of research would be the relative rewards in salary and position attached to each of the various categories.⁵⁷ While the standards of "good" and "bad" work are largely determined within the profession, the allocation of research grants, new professorships, and thus the relative monetary rewards and rewards of "professional prestige" attached to the various specialities, are largely a function of the social problems

which seem pressing to non-economists. (Undoubtedly, there is a distinction between the determinants of standards of "good" and "bad" work and the standards of "interesting" and "uninteresting" research.) The question of which sub-areas of economics will receive the most attention and which will fall into relative obscurity has thus become more dependent on exogenous influences as economists have become increasingly recognized as professional experts who can provide valuable services to those outside the academy. Although it is conceivable that further advances in some areas of the theory will "dialectically" require a reworking of basic propositions in other areas, the emphasis of theory development and problem solving will, for the foreseeable future, remain with those particular specialities best able to tap the public purse.⁵⁸

All of this is not to deny that there is a valid distinction between pure economics and the "doctrines" or applications of economic theory. That distinction must, however, be examined more fully in order to place it in its proper perspective. The "pure theory" or "core" of economics, referred to by both Marshall and Spengler, can only be the set of definitions and methodological proscriptions which define and distinguish economics as a distinct social science, separate from sociology, political science and psychology. (This distinction is one apparent interpretation for Marshall's oft-quoted comment that "Economics is not a body of concrete truths, but an engine for the discovery of concrete truths.")

While exogenous influences cannot, by definition, affect the methodological conventions or basic identities of economics, as long

as this particular form of social inquiry is accepted as a viable enterprise, neither are these conventions or identities alterable through the action of endogenous influences.⁵⁹ One cannot play chess by changing the rules for playing chess, neither can one do "economic research" which violates the rules or framework in terms of which "economic research" is defined. (While this point is, of course, "trivial," in the sense of tautological, I am afraid that something of just this sort is basic to the "absolutist" conception of the development and future paths open to economic thought.) If demand curves slope up rather than down, then a new and different type of scientific study is created, one similar to the old study in name only. If behavior is described using models other than those which involve the constrained maximization of some behavioral (?) variables, then a similar anomaly is generated within "the theory."

The only escape from this dilemma is to reject the position of absolutism and to reject along with it the characterization of scientific development currently in vogue⁶⁰ (i.e., that of Thomas Kuhn). Both the absolutists and Kuhn seem to view economics as a game (perhaps an ideological or "religious" game) which is judicable only by its own internal rules and which proceeds by "puzzle solving" rather than "problem solving."⁶¹ Although Kuhn's theory was originated to explain "scientific revolutions" or, using the terms of absolutism, changes in the pure theory or "core" of economics, neither Kuhn nor the absolutists are able to provide any real explanation for peoples' decision to change the rules of the

game which they are playing. All suggested (perhaps, all possible) explanations seem to turn upon such non-logical criteria as "boredom," "a general uneasiness," or "a feeling that things aren't going right." The idea that the Kuhnian or absolutist view can justify paradigm change (a change in "the core") on the grounds of consistent "failures" of "the theory" simply begs the question of what, under this view, can constitute an instance of "a failure." In fact, a "failure" of the the theory cannot be connected to any intersubjective test of "the theory" (i.e., of "the core"), for there are no intersubjectively observable implications of "the theory" until it has been empirically interpreted. "Failure" is thus reducible only to psychological discontent.

Faced with their own inability to provide an explanation of the logic of (or for) scientific change, Kuhn's followers have concluded that his speculations constitute a sociological explanation or description of what it is that scientists do, rather than an analysis of the criteria they use (or "should" use) in deciding whether to retain or reject a portion of "the core." (Spengler, not surprisingly, adopts the same tactic.⁶²) It is evident, however, that the sociological causes for the actions and orientation of economists must, at some point, be traceable precisely to those extra-professional influences which the absolutists consider to be insignificant. Economists are simply not their own judges in every respect since they are not the exclusive or ultimate consumers of their own efforts.

J. E. Cairnes and the Historical School of Economics

Despite his defense of several doctrines held dear by historical writers, Cairnes was rightfully known as the most outspoken and uncompromising critic of the Historical School. He quite correctly viewed the meta-economic upheaval in Nineteenth Century economics as the main barrier to the continued progress of the study, and he fully realized that the future survival of the discipline depended upon the speedy resolution of this divisive struggle. His arguments although unreserved in their condemnation of the major planks in the Historical program, were so appealing and well-reasoned that he gained the respect, if not the agreement, of many of his Historicist opponents.⁶³ At the very least they uniformly agreed that he had done an admirable job in systematizing and clarifying those "orthodox" views which they were so anxious to destroy, and that he had said "everything which could be said" in their defense.⁶⁴

Cairnes' rejection of induction, in the sense defined by Mill, has previously received our attention. It is worthy of note, however, that he was equally opposed to "inductivism" (to the excessive use of inductive methods in economics) even when that term was defined in a weaker sense. Thus Cairnes cautioned against all attempts to "turn economics into the study of economic statistics," holding that economic statistics could, at best, disclose "the succession of phenomena" which it is the business of science to explain.⁶⁵

As already stated, Cairnes also opposed the reduction of economics to an all-encompassing historical sociology (as proposed

by the Comtists). His most persuasive and pervasive reason in defense of a separate and legitimate science of wealth was based upon the increased productivity which he believed to be the result of the division of scientific pursuits into separate areas of specialization. Just as the principle of a division of labor resulted in increased output in manufacture, it also had a place in the production of new knowledge.⁶⁶ The increase in knowledge resulting from a specialization of intellect was not, however, Cairnes' sole justification for the autonomy of the economics discipline. He clearly believed that there was no more of a "natural" boundary separating wealth maximizing behavior from human behavior in general, than there was separating astronomy, chemistry and physiology from some overreaching science of physical laws and relations. The test of science lay in its ability to construct well-ordered and appealing hypothesis systems and not in its correspondence to any presumed ultimate essences of reality.

While admonishing economists to exercise care in determining the truth or falsity of their premises,⁶⁷ by determining their correspondence with known physical and psychological laws, Cairnes vigorously combatted all attempts to reduce economics to either mechanics or psychology. While the "principles" of these disciplines provided the basis on which most economic investigations were based, the laws of economics, he believed, added "additional insights" to the study of human action not attainable directly from a knowledge of either mechanical or psychological relations. Economic laws were thus not eliminable from the class of independent scientific

hypotheses through the use of Occam's Razor.⁶⁸

Medievalism and Social Darwinism: The Other Heresies

Two remaining views that received Cairnes' harshest expression of scorn were "medievalism" and social Darwinism. Medievalism, which was popular during the Seventeenth and early Eighteenth Centuries and was resurrected by the Social Romantics of the Nineteenth Century, held that economic laws or the laws of any social science, were illegitimate intellectual constructs. Medievalists believed that social relationships were better left to the inquiries of ethics or religion or to the customary politics of the polis rather than being manipulated on the basis of abstract and purportedly Wertfrei social speculations. In response to this reasoning Cairnes replied that economic laws possessed all the properties common to laws in the physical sciences and social behavior was as proper an object for scientific investigation as were the objects of the non-social world. Indeed, Cairnes believed "that Political Economy does for the phenomena of wealth ... what Astronomy does for the phenomena of the heavenly bodies; what Dynamics does for the phenomena of motion; what Chemistry does for the phenomena of chemical combination ... it expounds the laws according to which those phenomena co-exist with or succeed each other ..."⁶⁹ As he also stated in response to those who feared that political economy would come to replace ethics, right actions are seldom the result of ignorance about human beings.

The doctrines of social evolution and social organicism,

championed by Herbert Spencer and, to a lesser extent, by Auguste Comte, were also to feel the sting of Cairnes' pen. Cairnes found the social organicist methodology to be "philosophically unsound and practically mischevious," and he believed that its logic was "entirely destitute of cogency." Spencer's analogies between the growth, decay and death of organisms and the growth, decay and death of civilizations prompted Cairnes to a lengthy critique in which he sought to use the main features of the proposed analogies to demonstrate their own inapplicability. The entire course of social Darwinian thought was, he believed, politically pernicious and "fitted more to obscure and confound, rather than elucidate, the problems of social existence." Cairnes' goal was clearly not to reform these doctrines and perspectives on social theorizing, but to eliminate them from all future discussions of social policy.⁷⁰

In Summary

Cairnes' opposition to the programs of historical economics and his defense of "the deductive view" left no room for a distinction, either in his eyes or in the eyes of his followers, between the older and newer branches of the British Historical School. Cairnes emphasized deductive methods to the virtual exclusion of inductive techniques, denied the role of statistics in economic inquiry, characterized economics as a pure study of cause and effect relationships rather than a practical study of "applied" problems, argued for the value-free status of economics and its autonomy from all other social and physical sciences, defended the

legitimacy of economic laws and their origination apart from either systematic fact-gathering or the laws of a universal social theory and waged war on the doctrines of social evolution and social organicism, both popular doctrines of his day. Even when he hedged his case, by admitting the introduction of statistics and casual observation in specialized instances, Cairnes was always ready to assure his critics that these procedures were no more than window-dressing, covering the corpus of intuitive theories and systematic deductions on which economic theory was essentially based. A priorism and "right intuition" were always more important in Cairnes' methodological outlook than were any collection of (probably meaningless) facts, and this was never more the case than in a science which rested on the firm intuitive generalization of "the desire for wealth."

Footnotes to Chapter III

1. J. E. Cairnes, The Character and Logical Method of Political Economy (London: Macmillan and Co., 1875), pp. ix-x, 88, 100-106.

2. For Whateley's views on meta-economics see Richard Whateley, Introductory Lectures on Political Economy (London: B. Fellows, Printer, 1832), Lecture IX. For a brief summary statement also see Richard Whateley, "On Certain Terms Which Are Liable to be Used Ambiguously in Political Economy" reprinted in Naussau Senior, An Outline of the Science of Political Economy (New York: Augustus Kelley, 1965), p. 227.

It should be noted that Cairnes was appointed to the Chair of Economics at Trinity College through the intervention of Whateley. For these and other interesting facts about Cairnes' life and career see L. L. Price, A Short History of Political Economy in England, Fifteenth Edition (London: Methuen & Co., 1937), p. 115.

3. J. E. Cairnes, Some Leading Principles of Political Economy Newly Expounded (London: Macmillan & Co., 1874).

4. For Mill's recantation of the wages-fund theory see J. S. Mill, "Thornton on Labour and its Claims," Fortnightly Review (May and June, 1869), pp. 505-518 and pp. 680-700. For Cairnes' criticism of Mill's recantation see J. E. Cairnes, Some Leading Principles of Political Economy Newly Expounded, *op. cit.*, p. 101, and George O'Brien's "J. S. Mill and J. E. Cairnes," Economica, N.S., X (November, 1943), pp. 283-285.

An exclusive concern with Cairnes' Some Leading Principles to the exclusion of any detailed consideration of his meta-economic writings is found in John F. Bell, A History of Economic Thought (New York: Ronald Press, 1967), pp. 271-272, William H. Spiegel, The Growth of Economic Thought (Englewood Cliffs, New Jersey: Prentice-Hall, 1971) p. 394, and even in the usually more comprehensive works of T. W. Hutchison, A Review of Economic Doctrines, 1870-1829 (Oxford: Clarendon Press, 1953), pp. 22-28 and Robert B. Ekelund and Robert F. Hebert, A History of Economic Theory and Method (New York: McGraw-Hill, 1975), pp. 135-138.

5. J. E. Cairnes, The Slave Power (New York: Carleton, 1862).

6. J. E. Cairnes, The Character and Logical Method of Political Economy (First edition, London: Longman, Brown, Green and Longmans, 1857), (Second Enlarged Edition, London: Macmillan & Co., 1875). The second, enlarged edition which included a virtual reprint of J. E. Cairnes, "Political Economy and Laissez-Faire," Fortnightly Review, Vol. 16 (1871), pp. 80-97, is used as the primary reference for much of the following.

7. Joseph A. Schumpeter, A History of Economic Analysis (New York: Oxford University Press, 1954), p. 534fn.
8. George O'Brien, "J. S. Mill and J. E. Cairnes," Economica, N.S., Vol. X (November, 1943), pp. 273-285.
R. D. Collison Black, "Jevons and Cairnes," Economica, N.S., Vol. XXVII (August, 1960), pp. 214-232.
9. An exception to the neglect accorded Cairnes' meta-economic views is Jacob Viner, "Some Problems of Logical Method in Political Economy," reprinted in Earl L. Hamilton (ed) Landmarks in Political Economy, Vol. 1 (Chicago: University of Chicago Press, 1962), pp. 101-124.
10. Robert B. Ekelund and Emilie S. Olsen, "Comte, Mill and Cairnes: The Positivist-Empiricist Interlude in Late Classical Economics," Journal of Economic Issues, Volume VII (September, 1973), pp. 383-416.
11. Emilie S. Olsen, The Positivist Philosophy of Auguste Comte and Its Relation to the Philosophy and Writings of J. E. Cairnes (College Station, Texas: unpublished thesis submitted to the department of economics, Texas A&M University, December, 1970).
12. Cairnes' position concerning the purpose and justification for scientific inquiry is highly ambiguous. At one point he states that:

The purpose of a science is ... not to obtain tangible results, not to prove any definite thesis, not to advocate any practical plan, but simply to give light, to reveal laws of nature, to tell us what phenomena are found together, what effects follow from what causes.

The Logical Method of Political Economy, op. cit., pp. 17-18.

And to assure that there was no possible confusion regarding his intentions, Cairnes later noted that:

... whatever takes the form of a plan aiming at definite practical ends ... it matters not what the proposal be ... if its object be to accomplish some definite practical ends, then I say it has not of the characteristics of a science, and has no just claim to the name.

J. E. Cairnes, Essays in Political Economy: Theoretical and Applied (London: Macmillan, 1873), pp. 252-253, hereafter cited as "Essays."

Yet despite these apparently unconditional statements the material in the two following footnotes (numbers 13 and 14) seems to indicate that Cairnes may not have always wanted to restrict scientific inquiry to "pure theory."

13. Cairnes was quite definite regarding the "practical fruits" to be expected from science and scientific investigations:

In short, let it once be made clear that abstract speculation is not barren speculation, that scientific doctrines have a real bearing on the practical concerns of life ... [on] all that concerns human beings in shaping their conduct to [sic] the world.

Essays, op. cit., p. 237.

Yet, as we have already seen, Cairnes clearly expressed the view that science was not to be pursued for its practical benefits.

14. Perhaps the best rationalization which can be offered for Cairnes' thirst for "pure science" and his justification of the enterprise by reference to its practical benefits is found in the following passage from his essay on August Comte:

Practical applications of scientific principles are ... not the proper fruit, but the accidental consequences of scientific knowledge ... these tangible results may, and in the end generally will, come in abundant supply, but they are not of the essence of the plant; it is not in these, but in that power which is the end and aim of scientific knowledge--the power of interpreting nature, of explaining phenomena ...

Essays, op. cit., pp. 298-299.

The curious contradiction which seems implicit in all this is the extensive use which Cairnes made of the supposedly abstract conclusions of political economy when he was defending his own political positions. See, for example, the material on page 9 of the present chapter and the corresponding footnote.

15. The Character and Logical Method of Political Economy, op. cit., pp. 31-37.

16. "Valued matter," it should be noted, connotes a different orientation toward economic inquiry than that adopted by Symes or other more "psychological" economists. It is more expressive of the ambiguous tradition of philosophic idealism or associationist psychology, and is a much more flexible concept with which to deal in describing the nature of economic inquiry. As such, this definition of economic concerns forms a firmer base on which to con-

struct the later divisions of economics into both demand theory and production or cost theories than did the views of Symes, Jevons or the Austrians.

On the "complex character" of political economy, which resulted from its concern not with mind or matter but with "valued matter" see Character and Logical Method, op. cit., pp. 31-37.

17. For Cairnes' connection between induction and experimentation see Character and Logical Method, pp. 63-64, Essays, p. 303 and footnote 23 below.

Cairnes sometimes used "induction" in the more ordinary sense of drawing an empirical generalization from a set of facts or inferring an invariant relationship from only a few instances of that relationship. He also criticized what he called the use of induction in the "large sense" as nothing more than the antonym for the term "metaphysical." A detailed discussion of the different senses of "induction" appears in Character and Logical Method, pp. 60-62.

It is interesting that Cairnes believed that physics, which most historicists understood to be the paradigm of an inductive study, was inductive only in its early primitive stages: when men had no knowledge of "ultimate physical principles." In its more advanced forms, however, the "more powerful" tools of deduction came into use in the studies of physical problems, and it was only then that dramatic advances were possible in a short span of time. Cairnes held a similar view of the history. (Character and Logical Method, pp. 69-75).

18. Character and Logical Method, pp. 46-47.

19. The doctrine of tendencies was first applied in an organized form to economic research by J. S. Mill (see Appendix A to Chapter 1 of the present work). It was probably from that source that Cairnes derived the following notions:

... the doctrines of Political Economy are understood as asserting not what will take place but what would or what tends to take place, and in this sense only are they true. If this admission constitutes an objection to Political Economy, it is equally an objection to Astronomy, Mechanics, and to all those physical sciences which combine deductive with inductive reasoning.

Character and Logical Method, op. cit., pp. 55-56.

See also Essays, op. cit., p. 303 for Mill's influence on Cairnes in this regard.

20. The reputed "complexity" of social phenomena was only more severe than the complexity of physical phenomena, not different in type. Yet the physical scientist had open to him the tools of controlled experimentation which allowed him to determine the

relative weights to be attached to the component variables which themselves determined various physical events. The economic scientist, being denied this tool, was also denied the powers of prediction which resulted from it. The most he could hope to accomplish was, according to Cairnes, a satisfying explanation (after the fact) of why things had turned out as they had.

In a slightly different vein, however, we find Cairnes asserting that perhaps political economy could, perhaps, one day become predictive, given certain conditions. In his defense of the science against charges of sterility leveled by Comtists, Cairnes noted that so far as economics was not a perfect instrument for social investigations, much of its imperfection was attributable to the relatively underdeveloped states of complementary studies: the results of these other sciences being necessary, along with the theorems of economics, for predictive accuracy:

This incapacity ... of forecasting events ... argues no imperfection in economic science; ... but in those other cognate sciences to which belongs the determination of the non-economic agencies which are the unknown quantities in the problem. When these cognate sciences shall have been brought up to the same stage of advancement which has been obtained by Political Economy, something approaching to that systematic prevision of events contemplated by M. Comte will be possible. Meanwhile it is no slight gain, in speculating on the future of society, to have in our power to determine the direction of an order of tendencies exercising so wide, constant and potent an influence on the course of human development as the conditions of wealth.

Essays, op. cit., p. 306. See also pp. 269-270.

21. Character and Logical Method, op. cit., p. 226. We have already noted in the Appendix on William Whewell what that genius' opinion was of Comte as a "universal scientist." It was, in short, decidedly unfavorable.

22. The complex character of economic phenomena as a compound of both physical and mental aspects is explained by Cairnes in the following passage from his Character and Logical Method of Political Economy:

Neither mental nor physical nature forms the subject-matter of the investigations of the political economist. He considers, it is true, physical phenomena, as he also considers mental phenomena, but in neither case as phenomena which it belongs to his science to explain.

The subject-matter of that science is wealth; and though wealth consists in material objects, it is not wealth in virtue of those objects being material, but in virtue of their possessing value--that is to say, in virtue of their possessing a quality attributed to them by the mind. The subject-matter of Political Economy is thus neither purely physical nor purely mental, but possesses a complex character, equally derived from both departments of nature, and the laws of which are neither mental nor physical laws, though they are dependent, and, as I maintain, dependent equally on the laws of matter and on those of mind.

Character and Logical Method, p. 32.

This question was of prime importance for the other aspects of Cairnes' methodology since the introspective techniques of the mental sciences were inappropriate to research in the physical sciences and the empirical techniques of the physical sciences were inappropriate to research in the mental sciences. To put the matter differently, by claiming that economics had elements of both mental and physical studies, Cairnes could "switch-off" between introspective and empirical methods as he desired.

23. The tie between the ability to perform controlled experiments and the use of induction is clearly recognized by Cairnes in at least two separate writings.

The foregoing considerations suffice to show the utter inadequacy of the inductive method, in the narrower sense of that expression, as a means of solving the class of problems with which Political Economy has to deal, arising from the impossibility of employing experiment in economic inquiries under those rigorous conditions which are indispensable to give cogency to our inductions.

Character and Logical Method of Political Economy, p. 68. See also Essays in Political Economy, p. 303.

24. Character and Logical Method, op. cit., pp. 15-16, 44.

25. The question of the positive and, at the same time, hypothetical nature of political economy is tied to the doctrine of "abstraction" in the following passage from Cairnes' Character and Logical Method:

... it is surely possible that the premises

[of a hypothesis] should be true, and yet incomplete--true so far as the facts they assert go, and yet not including all the conditions which affect the actual course of events. The laws of motion and of gravity are not arbitrary assumptions, but have a real foundation in nature; and it is a strictly logical deduction from those laws that the path of a projectile is in the course of a parabola; yet, in point of fact, no projectile accurately describes this course; the friction of the air coming in to disturb the other principles.

Character and Logical Method of Political Economy, op. cit., p. 54.

26. Examples of Cairnes' Wertfrei position regarding economics are numerous, the following are only a representative sample:

Political Economy stands apart from all particular systems of social or industrial existence. It has nothing to do with laissez-faire any more than with communism ...

Essays, op. cit., p. 255.

Economic science has no more connection with our present industrial system than the science of mechanics has with our present system of railroads.

Character and Logical Method, op. cit., p. 22.

... the maxim of laissez-faire has no scientific basis whatever, but is at best a mere handy rule of practice, useful, perhaps, as a reminder to statesmen ..., but totally destitute of all scientific authority.

Essays, op. cit., p. 244. See also Character and Logical Method, op. cit., pp. 13, 14, and 22-26.

27. For a detailed discussion of the expressions of political views by Classical economists see Gunnar Myrdal, The Political Element in the Development of Economic Theory (New York: Clarion Books, 1969). The volume contains extensive, but not detailed, references to Cairnes. Another, more recent, discussion of the issue of value judgements in economics with reference to the writings of both Classical and Neoclassical authors is T. W. Hutchison's "Positive Economics and Policy Objectives" (Cambridge: Harvard University Press, 1964). For a consideration of Cairnes' views see especially pp. 32-34, 40-41.

28. Joseph Schumpeter, A History of Economic Analysis (New York: Oxford University Press, 1954), p. 824fn.

29. J. E. Cairnes, "M. Comte and Political Economy," Fortnightly Review, Vol. 13 (1870), pp. 579-580. Quoted in Ekelund and Olsen, op. cit., p. 400.

30. See Ekelund and Olsen, "Comte, Mill and Cairnes," op. cit., pp. 403-405 and O'Brien, "J. S. Mill and J. E. Cairnes," op. cit., pp. 277-278 for references to Cairnes' policy pronouncements.

31. J. E. Cairnes, Essays in Political Economy, Theoretical and Applied, op. cit., pp. 245-251.

32. J. S. Mill's attitude of polite disdain (one would not like to say "contempt") for Cairnes' more doctrinaire opinions is commented on and illustrated by quotation in O'Brien, op. cit., pp. 276-277. See footnotes 57 and 58 below for the British Historical economists' opinions of Cairnes.

33. Character and Logical Method of Political Economy, op. cit., pp. 224-225.

34. Ibid., pp. 38-39.

35. See, for instance, Gary S. Becker, Economic Theory (New York: Alfred A. Knopf, Inc., 1971), pp. 1-4, 25-26.

36. The idea of a "secret world" of "hidden motives" which "only the individual knows 'for sure'" is dissected in Gilbert Ryle's The Concept of Mind (New York: Hutchison's University Library, 1949). For a discussion of "Descartes' Myth" of motives as something apart from actions, see pp. 11-24; and for a discussion of the logical and linguistic status of motives and emotions, see pp. 83-115.

The idea that economists could perform "mental experiments" in their "inner worlds" of thought and reflection is well illustrated in the following passage from Cairnes' writings:

The economist may thus be considered at the outset as already in possession of those ultimate principles governing the phenomena which serve for the subject of his study, the discovery of which in the case of physical investigation constitutes for the inquirer his most arduous task: ... although precluded from actually producing the conditions suited to his purpose, there is nothing to prevent the economist from bringing such conditions before his mental vision, and from reasoning as if these only were

present ... It is true that the conclusions arrived at would represent hypothetical truth merely--that is to say, would express a law true only in the absence of disturbing causes; but, as I have already explained, so much qualification as this must be understood of all scientific laws ... The process, then, which I have been describing ... is in the nature of an experiment conducted mentally.

Character and Logical Method, op. cit., pp. 78-80.

The curious feature of this doctrine is not only that it separates the world of ideas from the world of physical events, but also that it then makes the latter dependent upon the former in an almost Kantian fashion (i.e., through the assertion that our "true" knowledge of the social world rests upon synthetic a priori propositions). If men can conceive of some way of explaining their most elemental impressions about social organization, then that way of looking at the matter must be fundamentally correct, even though it can be subject to further modification on the basis of further reflection. The idea that some ways of organizing "common sense knowledge" about society and social relationships might ultimately prove factually false, rather than internally inconsistent, never seems to have deeply impressed Cairnes.

37. Character and Logical Method, op. cit., pp. 83-88. Although Cairnes sometimes hedged on the exclusivity of introspective techniques in social investigations (see p. 67 and footnote 41 of this chapter) he stated upon several occasions that any proper methodology of economic science must ultimately rest upon our inner perceptions. For additional comments regarding the irrelevance of empirical tests in economics see p. 14 of the present chapter.

38. Schumpeter has correctly evaluated Cairnes' relationship with Mill in his History of Economic Analysis:

He [Cairnes] may be called Mill's pupil for he always reasoned with reference to the latter's teaching--even where he did not mention the fact explicitly--and he entertained toward Mill, as his correspondence shows, feelings that can be rendered only by the term "reverence."

History of Economic Analysis, op. cit., pp. 533-534.

Yet Schumpeter was also correct in stating that:

Nevertheless, he [Cairnes] sometimes criticized Mill sharply and, by virtue of this

criticism, constructed something that, though entirely within the Millian groundwork, was in some measure his own.

Ibid., p. 534.

It should be kept in mind, however, that Cairnes was not totally aware of his originality, especially in methodological matters. Frequently he assumed that he was merely repeating, clarifying and systematizing the earlier Classics, and when he criticized Mill he often argued from what he believed to be a traditionalist standpoint.

39. Character and Logical Method, op. cit., p. 78.

40. Ibid., pp. 76-77. Cairnes goes on to state that:

The economist starts with a knowledge of ultimate causes. He is already, at the outset of his enterprise, in the position which the physicist only attains after ages of laborious research. (Emphasis in original)

It is not necessary to ... [resort to induction for empirical generalizations or an understanding of the facts of the case] ... for the reason, that we have, or may have if we choose to turn our attention to the subject, direct knowledge of these causes in our consciousness of what passes in our own minds, and in the information which our senses convey, or at least are capable of conveying to us of external facts. Everyone who embarks in [sic] any industrial pursuit is conscious of the motives which actuate him in doing so ...

Character and Logical Method, op. cit., pp. 75, 76-77.

41. Ibid.

42. The distinction between observation for the purpose of falsification and observation as a device to insure the completeness of intuitive reasoning is clearly apparent in those passages where Cairnes does make some concessions to a loose form of empiricism:

... there is in a hypothetical experiment always the danger, not only that some of the conditions supposed to be present may, in the course of ratiocination, be overlooked, but also of a flaw in the reasoning by which the action of the particular cause under consid-

eration is established. And this renders it expedient that the process in question should as far as possible be supplemented by such sorts of verification as economical inquiry admits of. For example, it is open to the economist, having worked out his problem in the manner described, to look out for some actual instance which approximates in as many of its principal circumstances as possible to those of his hypothesis. Having found one, he can observe how far the results realized in the conclusions; and in case, as would usually happen, the correspondence was not complete, he would have to consider how far the discrepancy admitted of being explained by reference to the presence of known disturbing causes. Unfortunately, for reasons already indicated, verification can never, in economic inquiry, be otherwise than imperfectly performed.

Character and Logical Method, op. cit.,
pp. 80-81. See also pp. 84-85.

43. That Cairnes was willing to admit any role for factual evidence in economic inquiry can only be viewed as an anomaly in his perception of "correct" methodological procedures. However, he never approached the idea that such evidence might be used to test the theories of economics with a view to possible falsification. Note the passage from his works reproduced on p. 70 of this chapter.

44. R. B. Ekelund, Jr. and E. S. Olsen, "Comte, Mill and Cairnes," op. cit., p. 405. Despite his concessions to a loose form of empiricism, Cairnes' attitudes toward the use of statistical data in economic investigations remained highly ambivalent. Noting that economic hypotheses are "imperfect," or, in more modern terms, that they do not include all relevant causes affecting the phenomena (they state only sufficient and not necessary conditions), Cairnes argued that statistical evidence seeming to contradict the hypotheses of political economy was not, in itself, enough to indicate whether the hypotheses were actually in error or whether "disturbing causes" (changes in one of the variables implicitly held in the pound of ceteris paribus) had in some way affected the prediction. See his Character and Logical Method, op. cit., p. 99.

45. R. D. C. Black, "Jevons and Cairnes," op. cit., p. 214. There was a rather lengthy correspondence between Jevons and Cairnes regarding their mutual contributions to the question of price dispersal (the differing local impacts of fluctuations in the money stock on differing geographical locations). During the course of this interchange Jevons remarked that he had learned much from

Cairnes' published articles on the subject, articles which were, at least in part, "statistical" in content.

46. Those interested in Mises' methodological views should refer to his Epistemological Problems of Economics (Princeton: D. Van Nostrand, 1960), his Ultimate Foundation of Economic Science (Princeton: D. Van Nostrand, 1960) and his Human Action (New Haven: Yale University Press, 1949), pp. 10-89. In short, Mises claims to have deduced the whole of economics from some rather elementary "facts of human consciousness." The claim is, of course, fallacious.

47. The quote occurring in the text is drawn from Character and Logical Method, op. cit., p. 99. The latter passage, referred to in the text, reads as follows:

In economic reasoning, therefore, supposing the logical portion of the process to be sound, the appeal must in all cases ultimately be to consciousness or to some external fact--to some mental or physical law.

Ibid.

48. J. E. Cairnes, "New Theories in Political Economy," The Fortnightly Review, Vol. 38 (1882), pp. 579-602.

49. W. S. Jevons, The Theory of Political Economy (New York: Kelley and Millman, 1957), pp. 7-22.

50. J. E. Cairnes, "New Theories in Political Economy," op. cit., p. 583 and Character and Logical Method, op. cit., pp. 109-110.

51. Character and Logical Method, op. cit., pp. vii-viii and p. 19.

52. Leo Rogin, The Meaning and Validity of Economic Theory (New York: Harper and Brothers, 1956).

53. George J. Stigler, "The Influence of Events and Policies on Economic Theory," American Economic Review, Vol. 50 (May, 1960), pp. 36-45.

54. Joseph J. Spengler, "Exogenous and Endogenous Influences in the Formation of Post 1870 Economic Thought: A Sociology of Knowledge Approach," in Robert Eagly (ed.), Events, Ideology and Economic Theory (Detroit: Wayne State University Press, 1968); Robert Eagly, "Comment," Ibid., pp. 188-190; and Frank Fetter, "The Relation of the History of Economic Thought to Economic History," American Economic Review, Vol. 55 (May, 1965), pp. 136-142.

55. Fetter, op. cit. Quoted in Robert B. Ekelund, Jr. and Robert F. Hebert, A History of Economic Theory and Method (New York: McGraw-Hill, 1975), p. 14.

56. Character and Logical Method of Political Economy, p. 23. Also see J. E. Cairnes, Essays in Political Economy, Theoretical and Applied (London: Macmillan, 1873), pp. 258-260.

57. It might be believed that this critique of the absolutist position is incomplete, in that it leaves unanswered the question of where the axioms of political economy originally came from and the question of why they are what they are rather than something else. This is, however, a historical, or, perhaps, psychological, issue which is concerned with unique unrepeatable events. As such it is not open to either the analysis of logical structures or the repetition of scientific tests. Viewed in this way, then, the entire relativist-absolutist controversy reduces to a quibble over which historical explanation (that is, which well-constructed myth) we feel most comfortable with when describing the development of economic thought. The critical question then appears to be not what caused economics to develop the particular doctrines which it has but, rather, what constitutes the criteria for "successful" and "unsuccessful" doctrines (assuming that the criteria are not exhausted by the canons of empirical research).

In a comment on an earlier draft of this chapter, Professor R. B. Ekelund has raised the following question: "Don't economists have economic incentives to maintain 'the core' of their theories as distinct from theories in the other social sciences? Doesn't this consideration go some way toward undermining your arguments for relativism on the basis of the economics of Economics?" (I have paraphrased freely.) I will attempt to sketch what I believe may be an answer to this question, for it is important to the argument presented in the text of this chapter. The following is, however, only a sketch; an exhaustive answer to this question would require a chapter of its own.

On pages 20-21 of this chapter I have expressed the belief that there is a valid distinction between the "core" of economics and the "shell" of economic doctrines. However, I also believe that the true character of this distinction has been obscured in many of the articles which employ this terminology. The distinction, in short, is that "the core" is composed either of tautologies, which express basic definitions used in economic research, or of methodological conventions for carrying out such research. The "shell," on the other hand, is composed of attempts at the empirical interpretation of such purely tautological concepts as "utility maximization." Since the "shell" is the only part of economic theory which can be modified without reinterpreting the entire enterprise, it should be apparent that my arguments from the economics of Economics can apply only to it. I thus agree with the modified absolutist position, but believe that this position asserts nothing of significance: that is,

that it is reducible to the statement, "As long as economists continue doing Economics they will continue to use "pure theories" such as utility maximization, demand curves and production functions."

In regard to the economic incentives to differentiate economic research from other forms of social science, it must be explicitly recognized that this preumes an imperfectly competitive academic market in the production of economic research (most particularly, in the production of economic theory). While I personally believe that this assumption is consistent with other casual observations (such as the preponderance of articles from certain schools in the major journals and the neglect of, if not hostility toward, methodological inquiry within the economics profession) such observations are without any formal basis.

58. It might be interesting to consider the extent to which economics, or any publically subsidized intellectual speciality, would have been different in the absence of outside funding. The increasing emphasis on "applied" sub-fields of economics within the discipline today is perhaps more connected with the incentives to develop these fields as a way of making the "product" marketable, than it is a reflection of a rising concern over the importance of empirical research in social science. That many economists are quite satisfied with the "correctness" of their more vivid "intuitions" is evident from their frequently expressed willingness to "fudge" empirical studies so they "come out right." On the other hand, it might be argued that there have been significant "spillover effects": that those areas which would have developed even without public support have developed even further in a subsidized environment and that, although the number of reliable and competent empirical researchers is still "too small," it is larger than it would have been otherwise. All such arguments, either pro or con, are, of course, in the nature of "story-telling" since we are dealing with the "what if ..." of historical counter-factuals; but the speculation is, in any case, intriguing.

59. This argument is, in part, based upon a brilliant criticism of Kuhn's philosophy of scientific revolutions authored by John Watkins of the London School of Economics, "Against 'Normal Science'," contained in Imre Lakatos and Alan Musgrave (eds.), Criticism and the Growth of Knowledge (Aberdeen, G. B.: Cambridge University Press, 1970), pp. 25-37. See especially pp. 30-31, 35.

60. The central and most familiar work of the recent revival of a Kantian view of science is Thomas Kuhn's The Structure of Scientific Revolutions, 2nd edition (Chicago: Phoenix Books, University of Chicago Press, 1970). A more recent restatement and refinement of these same views is found in Thomas Kuhn, "Logic of Discovery or Psychology of Research?," Lakatos and Musgrave, op. cit., pp. 1-23 and "Reflections on my Critics," Ibid., pp. 231-278.

61. The reduction of science to a "puzzle-solving" rather than a "problem-solving" activity is found in Kuhn's "Logic of Discovery or Psychology of Research?," op. cit., pp. 4-10, 21-22.

62. Joseph Spengler, "Exogenous and Endogenous Influences in the Formation of Post 1870 Economic Thought: A Sociology of Knowledge Approach," op. cit., p. 45.

63. Ingram found Cairnes' Some Leading Principles of Political Economy Newly Expounded to be "marked by great ability" (J. K. Ingram, A History of Political Economy (New York: Macmillan, 1888), p. 157) although he then proceeded to devote some five pages of his History to a criticism of its contents. He also believed that Cairnes' "... Slave Power (1862) was the most valuable work which has appeared on the subject of the great American conflict." (Ibid., p. 162.)

Leslie also had words of both praise and criticism for Cairnes. In his 1875 obituary notice he wrote that Cairnes' reputation was second only to that of J. S. Mill, who had had the advantages of a prestigious background and a term in Parliament to his advantage. He described Cairnes' The Slave Power as "one of the most masterly essays in the literature of political controversy," and his Leading Principles was, in Leslie's opinion, "a work which ought to be regarded, even by those who dissent most from some of its principles, as an important contribution to economic science." The most extravagant combination of both praise and criticism was reserved for Cairnes' main methodological work, however. Of his Logical Method Leslie wrote that it "ought ... to be welcomed by those economists who incline to the inductive or historical method, not only for the intellectual interest which the reasoning of a powerful mind must always excite, but also as a masterly exposition of the deductive method, and a complete presentation of all that can be said for it or got out of it." (T. E. C. Leslie, Essays in Political Economy, 2nd edition (New York: Augustus M. Kelley, 1969), pp. 60, 62.)

64. Bagehot's obituary of Cairnes is filled with many words of high praise, but the phrases which he employs in expressing his admiration are often so ambiguous that they can be read as either criticisms of or paeans for Cairnes' writings:

The constant rigor with which Mr. Cairnes withstood these temptations [to popularize political economy or tie it to particular cases] has given his writings a very peculiar character. There is a Euclidian precision about them which fits them for a tonic for the mind and which makes much other writing seem but "soft stuff" ... at any rate, you feel that you have seen in all likelihood the worst of the subject ...

Reading his works is like living on high ground; the "thin air of abstract truth" which they give you braces the mind just as fine material air does the body ...

Why a mind like his should have been created, and then the power to use it at all withheld, is one of the mysteries of which in this world we have no solution (emphasis added).

E. F. Hutton (ed.), The Works of Walter Bagehot, Vol. III (Hartford: Traveler's Insurance Company, 1891), pp. 443-444.

65. Cairnes' separation of empirical generalizations and the laws of science is emphatic and totally unambiguous:

... [Universal generalizations] ... afford no explanation of any phenomenon connected with the production and distribution of wealth, but is itself an expression of a complex and difficult phenomenon which it is the business of the political economist to explain. To bring forward this as a final result in economic speculation--to deprecate all analysis of the causes on which the so-called "law" depends ... is to simply abandon all pretensions to solving the problem of wealth--is to give up at once the cause of Political Economy as a branch of scientific research.

Character and Logical Method of Political Economy, p. 211.

66. See Character and Logical Method of Political Economy, pp. 225-226 and Essays in Political Economy, Theoretical and Applied, pp. 271-276, 306.

67. Character and Logical Method of Political Economy, p. 18.

68. Ibid., p. 226.

69. Ibid., p. 18. See also Essays in Political Economy, Theoretical and Applied, p. 254.

70. J. E. Cairnes, "Mr. Spencer on Social Evolution," Fortnightly Review, Vol. 23 (1875), pp. 63-82, 200-216.

CHAPTER IV

T. E. C. LESLIE AND THE REVIVAL OF BRITISH HISTORICISM

Evaluation of Leslie by Other Economists

Of the many major and minor Historical economists who succeeded Jones and Whewell, T. E. C. Leslie was the one individual most capable of preserving and building upon the tradition of the early British Historical School. He not only penned a devastating critique of the orthodox methodology which he was in the process of revising into a systematic treatise at his death, but he also was one of the few British Historical economists to put to practice the principles of empirical research which the School had long professed. In Schumpeter's evaluation, that part of Leslie's work which was of "the descriptive kind" was often "high-grade,"¹ especially when it dealt with conditions and consequences of British and Continental land tenure; the praise of Leslie's elder contemporary, J. S. Mill, was even less constrained. Mill referred to Leslie as "one of the best living writers on political economy" and wrote an extensive and laudatory appraisal of his Land Systems for the Fortnightly Review.²

Leslie was, in fact, well-received by "friend" and "foe" alike. He was one of the few "non-Positivists" to receive the unguarded praise of J. K. Ingram,³ who both edited the later edition of his Essays in Political Economy and borrowed heavily from his writings on Adam Smith in the interpretation of that author presented in his

own History.⁴ Although his reputation carried over into the early Twentieth Century in the histories of Haney⁵ and Scott,⁶ Leslie was gradually "weeded out" from more modern texts, including the standard works by Roll and Blaug.⁷ Of the accounts of the development of economics published during the second half of the Twentieth Century only Schumpeter, Ekelund and Herbert, and Bell⁸ make reference to Leslie's life and work, and none of these volumes contain anything approaching a developed and systematic consideration of his methodological views.

His Influence on Contemporaries

In a history of Victorian economic methodology and the development of the British Historical School it is, however, not only improper, but, indeed, impossible to overlook the overwhelming force of Leslie's thoughts and writings. By the time his views had gained their full audience in the mid and late 1870's, the empirical orientation of Jones and Whewell had all but disappeared from British economics. There may be some dispute regarding Leslie's priority as the instigator of a revival of methodological controversy in British economics, for his "On the Philosophical Methods of Political Economy"⁹ appeared in the same year (1876) as Bagehot's "Postulates of Political Economy"¹⁰ and Symes' Outlines of an Industrial Science.¹¹ However, Leslie had published essays dealing with methodological issues as early as 1862,¹² with Symes' first essay not appearing until 1871.¹³ It is also notable that Symes was reputed to have been virtually unknown in Great Britain and that he

acknowledged Leslie as his mentor in the introduction to his Outlines of an Industrial Science.¹⁴ Among other evidence for Leslie's priority we may mention that his Land Systems,¹⁵ which Mill rightly acknowledged as a truly significant contribution to both empirical economics and the extension of economic theorizing, had appeared in 1870, and several of the papers collected in it had been separately published as early as 1867.

While Bagehot and Symes arrived at valuable and original insights contemporaneous with Leslie's more mature publications, Leslie led the way in applications of the "historical method" (rightly conceived) and served as an inspiration, if not a direct source, for the bulk of those issues debated during the late 'Seventies. Indeed, those sections of Bagehot's Economic Studies¹⁶ composed after the publication of the Postulates, in 1876, contain many points which are little more than a "rewrite" of Leslie's basic methodological contributions. It would be unjust, however, to underestimate the role of Bagehot and of later writers such as Ingram in the significant, if fleeting, popularity enjoyed by Leslie's views. For although he often wrote in literary and popular journals, Leslie's own style was more often that of the philosopher or the pure social scientist, rather than that of a popularizer of vital issues.

Plan of This Chapter

In this chapter, I have attempted to trace the historical development of Leslie's methodological views and to summarize the central features of his mature writings. In this manner I have hoped

to indicate those aspects of his thoughts which, for good or ill, would eventually gain some public recognition, and to uncover those "hidden insights" in his methodological writings which were, unfortunately, neglected by his contemporaries. While some of the doctrines in Leslie's methodological system can only be judged as faulty and productive of much later confusion, it is my general conclusion that he was the last, and in many senses, the best of the early English Historical economists. After his death in 1882 there remained only J. K. Ingram as a standard bearer of the "early" historical tradition in England. Whatever else might be said in Ingram's favor, he was unequal to the enormity of that task and was, in fact, ill-equipped for it by virtue of his own philosophic preoccupation with the Positive Philosophy of Auguste Comte. Throughout the 'Eighties and 'Nineties the Historical movement in England rapidly disintegrated into Comtist-Historicist (in the Popperian sense) and German Historical factions. By the time of Marshall's Principles and Keynes' On the Scope and Method of Political Economy there is some question if anyone still understood the intent of the earlier British School, or whether, perhaps, its perspective had not been wholly obscured by the pseudo-debates of the 'Eighties and 'Nineties.¹⁷

Leslie's Life and Intellectual Foundations

The biographical accounts of Leslie's education and youth are abbreviated to such a degree that we have only the barest sketch of those influences which imparted the cast to his mature thoughts. In

his introduction to the second edition of Leslie's Essays in Political Economy, Ingram tells us that his subject began his education at King Williams College at an exceptionally early age and left there to enter Trinity College, Dublin, when he was yet only fifteen. Within three years he had gained a scholarship in classical studies. And the following year he was awarded a gold medal in mental and moral philosophy for exceptional performance at his degree examination. At nineteen, with degree in hand, Leslie took up the practice of law but gladly abandoned that profession some seven years later when he received an appointment as Professor of Jurisprudence and Political Economy at Queen's College, Belfast.¹⁸

As a part of his own autobiography Leslie credited Sir Henry Maine's early lectures on historical anthropology as a telling influence on his own early intellectual development. But he quickly added that "... the English economists of the future must study in the schools of both ... Sir Henry Maine ... [and] ... J. S. Mill."¹⁹

Whether we should accept Leslie's own hindsight account as an accurate appraisal of the influences which played a primary role in his intellectual development is, however, open to several major questions. Although his essays often dwell upon the institutional differences of the various nations of Europe and on the process of historical evolution of these institutions (both themes in Maine's lectures), there is a deeper and more fundamental strain present in his writings. Virtually all of Leslie's criticisms of Orthodox methodology turn upon an implicit parallel between the thought patterns (or "games") traditionally encountered in philosophy and

the corresponding types of methodological arguments often employed by economists. It was in this light that he characterized orthodox economics as "being infested" by "the disease of language which metaphysicians call the realism of the Schools."²⁰ ("The Schools" referring to the Thomistic-Aristotelian tradition in metaphysics and epistemology.) In this same regard it is notable that Leslie never actually distinguished economics as a separate science independent from other forms of social study, and frequently referred to its theoretical and methodological aspects as "philosophy."²¹ It is perhaps safest, then, to view Leslie's methodological discussions as a combination of these two elements: the institutional relativism which he derived from the lectures of Maine and philosophic concerns which undoubtedly arose from his early studies in metaphysics and British empiricism. As we progress through the various stages of Leslie's intellectual development, it will become apparent that he continually wavered back and forth from one of these issues to the other, first taking up the study of economics as a science of human motives, then turning to a descriptive and statistical study of the conditions of land tenure in various nations, only to return once again to the issue of economic a prioriism, and finally, to consider the historical and geographic patterns of price and wage fluctuations.

Early Essays

Leslie's earliest essay on an economic topic, "On the Love of Money,"²² appeared in 1862 in an obscure journal which soon thereafter "ceased publication." The contents of this article were not,

as might be assumed from the title, a moralistic disquisition on why one should not love money, but rather comprised a detailed analysis of what had served as money or wealth throughout the ages and what different types of conduct had been promoted by the desire for its acquisition.²³ Although at this early date Leslie was less openly critical of the orthodox position than he would eventually become, it is not difficult to discover passages which are reflective of the serious doubts over orthodox methodology which were even then troubling his mind. As one instance we quote the following:

... perhaps political economists have not escaped a bias from their own phraseology, and are apt to imagine in their scientific discussions a much fuller explanation of the complete phenomena of wealth, and a much closer approximation to the complete philosophy of the subject, than lies within their providence as completely circumscribed by themselves at present.²⁴

Although this essay was later relied upon by Symes in his paper "On the Method of Political Economy" and was once again returned to in his Outlines of an Industrial Science, it is questionable whether either of these performances came close to the sophistication of Leslie's early essay.

During the eight years following the publication of his "The Love of Money" Leslie turned away from methodological concerns and toward more topical issues. In 1863 he published a lengthy essay entitled "The Wealth of Nations and the Slave Power"²⁵ in which he traced the history and intellectual arguments opposing the institution of slavery. (Leslie thereby anticipated and may have inspired the article on "Slavery and Serfdom" which Ingram prepared for the

ninth edition of the Encyclopedia Britannica in 1885. Even so, Ingram claimed in the book-length revision of this article that it was the first systematic treatment of the subject in English,²⁶ omitting any acknowledgement which might have been due to the then-deceased Leslie.) In the subsequent two years (1864-1865) Leslie published the first of a series of five articles dealing with the quantity theory of money and prices and its application to the economic history of European price fluctuations.

It was not until 1870 that Leslie again took up methodological issues, this time in the context of a history of thought study concerning "The Political Economy of Adam Smith."²⁷ In Smith's writings Leslie discovered a kindred spirit: a concern, like his own, with inquiries into the particular customary and formal institutions which distinguished and differentiated ages and societies and with theories which were themselves built upon these differentiations. Smith, Leslie claimed, was the founder of "historical research" in British economics,²⁸ a claim which was subsequently adopted by many of the British Historical economists. Yet Leslie was not so blinded by the virtues of the Wealth of Nations as to become insensitive to its flaws. He warned that many of Smith's views were the product of an age wedded to a belief in natural law and absolute truth,²⁹ views which he, himself, rejected.

While Leslie acknowledged that natural law arguments had served as a powerful weapon in the liberal "revolt against the tyranny of the folly and inequality of such human codes as the world had known ..."³⁰ (i.e., those of Mercantilism), he argued vigorously against

the retention of natural law appeals as a part of the theoretical underpinnings of "modern" economics.³¹ Smith, himself, was partly excused from the force of these arguments since, according to Leslie, his writings contained:

Two essentially opposite systems of reasoning respecting the fundamental laws of human society ... the former speculating a priori about "Nature," and seeking to develop from a particular hypothesis the "Natural" order of things, the latter seeking to investigate in history and the phenomena of the actual world the different states of society and their antecedents or causes--or, in short, the real as contrasted with the ideal, order of things.³²

And because Smith, unlike many of his followers:

... subjected the phenomena of history and the existing state of the world to a searching investigation, traced the actual economic progress of different countries, the influences of laws of succession, and of the political distribution of property, the action and reaction of legal and industrial changes, and the real movements of wages and profits so far as they could be ascertained. Nor was he content with the inductions of the closet from written evidence--though necessarily the most important field of inductive investigation in social philosophy--he compared all the phenomena which careful personal observation, both in his own country and in France, had brought under his view.³³

For Smith, then, "the Code of Nature" was not the idle daydream of a spinner of social mythology, it was a very real empirical order discovered from the extensive observation of many times and many places.

While Leslie usually assumed a most tolerant and generous attitude in his interpretation of the writings of the founding fathers of economics, including Ricardo, his judgement of those orthodox

writers who were more nearly his contemporaries was not always so beneficent. He discovered in most post-Ricardian economists, with the exception of J. S. Mill, a strain of Continental Rationalism and a priori reasoning à la Descartes and the Scholastics:

That the clearness with which a conception is entertained gives evidence of its truth is a proposition for which the maxim of an illustrious philosopher might be cited: "Credid me," said Descartes, "pro regula generali sumere posse omne id quod valde dilucide et distincte concipiebam verum esse." Modern logic, nevertheless rejects the presumption, and, as Mr. Mill has observed, no one can have examined the sources of fallacious thought without becoming deeply conscious that a nice coherence and concatenation of our ideas are apt to pass off with us for evidence of their truth.³⁴

His acceptance of intuitive certainty as the test for the falsity or correctness of economic hypotheses inexorably led the orthodox economist to a concern with abstract notions: notions which were introduced into the science without the least regard for their connection with observable phenomena. In addition to the concept of "wealth maximization," on which we have already commented, Leslie also scrutinized the "excessive generalization" represented in the concepts of "an equality of wages and of prices," the Ricardian theory of land rent and the equalization of profits under conditions of long run market equilibrium.³⁵ In each of these cases he discovered that the orthodox theory required major modifications, concerned mostly with institutional or customary constraints, before it could become even a rough guide to a discussion of those economic conditions actually observed in the world.

It was due to the eventual recognition of similar criticisms that modern economics would later develop the specialized branches of "industrial organization," "resource economics," "agricultural economics" and other fields dealing with "applied" problem solving. While the orthodox economists may have provided a basic theoretic perspective within which the discipline was able to develop a sophisticated analytic structure, their unwillingness to acknowledge the methodological validity of empirical studies, aimed at a determination of the nature and effects of informal and formal constraints upon the maximizing behavior of individuals and firms, was to significantly impede the progressive development of economics from a social philosophy to a social science.³⁶

Early Empirical Studies

From the foregoing it might be presumed that Leslie was merely a critical intellect, concerned only with launching destructive arguments against the methodological traditions of Nineteenth Century economics. Yet this was far from the case; Leslie desired not only the repudiation of erroneous doctrines, but, further, their replacement by a positive program for continuing economic research. In pursuit of these goals he published in 1864 his historical study of "The Distribution and Value of the Precious Metals in the Sixteenth and Nineteenth Centuries"³⁷ and followed this in 1865 by an article dealing with contemporary data on this same topic.³⁸ The most impressive of Leslie's early empirical studies, however, was his Land Systems and Industrial Economy of Ireland, England and Continental

Countries (1870),³⁹ supplemented in 1869 by an essay on "The Land System of France"⁴⁰ and in 1871 by a lengthy study of "Financial Reform."⁴¹ Leslie had attempted to develop a modified "relativist" perspective on both the history of economic thought and the application of economic theory in his 1870 "Political Economy of Adam Smith" and had argued for a consideration of the importance of structural constraints in this same essay.⁴² Yet it is only within the context of his more empirical studies that we can fully sense the significance which he attached to these doctrines.

In an article on Irish land tenure first published in 1867 and later reprinted as a chapter of his Land Systems, Leslie traced the perpetual poverty and continual instability which plagued the region to the historical and contemporary arrangement of "leases to farmers [which, where they] existed at all, ... were for the most part too short to permit of permanent improvements essential to husbandry being made by tenants."⁴³ Any improvements on the land, he noted, became automatically the property of the landlord at the expiration of the tenant's lease. There were thus no incentives for the workers of the land to either improve the land's productivity or even to preserve any improvements which might have been accomplished by others. Although the solution to these difficulties was, in Leslie's eyes, a simple matter of allowing for the legal enforcement of rental contracts of a longer term and providing to tenants some guarantees of reimbursement for those improvements which they might add to the land,⁴⁴ these proposals were considered by other economists as "interference" with the operations of "free competition."⁴⁵

While Leslie might have considerably strengthened his position and reemphasized his perspective on property structures by an appropriate consideration of the meaning of "free competition" considered in abstracto without reference to a body of laws (or "rules of the game"), he instead chose the Ricardian path of arguing that land-rental contracts could not be free because land was a naturally monopolized resource.⁴⁶

From these rather simple and hardly very original beginnings, however, Leslie began to delve more deeply into the interaction between the institutional or legal framework in which economic actions took place and the quite different forms taken by these actions. In an 1868 essay entitled "Political Economy and Emigration" (also reprinted in his Land Systems)⁴⁷ Leslie abandoned his former attitude of deference toward the prevailing authorities of orthodox political economy. In sharp and uncompromising language he ridiculed their presumptions to universal knowledge without observation:

... a school of economists of no small pretensions, strongly represented in Parliament, supposes itself to be furnished with a complete apparatus of formulas, within which all economic knowledge is comprised; --which clearly and satisfactorily expounds all the phenomena of wealth, and renders all further investigation of the causes and effects of the existing economy of society needless, and even mischievous as tending to introduce doubt and heresy into a scientific world of certainty and truth, discontent and disturbance into a social world of order and prosperity. Political writers and speakers of this school have long enjoyed the double satisfaction of beholding in themselves the masters of a

difficult study, and of pleasing the powers
that be, by lending the sanction of science
to all established institutions and customs
...⁴⁸

It is a matter of some interest that J. S. Mill quoted this passage with approval in his review of Leslie's Land Systems, and he himself reiterated what he believed to be a warning against the too hasty derivation of policy from theory:

The founders of Political Economy have left two sorts of disciples: those who have inherited their methods, and those who have stopped short at their phrases; those who have carried on the work of the masters, and those who think that the masters have left them no work to do. The former follow the example of their teachers in endeavoring to discern what principles are applicable to particular cases, by analyzing its circumstances; the latter believe themselves to be provided with a set of catch-words which they mistake for principles ... which supersede analysis, and are applicable to every variety of case.⁴⁹

In another essay also issued in 1868,⁵⁰ Leslie added to his other interests a fascination with the effects resulting from customary or cultural constraints on such "economic matters" as the choice of a lifetime occupation or the rules of "fair dealing" in the transaction of exchanges.⁵¹ This article was the first of a number of cultural studies of which Mill stated, with obvious oblivion to their true worth, that "No one [besides Leslie] was able to write narratives of foreign visits at once so instructive and so interesting."

It is perhaps no coincidence that during the same period when Leslie was most intensely involved in his research on the legalities

of land tenure and the customary constraints to free trade and the free movement of labor, he also came to consider to wages-fund doctrine as "excessively abstract." Although explicitly exempting Mill from his attack on those who espoused this doctrine (even though Mill's own repudiation of the wages-fund did not appear until over a year later),⁵² Leslie vigorously opposed the continued use of the wages-fund explanation for the determination of an average wage rate.⁵³ In its place he suggested a micro-economic theory of wage determination in somewhat the same vein as had Adam Smith, i.e., that wages were determined partly by the relative bargaining strength of employees and employers and partly by the productivity of labor in the production of goods. Although less explicitly "structuralist" than other of his doctrines, this critique of the wages-fund theory once again illustrated Leslie's omnipresent tendency to reduce theory from a generalized or "philosophic" position to a more applied and testable form.

The Tone and Emphasis of Leslie's Empiricism

Throughout his inquiries into land tenure Leslie always referred to "the facts,"⁵⁴ citing them in whatever form they were available (either as personal observation of the phenomena considered, as historical accounts or as statistical tables). The bulk of his discussion concerning the conditions of land tenure in various nations of Europe did not, however, rely upon "casual observations" of the type so favored by British economists of the turn of the century, but was rather comprised of correspondence

with those who had long inhabited the respective regions and who were thus in a position of greatest familiarity with their institutions and peculiarities. Instead of being repelled by numerical data, Leslie was always at great pains to include it in those instances when it was available.⁵⁵

Leslie was far from disintegrating into a mere collection of statistical data and historical examples, however. Instead of becoming more and more of a narrow historical-statistician, Leslie's interests in property and legal structures was ever on the increase. As late as 1868 he penned what was perhaps his definitive statement concerning the economic effects of the legal structure of land tenure in Ireland.⁵⁶ In 1872 we find him concerned with the effects of geography and distance on the isolation of markets and the smooth operation of a quantity theory of money and prices,⁵⁷ and in 1871 his most sophisticated structural analysis, entitled "Financial Reform," appeared as a contribution to a volume issued by the prestigious Cobden Club.⁵⁸

Although Leslie was ever the firm advocate of increasing empirical research in economics, he was far from the German Historical ideal of the economic historian who collected random bits of data which neither related to, nor were meant to relate to, any unifying explanation of human behavior. As he himself expressed the critique of the purely historical-statistical approach to economics:

It is curious that some who ... regard the numerical statement of facts, and the marshalling of tables of figures as the proper business of the statistician, nevertheless

speak of statistics as science. But as the eminent economist Roscher has observed, numbering or numerical statement is only an instrument of which any branch of science may avail itself, and can never, in itself, constitute a science ...

No branch of science, no scientific body, confines itself to the observation of phenomena without seeking to interpret them or ascertain their laws ... serious error, and even practical mischief, have followed from attention merely to the recurrence of statistical facts without inquiry into their causes.⁵⁹

While "facts" of a specific nature could be useful in testing economic theories and suggesting modifications for the improvement of these theories, facts of a more general type, i.e., those concerned with the basic social structures which differentiated economies, were, Leslie realized, the fundamental concern of the competent theorist. As he himself stated:

No ... theory respecting the effect of consumption on either the nature or the amount of wealth, can be forthcoming without a study of the history and the entire structure of society, and the laws which they disclose ... we need an investigation, not only of the motives and impulses which prompt to the acquisition of wealth, but also of those which withdraw men from its pursuit, or give other directions to their energies.⁶⁰

Yet even in Leslie's writings we find the foreshadowing of the belief in a science of history and of historical laws. In both his empirical work on British and Continental land systems and in his later methodological essays, he paid lip service to the idea that economics "... should investigate the laws of evolution of which the present economic structure and state of (any particular) ... society

is the outcome."⁶¹ Although this orientation never really played any major part in Leslie's research other than to make him sensitive to those features of particular property arrangements which would lead to their increasing stability or instability,⁶² it nevertheless was frequently lurking in the background of his writings, ready to be seized upon and developed by the Ingrams, Cunninghams and Marshalls of future years.⁶³

Leslie's Mature Methodological Views

To a great extent Leslie's mature views concerning methodological subjects were merely a more consistent and completed version of the positions first outlined in his early essays on "The Love of Money," "The Political Economy of Adam Smith" and his various empirical studies. The "Realism of the Schools" as applied to economic concepts was discussed early in the course of an essay Leslie composed on demography entitled "Political Economy and Emigration" (1868):

In few countries (of Europe) ... is this branch of political philosophy (economics) less carefully or commonly studied (than in England), however commonly its terms are in use; and it becomes daily more evident that the air ought to be cleared of clouds of confusion enveloping these very terms. For instead of facilitating thought, as the terms of a science should do, they have come to supersede it; they are taken to settle several problems about which economic inquiry is almost in its infancy; and, what is yet more misleading, they have caused different and even opposite things to be confounded under one name ...⁶⁴

And the extension of his criticism of "realistic" concepts to such

issues as the existence of a wages-fund, the average rate of wages and the long-run equality of profits was discussed at length in his 1873 "Economics and Statistics."⁶⁵ As a replacement for these "generalizations of which the world ... has grown a little doubtful and not a little weary," Leslie suggested that "the collection of statistics and careful inquiry into facts" might lead the economist to "statements which were much closer approximations to the truth."⁶⁶

Even though Leslie recognized that the a priori approach to political economy had been a useful technique for impressing the subject on the minds of the uninitiated and was thus responsible for much of the original esteem attached to the subject, he nevertheless believed that this same methodology had largely inhibited any real scientific growth which might otherwise have occurred during the history of the subject. It was in an attempt to reorient economic investigation toward a more empirical and scientific methodology that Leslie eventually declared war on what he characterized as "deduction" and the excessive use of "deductive techniques."⁶⁷

In evaluating Leslie's attack upon the use of deduction in economic investigations, it must be recognized that he was not directly concerned with those "epistemological" or meta-scientific issues addressed by Whewell. Nor were his remarks intended to exalt history and historical inquiry over "theory," as were the similar-sounding anti-deductive writings of the German Historical School. Leslie was, in fact, quite "moderate" in his stance on this issue, although not oblivious to the passionate debates which

were then raging on the Continent over this very question. "Deduction" for Leslie was by no means to be abandoned in economic research, at least when that term was restricted to strictly inferential reasoning from premises to testable conclusions.⁶⁸ Leslie was mainly anxious that the past damage done to the science by deduction from "false premises" would not continue into the future, that arguments based upon the faulty concepts of an economic man or of wealth maximization would no longer be used in political debates thus bringing scorn to bear on anyone professing to be a political economist.⁶⁹

As we have already mentioned, Leslie was always generous in his evaluation of the founders of political economy, and this attitude was by no means eschewed when he turned to their opinions regarding deduction. Both Smith and Ricardo were explicitly exempted from his strictures against deductivists, Smith for the obvious reasons already discussed, and Ricardo for reasons dissented from by all other British Historical economists. Although Leslie was by no means perfectly sanguine about Ricardo's role in the development of political economy, he did believe that he had been unjustly characterized as the founder and chief advocate of deduction in economics. The differences between Ricardo and Roscher, the founder of the German Historical School, were, Leslie believed, "for the most part, matters of tone rather than of principle."⁷⁰ Similarly, Leslie held that the widespread debate over inductive vs. deductive methods in economics and the other social sciences was somewhat of a misunderstanding. The correct perspective was to view the conflict as

a matter of timing rather than of substance.

For Leslie, then, both inductive techniques (i.e., empirical studies of certain types of economic and economic-related social phenomena) and deductive techniques (i.e., the construction of a systematic body of theory to explain economic activity) were vital to any mature science of society. Deductions (i.e., theories or hypotheses) were, however, properly founded on (or formulated with regard to) "the facts," and the enterprise of fact gathering (of induction) had been largely neglected in British economics since the days of the Wealth of Nations. Induction was, therefore, "the urgent work of the present" for economics had yet to properly attain "the deductive stage"⁷¹ concerning a large number of the questions which it was called upon to answer.

Tendencies, Disturbing Causes and Incomplete Hypotheses

From his basic beliefs concerning the scientific character of economics and the role of inductive and deductive techniques within its methodology, Leslie evolved a number of secondary positions regarding the orthodox doctrines of tendencies, disturbing causes and the incomplete nature of economic hypotheses and predictions. While recognizing that there was some sense in the Classical's talk of "disturbing causes," in that a theory could not usually include all possible variables and thus was subject to changes in the ceteris paribus conditions on which it rested, Leslie stressed the importance of being specific about the types and relative weights attached to any potential disturbing cause.⁷² Any attempt to

"patch up" the predictions of a theory after the failure of the theory he recognized as little more than the arbitrary introduction of ad hoc hypotheses.⁷³ Whereas a more proper procedure might be to search for those particular formal and informal social constraints which adversely affected the consequences predicted by the theory.

Leslie also attacked the orthodox economists who claimed that political economy could not and should not be expected to predict events actually occurring in the world.⁷⁴ He fully recognized the close relationship between this point of view and the characterization of economics as an "incomplete" study of human action, yet his response to this connection was quite different from that of either Cairnes or J. S. Mill. Instead of rejecting the predictive power of economic theory due to its "incomplete" character, Ingram rejected the incomplete and isolated status of economics as a discipline concerned only with "economic" or "wealth-related" motives.⁷⁵ That Leslie sought for a social science capable of dealing with the actualities of human behavior and untied to artificial distinctions between "economic" and "non-economic" behavior is clear from the following passages from his essay on "Political Economy and Sociology":

All men, it may be said, desire health, and "in the absence of disturbing causes" will seek it. But can a science of health be based on this assumption, or the conduct of mankind be predicted from it?

No such principle as "the desire for wealth," in the sense of a single, universal motive, whose consequences are uniform and can be foreseen, really exists. Adam Smith does

not use the phrase, and his doctrine respecting the nature of wealth shows the impossibility of using it as a key to the movements of the economic world.⁷⁶

His attack upon the purely abstract, "philosophic" or "metaphysical" character of orthodox economic speculation is also summarized in several pages in that essay and in a later study entitled "The Known and the Unknown in the Economic World." We again quote a few short excerpts for reasons of illustration:

Mr. Cairnes ... concurs with Mr. Mill that positive, unconditional conclusions are beyond the reach of the economist, since he does not take into account, or even know, all the forces at work, much less can measure them with precision. An entire lecture in Mr. Cairnes' Logical Method of Political Economy is devoted to prove that quantitative exactness is unattainable in the science, and that its conclusions being only hypothetically true, and representing only several tendencies "in the absence of disturbing causes" ought not to affect the semblance of numerical exactness.⁷⁷

Political economy, (the orthodox economist) tells you, with an air of offended dignity, is a science of tendencies in the long run, and in the absence of disturbing causes; it does not predict in individual cases. A great general used to say that a man who was good at excuses was never good for anything else; and nearly as much may be said of a theory.⁷⁸

Miscellaneous Methodological Issues in Leslie's Writings

In concluding this evaluation of Leslie's methodology, it is desirable to consider briefly two of the more minor, but still well integrated, features of his general perspective on social investiga-

tions. First of all, Leslie, like most Nineteenth Century economists of both the Orthodox and Historical Schools, was a "relativist" in matters of the history of economic thought. "Throughout the history of political economy," he wrote, "... the personal history, education, and character of particular writers has borne no small part in its developments and forms."⁷⁹ And, indeed, it appears that he was somewhat attracted to J. S. Mill's characterization of economics as a "mentalistic" science, as well as to psychologistic investigations into the social-environmental "causes" for the proffering of particular social theories. Although this extreme form of relativism, which might be labelled "epistemological relativism," was common in the writings of the later British and German Historical Schools, it was in no way implied by other of Leslie's own views. It can only be usefully interpreted as a support for his views concerning social and economic policy (i.e., as a subtle form of ad hominem to be used against political opponents.

Finally, Leslie must be understood as both a social scientist and a social theorist. He clearly recognized that economics played a role not only in determining the "truth or falsity" of certain hypotheses, but also "as a factor in the formation of public opinion and policy."⁸⁰ Although there is some indication that Leslie regretted this dual character of social theories, he was quite ready to act upon it and to voice his own views concerning the optimal set of social policies. Here again the main difference between Leslie's involvement in policy issues and the similar involvement of his

contemporaries in both the Orthodox and Historical camps was that Leslie seldom claimed the authority of either "absolute theory" or "inevitable historical trends" in justification for his opinions. Although often passionate in his denouncement of contemporary social practices and institutions,⁸¹ Leslie was always at pains to emphasize the conditional and transitory character of the knowledge which social science could contribute to the arena of public controversy.

Concluding Remarks

Leslie was unquestionably the last of the truly great figures in the British Historical tradition. Although economics would probably have been more deeply influenced by the dogmatic treatises of J. K. Ingram, the popularizations of Walter Bagehot or the ponderous tomes penned by various economic historians around the turn of the century, it was Leslie's writings that provided the inspiration for whatever remnants remained of an empirical economic methodology. The supporters of a nominally "historical economics" could be found in British academies for many decades after the 1880's, but the movement no longer possessed any of its former dynamic originality. Its advocates were restricted to mouthing worn and often irrelevant slogans directed against an "Orthodox economics" which had long since died and been resurrected in new clothing by Marshall and the early Neoclassicals. The "historical economists" of the closing decades of the Nineteenth Century would either be justifiably ignored as cranks, crackpots and methodological quacks or they would be, often just as justifiably, lumped into a heterogeneous category

containing Marxists, Georgists and other assorted political malcontents. The burning intelligence and dedication to authentic social science which Leslie brought to his study of economics would not again be seen until well into the Twentieth Century, and in the interim all memory of the fundamental character of his methodological critiques would be erased from the consciousness of new generations of economists. F. A. Hayek has observed that of all the periods of intellectual development, today's scholars are least familiar with the mid and latter Nineteenth Century, especially in Britain.⁸² Yet it was during that period that most of the basic meta-economic attitudes still dominating our science first received their systematic formulation. It is in the debates of that period that we can first recognize the "as if" doctrine of Milton Friedman, the "operationism" of T. W. Hutchison and the extreme rationalism of Mises and Machlup. It was during this period that the irreverent attitudes of many economists toward "empirical realities" first gained professional support, and it was during this period that the case for economics as a science of actual human behavior and existing social structures was first openly defended. To reduce the debates of the period, especially those debates centering around figures such as Leslie and Whewell, to a conflict between "historians and theorists" is to entirely obfuscate the true issues at hand. It was in the writings of Leslie and of his antagonist, J. E. Cairnes, that those issues were most lucidly stated.

Footnotes to Chapter IV

1. J. A. Schumpeter, A History of Economic Analysis (New York: Oxford University Press, 1954), p. 823. Despite Schumpeter's high regard for Leslie's empirical work, however, his assessment of his overall methodological position was somewhat less than totally accurate. He says in part:

The two papers that present his methodology or, as he preferred to call it, philosophy of social sciences ... read much like a reformulation of the Schmollerian program; in view of the dates of their first publication (1876 and 1879) this should not induce us to deny them originality.

Ibid.

As we will show conclusively in this chapter and the supporting footnotes, Leslie's point of view had very little in common with "the Schmollerian program" and was formed long before 1876.

2. A brief account of Mill's assessment of Leslie as an economist and as a journalist is presented in J. K. Ingram's "Biographical Notice of the Author" appended as a preface to T. E. C. Leslie's Essays in Political Economy, 2nd edition, original printing 1888 (New York: Augustus M. Kelley, Publishers, 1969), especially pp. x-xi.

Mill's review of Leslie's Land Systems and Industrial Economy of Ireland, England and Continental Countries (London: Longmans, Green and Co., 1870) is reprinted in J. S. Mill, Collected Works, Vol. V, one of two volumes appearing under the subtitle of Essays on Economics and Society, edited by J. M. Robson with an introduction by Lord Robbins (Toronto: University of Toronto Press, 1967), pp. 669-685. The evaluation of Leslie as "one of the best living writers on applied political economy ..." appears on p. 671.

3. In his "Biographical Notice," op. cit., J. K. Ingram refers to Leslie as "... one of the ablest and most original English economists of the present century; and in his History of Political Economy (New York: Augustus M. Kelley, Publishers, 1967) he favorably reviews his works and compares him to Comte, pp. 222-225. See also p. 141. Ingram's treatment of Smith's economics refers explicitly to Leslie several times (Ibid., pp. 87, 107, 109), and it obviously owes a great deal more to Leslie's pioneering study than is indicated in these references.

4. See the previous footnote for Ingram's references to Leslie in his treatment of Smith.

5. L. H. Haney's History of Economic Thought, 4th enlarged edition (New York: Macmillan, 1949), pp. 529-532, 540, contains a brief but excellent sketch of some of Leslie's main methodological

positions and his general political perspective.

T. W. Hutchison's A Review of Economic Doctrines, 1870-1949 (Oxford: Clarendon Press, 1953) also contains numerous references to Leslie and to the British Historical School in general, but it does not contain any extensive consideration of his meta-economic views.

6. William A. Scott, The Development of Economics (New York: D. Appleton-Century Co., 1933), pp. 510-514.

7. Of the leading histories of economic thought, Mark Blaug's Economic Theory in Retrospect, revised edition (Homewood: Richard D. Irwin, 1968), Eric Roll's A History of Economic Thought, 3rd edition (Englewood Cliffs: Prentice Hall, 1964) and Jacob Oser and William C. Blanchfield's The Evolution of Economic Thought (New York: Harcourt, Brace and Johanovich, 1975) all omit any reference to Leslie.

8. R. B. Ekelund, Jr. and R. F. Hebert, A History of Economic Theory and Method (New York: McGraw-Hill, 1975), pp. 199-201, contains a discussion of Leslie in conjunction with a broader discussion concerned with J. K. Ingram and Auguste Comte.

9. Leslie's essay "On the Philosophical Method of Political Economy" was first published in Hermathena, Vol. ii (1876) and is reprinted in his Essays in Political Economy (hereafter abbreviated as Essays), pp. 163-190.

10. Walter Bagehot, "The Postulates of English Political Economy," Fortnightly Review, Vol. 19 (1876), pp. 215-242, 720-741.

11. David Symes, Outlines of an Industrial Science (London: Henry S. King and Co., 1876). See chapter 5, page 101 of this dissertation for a discussion of Symes' lack of notice in England.

12. T. E. C. Leslie's "The Love of Money" is reprinted in his Essays, op. cit., pp. 1-8.

13. David Symes, "On the Method of Political Economy," Westminster Review, N.S., Vol. 40 (July, 1871), pp. 206-218.

14. David Symes, Outlines of an Industrial Science, op. cit., p. ix.

15. T. E. C. Leslie, Land Systems and Industrial Economy of Ireland, England and Continental Countries, op. cit. (hereafter abbreviated as Land Systems).

16. Walter Bagehot, Economic Studies (Stanford: Academic Reprints, 1963), pp. 66fn.

17. Beyond several scant citations, J. N. Keynes, the methodologist of the early Neoclassicals, hardly recognizes Leslie's existence. When he does, he is quick to note that "the problems which he (Leslie) asserts are left entirely unsolved by the deductive method are mostly of a purely historical character," and that Leslie's own attempts at problem solving "constantly imply or presuppose the use of a deductive or a priori method of reasoning on fundamentals" (J. N. Keynes, The Scope and Method of Political Economy, 4th edition (New York: Macmillan, 1926), p. 318fn.) See also Ibid., pp. 314-315, 321 where Keynes attempts to establish a link between Leslie and the later German Historical School.

In appreciating the attitudes of the Cambridge Neoclassicals toward the Historical School, it is also of some significance that Marshall, in attempting to maintain his usual pose of impartiality in correspondence with Foxwell, described his own views as follows:

Most of the suggestions which I made on the proofs of Keynes' Scope and Method were aimed at bringing it more into harmony with the views of Schmoller ... It still remains true that as regards method I regard myself midway between Keynes + Sidgwick + Cairnes and Schmoller + Ashley.

R. H. Coase, "Marshall on Method," Journal of Law and Economics, Vol. XVIII, No. 1 (April, 1975), pp. 27-28.

The only "historical" alternative to the orthodox position of "Keynes + Sidgwick + Cairnes" was, thus, in Marshall's mind, the evolutionary history of Ashley or the pseudo-Marxism of Schmoller.

18. See Ingram's "Biographical Notice" appended to Leslie's Essays, op. cit., pp. xix-x. A short sketch of Leslie's life and writings is also to be found in Henry Higgs (ed.), Palgrave's Dictionary of Political Economy, Vol. II (London: Macmillan, 1926), pp. 596-598 and in Sir Leslie Stephens and Sir Sidney Lee (eds.), Dictionary of National Biography, Vol. XI (London: Oxford University Press, 1921), pp. 987-988.

19. T. E. C. Leslie, Essays, op. cit., p.xfn.

20. Ibid., p. 3; see also Essays, p. 166 and Land Systems, op. cit., p. 85.

21. Thus, for instance, Leslie states, "No other branch of philosophy is still so deeply tinged with the realism of the schools as economic science." (Essays, op. cit., p. 166) See also Land Systems, op. cit., p. 85.

22. "On the Love of Money," contined in Essays, op. cit., pp. 1-8.

23. The theme is a familiar one taken up by Symes in his "On the Method of Political Economy" and his Outlines of an Industrial Science and referred to in a lesser extent by Bagehot in his Economic Studies. Yet Leslie's illustrations of this principle, which may, in part, have been drawn from Maine's lectures, are of special interest for the systematic way in which they arrange the central motivational objects of different types and different stages of society. Thus pastoral and nomadic peoples hold wealth in livestock or in those items which are highly portable, and their descendents may follow suit for numerous generations after this practice has ceased to be functional. Agricultural peoples, however, develop wealth holdings primarily in the form of land and large families, while urban dwellers hold wealth in a multiplicity of forms, depending on the political stability and the cultural backgrounds of their populations.

24. Essays, op. cit., p. 3.

25. Leslie's article on "The Wealth of Nations and the Slave Power" first appeared in the February, 1863, issue of Macmillan's Magazine and is reprinted in his Essays, op. cit., pp. 9-20.

26. Ingram stated of his Encyclopedia Britannica article on "Slavery" that "That article was, so far as I am aware, the first attempt in English to give a complete account of slavery and serfdom in ancient, medieval and modern times." (J. K. Ingram, A History of Slavery and Serfdom (London: Adam and Charles Black, 1895), p. ix.) To take this claim at all seriously, however, we must place the stress solely on the modifying clause "a complete account of slavery in ancient, medieval and modern times," for not only had Leslie's essay appeared in 1863, but J. E. Cairnes' massive study of slavery in the Americas was published and widely known in 1862, twenty-three years before Ingram's article appeared in the 1885 Encyclopedia Britannica. For Cairnes' contribution to this issue, see his The Slave Power (New York: Carleton Publishers, 1862).

27. Leslie's "The Political Economy of Adam Smith" first appeared in the Fortnightly Review of November 1, 1870, and was reprinted in his Essays, op. cit., pp. 21-40.

28. Leslie stated of Smith's approach to economic inquiry:

... his method, though combining throughout a vein of unsound a priori speculation, was in a large measure inductive.

Essays, op. cit., p. 23.

See also the quote from Leslie's Essays reproduced on p. 107 of this chapter and Essays, op. cit., p. 37. In his earlier essay, "The Wealth of Nations and the Slave Power," Leslie had also expressed his appreciation for the historical character of much of Smith's

writings (Essays, op. cit., pp. 15-16).

29. In referring to Smith's belief in natural law and his tendency toward an absolutist conception of social theory, Leslie stated that:

What he did not see was that his own system, ... was the product of a particular history; that what he regarded as the System of Nature was a descendant of the System of Nature of the ancients, in a form fashioned by the ideas and circumstances of his own time, and coloured by his disposition and course of life.

Essays, op. cit., p. 22.

30. Ibid.

31. According to Leslie, the Code of Nature was a descendant of the Greco-Roman belief in a pre-societal order. In all of its "variety of forms and disguises" it involved "one fundamental fallacy, of reasoning a priori from assumptions obtained, not by the interrogation but by the anticipation of Nature; what is assumed as Nature being ... a mere conjecture respecting its constitution and arrangements." All the various reformulations of this doctrine undertaken in the Eighteenth and early Nineteenth centuries only helped "to thicken the confusion perpetually arising between the real and the ideal, between that which by assumption ought to be and that which actually is." (Essays, op. cit., pp. 24-25.)

32. Essays, op. cit., pp. 23-24, 31. From Smith, Leslie believed, had descended:

...two systems of political economy ... one ... reasoning entirely from hypothetical laws or principles of nature, and discarding induction, not only for the assessment of its premises, but even for the verification of its deductive conclusions; the other ... reasoning sometimes, it is true, from pure hypotheses, but also from experience and shrinking from no corrections which the test of experience may require in deductions. Of the two schools distinguished by their methods, the first finds in assumptions respecting the nature of man, and the course of conduct it prompts, a complete "natural" organization of the economic world, and aims at the discovery of "natural prices," "natural wages," and "natural profits."

Essays, op. cit., p. 24.

33. Essays, op. cit., p. 33. For Leslie's interpretation of Smith's Code of Nature as ultimately empirical, see Ibid., p. 35.

34. Essays, op. cit., p. 143.

35. Leslie's criticism of the doctrine of a "tendency to equality" in both wages and profits was first expressed in his essay on "The Political Economy of Adam Smith" in Essays, op. cit., pp. 37-39. He stated, in part, that, "The truth is that the doctrine of a tendency to equality is a mere theorem in political economy; and a theorem which imports the tendency only under special conditions ... conditions the opposite of those which prevail in the present industrial world." (Essays, op. cit., p. 39.) It was not until much later that Leslie sought to make specific all those conditions required for a movement toward equilibrium. Among the conventional considerations dealing with monopolized markets, institutional constraints, the effects of distance on market separation and dynamic effects of economic growth (Ibid.), Leslie would eventually "center in" upon the assumption of perfect knowledge. Not only did he believe that this assumption obscured the true dimensions of economic decision-making, but he was especially concerned to stress its increasing inapplicability in consideration of advanced societies, rather than simple tribal groups:

The full knowledge and foreknowledge lately claimed for political economy in modern commercial society can exist only at an opposite stage of development, at which human business and conduct are determined, not by individual choice, or the pursuit of wealth, or commercial principles, but by immemorial ancestral customs.

Essays, op. cit., p. 222.

It might ... be not irrationally conjectured that in a little village at the present day every man knows all his neighbors affairs. To jump from that to the conclusion that everybody in England knows the affairs of everybody else is the leap that Ricardo and his followers have made.

Ibid., p. 232.

(It is rather startling that Leslie seems to have had a well-developed notion of the concept of a market as a social institution "the consequence of human action, but not of human intention" some seventy years before Hayek developed this idea into his critique of socialism and centrally planned economies:

It is a fundamental error of the a priori or deductive political economy that it takes no

cognizance of the cardinal fact that the movement of the economic world has been one from simplicity to complexity, from uniformity to diversity, from unbroken custom to change, and, therefore, from the known to the unknown.

Essays, op. cit., p. 224.)

For other references to Leslie's critique of the perfect knowledge assumption of early Neoclassical economics, see his Essays, op. cit., pp. 228-229; and for the extension of this argument to the quantity theory of money, to which he preferred a theory of regional price changes and an examination of the determinants of price levels between market areas, see his essay on "The Distribution and Value of the Precious Metals in the Sixteenth and Nineteenth Centuries," Essays, op. cit., pp. 269-300.

Perhaps the most famous of Leslie's attacks on the "generalizations" of orthodox political economy was his critique of the wages-fund theory, which first appeared in his "Political Economy and Emigration," reprinted in his Land Systems, op. cit., pp. 85-116 (see especially pp. 87-88), and which was subsequently expanded upon in his 1868 essay on "political Economy and the Rate of Wages," reprinted in Land Systems, op. cit., pp. 357-379. It was further systematized and refined in his review of Cairnes' Leading Principles (1874), reprinted in Essays, op. cit., pp. 41-53 (see especially pp. 44-46), and was finalized in his "The Movement of Agricultural Wages in Europe," (1874), reprinted in Essays, op. cit., pp. 364-383 (see especially pp. 379-383).

36. I do not mean to imply by these comments that the economists of the early British Historical School were "without error." Their involvement with historicism, in Popper's sense, was certainly to their demerit as was their often excessive appeals to a purely "verbal realism" (discussed in the concluding chapter of this dissertation). Leslie, while frequently over-zealous in his attacks on orthodox writers of his own time, was, however, seldom at fault in any of these more common ways. His most grievous error was to become excessively involved with the sociology, or better, the psychology, of social investigation and to too frequently partake of the psychological interpretation of political economy emphasized and developed by J. S. Mill.

37. Essays, op. cit., pp. 269-300.

38. "The New Gold Mines and Prices in Europe," first printed in the June, 1865, issue of the North British Review and later reprinted in Essays, op. cit., pp. 301-331.

An even later article concerned with a summary of previous arguments and their application to a more limited question was "Prices in Germany in 1872," Fortnightly Review (November 1, 1872),

also included in Essays, op. cit., pp. 332-355.

39. T. E. C. Leslie, Land Systems, op. cit.

40. T. E. C. Leslie, "The Land System of France," appearing in J. W. Probyn (ed.), Systems of Land Tenure of Various Countries, A Series of Essays Published Under the Sanction of the Cobden Club, new edition, revised and corrected (London: Cassell, Petter, Galpin and Co., 1869), pp. 291-312. Leslie demonstrated once again his concern with alternative property structures in his first introductory paragraph to this essay:

The object of this essay is to describe the Land System of France in respect of the distribution of landed property in that country, with the rural organisation in which it results, and to examine its causes and effects. In considering its causes, laws and customs relating to property (including succession and transfer), and to tenure, of necessity form prominent objects of inquiry; but their operation is so bound up with that of economical causes and conditions, that we should miss in place of obtaining clearness by separating what may be termed the legal from the economical class of subjects ...

Ibid., p. 1.

41. T. E. C. Leslie, "Financial Reform," appearing in Cobden Club Essays, Second Series, 1871-1872 (London: Cassell, Petter and Galpin, 1872), pp. 189-264. This is by far the best example of Leslie's skills as a structural political economist. Although the amount of material available for a study of questions concerning customs and excise taxes was, no doubt, of vast proportions, Leslie's essay skillfully combined the central points which should be found in such a study with an unusually rich assortment of original suggestions. In the first few pages of the essay we discover an anticipation of Hayek's conception of the market as a vast and supra-intelligible calculating machine (see fn.35 of this chapter for an additional reference to this same concept) and a noteworthy expression of the little-researched connection between changing prices and long-run effects on changing tastes (Ibid., pp. 195, 200). Also included in the essay are anticipations of Mises' doctrine of "the effects of prior market intervention as a justification for further intervention" (Ibid., p. 206), an appreciation for the intra-national redistributive effects of tariff legislation (Ibid., p. 213), numerous examples of the inflexibility of bureaucratic administration of economic affairs (Ibid., pp. 225-227) and a realization of the increased administrative and uncertainty costs borne by those businesses liable to possible government intervention

(and thus the flow of capital funds from these enterprises to others) (Ibid., p. 240).

42. Leslie's expressions of the historical relativism of economic doctrines sometimes did border upon an assertion of the historical relativity of all social science. In his essay on "The Political Economy of Adam Smith," for instance, he states that:

I venture to maintain ... that political economy is not a body of natural laws in the true sense or of universal and immutable truths, but an assemblage of speculations and doctrines which are the result of a particular history, coloured even by the history and character of its chief writers; that, so far from being of no country, and unchangeable from age to age, it has varied much in different ages and countries, and even with different expositors in the same age and country ...

Essays, op. cit., p. 21.

Later in this same essay he says of Adam Smith that, "... had he lived even two generations later, his general theory of the organization of the economic world and the results of the competition for economic life would have been cast in a very different mode." (Essays, op. cit., p. 39)

43. T. E. C. Leslie, "The State of Ireland in 1867," reprinted in Land Systems, op. cit., pp. 5-33 (see especially p. 14).

44. Ibid., pp. 28-29.

45. It is noteworthy that J. S. Mill defended Leslie's stand in his review article on the Land Systems, and that he was particularly sharp in rebuking those who "believe themselves to be provided with a set of catch-words, which they mistake for principles--free-trade, freedom of contract, competition, demand and supply, the wages-fund, individual interest, desire of wealth &c.--which supersede analysis, and are applicable to every variety of cases without the trouble of thought." (J. S. Mill, Collected Works, op. cit., p. 671.)

Leslie's flexibility in considering matters of economic legislation and his scientific attitude toward even those issues with which he was most passionately involved is well illustrated by his refusal to become caught up in the issue of which form of economic organization was "best" in agriculture. As he expressed this matter in another of his 1867 essays entitled "The Peasantry and Farms of Belgium, 1867":

... to Mr. Harrison's question--"Are small farms or large farms best?"--we answer, Both

are best. Not only because there are in all countries ... places specially adapted for each, but also because the existence of both creates various experiments and improvements, which may be transferred from one to the other ...

Land Systems, op. cit., p. 337.

46. Unfortunately, Mill's defense, like Leslie's original argument, turned on the "natural monopoly" characteristics of land ownership rather than stressing that there was no such thing as "free competition" without the prior specification of a legal code (a property rights structure) for defining the rules of legitimate competition. For Leslie's argument, see Land Systems, op. cit., p. 28; and for Mill's discussion, in a similar vein, see his Collected Works, op. cit., pp. 672-673. Mill even went so far as to rely upon a mixture of arguments made popular by Locke, and later by Proudhon, arguments which maintain that land is properly a social good which has been provisionally allocated to the care of private individuals as a public trust.

47. Leslie's article on "Political Economy and Emigration" was originally published in Fraser's Magazine for May, 1868, and is reproduced in Land Systems, op. cit., pp. 85-116.

48. Land Systems, op. cit., pp. 89-90.

49. J. S. Mill, Collected Works, op. cit., pp. 671-672. In the extension of his remarks, Mill states:

May I venture to suggest that there are no such principles of political economy as those which (Leslie's critics) believe themselves to be violating? The principles of political economy, as of every other department of knowledge, are a different thing from its practical precepts. The same principles require different precepts, wherever different means are required for the same ends ...

Ibid., pp. 674-675.

50. For Leslie's analysis of the customary constraints operating on the residents of a small farming village, see his "Ireland in 1868," Land Systems, pp. 39-40. A further example dealing with the case of the informal (non-legal) institution of primogenitur and of the differences in job choice between eldest and younger sons of any given family is found in Leslie's "Auvergne" (1874), reprinted in his Essays, op. cit., pp. 415-437 (see especially, pp. 419-421).

51. It was during this same period that Leslie came to oppose

economic a priorism and to connect it with the "realism of the Schools," which he had formerly attacked in his 1862 "The Love of Money." We quote the central passage:

In no other branch of philosophy indeed, unless metaphysics itself, does the ancient mist of realism continue so to "darken counsel by words without knowledge." A resemblance has been seen by a philosopher in a number of different things viewed in one particular light, and a common name has been given to them with reference only to that point of resemblance ... In like manner, a phrase used at first to signify merely a tendency of things under particular conditions comes to stand for a universal law or principle of nature, and a generalization, which originally threw a new light upon phenomena, finally involves them in almost impenetrable obscurity.

"Political Economy and Emigration" (1868, reprinted in Land Systems, op. cit., pp. 85-86.

The quote from Mill is found in Ingram's "Biographical Notice" to Leslie's Essays, op. cit., pp. x-xi.

52. J. S. Mill's recantation of the wages-fund doctrine is to be found in his "Thornton on Labour and its Claims," Fortnightly Review, Vol. 32 (May and June, 1869), pp. 505-518, 680-700.

53. See Leslie's "Political Economy and the Rate of Wages," Land Systems, op. cit., pp. 362fn. Leslie was not unaware of the differences between a macro theory concerned with labor's share of GNP (see Ibid., p. 361) and a micro theory of wage determination in particular occupations or localities, but he clearly preferred the latter, and, indeed, seems to have believed that the former was nearly useless.

54. We must, of course, be cautious when commending those who speak with great fervor of "the facts," for a criticism of "abstract theorizing" based on "the facts" may be just as misplaced as the error being criticized. We must first establish some sort of rules or procedures to guide us in the determination of what does and does not count as a relevant fact, i.e., one which will cause "significant" modifications in our predictions and thus should be explicitly considered in our theory. Without such procedures we are only engaging in word games over what seems, to us, as an "important" element of "reality," viz., we are engaged in the same type of misplaced metaphysics as the absolutist concern with the model of profit maximization and perfect knowledge.

55. Examples of Leslie's use of correspondence and other supporting documents are far too numerous for citation. However, it

may be mentioned that in his Land Systems alone there are at least eleven instances of his inclusion of significant amounts of numerical data (see Land Systems, op. cit., pp. 62, 65, 68, 69, 71, 72, 92, 98, 103, 105 and 313).

56. See Leslie's "Ireland in 1868," op. cit.

57. See Leslie's "Prices in Germany in 1872," Essays, op. cit., pp. 332-355, especially pp. 333-334, 339-342.

58. See footnote 41 of this chapter for a discussion of Leslie's essay on "Financial Reform."

59. Essays, op. cit., p. 156. Leslie was, in many ways, enthusiastic about the union of economics and statistics, despite his caveats concerning an overemphasis on this empirical tool. As he saw the matter:

The formal incorporation of economic science with statistics ... tends to correct the error to which economists as well as that to which statisticians are specially prone. If the latter have been prone to think only of facts, it has been the besetting sin of the former to neglect facts altogether ... if statisticians have often been content to collect phenomena without heed to their laws, economists more often still have jumped to the laws without heed to the phenomena; if statistics have [sic] lain chiefly in the region of dry figures and numerical tables, economics have [sic] dwelt in the region of assumption, conjecture and provisional generalization, which other sciences, indeed--geology to witness--have not escaped, but from which they are triumphantly emerging by combining the closest observation of phenomena with the boldest use of speculation and scientific hypothesis.

Essays, op. cit., pp. 157-158.

Although quite lengthy, the above extract is especially worthy of attention, not only for the light which it throws on Leslie's mature attitudes toward the use of statistical tools in economics but also as a summary of his entire methodological position.

60. Essays, op. cit., p. 172.

61. See Scott, The Development of Economics, op. cit., p. 513 and Essays, op. cit., pp. 175, 212, 210.

62. The tendency in Leslie's writings to employ an analysis

of economic structures as a guide to their future change or "evolution" (in a non-dialectical or Darwinian sense of that term) is quite similar to the modern turns in the economics of politics; see, for instance, James Buchanan's The Limits of Liberty, Between Anarchy and Leviathan (Chicago: University of Chicago Press, 1975).

63. In regard to his occasional expressions of belief in a theory of social evolution, Leslie might well be associated with Comte as, indeed, he has been by several authors. Yet Ingram was probably correct in classing him in with the non-Positivists (J. K. Ingram, A History of Political Economy, op. cit., p. 157). It is true, however, that on those occasions when Leslie did refer to Comte, he had nothing but words of praise for his work (see Essays, pp. 213-215). Yet it is possible to find many more references in his writings to the German Roscher or the Frenchman Leonce De Lavergne, to say nothing of English writers, than to Comte.

64. Land Systems, op. cit., p. 85.

65. Essays, op. cit., pp. 158-159.

66. Ibid.

67. The debate over "deduction vs. induction" in economics was subject to continual misunderstandings from its origins in the writings of Whately and Whewell to its eventual disappearance in the early decades of the Twentieth Century. Regarding this debate, however, at least two major interpretations and three major points of view may be readily distinguished. Leslie and most other British Historical economists interpreted the question as either: (1) having to do with the choice of premises or axioms for economic models, i.e., whether the premises were in accord with established facts or contradicted them (the "inductive" or "historical" concern) or whether they were "intuitively appealing" and sufficiently "simple" for ease of manipulation (the "deductive," "a prioristic" or "orthodox" approach), or as (2) having to do with the claim of empirical truth (as opposed to inferential validity) which was often advanced for theorems derived from a prioristic systems. In either of these interpretations the Historical economists were opposed to "deductivism," believing that premises should be chosen with due regard for "the facts" so they were not immediately falsified by them, and also believing that any of the deductive consequences of higher level theories were properly candidates for testing (that they were not necessarily true just because they were derived from "intuitively obvious" premises). In many cases the Historical economists also opposed higher level theories, believing that any theory not immediately connected with observable phenomena was little more than metaphysical speculation about the ultimate essence of things.

The other major interpretation of the issue debated under the label of "induction vs. deduction" was shared in common by the

early Neoclassicals (i.e., Marshall and Keynes) and by the later German Historical School of Schmoller. According to this interpretation deduction meant little more than the extensive use of inferential reasoning (Marshall's "long chains of reasoning") and induction meant a refusal to engage in (explicit) inferential reasoning, relying instead on "history" and "the facts" to structure themselves. This interpretation of inductive methods was, in short, little more than a return to the belief in a mystical nature-force, or a Hegelian Weltgeist, which underlay and directed the flow of historical events. While the German Historical authors of the time of Schmoller were almost exclusively caught up in the cult of induction, Marshall would, as usual, choose to compromise between the two views (as he understood them). In his recent article on "Marshall on Method" in the Journal of Law and Economics, Vol. XVIII, No. 1 (April, 1975), pp. 25-31, R. H. Coase has noted that although Marshall had declared himself to be opposed to "philosophical economics" he still believed that both "induction" and "deduction" had their places in economic inquiry (Ibid., p. 27). Of course, he was never very clear in defining either of these two terms and, as Coase has noted, never really tried except in the structure of his outline of an ideal course of study (Ibid.).

68. In attempting to clarify his critique of deductive methods Leslie states, in a review of Jevon's Theory of Political Economy, that:

We are, it is true, for deletion of the deductive method of Ricardo: that is to say, of deduction from unverified assumptions respecting "natural values, natural wages, and natural profits." But we are not against deduction in the sense of inference from true generalizations and principles, though we regard the urgent work of the present as induction ...

Essays, op. cit., p. 72.

and in his essay on "Political Economy and Sociology" Leslie expands upon this notion of induction and presents his alternative:

The deductive theory of wages, profits, prices, rents and taxation is substantially a set of predictions respecting the distribution of wealth, which affects to foretell exactly the gain in every business and the rates at which goods of every kind will be sold. It has been well said that before predicting the future, we must learn to predict the past; and before predicting the past, it might be added, we should learn to predict the present, by studying the forces at work in the world around us, the conditions under which they operate, and their actual results.

Essays, op. cit., p. 203.

Finally, in "Economic Science and Statistics," Leslie clearly differentiates between his own views and the "inductivist" approach of many Baconian oriented philosophers:

(Quetelet) assumed that by enlarging the number of instances we eliminate chance, and arrive at general or stable laws or conditions. But a great number of instances does not give us their law or justify us in any positive conclusion respecting the future.

Essays, op. cit., pp. 161-162.

69. Leslie was keenly aware of the harm done to the reputation of political economy by the absolutist arguments of dogmatic free-traders; and, although his own political sentiments were often in accord with theirs (see the references to his "Financial Reform," op. cit.), he was anxious to free economics from any such normative associations. As he expressed the matter, it was not necessary that economists have immediate knowledge of the total structure of the social world or that they be able to offer sweeping statements on social policy, but only that they follow "a right method" in the progressive development of their science (Essays, op. cit., p. 215). For further warnings against premature construction of elaborate and all-embracing deductive systems, see Essays, op. cit., pp. 213, 214.

70. Essays, op. cit., p. 96.

71. Essays, op. cit., pp. 72, 241.

72. Essays, op. cit., p. 197. In speaking of the "disturbing causes" or "frictions" which Orthodox economists constantly referred to in justification of their theories, Leslie stated:

The real defect of the treatment by economics of these other principles (or disturbing causes) is, that it is superficial and un-philosophical; that no attempt has been made even to enumerate them adequately, much less to measure their relative force in different states of society; ... they are emphasized simply to prop up rude generalizations for which the authority of "laws" is claimed.

Essays, op. cit., p. 173.

And once again, "... with respect to the deductive economist's practice of setting aside a number of forces as 'frictions,' ... the best corrective would be that this so-called friction is capable of scientific analysis and measurement ..." (Essays, op. cit., p. 193.)

73. For at least one of Leslie's comments concerning ad hoc hypotheses, see Essays, op. cit., p. 197. This critique, indicating the "saving effects" of calling upon disturbing forces or the partial nature of economic deductions, is reemphasized at many points in both the Essays and the Land Systems as, for instance, in the following:

(these "other principles" to which political economists often appeal) ... serve, along with other conditions, to give some sort of support to saving clauses--such as "allowing for differences in the nature of different employments," "caeteris paribus," "in the absence of disturbing causes," "making allowance for frictions"--by which the "law" that wages and profits tend to equality eludes scrutiny.

Essays, op. cit., p. 173.

74. See p. 121 of this chapter and fns. 76 and 77 below. The following passage, although somewhat lengthy, is also instructive:

A bone fairly enough represents the sort of wealth coveted by a dog, who has a comparatively simple cerebral system, and few other objects. Yet you cannot predict the conduct even of a dog from his love of bones, or not one would be left in the butchers' shops. The dog has a regard for his master and a fear of the police, and he has other pursuits ...

Everybody, it might be affirmed, loves virtue "in the abstract," and "in the absence of disturbing causes" would be virtuous; yet, policemen, prisons, and the Divorce Court show that no theory of morals, much less absolute predictions, can be drawn from this abstract principle.

Essays, op. cit., p. 198.

75. The Comtian program of submerging economics into a general science of society was frequently referred to in Leslie's writings, mainly in connection with his realization of the impossibility involved in both (1) a science which dealt strictly with the maximization of wealth and (2) a science which was at the same time predictive and descriptive of human action. Thus we find the following passage among many similar ones:

Political economy is ... a department of the science of society which selects a special

class of social phenomena for special investigation, but for this purpose must investigate all the forces and laws by which they are governed. The deductive economist misconceives altogether the method of isolation permissible in philosophy. In consequence of the limitation of human faculties, not that the narrowing of the field is in itself desirable or scientific, it is legitimate to make economic phenomena ... the subject of particular examination, provided that all causes affecting them be taken into account. To isolate a single force, even if a real force and not a mere abstraction, and to call deductions from it alone the laws of wealth, can lead only to error, and is radically unscientific.

Essays, op. cit., p. 212.

Yet sometimes the theme of a unified social science was interwoven with that of historical evolution as in the following:

The truth is, that the whole economy of every nation, as regards the occupations and pursuits of both sexes, the nature, amount, distribution, and consumption of wealth, is the result of a long evolution, in which there has been both continuity and change, and of which the economical side is only a particular aspect or phase. And the laws of which it is the result must be sought in history and the general laws of society and social evolution.

Essays, op. cit., p. 175.

76. Essays, op. cit., pp. 197,198.

77. Essays, op. cit., p. 202.

78. Essays, op. cit., p. 282.

79. Essays, op. cit., p. 144.

80. The complete quote outlining Leslie's total perspective on the social functions and scientific character of economic theories, reads as follows:

Economic theories and systems may be regarded in several different lights:

- (1) in reference to their causes, as the products of particular social, political and physical conditions of thought;
- (2) in reference to their truth or error;

(3) as factors in the formation of public opinion and policy.

Essays, op. cit., p. 142.

81. Lewis Haney, History of Economic Thought, op. cit., p. 531, contains several quotes illustrating Leslie's dissatisfaction with the political policies and institutions of his time.

82. F. A. Hayek, Studies in Philosophy, Politics and Economics (Chicago: University of Chicago Press, 1967).

CHAPTER V

DAVID SYMES AND THE AUSTRALIAN SCHOOL

David Symes (1827-1908) differed from most other British Historians both in his cultural surroundings and in his choice of a profession. Although born in Scotland and educated at home by his schoolmaster-father, Symes departed his homeland at the age of twenty-two, never again to return. After a year spent broadening his education at various German universities, he travelled to the gold fields of California to "find his fortune." There he labored long hours under dreary and exhausting conditions, yet his luck yielded him little better than the existence of a common laborer and nothing in the way of intellectual satisfaction. In 1851, after a year of unpleasant experiences, family duties and rumors of a new gold strike in Australia lured him to Melbourne where two of his brothers had already settled with their families. Although the gold fields of Australia provided only a slightly better income than had those of California, a turn of fate allowed Symes to purchase a small newspaper, The Age, which, through much dint of effort and his obvious skills as a "promoter," he eventually built into Australia's most influential daily. By the mid 1860's Symes was known as a major force in Australian politics and the leading light of the Australian Liberal Party. By the 1880's he was powerful enough to veto legislation planned by government officials and exercise the deciding influence in the appointment of premiers and cabinet ministers.¹ Symes was more than a publicist with an

interest in economic methodology, however. In addition to journalistic duties and multitudinous political crusades, he authored volumes in the fields of political science, evolutionary biology, theology and political economy.² It is his main work in political economy, Outlines of an Industrial Science (1877),³ and his two earlier articles on land tenure and economic⁴ that serve as the primary basis for the following discussion of his economic and meta-economic views.

Symes' reflections on issues of social and economic policy were obviously influenced by his education in German philosophy at the University of Heidelberg. They incorporated the collectivist and holistic orientations of German social thought during the Nineteenth and early Twentieth centuries and were in many ways remarkable anticipations of views held by the dominant school of English social and political theorists several decades later.

The Germanic strain in Symes' methodological writings was, however, contradicted by the equally strong themes of subjectivism and empiricism, evident, if not dominant, in his early (1871) Westminster Review article "On the Method of Political Economy." As a result of the diversity in his philosophic training and views, Symes' mature methodological position was an eclectic conglomeration of elements associated with the subjectivist-psychologistic tradition in British philosophy and economics and of views derived from the "objective"-historical tradition in German philosophy.

While Symes' experiences in Germany turned him against organized Christianity and caused him to abandon his family's tradition

of adopting the ministry as a profession, his intellectual outlook was permanently cast in the moralistic modes of social "reasoning" so closely associated with Victorian Christianity. Symes' omnipresent concern with the ethical consequences and dimensions of human acts played a decisive role in his critique of the Wertfrei cloak in which later classicals had wrapped their ideological views.⁵ It was also one of the roots from which sprang his nearly medieval perspective on public policy.⁶

Previous Discussions of Symes' Methodology

Secondary sources dealing with Symes' economic or meta-economic views are exceedingly rare, even though lengthy biographical studies of his personal affairs and political activities have appeared in abundance. In England during the Nineteenth Century his methodological writings received no recognition whatever beyond a single sentence in J. K. Ingram's History of Political Economy.⁷ Even his close friend, T. E. C. Leslie, whom Symes had credited as the inspiration for his Outlines, repaid the gracious acknowledgement of his Australian colleague by a stoney silence.

In Germany, however, the response to Symes' methodological writings was more widespread and more generally appreciative. He received the praises of Schmoller in an early Twentieth Century article written for Conrad's Handwörterbuch (1911),⁸ and his writings were examined at some length in Cohn's The Progress of Political Economy in England and America.⁹ Symes' Outlines of an Industrial Science was translated into German and apparently

engendered some significant interest among the economists of the later German Historical School. It was also widely used "as a textbook in elementary political economy ... in U. S. colleges and schools"¹⁰ and was reported to have been favorably reviewed by Henry Carey.¹¹

Interest in Symes' economic writings during the recent decades of the Twentieth Century has been, however, even less enthusiastic than that of his own period. The only major exceptions to his continued anonymity are a two line footnote in Hutchison's Review of Economic Doctrines, 1870-1929¹² and passing notice in an appendix of Marshall's Principles.¹³ His name is not to found even in Schumpeter's History of Economic Analysis, despite the reputation of that volume as the ultimate depository of references to both the obscure and famous. Fortunately for those interested in Symes' economic speculations there has appeared one reasonably satisfactory consideration of his economic and meta-economic writings. As a part of a survey of little-known Australian economists of the Nineteenth Century, J. A. La Nauze of the University of Sidney included a thirty-six page section on the doctrines of Symes. Although La Nauze's contribution to the literature on Symes' economics and meta-economics is certainly the most significant assessment of his views to date, and although it has been relied upon in the preparation of the following material, it unfortunately suffers from many of the errors common in evaluations of British Historical authors. La Nauze, for instance, seems to associate Symes in particular and British Historicism in general with the quite different trends in

German economic thought. He places undue emphasis on the policy aspects of the Historicist-Orthodox debate over the model of an "economic man," comprehending incompletely the meta-economic issues involved in this controversy. He also neglects the Historicists' more central concern with operational theories and with the impact of institutions on the forms taken by "maximizing behavior."

Finally, like so many commentators on British Historicism, La Nauze underrates the significance of the Historicist-Orthodox debate concerning the uses of inductive and deductive methods in the social sciences. In finding the Historicists' arguments against a priori methods to be "crude," "uninteresting" and unscientific,¹⁴ La Nauze displays either his own ignorance regarding the study of scientific methods and procedures or a lack of appreciation for the intent and setting of the methodological debates in Nineteenth Century economics.

In a study dealing with Symes as an isolated author, viewed apart from the British Historical tradition, some of La Nauze's errors might be to a degree justified. Symes was at times obscure, if not muddled, in his writings, and an obscure passage may easily be interpreted as one pleases. It might, indeed, be noted that La Nauze's summary and evaluation of Symes' works has the virtue of correctly identifying his most important and unique contributions to British Historicism, i.e., his arguments attacking the possibility of a Wertfrei theory of economic optimality.¹⁵ Despite this virtue of La Nauze's evaluation, however, there is much more to Symes' writings than the superficial characteristics which La Nauze has

chosen to concentrate upon. It is the purpose of the following pages to delve into the depths of Symes' meta-economic writings and thus lay bare the full scope of his contributions, both in terms of the debates of his day and in the light of more modern methodological research.

Symes' Classification of Economic Science

The key to an understanding of Symes' overall perspective on the character of economic investigations is to be found in his remarks regarding the classification of political economy. Like Cairnes, Symes divided all sciences into the mental and the physical according to the nature of their subject matters. Unlike Cairnes, Symes classified economics as a study dealing with strictly "mental" phenomena, as opposed to "valued matter" of a complex mental and physical character.¹⁶ Symes' explanation for the meaning to be attached to the term "mental science," and his justification for disregarding the "physical side" of those objects investigated by economists, illuminates difficulties and questions still unresolved in economic research. In summarizing his position on this question, Symes stated that:

... mental science does not concern itself with the external objects, being occupied exclusively with the sensations and ideas of which they are merely the exciting cause. So it is with the material objects which constitute Wealth. It is not with these that Political Economy has to deal, but with the impressions which they produce, the mental associations connected with them, and the Desires which their presence or absence incite.¹⁷

While one may agree, disagree or consider controversies concerning the "mental" or "physical" nature of social objects to be meaningless (in the philosophic sense of that term¹⁸), Symes' treatment of this problem does illustrate one strain in the often ambiguous way in which economists have historically used terms such as "good" (n.) or "wealth." Those who agree with a utility (or "psychological") oriented definition of these terms, i.e., those who agree with Symes that goods should be classified solely on the basis of the "mental impressions" or "satisfactions" they excite for given individuals, have had no difficulty in explaining cases of Veblen goods, "snob goods" or "bandwagon effects." Two things that are "physically identical" need not, under this view, command the same price, even in markets characterized by perfect knowledge and free access to sellers. If it is the case that consumers attach more prestige of ownership (or prestige of consumption) to one rather than the other, then the two items are ab definitie "different" goods.

Yet this psychological schema for the classification of goods renders economic theory tautologous, and, therefore, untestable. Since only patterns of consumer demand are identifiable in the world, preference maps being empirically indeterminate,¹⁹ any failure of economic laws (or better, of economic predictions) can always be dismissed as a consequence of a supposed mis-specification of the particular good(s) being considered. If demand curves slope up over some range of consumption, it is not, according to this view, because our theory is in need of further qualification, but,

rather, because we have failed to notice important "psychological" distinctions in physically homogeneous consumer goods.

The alternative to a psychologistic theory of goods is one specifying the homogeneity of goods in terms of some set of physical or observable properties of the goods themselves, or of the markets in which they are exchanged. Although certainly more attractive from an operational standpoint, this view of the way in which economic concepts should be tied to observable phenomena possesses defects from the perspective of "pure theory." A physical standard for the classification of goods might well prove as an embarrassment to the purist in matters of theory since it would probably lead to the conclusion that our basic economic relationships were not "universally" applicable (i.e., that they were not unconditionally true). It would also seem to suggest, if not imply, however, that individual preferences do not really "count," at least as the only criterion for determining a consumer's "better-offness." If the homogeneity of "goods" is determined by their physical aspects, we are forced to ignore any purely social and/or psychological distinctions which consumers might find of importance. Symes fully realized this latter aspect of a strictly physicalistic view of economic goods and discussed the problem of determining the consumer's welfare under this type of classification schema.²⁰

The Proper Concerns of Economic Science

Symes was not, however, content to rest upon his definition of economics as a study of psychological or mental objects. He

extended his attack upon orthodox methodology to the traditional limitation and definition of economics to "the science dealing with wealth."

In Symes' interpretation, restricting economics to "wealth maximization ruled out the consideration of other motivations for human action: motives of Health, of Power, of Honor and of Fame."²¹ Such an arbitrary limitation on the scope of the science also led to the neglect of those wealth-connected activities of production, consumption and exchange, which were the true object to be explored and explained by an "Industrial Science" (i.e., by economics, or the study of "industrial activity" properly conceived).

Yet even more important for Symes than the question of the scope of human motivation was his contention that wealth was not itself the motive power of human action. Wealth, according to Symes, was only psychologically associated, in certain cultures, with the basic goal of all human endeavors--happiness. He observed that: "Wealth is not pursued for its own sake, but on account of the pleasures it may bring, or the pains it may advert. The possession of even an enormous amount of wealth will never impel to exertion if it is believed its possession would not conduce to happiness."²²

This distinction between wealth and happiness was not merely analytic or "philosophic" in Symes' treatment of the subject. It had very real implications for the application and meaning of economic theory. Happiness was associated with matters of custom, habit, charity, propriety, friendship and security quite as much as

it was with the maximization of wealth. The description of all human behavior as a quest after increasing amounts of wealth was, for Symes, an empirical absurdity. Orthodox economists had attempted to evade the issues involved in a dispute over the maximization of wealth vs. a maximization of happiness through the introduction into their theories of ad hoc hypotheses designed to cover those "special cases" in which motives other than the desire for wealth "predominated." Symes, however, pointed out that this maneuver necessarily violated the spirit of a priori methodology and thus debased the orthodox approach to economic inquiry:

The very fact that writers on economic science are under the necessity of going outside their premises is an admission that these premises are incorrect. But this going outside should in no case be permitted. In investigations of this kind, when the a priori method is rigidly insisted on as not only a proper method, but the only method applicable, no matter foreign to the premises, far less what is expressly excluded, as is the case in the subject before us, should be imported into the discussion.²³

The circle of refutation was thus complete. Wealth maximization was neither necessary nor sufficient for the maximization of happiness. Yet other factors which were associated with happiness were excluded by the axioms of the classical system. To change the axioms or to insinuate extraneous material into the chain of inference flowing from them was to abandon the orthodox approach to an explanation of human behavior, or to replace the "deductive" and a prioristic procedures of the Classics with different techniques. That type of methodological reorientation, from abstract rationalism to

a concern with actual institutional constraints, was, of course, what Symes had sought after from the outset.

The Social Constraints on Maximizing Behavior

Another, more positive, contribution which flowed as an indirect consequence from Symes' discourse on wealth and happiness, was a recognition of the importance of rule-bound behavior to social stability and social organization. Symes argued that individual wealth maximizers might gain, in the short-run, from anti-social acts, but that the conditions required for long-run wealth maximization by the many individuals composing a social system required the existence of legal restrictions on the behavior of each individual and the willingness of each individual to abide by certain non-legal codes of "right conduct."²⁴

Although similar arguments had been advanced at least as far back as the time of Thomas Hobbes, Symes' perspective on the analysis of human actions, as conditioned by specific systems of formal and informal constraints, once again illustrated the concern shown by British Historical economists for analyzing the details of the legal and cultural framework of which individual "maximizing" behavior is a product. Although Symes' orthodox contemporaries would have undoubtedly agreed to the necessity for some type of formal and informal constraints on individual action as a way of preserving and defining "social stability," they only infrequently chose to consider the particular character of the behavior which would be elicited by specifically different institutions and customs. The only type

of social parameters popular in orthodox analysis were those imposed by nature (i.e., the scarcity of the best grades of land) or those which arose from broad historical or biological trends (the historical decline of profits on stock or the Malthusian laws of population). All more mutable institutional arrangements, with the possible exception of legal restrictions on international trade, or, in rare instances, the conditions of land tenure, were considered as "givens," seldom mentioned or explicitly recognized as assumptions of the analysis. This characteristic of classical theory, which most Historicists confusingly referred to as a priorism or the deductive method (and which Bagehot more correctly identified as "the extravagant claims" of the Classics) lay at the core of Orthodox analysis. The assumption that economic theory was neither "generically specific" nor "space-time specific" led the Classics into methodological blind alleys from which they would never escape.²⁵

The Formulation and Testing of Economic Theories

Symes' concern with the empirical content and significance of economic hypotheses was further illustrated in his analysis of the Classical doctrine of "disturbing causes" and in his critique of the motivational model of economic explanation. The "doctrine of disturbing causes" may be crudely summarized as a general insistence on the necessary truth of economic theories, viz., the assertion that any "failure" of an economic theory to predict "accurately" necessarily was due to unexpected fluctuations in the values of those variables assumed constant in the ceteris paribus clause of

the theory.²⁶ Although Classical economists had generally found it unnecessary to specify the variables which were to be impounded in ceteris paribus before applying their theories to "real world" cases, they were never at a loss to explain failures of their theories (in yielding accurate predictions) on the basis of an "implicit" and seemingly endless list of such disturbing influences. A statement which seems to suggest this very doctrine in a way which would render it easily available for abuse is found in J. S. Mill's Unsettled Questions of Political Economy, and is quoted by Symes both in his early Westminster Review article on political economy and in his Outlines of an Industrial Science, as follows:

... we must make proper allowance for the effects of any impulses of a different description (other than the desire for wealth), which can be shown to interfere with the result (of our predictions) in any particular case ... (our economic laws) will so far fail of being applicable to the explanation or prediction of real events, until they are modified by a correct allowance for the degree of influence exercised by other causes.²⁷

Symes' criticism of this methodological perspective is both comprehensive and of a somewhat complex nature. It is based upon the three separate issues identifiable in the above quote from Mill and upon expressions of similar views found in the writings of other Orthodox economists.

The first and most basic issue to be confronted in any intelligible rendering of the doctrine of disturbing causes is the question of how to identify all the potential sources of disturbance which could possibly affect the predictions yielded by a given theory,

and then how to sort these possible disturbing causes into those which are potentially significant and those which would have only a negligible impact on the predictions of the theory.²⁸ The distinction by Symes between all conceivable disturbing causes and those which were actually of potential significance is similar to Stigler's decomposition of Ricardian value theory into an "analytic" cost-of-production theory and an "empirical" labor theory. That is, the proposition that alterations in the value of some variable could conceivably affect the character or values of our predictions is considerably different than the proposition that any change in the value of a "significant" independent variable will result in a "relatively large" change in the value of the dependent variable. What is the magnitude of the relative variability which we choose to consider as "significant" is, of course, a matter of convention or of personal taste.

Combined with the question of which exogenous variables can really act as "disturbing causes" empirically, Symes also considered the effects of different cultures and varying paths of individual development on the relative strengths of different motivational influences. While Orthodox economists were guilty of a sin of omission as well as a certain ambiguity in interpretation in failing to provide an explicit listing of variables which they considered to be significant disturbing causes, they had also been guilty of an error of commission by assuming that the "same type" of motive (i.e., that of wealth maximization) would always lead human beings to act in similar and a prioristically identifiable patterns.

The asserted universal connection between types of motives and the expected types of actions resulting from these motives was decisively refuted by Symes. In intercultural cases, Symes noted, a motive as specific as love and devotion to one's aged parents had led dutiful offspring to the quite different behavioral responses of slaughtering their parents, exposing them to the elements, or meticulously attending to their health and protection, depending on whether the individuals considered were the cultural products of Sparta, the land of the Hottentots or the various nations of modern Western Europe. Symes also noted less dramatic intracultural cases in which the "different tastes" of different individuals would lead to different behavior patterns being associated with "identical" motives.²⁹

As another point in his indictment of the Classical's doctrine of "disturbing causes," Symes argued that a mere enumeration of all the possible background variables connected with the applicability of a theory was, by itself, insufficient to transform the theory into a useful tool for formulating predictions. If the relative weightings to be attached to each of the significant economic and non-economic [sic] variables in any decision situation remained unspecified, any theory would still remain useless in rendering quantitative predictions. The admission of any influence beyond the crude concept of wealth maximization thus entangled the Orthodox economist in an inexorable web of difficulties.³⁰ Not only would he then (1) have to explicitly identify all other motivational factors, but he would also be required to (2) separate these into the significant and the insignificant, (3) note modifications in this

list for each of the different cultures or individuals to which the theory was applied and (4) provide explicit estimates of the relative magnitudes of the partial derivatives of the functional relationship with respect to each (motivational) independent variable.

As a conclusion to his rejection of the doctrine of disturbing causes, Symes questioned the entire sequence of economic explanation, leading as it did, from motives to acts, or from a priori intuitions about motives to conclusions concerned with "hypothetical" tendencies. Symes suggested, in refutation of the traditional form of analysis, that it was impossible to "get at the motives except through the phenomena," and that it was thus a pure conjecture to associate any given motive with any given type of action. Symes, in fact, approached the modern behaviorist view of social science and, in several passages from his writings, endorsed a complete abandonment of the "motive-talk" of Nineteenth Century economic studies. In his essay on economic method, for instance, he stated that:

Motives are multitudinous, variable and often inscrutable. The individual looking within his own heart finds it difficult to tell the precise motive that influences him in a given course of action; and if it be difficult in the case of an individual where his own feelings are alone concerned, the difficulty is immensely increased in the case of an aggregation of individuals existing under conditions different from his own, or of mankind at large. It is clear therefore that if we have first to determine the particular motives that may have produced the phenomena, the inquiry will become complicated if not an interminable one.³¹

Thus, not only would a psychology of individual action be most com-

plex, and perhaps indefinitely difficult, but a social science which, of necessity, would treat of the actions and interactions of many individuals would be faced with the compounding of this difficulty to the point of impossibility. Although Symes' intersubjective approach to the study of social phenomena would eventually become popular during the Twentieth Century, he himself eventually lapsed back into an analysis of economic actions in terms of motives and intentionality.³² Perhaps little else could be expected, for Symes' general methodological orientation became increasingly psychological (or, perhaps better, psychologistic) as the years passed; and psychology, under the influence of German and French literary figures, was itself becoming more and more of a "mentalistic study."

The Problem of Induction

Symes' discussion of problems involved in the concept of undefined "disturbing causes" and in the use of a model involving motivational causation was closely linked to his views regarding the proper uses of "inductive" and "deductive" techniques in economic investigations. Having examined the Classical economist's obsession with the motives "causing" certain human actions and his "artificial" separation of "economic" from "non-economic" motives, Symes launched a frontal attack on the heart of Classical methodology:

So untenable ... is the hypothesis (of wealth maximization) ... that the very writers who have adopted it continually ignore it. They start with a philosophic abstraction of humanity, but they put it aside and accept the concrete man as soon as their premises are

stated ...

The hypothesis in question is not of itself sufficient to explain the phenomena without the aid of other hypotheses. Indeed ... (it) ... involves a whole series of hypotheses. Those who adopt it assume not only to have accurately determined the human motive which is at work, but its precise force and direction ...

The deductive method (according to Mill) prevails in geometry, there accordingly he thinks he finds the analogy he is in search of. Geometry, he says, assumes an arbitrary definition of a line. A line it defines to be that which has length without breadth; whereas, he says, we all know that a line has breadth, more or less ... The definition is not strictly correct, but sufficiently so for all practical purposes, and is therefore justifiable. In the same way, he maintains, it is justifiable in economic science to assume the exclusive influence of ... (certain) ... motives. But there is really no analogy between the two cases. In the one we have simply the definition of a term, and it is quite immaterial whether the definition be strictly accurate or not ... In the other case it is not the definition of a term which is assumed, but an hypothesis which materially affects the whole inquiry.³³

For Symes, the extreme abstraction involved in the a prioristic model of an economic man constituted an inappropriate form for economic inquiry. It was both less fruitful than other more "inductive" techniques in producing specialized (or "applied") economic hypotheses, and it resulted in the construction of analytic systems supportable only through the introduction of non-intuitive auxiliary hypotheses.³⁴ Symes further maintained that "deductive" procedures such as those involved in the speculative contemplation of the probable responses of an "economic man" provided no means for determining the "completeness" of hypotheses (Mill's own concern in

formulating the "rules of induction"), and that these procedures ignored the fact that "... in Political Economy the effects (that is, the observable behavior of individuals) are more accessible than the causes."³⁵

Although Symes proposed as an alternative to the "deductive" method of Orthodox economics his own special form of "induction," the meaning he attached to that term changed as the years passed. In his Westminster Review article of 1871 Symes had written that "all economic phenomena are within the reach of ordinary observation,"³⁶ but by 1876 he was including within "ordinary observation" not only "the external facts of human activity" but also "the internal facts of human consciousness."³⁷ In his later writings, Symes adopted a position concerning methodological procedures which was much like Cairnes', i.e., one expressive of the belief that economists, and other social scientists, had available to them a special source of information in introspection and the examination of other people--that they were, in this respect, more fortunate than the physical scientist, who dealt with mute phenomena. Symes gradually reworked his views concerning other aspects of economic method to conform to his changed characterization of economics as a purely "mental science." Yet certain inconsistencies remained between his new view of the "mentalistic" character of economics and his persistent desire to retain induction and the study of institutional constraints as an important part of the science.

Because of the prominence of the concept of "induction" in Symes' methodological views, it is important to be as clear as

possible regarding the meaning of the term. It is certain, first of all, that Symes did not mean to oppose induction to deduction in the same manner as had the Baconians. His main concern was that economics become more empirical and less dependent upon contrived and oversimplified models of human behavior. He thus advocated deduction, in the sense of inferential reasoning, "once the facts have been correctly ascertained" through the use of "induction," and he summarized his position concerning this question by stating that "Deduction properly begins where induction ends."³⁸ Symes' attempt to replace deductive by inductive techniques, in at least the formative stages of economic investigations, was, however, foredoomed to failure. It ran afoul of Kuhn's Law: that scientists will never reject a prevailing methodology, "paradigm" or research program unless presented with a clear-cut and productive alternative.³⁹ Since Symes remained somewhat vague about the character of those techniques to be applied in "inductive investigations," and since he was remiss in not illustrating the ability of this program to yield new and "interesting" types of economic questions, his crusade in behalf of induction became as futile as a Quixotic quest.

Economic "Experimentation"

Symes' abuse of methodological terminology was further illustrated by the manner in which he used and, obviously, misinterpreted the term "experimentation." While incensed at J. S. Mill's denial of a role for the experimental method in economics, Symes himself was no more aware of the standard usage of this term than were later

Neoclassicals. His view of what constituted an economic experiment was stated, in part, in the following passage:

Experiments enough are already made to his (the political economist's) hand, and all that is requisite is that he should collect and apply them ... indirect experiments of the very greatest value occur frequently, with every change of the business cycle or in legislation. And ... we can extend these in any direction we think proper.⁴⁰

This view of informal and uncontrolled "experimentation" is examined further in the conclusions to this dissertation, but it might be mentioned at this point that the view ignores problems arising from the quality or accuracy of observations as well as difficulties involved in separating out unique events from uniform casual sequences in those situations in which conditions are unrepeatable and where the underlying distribution of possible alternatives is unknown.

The Anti-Positivist Base of Political Economy

While the broad strokes of Symes' methodological writings resemble those of Cliffe-Leslie (to whom he acknowledges a debt in his Outlines of an Industrial science),⁴¹ his position on the relationship between morals, public policy and economic speculations was more extreme than anything imagined by Leslie or any other early British Historicists. Regarding this topic his criticisms were both relevant and devastating to the welfare position often implied, but seldom openly defended, by Orthodox writers:

Demand and supply is not essentially just, for

it recognizes no moral distinctions. It is not universally and invariably beneficent, for it ignores the difference between wants and desires ...

There is nothing just or beneficent in one man outbidding another for the possession of an article, or in one underselling another in order to secure a purchaser or a market. In either case the successful competitor attains his end at the expense of his rival; and in neither case is it intended that others than himself should derive any benefit whatever from the transaction.⁴²

Like many of Symes' other criticisms of Orthodox methodology, his statements concerning the ethical judgments implicit in conventional economic analysis apply with much the same force today as they did in the time of the Classical economists. In both the welfare analysis of Classical economics and the more modern Paretian welfare test, there exists a presumption, implicit, but still present, that the explanation of how competitive markets would organize exchange and production in any area of human endeavor is sufficient justification for preferring their adoption over competing non-market forms of organization. The realization that the ethical question is separate from (although partially dependent upon) the positive analysis of economics does not, of course, prejudge any case against market decision-making. Yet many modern defenders of a free-market system have sought to obscure, so far as possible, the fundamental valuative character of their social and political preferences. The many attempts which have been made to "derive" an ideological position from a positive theory of social action may be no more than a consequence of the superficial impression made by the

positive-normative distinction upon the modern consciousness. In economics, however, the attempts to intermingle ideology and science have a long and "respectable" history dating from the "absolutist" views of the orthodox Classics.

The fact that collectivist economists have been burdened with the same historical baggage as the advocates of market-systems, e.g., through the Classical and Hegelian roots of Marxist methodology, is but scant comfort to the honest social scientist. What we have seen historically, and what we see yet too frequently today, is a battle between two ideological movements, each claiming the honorific title of "scientific" for its program of social reconstruction, and neither interested in pursuing bona fide empirical research into social questions.

The issue involved in the separate "approval" or "disapproval" of a distributional mechanism, apart from the approval or disapproval of the existing property distribution (i.e., the recognition, by Symes, of the fact that "market control" or "state control" might themselves be goods or bads in an individual's utility function⁴³) was an advance in welfare theory not repeated until the recent writings of Mishan and Boulding. In both economics and political philosophy alternative social systems have been, and still generally are, judged on the basis of the existing distribution of property in those societies under their control. That the mechanism through which property may be gained or lost is an additional consideration requiring further valuative judgments was an issue overlooked in the new welfare theory of Samuelson and Bator and only recently intro-

duced into political philosophy by Robert Nozick of Chicago.

Symes' Own Views Concerning Public Policy

Even though Symes correctly criticized the Classics for allowing the merger of positive and normative elements in their economic discussions, his realization of their error did not exempt him from the same mistake.⁴⁴ In many of his economic writings Symes displayed a moral fervor quite unmatched by other British Historicists, and too often he allowed his ethical sensibilities to run amuck. In addition to intertwining policy arguments with discussions of economic methodology and theory, Symes frequently seemed unable to distinguish conditions of monopoly and fraud from the more normal workings of unregulated markets. La Nauze has excused Symes' rather blatant display of the medieval spirit as "crude empiricism,"⁴⁵ interpreting Symes' comments as an empirical generalization about the actual workings of markets in the Australia of his day rather than a theory of market operations. La Nauze's speculations in this regard, while interesting, fail to come to grips with Symes' virtual identification of disinterested market exchanges and immoral acts. According to Symes' perspective on economic transactions, any exchange not based on charity and altruism, that is, any exchange not based on a due consideration for the personal attributes and situation of the other party, is necessarily dishonorable and contrary to "a scrupulous sense of duty."⁴⁶ More modern economists would no doubt find such moralizing to be out of place in a serious work on economic methodology, to say nothing of the view which they would take of

such an ethical stance. Yet the introduction of this normative element into Symes' writings was not an abridgement of his personal standards for social inquiry. It was, rather, the necessary consequence of his belief in the inseparability of the positive and ethical dimensions of human acts.

Symes, it should also be noted, was further infatuated with a Golden Age interpretation of the pre-capitalistic order,⁴⁷ a view which, although popular in late Victorian literature of the Romantic School, has no correspondence to the known historical facts. In the happy days before the rise of the monied class, Symes fantasied "the strong arm of the law" prohibited fraud of even the most trifling variety. The goods that were produced were of only the highest quality (whatever that may mean) for the worker's pride in his product (and the system of guilds) would allow nothing inferior to come to market. Just prices, just wages and a fair distribution of the revenues from sales necessarily prevailed⁴⁸ under the pre-capitalistic order. The social and political order was supported on the firm base of the sturdy yeoman farmer class, and social peace as well as ordered prosperity prevailed within the nation.⁴⁹

The central problem of Nineteenth Century economic life, "excessive competition," arose with the increasing dominance of market forms of economic organization and the accompanying incentives to "greed" and "shoddy workmanship."⁵⁰ The enclosure movements of the Seventeenth Century completed the destruction of the Old Order by establishing a "monied monopoly" in land and under-

mining the social dominance of small proprietors.⁵¹

It is hardly surprising that even that staunch defender of socialism and collectivist ideology, V. I. Lenin, was repelled by the odor of moldy medievalism which arose from the "Progressive Movement" in Australia. With Symes acting as the primary force behind this movement, any other intellectual perspective would have soon been eliminated.

Subjectivism, Holism and Methodological Individualism

While it is frequently entertaining, though seldom very enlightening, to pick apart a man's political beliefs, one can sometimes discover in such beliefs, the reflection of a more general, and more interesting, perspective on society and social investigations. The integrated character of some men's political and social views is well represented in the case of Symes. It is the purpose of the following pages to illustrate the ties between Symes' general social philosophy and his political and policy views.

We have already examined in some detail how Symes' characterization of economics as a psychological study drove him to a position of extreme subjectivism as regards the determinants of economic acts (viz., the position of motivational causation, already endorsed by Orthodox economists). There is one respect, however, in which Symes continued to deviate from a pure subjectivist stance, even in his later writings. To fully appreciate both the significance of this deviation and the rather perverse social views which were engendered by it, some review of the history of social

thought on both the Continent and in Britain is, however, required.

Since at least the time of Locke, British philosophy was dominated by a form of subjective-psychologicistic-empiricism which had as its political and social counterpart the doctrine of limited individualism and as its methodological corollary in the social sciences the doctrine of "methodological individualism." The political philosophy of British liberals, simply stated, was based upon the notion that individuals were the product of their experiences and that their experiences were "subjective" or "mental" in character. Since every person's experiences were unique and "private" (or, at least, personal) and depended upon the different associative connections established by the different sequence of occurrences in each lifetime, everyone's tastes and values would be, to some degree, different from any other person's tastes and values. It therefore seemed best to Locke and his successors to leave each individual in charge of his own decisions and acts so far as possible, thus maximizing the "social good" by allowing each individual to maximize his own peculiar notion of his individual good.

Society, in the Lockian view, was merely an association of freely acting individuals bound together by "artificial" or contractual ties. The "rights of society" could be no more than an expression of the terms on which individuals had chosen to associate with each other. Expressed differently, there was no "society" as a separate entity which could be invested with rights superior to or different from the rights possessed by its individual members.⁵²

Symes' own perspective on society and the optimal social structure was quite different from the Lockian-subjectivist view. From the standpoint of German Holism, which he had first absorbed during his student days at Heidelberg, Symes declared that "society" had rights and desires quite as much as any individual, and that it was the function of the State to achieve Social Ends rather than private ends.⁵³ Although quite popular on the Continent, and expressed with considerable clarity in writings such as Hegel's Philosophy of Right, the Holistic view of social relations had made no significant inroads into England. It was no more than hinted at by J. S. Mill in his doctrine of "social oppression" and "social freedom" as presented in his On Liberty (1859), and it was not until the appearance of writings like Thomas Hill Green's Prolegomena to Morals (1883) that the English consciousness truly became aware of this rather peculiar perspective on social organization.

In Australia, however, Symes writings served as the catalysis for an early introduction of "social concern" and "social thinking" (i.e., for a distinctively "social" perspective on matters of public policy). Through his organ, The Age, Symes championed crusades for national protectionism on the basis that such policies would hasten the process of national economic development, and he campaigned for agrarian reform as a means of restructuring the balance of social and political power within Australian society. The modern prophets of doom and despair who bemoan the fate of the "lonely crowd" and the social rootlessness of an industrially

oriented society were "anticipated" in ludicrous detail in Symes' writings.

The methodological analogy to "social policy making," i.e., methodological holism, was also referred to in Symes' writings. Yet despite his explicit rejection of any individualistic stance, Symes never succeeded in freeing his social speculations from the form of micro, or individualistic, investigations. Although the unit of analysis from the viewpoint of methodological holism is properly the entire national economy or, at least, broad "sectors" of the economy, Symes' was usually much more concerned with questions of property relations and the ethical and behavioral dimensions of these relations. While his policy stances generally concerned "National Issues," his ever-pervasive ethical viewpoint on all social questions continually enmeshed him in the micro aspects of "social problems." Rather than the broad sweep of history and the trends of social development, Symes ultimately chose to analyze problems of alternative property structures and the "justness" of exchanges.

Morals, Property Structures and Economic Theory

Symes argued that the creation and distribution of economic goods necessarily involved the sanction of some (existing or desired) system for the enforcement of contracts and for the establishment and protection of property rights. As we have already seen, however, the selection of any given property system was a moral decision, and economics was thus, in Symes' view, unredeem-

ably ethical in character. As he himself traced the line of this deduction:

... when we come to treat of wealth from the standpoint of society, we are brought face to face with the question of adjustment or distribution, a question which is quite foreign to the premises of the deductionist, but is inseparable from the consideration of ... society.⁵⁴

The division of labor necessitates exchange, but there could be no exchange ... if the state did not enforce contracts, or if it permitted agreements to be broken with impunity. This shows the inseparable connection that exists between Industrial and Social Science.⁵⁵

(Industrial science) is subordinate to Social Science as the latter is subordinate to Ethics. Social Science is the key-stone of the arch of which Ethics is the foundation. It is the Social Sentiment that gives expression and force to the Ethical Sentiment that we owe the ideas of property and contract. There could be no contract without exchange, and there could be no property unless society sanctioned appropriation.⁵⁶

Although Symes' explicit recognition of the valuative base of property relations, and thus of markets themselves, was a major advance over the methodological views of Classical absolutists, his argument was not without its defects. That economic analysis rests upon the prior assumption of some particular property rights system does not mean that the entire study is irredeemably normative. It is only necessary to conditionally "accept" some property structure for the duration of any given analysis and for the purposes of the analysis alone. If the consequences of any given property structure should prove, on net, to be undesirable, then there is nothing which

would prevent the analysis of other, alternative, structures. The net benefits resulting from any given property system are, however, recognizable only as a result of some such economic analysis, and the benefits are only "desirable" in relation to the benefits possible under any of the constellation of other alternative systems. This conditional approach to a welfare comparison of alternative property structures is, in fact, the standard approach of many modern theorists,⁵⁷ and the detailed consideration of the behavioral consequences of alternative property systems has proved to be a powerful tool in predicting consequences of any given system.⁵⁸

Symes' Place in the History of Economic Thought

In the light of the foregoing considerations, it may seem odd that Symes was so completely neglected by his contemporaries and is still neglected by historians of economic thought, but there is, in fact, some justification for the attitudes of both these groups. While Symes demonstrated well-developed analytic abilities in his attacks against many of the Classics' methodological positions, he was frequently inconsistent in the positions he himself advocated, especially between his earlier and later periods. Neither was he as anxious as Jones, Leslie, or even J. S. Mill, to illustrate his various doctrines by reference to empirical evidence; and under the circumstances, words, without collaborating factual studies, were extremely cheap to come by and rather expensive to "sell." Finally, Symes undoubtedly offended many British Historicists and some late Classics by espousing unpopular, and often ill-considered, politi-

cal views in a rather inflammatory language. Leslie, who had written admiringly of British free-trade policies, and who saw the cause of many economic woes in the custom house, was surely quite upset by Symes' arguments in favor of economic protection. Further, Symes' advocacy of land reforms (mainly in the nature of dividing up large holdings for the benefit of a ressurected yeoman class) could have hardly been appealing to the English economists who had noted many virtues and few defects in the system of primogeniture.

The more modern prejudice against the serious consideration of methodological issues, especially in the context of an history of economic thought, has already been referred to. It seems highly unlikely that any major virtue apart from his methodological views can be discovered in Symes' writings.

Footnotes to Chapter V

1. This biographical summary of Symes' early life and later career is based upon the material in J. A. La Nauze, Political Economy in Australia (London: Cambridge University Press, 1949), pp. 98-100 (hereafter referred to as Australia), and the material in Sir Sidney Lee (ed.), Dictionary of National Biography, Supplement 2, 1901-1911, Vol. 1 (London: Oxford University Press, 1917), pp. 465-466.

2. A bibliography of Symes' most important works is contained in Australia, op. cit., p. 135.

3. David Symes, Outlines of an Industrial Science (London: Henry S. King and Co., 1876), hereafter referred to as Outlines.

4. David Symes, "The Land Question in England," Westminster Review, Vol. 94 (October, 1870), pp. 233-262 and "On the Method of Political Economy," Westminster Review, N.S., Vol. 40 (July, 1871), pp. 206-218, hereafter referred to as "On the Method."

5. "Positivist" here refers to the purported value-free character of classical economics and not to the doctrines of Auguste Comte.

It should also be added that German thought during the period of Symes' studies in that country was deeply moralistic, although the moralism was of a non-Christian variety. The leading advocate of a German reinterpretation of social and economic policies in Britain was T. H. Green, who is referred to at several points in the chapter on Marshall.

6. The rather odd character of Australian socialism, as a mixture of democratic and feudal elements, was noted by V. I. Lenin in his "Philosophic and Political Notebooks," Collected Works of V. I. Lenin, 5th edition, Vol. 33 (Moscow: Progress Publishers, 1967), p. 533. A rather extensive and quite sympathetic appraisal of the policy impact of Symes' thought on Australian society is to be found in Ambrose Pratt, David Symes, The Father of Protection in Australia (London: Word, Locke and Co., 1908).

7. J. K. Ingram, A History of Political Economy (New York: Macmillan, 1888), p. 222.

8. Australia, op. cit., p. 111.

9. Ibid., pp. 111-112.

10. Ibid., p. 113fn.

11. Ibid., p. 113.

12. T. W. Hutchison, A Review of Economic Doctrines, 1870-1929 (London: Clarendon Press, 1953), p. 7.
13. Alfred Marshall, Principles of Economics, 8th edition (New York: Macmillan, 1948), p. 783.
14. Australia, op. cit., p. 107.
15. La Nauze's confusion of the pursuits of German and British historicism is evident in Australia, pp. 101-102, 107-108. Symes' attack upon the implicitly normative character of classical political economy is discussed by La Nauze on pp. 108-109, 115 of Australia.
16. For a summary statement of Cairnes' views regarding "valued matter," see his Character and Logical Method of Political Economy, enlarged edition (London: Macmillan, 1875), pp. 32,38. For his attribution of this doctrine to Mill despite certain inconsistencies which he believed he had discovered in Mill's position, see Ibid., p. 30.
17. Outlines, p. 11.
18. The meaningfulness of any ultimate distinction between "mind" and "matter" was challenged many years ago by Gilbert Ryle in his Concept of Mind (New York: Hutchison's University Library, 1949). If this distinction is ultimately irrelevant, then the choice of a criterion by which to distinguish goods is merely the choice between two or more physically describable procedures.
19. The theoretical consequences of "Veblen" or "snob goods" are considered by Harvey Leibenstein in his "Bandwagon, Snob and Veblen Effects in the Theory of Consumer Demand," reprinted in Readings in Microeconomics, 2nd edition, edited by William Breit and Harold Hochman (New York: Holt, Rinehart and Winston, 1971).
20. Outlines, pp. 119-128.
21. Ibid., p. 3.
22. Ibid., p. 18.
23. "On the Method," p. 209.
24. Outlines, p. 25.
25. The distinction between the generic and space-time specificity of scientific theories is treated in G. C. Archibald's "Comment" to a round-table discussion on methodology chaired by Fritz Machlup, American Economic Review, Vol. 54 (May, 1963), p. 228.

26. The idea that scientific theories are unaffected by factual occurrences, but are retained or rejected on the basis of sociological factors within the various scientific disciplines, is examined in a comment by Imre Lakatos contained in Imre Lakatos and Alan Musgrave (eds.), Criticism and the Growth of Knowledge (New York: Cambridge University Press, 1970), pp. 97-100. This view was originally popularized in the writings of Thomas Kuhn, especially his The Structure of Scientific Revolutions, 2nd enlarged edition (Chicago: University of Chicago Press, 1970), p. 77, mainly with reference to the "hard sciences" of physics, chemistry and astronomy. There is much more decisive evidence, however, to bear out Kuhn's speculation as a proper description of methods in the histories of the social sciences.

27. This quote appears in a more lengthy version in Outlines, pp. 25-26; also see Outlines, p. 101 and "On the Method," p. 211.

28. "On the Method," p. 210.

29. Ibid., pp. 210-211. In the same passage Symes' also recognized that the "same motive" could cause any given individual to act differently at different times during his life. (According to changes in his information endowment? The exact reason is not stated.)

30. Outlines, pp. 25-26, 101-103.

31. The quote is from "On the Method," p. 206. In the same piece Symes linked together the a prioristic and introspective approaches to political economy as follows:

The Political Economist observes phenomena with a foregone conclusion as to their cause. His method is, in fact, the method of the savage. The phenomena of nature ... strike the savage with awe and wonder; but he can only look within himself for an explanation of these phenomena ... Like the Political Economist he works within the vicious circle of his own feelings, and he cannot comprehend ... how he can discover the laws which regulate the phenomena which he sees around him. The savage would reduce the divine mind to the dimensions of the human; the Political Economist would reduce the human mind to the dimensions of the ideal.

"On the Method," p. 218.

32. Symes would later hold that introspection and questioning of others concerning their motives for the commission of various acts were legitimate procedures for the political economist (Outlines, pp. 29-30).

33. "On the Method," pp. 207, 209, 212.

34. Ibid., pp. 209-210.

35. Ibid., p. 212.

36. Ibid., p. 313.

37. Outlines, pp. 29-30.

38. Ibid., p. 30.

39. For Kuhn's view of the role played by "competing theories" in the sociological process of "paradigm replacement," see his The Structure of Scientific Revolutions, 2nd enlarged edition (Chicago: University of Chicago Press, 1970), p. 77.

40. The quote is from Outlines, pp. 27-30. A more lengthy and detailed rendering of Symes' view of experimentation occurs in his earlier paper on methodology:

Mr. Mill, I am aware, maintains that experimentation is inapplicable to economic science. It is quite true that the economist cannot treat society as the chemist would treat matter, nor is it necessary that he should do so, as the experiments required are made to his hand ... The changes brought about in seasons of plenty and of scarcity, in periods of prosperity and of adversity, of activity and of stagnation in trade, affect production, prices, wages, and currency in a variety of ways, and are virtually, though not intentionally, experiments of a most important character. But we have direct as well as indirect experiments in political economy. What are all changes in Customs and Excise duties, and in the mode of taxation, but so many experiments, more or less successful.

"On the Method of Political Economy," p. 213.

41. Symes, in fact, acknowledged that: "I have to thank my friend T. E. Cliffe Leslie for his kindness in reading over the proof sheets and for some valuable suggestions which he has made to me, although this must not be understood as implying that our views are perfectly in accord on all points." (Australia, p. 110)

42. The quotes are from Outlines, pp. 38-39 and 56-57, respectively.

43. "On the Method," p. 209.

44. I am not, of course, asserting that the mixture of positive and normative elements in Symes' writings was an error from his own perspective. As is noted in the text of this chapter, Symes' view concerning the unity of moral and positive social issues was but the necessary consequence of his views on the character of social inquiry and the analysis of human action. He, in fact, stated this position explicitly in his Outlines of an Industrial Science: "... I shall probably be told that Political Economy has nothing to do with Morals ... But this position is altogether untenable. (An act) is still the same act whether regarded in its economic or moral aspects." (Outlines, p. 54)

That Symes criticism of the Classicals for introducing normative statements into their analytic works "through the backdoor" or via an appeal to the "absolute" (deterministic?) character of economic theories in no way contradicts his own practices or his belief in the validity of these practices. It is always legitimate to criticize another philosopher from the standpoint of his own professed beliefs, and the practice of covertly importing normative elements into purportedly positive theories would, in any case, be highly objectionable.

45. Australia, p. 114.

46. Outlines, pp. 40-46, 56, 60, 65-66.

47. Ibid., pp. 78-90 and "The Land Question in England," op. cit., pp. 234-237.

48. Outlines, pp. 65-66, 78-90.

49. "The Land Question in England," op. cit., pp. 238-240, 253.

50. Outlines, p. 58.

51. "The Land Question in England," p. 237.

52. An enlightened and lively discussion of the roles played by the concepts of Natural Law and of Moral Relativism in British social and political philosophy is to be found in Alfred F. Chalk's "Natural Law and the Rise of Economic Individualism," Journal of Political Economy, Vol. 59 (August, 1951), pp. 332-347.

53. Outlines, pp. 188-189.

54. Ibid., pp. 160-161.

55. Ibid., p. 164.

56. Ibid., p. 175.

57. For an approach to Welfare Economics which involves the

conditional acceptance of a particular property-structure for the purpose of "working out" its analytic consequences, see Vivian Walsh, An Introduction to Contemporary Microeconomics (New York: McGraw-Hill, 1970).

58. For excellent examples of the use of an analytic framework built around behavioral differences elicited by alternative property structures, see: Eirik G. Furubotn and Svetozar Pejovich, "Property Rights and the Behavior of the Firm in a Socialist State," Zeitschrift fur Nationalokonomie, 30 (1970), pp. 431-454; Eirik G. Furubotn and Svetozar Pejovich, "Property Rights, Economic Decentralization and the Evolution of the Yugoslav Firm," Journal of Law and Economics, 16 (October, 1973), pp. 275-302; S. N. G. Cheung, "The Structure of a Contract and the Theory of a Non-Exclusive Resource," Journal of Law and Economics, 13 (April, 1970), pp. 49-70; and S. N. G. Cheung, "Transactions Costs, Risk Aversion and the Choice of Contractual Arrangements," Journal of Law and Economics, 13 (April, 1969), pp. 22-42. A collection of significant articles in the more broadly defined field of "property-rights economics" is to be found in Eirik G. Furubotn and Svetozar Pejovich, The Economics of Property Rights (New York: Ballinger Publishing, 1974).

CHAPTER VI

WALTER BAGEHOT, POPULARIZER OF HISTORICAL ECONOMICS

"Walter Bagehot (1826-1877), banker, economist, political thinker and commentator, critic and man of letters, was Victorian England's most versatile genius," such was the appraisal of Norman St. John-Stevas in his definitive study of Bagehot's life and works. Yet both St. John-Stevas, who edited the critical edition of Bagehot's Collected Works, and those of Bagehot's friends and associates who memorialized his passing with lengthy eulogies did all within their power to discount his interest in the "dry science" of economics and disassociate him from the "hard" and "unfeeling" attitudes of Victorian political economists. Robert Griffin, for many years Bagehot's associate in business, prefaced his essay on "Bagehot as an Economist" with the remark that: "... I can only echo what has been said in protest against the common idea of Bagehot as being primarily an economist, instead of his being primarily a man of letters of strong genius and imagination, who happened, amongst other things, and subordinate to other things ... to take up [sic] with 'Political Economy'."¹ Sir Robert Giffin, for many years Bagehot's closest friend (and not to be confused with Robert Griffin), commented in a similar vein that: "So far from becoming absorbed in economic science as he grew older, though his later writings tend to be almost all economic, Bagehot to the last gave me the impression of only passing through one mental stage, which, being passed through he would leave political economy

behind."² The final blow to Bagehot's fame as a "true" economist was, however, delivered by the hand of John Maynard Keynes, who wrote in the Economic Journal of September, 1915, that while "... some of his (Bagehot's) contributions to the subject (of economics) are generally acknowledged to be of the highest degree of excellence it would be just to say that he was not an economist at all."³

Such evaluations of Bagehot's interests in and qualifications for economic studies seem difficult to justify, however, when faced with the evidence of his extensive writings on various economic issues and institutions, his active participation in the Political Economy Club of London,⁴ and the fact that "He was working on his Economic Studies (which he had hoped to revise into a comprehensive treatise on economic methodology) ... when he contracted the chill which was to lead to his death."⁵

The appraisals of Keynes' and Bagehot's contemporaries have had, however, a decisive negative impact on the treatment accorded his works by later historians of economic thought. Not one of the major references to the history of British economics devotes more than a few paragraphs to Bagehot's writings,⁶ and the overall assessment of his work is remarkably uniform between the various texts. He is usually quoted on the historical and spatial relativity of Classical economics and mentioned as the author of Lombard Street, "a classic study of the English money market,"⁷ and Physics and Politics, "Darwin applied to the political development of nations."⁸ His meta-economic contributions remained unnoticed by all except Schumpeter, however, and the dominant opinion was clearly that

nothing of importance remained to be said of his work. Although Bagehot attracted continued interest in literary circles, nothing concerning him appeared in an economic journal since Keynes' article of 1915, a lapse of over sixty years.

While the neglect of Bagehot by present day historians of economics is both mysterious and in many ways inexcusable, the attitudes of Keynes and of Bagehot's contemporaries can be more easily explained as either warped expressions of friendship or enthusiasm over the "new economics" of the Marshallian era. At the time of Bagehot's death in 1877, the reputation of economics, even in Britain, had reached its lowest ebb. Political economy had become identified in the public mind with the historical pessimism of the Malthusian theory of population and a belief in the approaching stationary state, as opposed to the more popular Victorian creed of unlimited and inevitable Progress. The subject was also widely regarded as an intellectualized apologia for the "discredited" and "outdated" policies of laissez-faire. The moralists attacked it as a new justification for greed, the socialists believed that it was a device used by reactionaries to retard needed political and economic reforms, and the historical and evolutionary economists laid bare and sought to undermine its "unscientific" methodology. In an intellectual climate of this sort, one would only refer to his worst enemy as "primarily an economist," and Bagehot's memorialists, mindful of social realities, were careful to disassociate his memory from the stigma which would inevitably result from a due emphasis on his interests in this lowly and somewhat degrading study.

By the time of Keynes's article in the 1915 Economic Journal public and academic opinions toward economics had changed, but the new conditions were no more favorable for an accurate reappraisal of Bagehot's contributions to meta-economic research than had been the conditions of the previous century. Marshall's reformulation of Orthodox theory into a well-defined "engine of analysis," supported by folksy generalizations and illustrations, had gained the eye of the British public. It would not, in fact, be an exaggeration to credit Marshall with being decisive in reestablishing economics as a recognized and "popular" field of scientific inquiry. The new Marshallian orthodoxy required, however, that future research into economic questions be based upon the theoretical system and methodological rules laid down in the Principles, centering upon such partial equilibrium constructs as the evaluation of consumer's surplus and relying upon casual observation and informal statistical studies to determine "the facts." Students were encouraged to become more and more systematic in presenting their ideas within and in terms of the Marshallian framework, to search out new "examples" to "illustrate" the usefulness and applicability of this framework and to publish works expressing their results in as lucid and non-technical a style as possible. Methodological investigations were, however, taboo in this new orthodoxy, having been judged by Marshall as both superfluous to the main objectives of economics as a science and as often destructive of the unity of the discipline. Keynes' The Scope and Method of Political Economy was the prescribed antidote for anyone infected by the virus of methodological dissent, for it

was generally believed that Keynes had carefully dissected and demonstrated the absurdities of the various non-Orthodox views.

Bagehot's speculations regarding the functioning of a market economy and the proper methodology for economic science were out of step with the newly created Neoclassical orthodoxy in numerous respects. Instead of the static equilibrium models of economic behavior favored by all Marshallians except Marshall himself, Bagehot's views were more suggestive of the "process" analysis later systematized by the Austrian School. His concern in "The Postulates of English Political Economy" (the only section of the Economic Studies fully completed and published at the time of his death) focused upon methodological controversies concerning the applicability of the Classical system to institutions and cultures outside the bounds of Nineteenth Century Britain, and was thus objectionable, if not offensive, to the Marshallians who believed, instead, in the basic historical continuity of economic theory from the time of Smith to the early Twentieth Century. Finally, Bagehot's economic writings were marred by being too often fragmentary, only suggestive of the further paths to be taken by economic inquiry, rather than systematically developing a case for a distinctively non-Orthodox methodology. Rather than a logical and well-structured writer, Bagehot was a keen observer with a prophetic cast of mind. St. John-Stevas has observed that "... Bagehot preferred to throw out his theories by way of allusion and digression, glancing at rather than developing them. He enjoyed, as he himself tells us, 'to play with his mind'."⁹

The effect of Bagehot's tendency toward loose speculation rather

than organized development of his position, was further aggravated in the case of his Economic Studies, that work in which he had "... intended to establish the aims and frontiers of economics ...,"¹⁰ by the unfinished state of the manuscript at the time of his death. His mature views on economic methodology were originally available only in scattered and fragmentary comments in his published and unpublished papers, and a number of years passed before even the bulk of these papers were published under the title originally intended for his completed treatise.

Despite the fragmentary and disorganized state in which Bagehot's meta-economic writings were left for his successors, they still contained important insights into the problems of economic methodology, some of which were not lost on future Historical economists and others of which had a significant impact on both the Historicists and their Orthodox brethren. Bagehot's contributions to methodological inquiry may be classed into two broad categories. First, he summarized the doctrines of Jones and other early Historical writers in a style noted for its lucidity and its appeal. The rather sudden revival of interest in Historical economics during the 1880's was in no small part the consequence of Bagehot's Economic Studies, even though that volume was not nearly so polished or "completed" as it would have been had Bagehot survived.¹¹ Second, Bagehot added to the published discussions of earlier Historical writers (i.e., Jones, Whewell and Leslie) a number of new and sometimes significant methodological doctrines (i.e., the pragmatic redefinition of the limits of political economy to serve the purposes of

empirical research and policy formulation).

His analysis of Orthodox doctrines and Orthodox writers was much more constrained than that of Jones, Whewell or Leslie, and was thus more conducive to fruitful conciliation between the two Schools. (As represented by the fact that even Marshall found merit in the Economic Studies.) His extensive practical experience with the complexity of actual markets also led to a better appreciation for the limits of economic inquiry and for the flexibility required of any potentially successful theory of social behavior. He was thus less doctrinaire than either the extreme Orthodox economists (i.e., Cairnes) or the utopian Historicists (i.e., Leslie) and was more willing to express an idea as a speculation to be discussed and debated, rather than as a point of dogma to be preached and defended against the heretical.

Although the most common and least interesting of Bagehot's economic ideas were obviously the result of a summary reading of Smith, Ricardo and Mill, his more original and important concepts may have been a psychological derivative of his extensive and intimate familiarity with the financial institutions of his day. His empirical orientation, although not as developed and systematic as might ideally be desired, was vastly superior to the "casual empiricism" of Marshall's weekend strolls through local factories or Cairnes' outright rejection of the theoretical relevance of economic relations observable in the world.

Criticisms of Classical Economics

Like many other critics of the Orthodox position, Bagehot was

concerned with the declining interest in and the widespread doubts being expressed about Political Economy in the England of his day:

... the position of our political economy is not altogether satisfactory: it lies rather dead in the public mind; not only does it not excite the same interest as formerly, but there is not exactly the same confidence in it. Younger men do not study it, or do not feel that it comes home to them ...¹²

Bagehot believed that the declining popularity of political economy was the result of a variety of factors, many of which were closely associated with the central flaws in the Orthodox stance on methodological issues. He provided a detailed analysis of each of these factors of interest both for its own sake, as a historical interpretation of the causes for the declining reputation of political economy during the Victorian period, and for the light that it casts upon Bagehot's own positive suggestions for the reform of the methodological techniques used in economic investigations.

One of the primary reasons for the growing unpopularity of political economy in the later Nineteenth Century, according to Bagehot, was its past association with the doctrines of free trade and laissez-faire. Although these policies had originally been favored by the majority of British intellectuals, they had engendered a reaction among the large class of professional state administrators and the growing number of those social reformers who were anxious to use the State in order to promote their own particular causes.¹³ Bagehot noted that while programs involving state action had often

immediately observable "results" which could be toted before the democratic majorities, the policies of laissez-faire had only long-term and indirect consequences. The case for increasing state intervention was thus direct and superficially plausible, while the case for laissez-faire rested upon long chains of abstract reasoning not easily understood by those untrained in the subtleties of economic thinking.¹⁴ By throwing their lot in with libertarian political and economic notions, political economists had jumped aboard a sinking ship. By overstepping the bounds of positive theory they had undermined popular support for their constructive and much needed research in the area of pure social science. While Bagehot himself fully recognized the existence of a positive core within Classical Political Economy, many other writers were neither so discerning nor so tolerant of the study. In their crusades for social betterment even many of the later British Historical economists were willing to discard the significant positive insights of the Classics along with their political creed.

A second reason cited by Bagehot for the growing dissatisfaction with political economy was the popular belief that its subject matter was the proper concern of every adult human being. Since it was a science which dealt with "human things," political economy inevitably excited "a great curiosity among the multitude of little cultivation," who proceeded to pass judgement upon its researches without the slightest bit of training in its methods. Those who read the works of political economists were often confused by "... reading words which were constantly used in common life ... about things

resembling ... those of that life ..." but with "... reasonings and ... conclusions ... [which did] ... not seem to apply to real life at all."¹⁵ "Uncultured moralists" often compounded public confusion over the significance of economics by emphasizing the moral aspects of social relations while excluding the possibility of a purely positive science of human action.¹⁶ Through such indirect paths to an "understanding" of the substance and significance of the subject, the man in the street would too often arrive at one of two equally misleading conclusions: either he would come to believe that the supposed science was confused and useless in dealing with the practical problems of the real world, or, alternatively, if he had a moralistic bent, he would view it as a new faith which sought to justify avarice and evil doings.

The cultivators of political economy were not themselves blameless for the declining reputation of their science, however, and Bagehot was not wont to let them off easily. They had too often used the conclusions, the technical language, and the authority of the discipline as a tool in the promotion of their own ideological views and had thus lent credence to the view of economics as a study of social ethics.¹⁷ Bagehot urged economists to publicly acknowledge that their science was merely an incomplete and hypothetical analysis of social conditions, purely positive and without any direct consequences for the ultimately valuative questions of social decision-making. He also recognized, however, the continual temptations for this type of political chicanery and the frequent opportunities open to those political economists who were unscrupulous

enough to engage in social myth-making. At least one of his comments regarding the ease and consequences of such inappropriate and ill-conceived practices is classic in both its content and its warning for all practitioners of a social science:

... so much are the practical impulses of man stronger than his theoretical tastes, that the cultivators of an abstract science are always in great danger of forgetting its abstract nature; they rush and act on it at once. In the abstract physical sciences there is an effectual penalty,--a person who acted on abstract dynamics would soon break his head; but in mental and physical [sic] sciences, unhappily, there are no instant tests of failure,--whatever happens, a man can always argue that he was right.¹⁸

A fourth and final reason offered by Bagehot for the declining popularity of economics was the growing "abstractness" and "dryness" of the subject. By this he meant not only that the theories of political economy were becoming more complex, so that they were ultimately intelligible only to specialists in the field, but also that the practitioners of the science were less and less willing to offer illustrative examples of their theoretical points.¹⁹

Bagehot interpreted the reluctance of Orthodox political economists to "verify" or illustrate their theories as evidence that these theorists realized their theories were not so "absolute" or universal as they had traditionally claimed. In Bagehot's view, the Classical theorists feared to search for "verifications" of their speculations in the new knowledge of other cultures because they were well aware that the analyses and conclusions of political economy applied only to those industrial forms of society closely resembling Nineteenth

Century Britain.²⁰

It should be noted that Bagehot's beliefs regarding the use of intercultural data to falsify the Classical System was not necessarily an expression of "cultural relativism." To claim that a theory is applicable to one society but not to another because the histories or racial characteristics of the peoples of the two cultures are "different" is not the same as the claim that the applicability of a theory depends on the conditions which currently prevail in different societies. The former claim is a metaphysical assertion of a supposed connection between certain "obvious" differences which exist in the populations or histories of two societies or cultures, and the asserted (but untested) significance of these differences for the applications of different social theories. (In this sense the claim is similar to the question, "Does History matter?". And the appropriate response is, of course, "'Matter' for what purpose? What is the real question being posed?") The latter argument is, however, concerned with test conditions or the "institutional" scope of a theory; it constitutes an equally valid and important consideration whether the theory being tested is a physical theory or a social theory. Although we might ideally desire theories which are "universal" or "absolute" in the sense that they require less and less severe restrictions on the domain of their applicability, such theories are usually arrived at only as the result of a prolonged process of scientific controversy and experimentation. They do not arise "instantaneously" through a recognition a priori of "important" features distinguishing individuals or societies.

One of the central goals of Bagehot's economic writings was to trace the true limits of economic inquiry and defend the theory, thus qualified, against those too voracious critics who attempted to reduce it back to moral philosophy. Bagehot's analysis of probable causes for the depressed reputation of political economy in Victorian England are important as a neglected and original interpretation of the development of the discipline in Nineteenth Century Britain. Yet more important still were his specific criticisms of Classical methodology and his more positive recommendations for reform in the goals and procedures of the subject. The remainder of this chapter is therefore devoted to a detailed analysis of Bagehot's contributions to these latter areas. We hope that the following will prove sufficient both to illustrate Bagehot's unrecognized virtues as an economic methodologist and to provide an introduction to the summary criticism of Classical Orthodoxy which is presented in the concluding chapter of this dissertation.

The Nature and Subject Matter of Economic Science

Bagehot, much like Cairnes, was adamant in his belief that political economy was a purely positive study, without the slightest intermixture of normative elements. What was cause and effect in social phenomena was properly its concern. What was good or bad, right or wrong, could only be dealt with by the "higher" and "more difficult" study of ethics.²¹ The guide to state management which Adam Smith had hoped to provide the world was banished from the legitimate concerns of the subject along with Ricardo's science of

the proper relation of classes in a developing economy and Mill's concern with the (normative) rules of income distribution and the evolution of society toward an ultimate cooperarian utopia.

Although Bagehot himself had quite definite views about politics and the evolution of societies, he was unwilling to resort to political economy as a justification for his views. For him there was no "art of political economy," no "practical" or "applied" subject concerned with social policy, which stood beside and sometimes united with the science. Bagehot may have even wished to limit economics to a study of the behavior of men without regard for their motives (as had Symes at an early stage in his intellectual evolution), but the evidence for or against this interpretation of his thought is itself so contradictory that it is impossible to draw any definite conclusions regarding his "true" position.²²

The Scope of Political Economy

Bagehot's proffered delimitation of the term "political economy" set the tone for his discussion of the applicability and limits of economic theory. He defined economics as "the science dealing with business activity," thus confining it to a much narrower sphere than the Orthodox concern with "wealth maximization" or Neoclassical investigations into all those things having to do with the "maximization of utility." Economics, in Bagehot's view, was not even so broad as the study of business activity as a whole but consisted only of those specific aspects of business behavior directly concerned with cost minimization and profit maximization.²³

In so far as nations are occupied in "buying and selling," in so far will political economy, the exclusive theory of men buying and selling, come out right and be true of them
...

As far as people are what we now always call "men of business," money, the thing they look for and the thing they want, is their sole object; and in that sense of the phrase, political economy may be fairly called the science of business.²⁴

It is somewhat notable that Alfred Marshall would later adopt a similar but not identical definition of economics as the study of all human actions which could be related to "the measuring stick of money."

Evolution and the Scope of Economic Inquiry

Bagehot's restriction of economic inquiry to those types of behavior that were associated with advanced industrial forms of social organization led him to also impose certain limits on the spatial and temporal scope of economic theory. Economics was by no means as universally applicable as the Classical economists had believed, at least not in the same sense which they had attached to the term "universal." Although it was not limited to an explanation of business and commercial phenomena which were uniquely British, "it is only true of ... states of society in which commerce has largely developed, and has taken the form of development, or something near the form, which it has taken in England."²⁵ In further elucidating his position regarding the proper scope of political economy, Bagehot considered the issue of social evolution and left

open the door for the expanded applicability of political economy to societies beyond the bounds of Western nations:

There is nothing capricious, we should observe, in this conception of political economy; nor, though it originated in England, is there anything specially English in it. It is the theory of commerce, as commerce tends more and more to be when capital increases and competition grows ... as the world goes on, similar characteristics are being evolved in one society after another. A similar money market, a similar competing trade based on large capital, gradually tends to arise in all countries. As "men of the world" are the same everywhere, so the great commerce is the same everywhere. Local peculiarities and ancient modifying circumstances fall away in both cases; and it is of this one and uniform commerce, which grows daily, and which will grow, according to every probability, more and more, that English political economy aspires to be the explanation.²⁶ (emphasis added)

Bagehot's age of "the Great Commerce" was remarkably similar to the Comtian concept of the "positive stage of society" in which the customs, prejudices and religions peculiar to each region of the world were to be superseded by "scientific attitudes" (including a scientific religion) and by scientifically designed social institutions. Comte did not, however, identify his ideal society with a steady progression toward a competitive free-market capitalism as did Bagehot. Instead he was rather enamored with the glories of neo-mercantilism or, perhaps more accurately, neo-feudalism.

Although Comte's influence on British social theorists is not to be discounted (indeed, much of British social thought in the 1870's might be fruitfully re-examined as a debate between Comtists and

anti-Comtists), it seems somewhat more reasonable to interpret Bagehot's outlook in the light of the political and evolutionary theory contained in his Physics and Politics. While his discussion in the Physics and Politics was historical (or historicist) in the sense of attempting to discover a law of social development, it was decidedly not Comtian.

In Physics and Politics Bagehot reinterpreted and expanded the Social Darwinism of writers such as Herbert Spencer into a doctrine which purported to describe the world-wide economic and political evolution of societies. In Bagehot's view, British policies of laissez-faire and industrialization resulted in a superior form of social organization, one which would eventually be emulated by the rest of the world or would absorb other cultures through conquest and trade.²⁷ The question Bagehot considered in his Physics and Politics was not how each society would separately evolve or whether they each would evolve along similar paths if left to their own devices. The question was rather the more practical one of "survival of the fittest" on a scale of national cultures and social organization. It was only because the "business form" of social organization was the most capable of providing an environment calculated to entice other peoples to adopt its methods, and because the level of productive activities resulting from it could better support the troops of conquering armies, that political economy would gradually become applicable to the remainder of the world. As long as the tradition and status-bound institutions of pre-capitalistic society were dominant in a country, there was no hope for a rational analysis

of the country's economic system.²⁸ "Equilibrium" in such pre-capitalistic societies was strictly a function of the balance of power between the various social castes. Supply and demand was replaced by the results of force and weakness.

The Unity of Science

For Bagehot, scientific laws were arrived at in the same basic way in both physical and social sciences, and economic laws were of the same stature (at least potentially) as the laws of physics. He, however, agreed with the majority of economists in his day and our own in classifying economics as a non-experimental science and in emphasizing the complexity of social phenomena as against the relative simplicity of physical phenomena.²⁹ Although the doctrines of social complexity and of the inaccessibility of controlled experimentation are key to the construction of any historicist methodology (in Popper's sense of the term),³⁰ the twist which Bagehot gave to his statement of these doctrines exemplified the best of what Popper later labelled the "critical spirit." It was thus that Bagehot decisively separated himself from those who wished to use historical criticisms of Classical economics as props for a new anti-economic ideology.

The special liabilities under which social scientists supposedly labored, instead of becoming an excuse for the abandonment of inter-subjective procedures in social inquiry, became, for Bagehot, a goad to magnified efforts. Bagehot viewed the inaccessibility of controlled social situations and the reputed complexity of the phenomena

as a justification for a more vigorous and thorough pursuit of social knowledge through a comprehensive and exacting application of the methods of the physical sciences.³¹ The only basic difference between economics and physics was that more was to be expected and demanded of the economic scientist. Since his problem was more difficult and he was deprived of a major tool for its solution, the social scientist could be expected to struggle more vigorously against the mysteries of society, not to take refuge in the mythologies of essentialism or dialectics.

Bagehot and the Baconian Method

Although advocating the unity of scientific method whether applied to social or physical problems (a position known as "naturalism"), Bagehot remained largely free from confusions in the methodology of the physical sciences which haunted discussions in the philosophy of science from the time of Francis Bacon to the mid-Twentieth Century. Many anti-naturalists, including some notable authors writing within the last twenty years, have opposed "scientism" (the "illegitimate" extension of the rules and techniques of physical investigations to social inquiry) because they believed that the methods described by Francis Bacon in the Sixteenth Century (observation and induction, without the actual formulation of hypothesis) were accurate descriptions of the procedures of the physicist.³² Conversely, many naturalists favored an adoption of the methods of physics because they wished to reduce social investigations to the collection of historical facts and eliminate universal

theories from the field of social investigation. Among the many justifications stated for this perverse form of naturalism were historical-cultural relativism (considered on pp. 192-193 of the present chapter), racial relativism and the belief that the statement of universal social theories runs counter to the philosophic doctrine of "freedom of the will."

Bagehot rejected the Baconian view of science, labelling it as the "all-case method" or the method of examining all "the facts which a complete historical and statistical inquiry would develop."³³ Instead of blindly accepting the authority of Bacon, he attacked the relativistic strain in the Historical tradition, noting that it endorsed "exactly that (procedure) which Lord Bacon himself followed, and owing to the mistaken nature of which he discovered nothing."³⁴ Against Bacon's view, Bagehot quoted the judgement of W. S. Jevons, who had just published his monumental study of scientific method. According to Jevon's judgement of Bacon's methodological recommendations, "It is difficult to imagine a less likely way of arriving at great discoveries."³⁵

Bagehot's more substantive criticisms of Baconianism were both telling and demonstrate an appreciation for the issues far advanced beyond his contemporaries. He argued, on the one hand, that the techniques of the "all-case method" were practically useless in the discovery of new scientific hypotheses; that such hypotheses, whether in political economy or in physics, were the product of what Popper has since called "the creative imagination." Hypotheses, according to Bagehot, are not drawn out of the facts but are tools in organizing

the facts.

On the other hand, Bagehot disputed the capabilities of the Baconians for carrying out their own proposed program of historical research. Against a program designed to determine "all the facts," Bagehot noted that many of the facts of commercial life were purposely kept secret by men of business, that they were, in any case, in a constant state of flux and, finally, that it was physically impossible to know everything (all "the facts") about any set of human events.³⁶ In a primitive form Bagehot also anticipated a part of Popper's refutation of historicism. He stated that if we can only know that which has already happened and must be content to formulate hypotheses only with regard to known facts, then it is logically impossible to say anything about the future. That is, the goal of science as a predictive tool must be abandoned.³⁷

The Failures of Orthodox Methodology

Although the all-case method was a false path for scientists to pursue, Bagehot believed that they were no better off following the "single-case method." The "single-case method," as advocated by Cairnes and other Orthodox economists, was simply to take one observation, or one's own intuitions and general impressions, as the only empirical input into a theory. Elaborate theoretical structures were then constructed on the basis of this casual empiricism without regard for or recourse to any further "verification."³⁸

Even though little direct discussion of the consequences of the one-case method is offered in Bagehot's writings, it is easy to

connect his comments on the limited applicability of Classical political economy with his antipathy toward the single-case method. Classical economics was limited in its predictive and descriptive powers to the types of phenomena prevalent only in Britain because the theorists who constructed the Classical System had relied too exclusively on a single-case method. Had they broadened their initial observations to include data from non-British sources, they might have been successful in constructing a more general system, applicable to both business and non-business societies. At the very least, they would have earlier recognized the limits of the theoretical system they had constructed.

The Empirical Content and Historical Development of the Sciences

Bagehot's own description of the process of scientific inquiry hinges upon the degree of "abstraction" represented in the hypotheses of any particular science. The decision of central importance to the success of any scientific endeavor was the decision over the degree of detail which must be embodied in an hypothesis in order to yield "correct" predictions. Since all possible aspects of any phenomena could not possibly be accounted for in any formulation simple enough to be dealt with by the human mind, it was necessary to decide both on the degree of complexity or simplicity in any given study and on the particular variables which would be included in or excluded from the study.³⁹

According to Bagehot, the hypotheses first formulated in any

area of scientific inquiry would necessarily be highly simplistic (that is, devoid of detailed content), and their predictive power would be correspondingly crude. As a field developed, however, the central explanatory hypotheses which formed the core of the subject could be made increasingly complex with an accompanying improvement in the accuracy of their predictions.⁴⁰ In economics in particular, the simplistic theories of David Ricardo and James Mill required modification for changes which had occurred in the institutional structures and for application to those non-British cases which did not conform to the model of purely economic (or business-type) behavior.⁴¹

In summary, then, it was Bagehot's view that the main tools of scientific inquiry were abstraction--the isolation of the more important aspects of any class of phenomena--and deduction from a set of premises, established with due regard for properly formed generalizations with the aim of arriving at testable hypotheses. Bagehot likened the pursuits of the scientist to the investigations of a detective seeking clues to the solution of a crime. Both scientist and detective had to decide which aspects of the case were important to its eventual explanation and which could remain unexamined, both would subsequently draw conclusions on the basis of the particular clues they had chosen and both would then test the truth of their deductions against other phenomena occurring in the world.⁴²

Specialized Problems of Economic Research

Even though the methods of abstract reasoning were similar in

both the physical and social sciences, Bagehot foresaw sociological and linguistic difficulties in their application to social phenomena which were not present, to the same extent, in the investigation of physical phenomena. Many individuals untrained in the techniques of abstract science could easily set themselves up as experts in a study dealing with "human things," and such untrained minds would inevitably object that the model of an "economic man," used in political economy, was an incomplete, and therefore illegitimate, representation of the characteristics and motives of human beings.⁴³ The use of ordinary terms in technical senses (as already mentioned on p. 190 of the present chapter) also resulted in frequent confusions among amateurs and the unprofessional critics of the Classical system.

Economics as a social science, intimately involved with the everyday activities of large bodies of men, was also at a disadvantage in the existence of separate groups of professional observers and professional theorists. Although businessmen would frequently have the best grasp of the subtlety and variety of "the facts," they distrusted the abstract theorizing and meddlesomeness of professional intellectuals. What theories they needed they believed they could easily concoct for themselves, and they often had little comprehension of the crudity of their own theoretical constructs. Professional economists, on the other hand, had access to a multitude of well-developed speculations about the economic system but possessed few facts useful in the correction or corroboration of their theories. Both groups viewed the other's knowledge as inferior, in type, to their own; and both found the other's speculations to be

less than perfect answers to the questions they held in common.⁴⁴

In the physical sciences there were also men who were primarily theorists and men whose main pursuits involved the application and testing of theories formulated by others, but a theorist was always anxious to demonstrate the testable consequences of his theories and would sometimes perform or direct the preliminary experiments himself. The applied scientists would always be eager to either suggest ways in which an old theory might be reformulated, in order to avoid the problems which resulted in its falsification, or to formulate new hypotheses explaining the anomalous test results. In the physical studies the division of labor between those primarily familiar with the facts and those primarily involved with theory formulation unambiguously aided in the development of the science as a whole, while in the social sciences it probably served as an impediment to rapid or significant advances in the development of a body of well-tested theories.

The problem of organizing research in economics today is somewhat different, although its significance has not changed from the time of the Nineteenth Century. While businessmen have become more convinced of the usefulness of economic theories, professional economists have themselves divided into two hostile, or, at least, indifferent, camps. Those who consider themselves as theorists only infrequently formulate their theories with regard for their testability, while those whose main concerns are with the testing of theories have perennially ignored the qualifications on test conditions which are built into the hypotheses they are testing.

We have too frequently been left with econ-metaphysics on the one hand and with non sequitur arguments arising from conclusions logically untied to test results on the other. Leontief has recently objected that the theoretical superstructure of economics is growing at a rate unmatched by the empirical base against which it must be tested.⁴⁵ He might have as well noted that the "tie rules" connecting economic theory to the observable world have never been standardized or clarified.

The Religion of Political Economy

Although Bagehot always defended political economy as a legitimate scientific pursuit (a position not particularly popular in his own day), he was careful to note those characteristics of the enterprise which rendered its procedures less than perfect and which were liable to abuse by the many economists who perceived themselves in the role of priests defending the faith rather than scientists pursuing knowledge. Most of these imperfections which allowed for dogmatic thinking have already been mentioned in different contexts, but we repeat them here in a more systematic form as a summary statement of what went wrong in Classical methodology.

According to Bagehot, the accuracy of economic predictions was closely related to the degree to which the empirical specifications, stipulated in the empirical interpretation of the formal theory, accurately reflected or corresponded to relations or institutional structures which actually existed in the world. The predictions of political economy could only be predictions of tendencies, however,

since "perturbing causes" analogous to "tensions" or "frictions" in physics would always cause a deviation of some magnitude between the observed and predicted values of the dependent variables.⁴⁶

Because all human behavior was not motivated by profit maximization, the empirical specifications of the interpreted Classical theory were seldom true of the world (although they would become more true, both extensively and intensively, as industrialization and market relationships invaded more areas of human behavior and extended geographically over all the nations of the globe).⁴⁷

When Classical theory was used to predict events outside of the narrow spatial and temporal confines of Nineteenth Century Britain, it would usually fail, and this failure would provoke perverse reactions both on the part of economists and on the part of the general public. Economists of the Orthodox variety had always claimed that their theories were true of man and society without restriction, and they would frequently seek justifications for any failure of these theories in "disturbing causes" or in the intuitive and a prioristic nature of economic inquiry (i.e., "if it seems right, it can't be wrong"). The public, on the other hand, would eventually conclude that either the predictions of economics were grossly in error, and thus that the subject itself was without foundation, or that a subject that constantly resorted to an unlimited collection of "disturbing causes" in order to explain away its failures was of no great practical importance and possessed no meaningful lessons for either the pursuit of business or the formulation of public policy.

Neither economists nor the public tended to view the "failures"

of the Classical theory as a result of the misapplication of the theory to phenomena which it was never intended to explain.⁴⁸

Economists would be led to defend it in ways in which it should not be defended and the public to condemn it for things for which only its practitioners were to blame. While the common man of the Victorian period thus came to believe that economics was "unrealistic," economists themselves soon arrived at the conclusion that "realism" (or a regard for any kind of intersubjective observation procedures) was superfluous to the main pursuits of their studies. The formation of a faith in economics (of a self-contained and self-consistent system of justifications operating without empirical testing) was thus complete.

The Artificial Boundaries of Economic Inquiry

A final meta-economic problem considered in Bagehot's writings was the question of the proper relationship between economics and the other branches of social science. This issue had been a point of bitter contention between economists before Bagehot's day and remained as an unsettled issue for many years after his death. Yet his own solution to the problem was both perceptive and is, in fact, not far different from the position arrived at today:

...the boundaries of political economy are arbitrary, and might be fixed here or there; but this is already implied when it is said that political economy is an abstract science. All abstractions are arbitrary: they are more or less convenient fictions made by the mind for its own purposes. An abstract idea means a fact or set of facts minus something thrown away. The fact or set of

facts were made by nature; but how much you will throw aside of them and how much you will keep for consideration you settle for yourself. There may be any number of political economies, according as the subject is divided off in one way or in another, and in this way all may be useful if they do not interfere with one another, or attempt to rule further than they are proved.⁴⁹

To suggest that the various fields of social inquiry were not separated by natural divisions, divisions in some way necessitated by the character of their respective subject-phenomena, or that "abstraction" (for those who believed in such things) was not a rigorous process much like "the rules of thought" was, in Bagehot's age, the purest form of intellectual heresy. The advanced character of his own view was, however, a reasonable corollary of his concern for and lifelong involvement with the intricacies of business activity (of which economics was to serve as an explanation). Just as a little knowledge of a subject sometimes leads the arrogant to claim expertise, so much knowledge, mixed with a more settled nature, leads to increased humility and an appreciation for the complexity of the world and for the necessity of maintaining flexible opinions about it.

While Comte, Cairnes and their followers were locked in endless squabbles about the "natural" boundaries of this or that branch of social science, Bagehot suggested that the boundaries of economic inquiry depended in any one instance upon the question proffered for investigation. The boundaries of any science were thus appropriate or inappropriate only so far as they aided or impeded the progress toward desired knowledge.

Economists in the Twentieth Century have certainly taken a more

flexible view of the limits of economic inquiry than did the Orthodox writers of Bagehot's time, but they have still been deficient in artificially restricting the type of variables to be included within the scope of their investigations. Like Bagehot in his more inflexible moods, they have wanted to restrict economic investigations to "economic variables," leaving matters of social interaction and political behavior to other disciplines. A less conventional and more comprehensive view of economic studies has grown up in the last decade, however, in the writings of George Stigler, Gary Becker and the economists of the Virginia School; and as a result of their seminal research into new areas of economic inquiry, we may yet see Bagehot's vision of multitudinous political economies become a reality.

Concluding Remarks

Despite the many faults that Bagehot discovered in the general sub-structure of political economy and the many objections he raised to its methods and claims of absolute truth, he was never to lose sight of the importance of the study or of the vital role it had to play in the development of social relations:

It will be asked, Why do you frame such a science, if from its nature it is so difficult to frame it? The answer is, that it is necessary to frame it, or we must go without important knowledge. The facts of commerce, especially of the great commerce, are very complex; some of the most important are not on the surface, some of those most likely to confuse are on the surface: if you attempt to solve such problems without some apparatus or method, you are as

sure to fail as if you try to take a modern military fortress--a Metz or a Belfort--by common assault; you must have guns to attack the one, and the method to attack the other.⁵⁰

While the bulk of Bagehot's economic and meta-economic comments were clearly drawn from contemporary and historical sources, his treatment of these concepts was often original and always entertaining. Yet Bagehot was far from being completely unoriginal. He considered many meta-economic doctrines which were new to his time and provided an analysis of these concepts superior to any others offered until well after the time of Marshall. Bagehot's comments on the limited scope of the Classical system, the relationship between the "practical man" and the economic theorist and on the proper boundaries of economics vis-a-vis the other social sciences each establish his claim to a reputation much superior to that which he presently enjoys.

Schumpeter once stated that, "His (Bagehot's) vigorous pen repeatedly touched methodological subjects," although, "Without questioning the validity of Ricardian procedures."⁵¹ The foregoing pages have shown, to the contrary, that Bagehot more than dabbled in methodological issues and that one of his central concerns in economics was to reform the traditional methods of the Classics. It is unfortunate that many of the Orthodox economists did not read him more seriously or with greater care, and that many later Historical economists absorbed only his negative doctrines while neglecting his points of positive reconstruction.

Footnotes to Chapter VI

1. Forrest Morgan (ed.), The Works of Walter Bagehot, Vol. I (Hartford: Traveler's Insurance Company, 1891), p. xiv.

2. J. Shield Nicholson, "Review of Mrs. Russell Barrington's Life of Walter Bagehot," Economic Journal, Vol. 24 (December, 1914), pp. 545-546.

3. J. M. Keynes, "Review," Economic Journal, Vol. 25 (September, 1915), pp. 369-375.

4. T. W. Hutchison, A Review of Economic Doctrines, 1870-1929 (Oxford: Oxford University Press, 1953), pp. 1, 367.

5. Norman St. John-Stevas, Walter Bagehot (London: Longmans, Green and Co., 1963), p. 8.

6. R. B. Ekelund, Jr. and R. F. Hebert, A History of Economic Theory and Method (New York: McGraw-Hill, 1975), pp. 198-201; L. H. Haney, History of Economic Thought, 4th edition (New York: Macmillan, 1949), pp. 527-529. Bagehot is not even mentioned in Eric Roll's History of Economic Thought, 3rd edition (Englewood Cliffs: Prentice Hall, 1974) or in Jacob Oser's and William Blanchfield's The Evolution of Economic Thought (New York: Harcourt, Brace and Johanovich, 1975).

See also J. A. Schumpeter, A History of Economic Analysis (New York: Oxford University Press, 1954), pp. 183, 445, 824 and 1111. Note the reference to Schumpeter's evaluation of Bagehot which appears on p. of this chapter.

An excellent discussion of Bagehot's technical economics and some mention of his methodological views are found in T. W. Hutchison, A Review of Economic Doctrines, 1870-1929 (Oxford: Clarendon Press, 1953), pp. 67, 367-368.

7. St. John-Stevas, op. cit., p. 14.

8. Ibid., p. 17.

9. Ibid., p. 20.

10. Ibid., p. 8.

11. Bagehot is in many respects the most brilliant and certainly the most adroit author considered in this dissertation. His works are rigorously logical and are, in tone, much like more modern tracts in philosophy of science. Yet his writing style is that of the journalist commenting on well known or easily fathomable issues. The effect is illusionary. Page after page of his works were filled with what is seemingly empty trivia. Then, in a few short paragraphs, important meta-economic issues were concisely summarized and

analyzed. The polished prose flows ever onward, and the careless reader may easily overlook the diamonds among the pebbles.

12. Walter Bagehot, Economic Studies (Stanford: Academic Reprints, 1963), p. 5.

13. Bagehot's analysis of the reasons for the declining popularity of economics was partly based on an economic and social analysis of the interests of politicians and of melioristic social reformers. His views on this topic are worth quoting even today since the truth of his opinions is observable everywhere around us. The "solutions" which our public officials offer for our economic woes and the elitist attitudes of our leading reformers correspond all too well to Bagehot's predictions.

And that English political economy is more opposed to the action of government in all ways than most such theories brings it no accession of popularity. All governments like to interfere,--it elevates their position to make out that they can cure the evils of mankind: and all zealots wish they should interfere, for such zealots think they can and may convert the rulers and manipulate the state control,--it is a distinct object to convert a definite man, and if he will not be convinced there is always a hope of his successor; but most zealots dislike to appeal to the mass of mankind.

Economic Studies, op. cit., p. 6.

14. As Bagehot so tersely stated the matter in one instance: "English Free Trade is almost everywhere unpopular. Experience shows that no belief is so difficult to create and no one so easy to disturb." (Economic Studies, op. cit., p. 26.)

15. For Bagehot's view on the special difficulties of the humane sciences, see Economic Studies, op. cit., p. 86.

16. Bagehot's comments regarding "uncultured moralists" are probably more applicable to the Victorian Christians than to more sophisticated methodological critics of economics like Symes. The complete quote, in context, appears in Economic Studies, op. cit., p. 86.

17. As Gunnar Myrdal has documented in his Political Element in the Development of Political Economy (New York: Simon and Schuster, 1969), pp. 1-9, many writers both before and after Bagehot had condemned the use of economics as a tool for political persuasion; but few of these economists adhered to their own prohibitions:

... the cultivators of the abstract science

itself (even those who fully understood its peculiar nature) did not always in practice remember the remoteness to practice of that nature. On the contrary, they rushed forth into the world with hasty recommendations to instant action; whereas the very justification of their reasonings, and the very ground of their axioms, was the necessity of beginning the investigation of the subject in a simple theory, and far away from the complexities of practice and action.

Economic Studies, op. cit., pp. 86-87.

18. Economic Studies, op. cit., p. 87. The second occurrence of the word "physical" in the quote is obviously a slip of the pen.

19. Ibid., p. 20. While the increasing "dryness" of political economy was a plausible explanation for the loss in status which economics had suffered in the public mind, this point was strongly discordant with Bagehot's expressed desire to reinforce the scientific character of the study. A discipline, it would seem, must at some point in its development abandon the interest and appeal of storybook illustrations, understandable by all, for the more serious pursuits of organized knowledge. If the goal of such an abandonment is to increase the scientific integrity of the study rather than to render it simply more obscure, then it should be hailed as a triumph rather than denigrated as a defeat.

20. After connecting the hesitancy of English economists in matters of verification with their feeling that "the most obvious phenomena of many nations did not look much like their abstractions," Bagehot expressed his own belief that "in the societies with which the science is really concerned, an almost infinite harvest of verification was close at hand, ready to be gathered in." It was due to the neglect of these rich sources of potential data that "much confidence in the science has been lost, and it is thought 'to be like the stars, which give no good light because they are so high'." (See Economic Studies, op. cit., pp. 20-21.)

21. Bagehot provided an exceedingly clear statement of the positive nature of economic studies in which he distinguished clearly between the pursuits of the economist and the "higher" pursuits of the ethical theorist:

... our political economy does not profess to prove this growing world to be a good world, far less to be the best. Abroad, the necessity of contesting socialism has made some writers use the conclusions brought out by our English science for that object; but the aim of that science is far more humble,--it says,

"These and these forces produce these and these effects," and there it stops. It does not profess to give a moral judgment on either; it leaves it for a higher science, and one yet more difficult, to pronounce what ought and what ought not be.

Economic Studies, op. cit., p. 23.

22. Ibid., p. 156.

23. In defining economics as the science of business and of the profit making activities of men, Bagehot was careful to note that these assumptions were not intended as full descriptions of the world, but only as a specification of the conditions under which economic analysis applied:

The science of political economy, as we have it in England, may be defined as the science of business, such as business is in large productive and trading communities ... Dealing with matters of "business," it assumes that man is actuated only by motives of business: it assumes that every man who makes anything makes it for money, that he always makes that which brings him in most at least cost, and that he will make it in the way that will produce most and spend least; it assumes that every man who buys, buys with his whole heart, and that he who sells, sells with his whole heart, each wanting to gain all possible advantage. Of course we know that this is not so, that men are not like this, but we assume it for simplicity's sake as a hypothesis; and this deceives many excellent people, for from deficient education they have very indistinct ideas what an abstract science is.

Economic Studies, op. cit., p. 7.

24. Ibid., pp. 88, 90.

25. Ibid., p. 8.

26. Ibid., p. 23.

27. Walter Bagehot, Physics and Politics (Boston: Beacon Press, 1956, pp. 37, 55.

28. Bagehot was most insistent about limiting the applicability of economics to the conditions of an advanced industrial state with predominant features of freemarket capitalism and in excluding all forms of "traditional" societies:

... no intellectual attempt can be more absurd than the attempt to apply the conclusions of our political economy to the lives of nations at a non-commercial stage of their existence. A great military nation based on slavery, like the Romans; a nation bound by fixed customs, like so many Oriental nations; tribes in a state of barbarism,--are not guided principally by the commercial spirit.

Economic Studies, op. cit., p. 90.

29. That Bagehot's view was primarily "naturalistic" despite certain differences which he saw between physical and social science is clearly illustrated by the following:

I do not claim for the conclusions of English political economy the same certainty as for the laws of motion; but I say that the method by which they have been obtained is the same, and that the difference in the success of the two investigations largely comes from this,-- that the laws of wealth are the laws of a most complex phenomenon which you can but passively observe, and on which you cannot try experiments for science' sake, and that the laws of motion relate to a matter on which you can experiment, and which is comparatively simple in itself.

Economic Studies, op. cit., p. 16.

30. The reader interested in the anti-scientific perspective which Popper has labelled "historicism" should refer to the Appendix on Terms to the first chapter of this dissertation and to Karl Popper's The Poverty of Historicism (New York: Harper and Row, 1959). An abbreviated presentation of many of Popper's key doctrines is found in Bryan Magee's Karl Popper (New York: Viking Press, Modern Masters Series, 1973), pp. 1-49.

31. Economic Studies, op. cit., p. 84.

32. For Hayek's early view of "scientism" and the method of the social sciences, see F. A. Hayek, The Counter-Revolution of Science: Studies in the Abuse of Reason (New York: The Free Press, 1955). Hayek subsequently recanted his earlier anti-naturalist views in Studies in Philosophy, Politics and Economics (Chicago: University of Chicago Press, 1967), pp. viii, 4-5.

33. Bagehot's comments concerning the "all case method" are found in Economic Studies, op. cit., pp. 13-14, 85. Although too lengthy to quote here, these comments contain an excellent characterization of the all-case approach to humane studies, including quotes from contemporary English and German sources.

34. Economic Studies, op. cit., p. 14.

35. Bagehot's quotes from Jevons' Principles of Science on the subject of Bacon's method are so devastating that they deserve to be presented in full:

... the method which Mr. Cohen suggests was tried in the physical sciences and failed ... the method which he suggests is exactly that which Lord Bacon himself followed, and owing to the mistaken nature of which he discovered nothing. The investigation into the nature of heat in the "Novum Organum" is exactly such a collection of facts as Mr. Cohen suggests; but nothing comes of it. As Mr. Jevons well says, Lord Bacon's "notion of scientific method was that of a kind of scientific bookkeeping: facts were to be indiscriminately gathered from every source, and posted in a kind of ledger, from which would emerge in time a clear balance of truth. It is difficult to imagine a less likely way of arriving at great discoveries."

Economic Studies, op. cit., p. 14.

36. Although Bagehot's criticisms of the all-case method cover many pages, we will quote only one short passage in order to obtain a flavor of his style of argumentation:

... the "All-case" method--is impossible. The facts of it are one thing to-day and another to-morrow; nor at one moment does any one know them completely. Those who best know many of them will not tell them or hint them; gradually and in the course of years they separately come to light, and by the time they do so, for the most part, another crop of unknown ones has accumulated. If we wait to reason till the "facts" are complete, we shall wait till the human race has expired ... In real life scarcely any one knows more than a small part of what his neighbor is doing, and he scarcely makes public any of that little, or of what he does himself. A complete record of commercial facts, or even of one kind of such facts, is the completest of dreams; you might as well hope for an entire record of human conversation.

Economic Studies, op. cit., p. 16-17.

The interested reader should also refer to Economic Studies, pp. 85, 108 and 152.

37. Ibid., pp. 14, 15-17.

38. Ibid., pp. 17, 173.

39. Ibid., pp. 84, 85.

40. "The maxim of science is simply that of common-sense-- simple cases first; begin with seeing how the main force acts when there is as little as possible to impede it, and when you thoroughly comprehend that, add to it in succession the separate effects of each of the incumbering and interfering agencies." (Economic Studies, pp. 83-84.) See also pp. 85 and 173.

In response to the closely related question posed by Symes: "Why postulate a hypothesis with exceptions?" Bagehot provided a most reasonable answer:

It may be asked, What is the use of laying down such a rule, if you admit and discuss exceptions to it? Why invent a hypothetical hedge, when you know that it does not include all you want, and that therefore you will be unable to keep within it? The answer is that the nearest way to the whole truth is by pursuing the clue which the partial truth first gave.

Economic Studies, op. cit., p. 95.

Such an answer, of course, implies an abandonment of the a prioristic examination of mental contents for the discovery of new economic relations. It is unlikely, however, that this implication would have much disturbed Bagehot since he was already a convinced empiricist.

41. Economic Studies, op. cit., p. 173.

42. "The discovery of a law of nature is very like the discovery of a murder: in the one case you arrest a suspected person and in the other you isolate a suspected cause." (Economic Studies, op. cit., p. 17.)

43. Ibid., pp. 7, 86.

44. Bagehot never doubted that the businessmen of his day possessed a wonderful knowledge of the details of the market activity that surrounded them, at least those businessmen who did not go down in the waters of turbulent competition: "Men of business have a solid judgment, a wonderful guessing power of what is going to happen, each in his own trade, but they have never practiced themselves in reasoning out their judgments and in supporting their guesses by argument." (Economic Studies, op. cit., p. 9.)

Yet he deplored the lack of communication and mutual

respect between businessmen and economists and considered it as one of the main barriers to the advancement of the science:

Men of business can no more put into words much of what guides their life than they could tell another person how to speak their language. And so the "theory of business" leads a life of obstruction, because theorists do not see the business and the men of business will not reason out the theories: far from wondering that such a science is not completely perfect, we should rather wonder that it exists at all.

Economic Studies, op. cit., p. 10.

... political economy--effectual political economy, political economy which in complex problems succeeds--is a very difficult thing; something altogether more abstruse and difficult, as well as more conclusive, than that which many of those who rush in upon it have a notion of. It is an abstract science which labors under a special hardship: those who are conversant with its abstractions are usually without a true contact with its facts; those who are in contact with its facts have usually little sympathy with and little cognizance of its abstractions.

Economic Studies, op. cit., p. 9.

45. Wassily Leontief, "Theoretical Assumptions and Unobserved Facts," American Economic Review, Vol. 61 (March, 1971), p. 1.

46. Bagehot's doctrine of the results of an imperfect fit of the theory to prevailing social conditions is found in several parts of his economic writings included among which is the following passage: "All this is as true of political economy as of any physical science; its deductions may be incontrovertible, and its results precisely true, whenever its assumptions are true; but these results will be very imperfect guides wherever those assumptions are impaired by contradictory matter." (Economic Studies, op. cit., pp. 87-88.)

As we have seen, Bagehot was frequently critical of the Classics for the way in which they had employed the concept of "disturbing causes" as a defense for the "universal applicability" and "absolute truth" of their theories. Unfortunately, however, Bagehot apparently saw no fundamental defects in the related treatment of economics as "a science of tendencies." We quote the key passage from his writings:

It is on account of its abstract character that political economy is often and justly described

as a science of "tendencies" only; that is, the object of it is, to work out and ascertain the result of certain great forces, as if these alone operated, and as if nothing else had any effect in the matter. But as in matter of fact many other forces have an effect, the computed results of the larger isolated forces will never exactly happen: they will only, as it is said, tend more or less to happen; that is, they happen more and more nearly in proportion as the resisting and perturbing causes in each case happen to be less and less.

Economic Studies, op. cit., p. 85.

It is a non sequitur to imagine that the less-than-universal applicability of a theory implies that the theory can only predict tendencies, but this was apparently the fallacy into which Bagehot had fallen.

47. Bagehot's recognition that the Classical theory nowhere fits the actual existing conditions is spelled out in the passages on pages 88, 90 and 7 of the Economic Studies.

48. The effects of advancing too comprehensive claims for the predictive power of economic theory are traced out in two lengthy quotes from Bagehot's writings:

It has often been put forward, not as a theory of the principal causes affecting wealth in certain societies, but as a theory of the principal, sometimes even of all, the causes affecting wealth in every society; and this has occasioned many and strong doubts about it.

... the greatest confusion arises if you try to fit on uneconomical societies the theories only true of, and only proved as to, economical ones. In my judgment we need, not that the authority of our political economy should be impugned, but that it should be minimized; that we should realize distinctly where it is established, and where not; that its sovereignty should be upheld, but its frontiers marked: and until this is done, I am sure that there will remain the same doubt and hesitation in many minds about the science that there is now.

If economists had distinctly set before themselves that they were dealing only with the causes of wealth in a single set of societies, they might have effectively pointed their doctrines with

facts from those societies; but so long as the vision of universal theory vaguely floated before them, they shrank from particular illustrations. Real societies are plainly so many and so unlike that an instance from one kind does not show that the same thing exists in other societies,--it rather raises in the mind a presumption that it does not exist there; and therefore speculators aiming at an all-embracing doctrine refrain from telling cases, because those cases are apt to work in unexpected ways, and to raise up the image not only of the societies in which the tenet illustrated is true, but also of the opposite group in which it is false.

Economic Studies, op. cit., pp. 19,20.

49. Ibid., p. 21.
50. Ibid., p. 12.
51. Joseph Schumpeter, A History of Economic Analysis (New York: Oxford University Press, 1954), p. 824.

CHAPTER VII

JOHN KELLS INGRAM:

THE TRANSITION IN BRITISH HISTORICISM

Perhaps the best known, though least understood, of the British Historicists was J. K. Ingram. Ingram was the author of the first systematic history of economic thought to be published in the English language,¹ he was a Professor of Literature at Trinity College, an ardent believer in his own Irish heritage, and a follower of the "Positive Philosophy" of Auguste Comte.² Ingram's History of Political Economy, which first appeared as an article in the Ninth Edition of the Encyclopedia Britannica (1887), has been cited as an authority by virtually every subsequent account of the field, while Ingram himself has gained the distinction of being one of the few "non-orthodox" writers to regularly attain at least passing notice in all the major histories of economic thought.³

Despite the widespread acknowledgement of Ingram's work, however, the assessments of his views have regularly followed a uniform and quite superficial pattern. His History is frequently the only of his economic writings mentioned by past historians, and his views are either considered as the paradigm of the Historical School in Britain, which they decidedly were not, or as a pale reflection of German Historicism, which they also were not. Of the many sources dealing with his perspective on economic investigation, only Ekelund,⁴ who attempted to place him within the intellectual milieu of his time, and Scott,⁵ who discussed more fully his meta-economic

doctrines, can be considered as having truly contributed to our understanding of and appreciation for Ingram's purposes and procedures.

Ingram's Approach to the History of Economics

Since Ingram is primarily known for his work in the history of economic thought, it seems appropriate to investigate his views regarding the procedures to be followed in that type of study and the nature of the lessons to be gained from it. It is clear, first of all, that Ingram would today be classed as a "relativist."⁶ He held that economists are both led to the questions which they will pose by the press of social events and that, in addition, the answers which they will proffer for these questions will be largely dependent upon the prevailing modes of intellectual thought, the stage of development reached by their respective societies and their own particular psychologies and past histories.

In Ingram's view, the history of social science could not be represented as a gradual advance toward a more and more correct body of social theory (in terms of the explanatory scope and freedom from individual peculiarities of its component hypotheses). Rather, the history of any study should be understood as the development of a series of justifications for the existing social situation, and, ideally, a primitive anticipation of the subsequent social state. "Theory" is thus a reflection of the age in which it is created, and it is only at a very advanced stage of social development that one may hope to arrive at something approaching a truly "scientific"

study of society.⁷

Since a social base adequate to support the scientific study of social phenomena did not come into existence until the end of the Nineteenth Century, Ingram concluded that it was a conceptual mistake to condemn past theories for speculations which were later seen as inappropriate to subsequent developments of society. Rather, past theories are properly viewed as the historical precipitate of earlier social stages: as, "elements in an ordered series, to be studied mainly with respect to their filiations, their opportuneness and their influences ..."⁸ (The "opportuneness" of a theory depended upon whether it accurately reflected and justified the "spirit of [its] age," while setting the stage for the passing of that age to the next.) Even those theories held dear by himself and his contemporaries were, for Ingram, conditional upon and applicable to only the conditions and the stage of social, economic and moral development attained, or nearly attained, by the British nation during his lifetime.⁹

While it is all too easy to applaud Ingram's seemingly self-critical methodology in an age obsessed with "absolute truth" and "absolute certainty," it should be recognized that his epistemological relativism (i.e., his seeming refusal to engage in a prioristic speculations about "all possible cases") approached the conception which Popper later described as historicism (viz., a concern with purportedly fundamental and irreversible changes in social structures and in the character of "cultural influences" which occur in the course of a society's "evolution"). We will see in following

sections of this chapter that these "relativistic" doctrines in Ingram's philosophy were by no means isolated aberrations, but were, instead, components of a complex of views which Karl Popper has described as "historicism."

It is, however, important to distinguish this historicist form of relativism from the quite different practices described in the chapter on Cairnes. So far as any "relativism" requires the replacement of social theorizing by theorizing about the sociology (or social psychology) of social theory (ies) or so far as it rejects theories which are universal in form (in logical construction) along with theories which claim to be universal in application, then to that extent, and to that extent alone, is it fairly characterized as "historicist." The other types of "relativist" doctrines have, however, no necessary connection with these historicist views. It was an unfortunate twist of intellectual history that these different senses of the term became associated with each other in the writings of Nineteenth Century economists and social theorists.

The Epistemology and Methodology of Social Knowledge

In his consideration of questions surrounding the construction of "a theory of society," Ingram repeatedly voiced extensive objections to the dominant position held to by the English economists of his day. Yet he was also among the first to sharply condemn Professor Bonamy Price of Oxford for suggesting that social theorizing, and most particularly economic theorizing, was fraudulent, that politicians and citizens were better advised to follow the dictates

of "common sense" than to rely upon the speculations and pronouncements of economists:

That economic phenomena are capable of scientific treatment is a proposition which I do not intend to spend time in demonstrating ... Nor do I intend to waste words in showing that, if there be a science of society, no other branch of investigation can compete with it in importance or in dignity.¹⁰

A more fatal suggestion (than that of Professor Price) could not, in my judgment, be made ... the prevalent methods of economic research and exposition are open to grave criticism but how can this be remedied by throwing ourselves on the undisciplined and random inspirations of so-called common sense? ... What security can there be in this as in other branches of inquiry against endless aberrations and confusions, but systematic observation and analysis of the phenomena, resulting in a body of ascertained and realized truth, and what is this but science?¹¹

Like Leslie, and other more sophisticated Historical economists, Ingram declared himself unopposed to the use of deduction in economic investigations provided only that it did not lead willy-nilly into aprioristic and metaphysical speculations about general human motives ("the desire for wealth"), did not obscure those peculiar features of different societies which might play an important role in the analysis of economic problems which arose in these societies, and did not completely displace the complementary methods of "historical research" and "induction" in those cases where these methods would prove more fruitful.¹²

Ingram's views concerning the character and function of a theory or "law" seem both conventional and unexceptional in the context of

his age, although he did commit the then-common error warned against by Bagehot, Whewell and a host of more modern writers¹³ (viz., the confusion of scientific laws and empirical generalizations): "Science is simply the ascertainment and co-ordination of laws; a law is the statement of a general fact; we explain a specific fact by showing that it is a case of a more general fact."¹⁴ It is over the question of the proper object of social inquiry that Ingram departed most sharply from the later Classical and from most Western economists of the present day.

In accord with Comte, Ingram distinguished "static theories" which deal "with laws of coexistence" from those "theories of social dynamics" which dealt "with laws of succession."¹⁵ This distinction was repeatedly related to a "justification" by way of analogy between the biological and social sciences: "As in biology we have, alongside of the theory of the constitution and actions of an organism, the further theory of its development in time; so in Sociology we have, besides the doctrine of the constitution and actions of society, the doctrine of the constitution and actions of society, the doctrine of its evolution from a primitive to a higher condition."¹⁶ Although this rather flimsy analogy may seem, superficially, as nothing more than an ad hoc imposition in support of a highly suspect extension of social inquiry, we shall see below (pages 233 and 236) that the parallel between the developmental history of an organism and the laws of social dynamics was actually an integral part of Ingram's systematic and well-structured views concerning the methodology appropriate to sociology. It is not clear that Ingram's analogy

between "the constitution and action of society" and the "constitution and action of an organism" can be fitted so well in his more general perspective. Yet it can be safely asserted that Ingram, like previous Historical economists, was insistent upon the importance of including institutional and customary constraints as variables of significance for use in and application to "real world" cases.¹⁷

It is somewhat ironic that while Ingram's prescriptions for the reform of static theory are both definite and emphatic, his own efforts in this area were limited to his brief but impressive History of Slavery and Serfdom¹⁸ (also first published in the Ninth Edition of the Encyclopedia Britannica, and later revised into a book) and his even more brief, and much less impressive, "Government Valuation of Ireland."¹⁹ If Ingram was not himself a "pure theorist," then he was, almost exclusively, a historian of thoughts rather than events.

In Ingram's consideration of "dynamics" or dynamic laws of social change we meet with what Popper has identified as the core of philosophic historicism--the belief in laws determining the path of social change or development (the belief in "a science of history") and the assumption of a unique type of "understanding" which arises from the study of "historical science." For example: "It is now universally acknowledged that societies are subject to a process of development, which is itself not arbitrary, but regular; and that no social fact can be really understood apart from its history."²⁰ And also:

... the method of Sociology must be not only inductive, but historical; and by the latter name it may best be characterized. By this is meant, not merely that it finds the materials for its studies in the general field of human history: we mean further that it institutes a comparison of the successive states of society in order to discover the laws of social filiation--a process similar in principle to the biological comparison of organisms of different degrees of development.²¹

While Comte's "dynamical element" of social investigation was frequently referred to in Ingram's writings, it is apparent that he never developed this aspect of his thought to any great extent. The role of "dynamics" was that of a crutch used to support his more central concern with the essential unity of all social science. It in no way served as an underlying structure for Ingram's speculations as it did for writers such as Hegel, Marx or many of the Continental philosophers.

As just indicated, Ingram's most pervasive and fundamental criticism of late Classical economists was concerned with their "artificial" separation of the "study of wealth" from all other factors affecting social life. The close tie which was established in his mind between "dynamics and a necessary unity" of social investigation is illustrated in his remarks to Section F of the Royal Statistical Society:

... nothing is plainer than that in the course of the [sic] human evolution the several social elements did follow separate and independent processes of growth. The present economic state, for example, of the nations of Western Europe, as a group, or of any individual one amongst them, is the result of a great variety of conditions, many of them not in their own nature

economical at all. Scientific, moral, religious, political ideas and institutions have all concurred in determining it. But if they worked in this manner in the past, it follows that they are working so in the present. It is therefore impossible rationally to conceive or explain the industrial economy of society without taking into account the other co-existing social factors.²²

This same association of "dynamics" and the unity of social science occurs repeatedly in Ingram's writings, and lengthy passages are devoted to this topic in his History of Political Economy, his Work and the Workmen and his History of Slavery and Serfdom. Yet despite such adamant declarations as, "This question as to the relation of economic studies to the general body of human knowledge, is really the most radical and vital that can be raised respecting them, and on it more than on any other depends, in my opinion, the future of these studies,"²³ Ingram was ultimately willing to soften his position regarding the requirement for a unitary Science of Society. In his address to the R. S. S., he distinguished between those research programs which were ultimately unified and those which required each social scientist to become a Renaissance man, fully qualified in a variety of studies.²⁴ While abandoning the later path to those few of superior intellect (i.e., Comte),²⁵ Ingram endorsed the notion that "... a separate class of savants be appropriated to each (of the sub-divisions of sociology)." In order to avoid excessive specialization, it was necessary, however, that the research carried out by each of the separate classes of workers be only "temporarily and provisionally" isolated from the general course of social knowledge. One of the central tasks of the Science of Society remained

as the continuing synthesis of the knowledge gained in the sub-fields of ethics, government and political economy.²⁶

Biological analogies were again resorted to by Ingram in his attempt to justify a unified social science. For, as we are reminded, "the study of society ... is in so many respects kindred to biology," and the job of the sociologist, it appears, is closely analogous to that of the medical doctor.²⁷

Ingram's Empiricism

Another derivative of Ingram's attachment to the unified science of Sociology was his hostility to many of those anti-empirical attitudes popular among the late Classical writers. J. S. Mill and J. E. Cairnes, it will be recalled, had justified a resort to the concepts of "tendencies" and "abstract cases" (i.e., that of a purely "economic man") by maintaining that social phenomena were of an especially complex character. Because of the difference between social and physical phenomena, it was necessary to mentally isolate each possible motivation from all others and deal exclusively with its effects. While Ingram assented to the complexity of the process of social development (something quite different from the static phenomena which Classicals like Cairnes had considered) and to the necessity for some degree of specialization in its examination,²⁸ he was compelled to attack "the a prioristic or deductive view" in order to maintain his own position concerning the essential unity of social sciences. Although Ingram criticized the "deductive view" both for its "abstract character" and for its "too extravagant" claims to

universal applicability, the concentration in his arguments seem to center upon the former of these points.²⁹ Ingram's fundamental concern seemed to be the Classics' method of reasoning from an empirically false premise. Both the hypothetical construct of an "economic man" and any argument which relied upon the generalized concepts of "man" or "man's nature" were, for him, both unscientific and "pernicious."³⁰

Ingram suggested as the optimal replacement for the a priorism of orthodox economics a careful historical study of the actual development of both social institutions and social mores within each existing society.³¹ Here again, however, we are faced with the two-edged character of these suggestions: on the one hand they imply the quite reasonable view that differing legal codes and differing social customs may well exercise a quite considerable impact on the success or failure of particular economic policies, while on the other hand they assert nothing less than one of the basic Histori-cist contentions that "the main agency in the social movement ... [is] the accumulated influence of anterior on subsequent generations of mankind,"³² or to put the same point differently, that there is really no such thing as two events of the same type. Thus, there is no such thing as an ahistorical science of society. A certain uneasiness must necessarily accompany any interpretation of Ingram's writings which does not attempt to account for his purely histori-cist-evolutionist views along with his more constructive empirical and historical arguments. Yet the rationalization of these two elements of his thought is not so difficult if considered in the

light of the moralistic social theory of his day and his own pre-disposition toward the construction of an objective social ethics.³³

Ingram as an Ethical Theorist

We have so far concentrated on what might be characterized as Ingram's views on the epistemological character (the "scope and method") of social theory. Yet this aspect of Sociology was, to him, only a necessary propaedeutic for the achievement of ultimate results aimed at by "the Science of Society," i.e., the development of an objective social-ethics. In order to fully appreciate Ingram's perspective on the aims of social science, one must first turn, however, to the general outlines of the Comtian system upon which he built. Comte, according to Ingram, portrayed Sociology (the master science of society) as possessing the following features:

(1) it is essentially one science, in which all the elements of a social state are studied in their relations and mutual actions; (2) it includes a dynamical as well as a statical theory of society; (3) it thus eliminates the absolute, substituting for an imagined fixity the conception of ordered change; (4) its principal method, though others are not excluded, is that of historical comparison; (5) it is pervaded by moral ideas, by notions of social duty, as opposed to the individual rights which were derived as corollaries from the jus naturae; and (6) in its spirit and practical consequences it tends to [sic] the realisation of all the great ends which compose "the popular cause"; yet (7) it aims at this through peaceful means, replacing revolution by evolution.³⁴

This blending of normative and positive (in the sense of Wertfrei) elements is perhaps the most striking feature of this summary

statement of Comte's views. It was perhaps the basic intermixture of the "is" and the "ought," combined with the Victorian belief in a progressive development of social morals, that served as the foundation for Ingram's confusion of history and historicism. The connection may be constructed as follows: if one truly believes that the moral constitution of a nation is a major, if not the most important, factor in its growth and development, and if one also believes in a necessary dynamics of moral Progress, resulting from equally determinate changes in social form and economic organization, then one is inexorably led (as were Marx, Hegel and a host of lesser figures) to the notion of a value-laden and value-determining "science." This entire complex of interrelated, if not inferentially connected, concepts also implies that it is desirable for a social scientist to concentrate his attention not upon the individual (no matter how important) but upon the underlying "movements" and convulsions of "social development." In Ingram's words, "The ensemble must preponderate [sic] over the individual; and the constructors of theories must be regarded as organs of a common intellectual and social movement."³⁵ Ingram's own writings in the history of economic thought are, to some extent, reflective of precisely that perspective in that they consider the individual peculiarities of the "great" economists as essentially unimportant to the path of the discipline's development.

In Ingram's view, the essential character of an author's economic writings was a derivative of the age in which he wrote. Thus the Greeks and Romans had engaged in little economic theorizing

beyond what was required to explain the operation of their immediate households,³⁶ the medievals' economic pronouncements were originally derived from theological considerations and only slowly evolved toward a metaphysical or "natural law" perspective,³⁷ and the Mercantilists exploited this same metaphysical perspective on the Natural Order to justify the equally metaphysical notion of "the Nation."³⁸ During an early phase in their development the Classicals preserved the idea of "the Natural" as a source of external appeal against the all-powerful state authority which the Mercantilists had helped to create.³⁹ At a later period, however, after their allies in the manufacturing and retailing industries had gained the upper hand, they were anxious to claim that the New Order of conditional laissez-faire was based on mental certainties and unchanging a prioristic "laws of political economy" rather than upon the anarchical "invisible hand."⁴⁰ It was in the late Classical period, however, that Ingram found the roots of a "mature scientific view," for it was during this period that English and Continental Society became engaged in the final transformation from the Age of (destructive) Criticism, necessary to clear away the traditional forms of the Middle Ages, to a Positive Age of "rational and scientific" reconstruction.⁴¹

From Ingram's perspective it would be mistaken to say that the Greeks, Romans, Mercantilists or early Classicals had erred in either their goals or their methods. Rather, they had each fulfilled their necessary roles in the sequence of social development. Due to their efforts society could now progress beyond the stifling customary

arrangements of the Middle Ages and beyond the Age of Negativism (i.e., the Enlightenment) to a newer, scientifically planned and run social order.⁴² It was the purpose of the Comtian Sociology and of its important branch, Political Economy, to plot the course along which this Positive society would develop and thus to hasten its realization.⁴³

In economics the reflection of the developing Scientific Society was the growth of an Historical Spirit, that is, the rejection of the metaphysical or a prioristic concepts of a "human nature" or an "economic man" in favor of a study of institutional structures and institutional changes. Even though the initial reaction against the "excessively abstract" theorizing of the Classics had taken the form of a resort to the pure "historicity" of the facts of economic development (i.e., to a mere accounting of the "life history" of each "social organism"),⁴⁴ Ingram himself was not at all satisfied that a simple accounting of economic history was exhaustive of the contributions that economics could make to the study of social phenomena. In the later editions of his History of Political Economy he summarized in highly sympathetic tones the state of economic inquiry in the late 1880's:

The continued influence of the historical school is evident in the large output each year of historical, statistical and descriptive works and in the large proportion of time and energy devoted by economists to studies of this kind ... The economist who devotes most of his time to such studies, however, constantly uses theory and is conscious of its importance. His attitude toward theoretical studies is at least tolerant, sometimes encouraging. He is less apt to

be an extremist than were the earlier adherents of the historical school.

Most economists of the present day cannot be classified as adherents of any school. They recognize the importance of both historical and theoretical studies and their place in the development of the science, and many of them divide their energies between the two. They also recognize the importance of both induction and deduction and of the abstract and empirical methods. They are open-minded to new doctrines, but at the same time critical. They are synthetic as well as analytic.⁴⁵

And in his address to the Royal Statistical Society Ingram cautioned against any attempt to completely overturn the foundations upon which political economy had historically arisen:

I am far from thinking that the results arrived at by the hitherto dominant economic school ought to be thrown away as valueless. They have shed important partial lights on human affairs, and afforded salutary partial guidance in public action. The task incumbent on sociologists ... is to incorporate the truths already elicited into a more satisfactory body of doctrine, in which they will be brought into relation with the general theory of social existence--to recast the first draughts of theory, which, however incomplete, in most cases indicate real elements of the question considered--and to utilize the valuable materials of all kinds which their predecessors have accumulated.⁴⁶

Ingram's conservative impulses, when considering the complete abolition of economic theory, did not, however, carry over to his attitudes concerning the social significance of scientific inquiry. The explicitly Wertfrei limitations placed upon economic investigations by virtually all of the Classical writers were, for him, no more than the necessary accouterment of their "abstract" and excessively general mode of theorizing. In his "The Present Position

and Prospects of Political Economy," for instance, he stated of Senior's writings that:

... when Senior is led to make some observations of the utmost importance and interest, on the very doubtful advantage to a labouring family of the employment of the mother and the children in non-domestic work, he thinks it necessary to apologize for having introduced such remarks, as not, perhaps, strictly within the province of political economy. And when he finds himself similarly induced to observe on the evils of severe and incessant labour, and the benefits of a certain degree of leisure--subjects so momentous to working men, and closely connected with their material as well as moral condition--he pauses and corrects himself, admitting that he should not only be justified in omitting, but perhaps was bound to omit, all considerations which have no influence on wealth. This is the very pedantry of purism; and the purism is not merely exaggerated, it is really altogether out of place.⁴⁷

And in summarizing his position in commentary on Cairnes' Logical Method of Political Economy, he stated that;

... this systematic indifferentism amounts to an entire paralysis of political economy as a social power capable of producing or confirming in the mass of the community just convictions on the most important of all subjects. How, it may be well asked, are sufficiently fixed and convergent opinions on such matters to be generated in the public mind? How are the scattered lights, supplied by the several partial and one-sided studies of human affairs, to be combined, so as to convey social truth to the understanding, and impress its practical consequences on men's consciences?⁴⁸

For Ingram, then, social theory and political economy were tools not only for the discovery of truth but also for its promulgation or, more accurately, its propagandism. This attitude toward the function of social science may be, as Popper has contended, the psychological

derivative of the notion of a Scientific History,⁴⁹ but regardless of the truth of that contention, it does seem to be true that a belief in a normative social science leads its practitioners to a position of political activism. In a general sense, Ingram had already attained to the role of political prophet at the time of his address to the Royal Statistical Society. For he stated on that occasion that:

I believe that the most effective weapons against ... economic errors will often be found in reasons not based on material interests, but derived from a consideration of the higher ends of society, and the ideal of the collective life of the race. And, a fortiori, when we have to deal with the larger economic subjects, now rapidly increasing in urgency, which are more immediately in contact with moral conceptions, these questions of the ultimate ends of the social union cannot be left out of sight.⁵⁰

It was not, however, until 1880 that Ingram took his first decisive step away from activities which were primarily academic and toward those which were wholly political. In his speech to the Trade Union's Congress of that year, he proceeded from an exceedingly brief introduction concerned with matters of economic methodology to a justification for normative social theory as the necessary consequence of any investigations into social matters. Although the passage summarizing this transition from the Wertfrei investigations of the social scientist to the concerns of the social reformer is somewhat lengthy, it is worthy of quotation:

Every particular social problem is only a case of this general one, how to subordinate all social forces to the highest permanent well-being of the

entire community. Now, the more we study this great question, the more we shall find that no material expedients--however useful in their proper place--will suffice for its solution. That solution must be essentially moral. The end in view can be attained only by means of a generally accepted code of social duties, continuously applied and brought to bear on practice by the systematic solicitude of society. The essential basis of this action is the establishment of stable intellectual convictions respecting the conditions of healthy social life--in other words, a scientific Sociology. Duties, in fact, are social functions freely performed, and, they cannot be fixed with the degree of definiteness necessary for practical discipline, without a study of the functions as they arise out of the natural constitution and historical development of the social body. The ideas appropriate to each function must thus be elaborated, in order to determine the corresponding duties. This is the high practical destination which lies before Sociology, and which gives it an importance and interest transcending that of every other department of human knowledge.⁵¹

The remainder of the address contains an expression of those social attitudes which Ingram associated with an advanced Positive Society. While these doctrines are of little interest from the standpoint of economic methodology or of economic theory, they do reflect the totally unjustified and arbitrary manner in which historicist-evolutionist economists have claimed for their own values the authority of a purported scientific analysis. In Ingram's case it is also remarkable that those social goals and institutions which he associated solely with Comtian Sociology were practically indistinguishable from the goals and institutions conjured up in the utopian writings of "orthodox" economists such as J. S. Mill and Alfred Marshall.⁵² Entrepreneurs, he tells us, are worthy of respect

not for their organizational abilities or their skills in anticipating shifting consumer demands, but rather because they are properly "social administrators" holding a position which is "really a public office."⁵³ Labor unions are to be encouraged, not as bargaining agents for workers, but as agencies to promote their moral ascendancy.⁵⁴ And, of course, the hope of future Progress depends solely upon the continued enlightenment and elevation of the labouring class.⁵⁵ It appears that the ultimate inspiration of Ingram's social program was actually the condescending and morally righteous statism⁵⁶ of late Victorian social theorists, rather than any purportedly scientific analysis of "social dynamics."

Relativism, "Progress" and Social Determinism

A final aspect of Ingram's historical methodology which has only implicitly been referred to in the preceding pages was his seeming preoccupation with a series of "social stages" through which any society must inevitably advance. In his History of Slavery and Serfdom, for instance, he stated that slavery was "a necessary step in social progress." (A statement which was followed by a lengthy justification for both slavery and national warfare as brutal but necessary elements in national development.)⁵⁷ In his History of Political Economy this same theme is repeated, if somewhat more subtly, with protection being justified as necessary to the early period of a country's development.⁵⁸ Ingram's conviction in the necessity of such practices and institutions was so firm that he was even willing to proclaim that, "If the thought of the period, instead

of being compelled by contemporary circumstances, could have been guided by sociological prevision [sic], it must have entered with zeal on the same path [of Merchantile and Protectionist policies] which it empirically selected."⁵⁹

The element of "relativism" in Ingram's thoughts was thus reinforced, and it in turn reinforced not only a rejection of universally formulated theories describing human behavior but also any universal code of ethics or universally "correct" political policies. The fervor which Ingram felt for the arising Positive Society would, in his view, have been as justifiably felt by a Roman landlord bidding for a new slave or a craftsman petitioning for the protection of his profession, provided only that the time was correct.

Conclusion--The Position and Importance of Ingram in the British Historicist Tradition

As we have seen, Ingram shared with Jones, Bagehot and Leslie many of the same hostilities toward both Orthodox doctrines and individuals and some of the same programs for methodological reform of an "Historical" character. Yet in a very significant sense his writings form a watershed between the views of the early British Historical School and the evolutionary-historicists and early Neo-classicals. Like Marshall, he had a profound suspicion of lengthy mathematical investigations into social phenomena, although he was willing to admit the use of mathematics as a teaching tool.⁶⁰ From his Comtian convictions he derived a belief in the importance of

"social dynamics" and historical science (in the Popperian sense of "historical"). From his English contemporaries he absorbed the doctrines of social organicism and the distinction between military and industrial societies.⁶¹ Combining this already eclectic social faith with the German view of social policy, still new to the England of the 1880's, Ingram was to anticipate many features of the reformed "orthodox" position, a paradigm which would prove stifling to the future progress of economic research but which served as fertile ground for the social reform movements of the late Nineteenth and early Twentieth Centuries.

Despite his extensive criticisms of past Orthodox economists, Ingram was more than willing to grasp the olive branch once it was extended. In one of his last writings we find him describing the "great thaw" experienced in the economics of the 1880's and the rise of "a more humane and genial spirit (which) has taken the place of the dryness and hardness which once repelled many of the best minds from the study of Economics."⁶² In the later versions of his History of Political Economy, he was willing to treat "orthodox" authors such as Marshall with a healthy measure of respect, if not with full acceptance, and to comment favorably upon the element of "open-mindedness" which they had added to economic studies.

From the theoretical and policy positions which Ingram ultimately arrived at, it was but a small step to the complete disintegration of British Historical economics into the diverging branches of economic history and evolutionary historicism. Once that step was taken, in the writings of later authors such as Ashley and Cunningham, the

unique features which had distinguished the Historical tradition in British economics from both the German Historical School and from orthodox British economics simply ceased to exist. The Cunninghams, Harrisons and other like-minded historicists eventually formed the theoretical component of the collective-evolutionism which overwhelmed British social, ethical and political theory in the last decades of the Nineteenth Century. The Ashleys and Toynbees, on the other hand, devoted themselves to "pure" historical research, either of a literary-descriptive or "statistical" form. "Theory" among the former group became identical to a sophisticated form of social prophesy, while among the latter group it was referred to only in demonstration of its inherent falseness.

That any coherent presentation of a social view relies upon some sort of "static" reasoning (upon a "social theory") was consistently ignored by everyone except the Orthodox economists. It was thus upon the Orthodox methodological foundations that subsequent economic speculation grew and supported itself. The consequences of this rather disappointing turn of intellectual history are traced in some detail in the concluding chapter of this dissertation. The attempts both to reinforce the orthodox perspective through the introduction of new and more subtle arguments and to contract out of the dead-end into which it inevitably led economic inquiry are also considered in this concluding chapter.

Footnotes to Chapter VII

1. J. K. Ingram's A History of Political Economy was first published as an article, "Political Economy," in the Encyclopedia Britannica, 9th edition, Vol. XIX (Edinburg: Adam and Charles Black, 1885), pp. 346-391. It later appeared in book form in a number of printings and editions of which the following were referred to in the preparation of this chapter: (New York: Macmillan, 1888); 2nd edition (New York: Macmillan, 1907); Second Amplified Edition with an introduction by Richard T. Ely and an added chapter by William A. Scott (New York: Macmillan, 1915). The Second Amplified Edition is the source for all following references unless otherwise specified.

2. There is no entry on Ingram's life in either Palgrave's Dictionary or the Dictionary of National Biography. Luckily, however, Ely does provide us with a brief but informative sketch of his character and interests, of which the main passages are quoted below:

He was an able mathematician and a fine philologist. He wrote on Shakespeare and Tennyson, and was himself a poet of distinction ... Several of his associates after his death said that he was probably the most learned man in the world.

... political economy was one among his many intellectual interests. Nor was political economy his main interest. His main interest was religion. The Religion of Humanity as founded by Auguste Comte and developed by the Positivists.

History of Political Economy, op. cit., p. xiv.

Although Ely does inform us of the little-known fact that "Ingram was one of the founders of the Statistical and Social Inquiry Society of Ireland ...," the effect of this information is somewhat diminished by the additional information that "As such he wished to encourage the use of statistics to promote social reform." (History of Political Economy, op. cit., p. xiii.) The basically normative goals which motivated Ingram's economic and social investigations were also noted by Ely in his introduction to the History of Political Economy:

... the chief animating motive in Ingram's life was his enthusiasm of humanity [sic]. His passion was the general welfare ... Ingram's activity in all the societies with which he was connected shows that his desire to promote human welfare was with him the chief consideration,

and that science with him did not embrace an end in itself ...

History of Political Economy, op. cit., p. xv.

3. References to Ingram include the following: John Fred Bell, A History of Economic Thought, 2nd edition (New York: The Ronald Press Company, 1967), pp. 349-350; L. H. Haney, History of Economic Thought, 4th Enlarged Edition (New York: Macmillan, 1949); Wesley Clair Mitchell, Types of Economic Theory, Vol. II, edited and with introduction by Joseph Dorfman (New York: Augustus Kelley, 1971), pp. 38-39; Eric Roll, A History of Economic Thought, 3rd edition (Englewood Cliffs: Prentice-Hall, 1954), p. 311; and Henry William Spiegel, The Growth of Economic Thought (Englewood Cliffs: Prentice-Hall, 1971), pp. 401-403.

4. Robert B. Ekelund, Jr. and Robert F. Hebert, A History of Economic Theory and Method (New York: McGraw-Hill, 1975), pp. 198-203.

5. William A. Scott, The Development of Economics (New York: D. Appleton-Century Company, 1933), pp. 514-517.

6. The ambiguities surrounding the term "relativism" (and the corresponding term, "absolutism") have already been discussed at length in the chapter on Cairnes and will again be returned to in the conclusion to this dissertation. The following quote from Ingram's History is, however, sufficient to establish his place within the "relativist" camp under most definitions of that term:

The rise and the form of economic doctrines have been largely conditioned by the practical situation, needs and tendencies of the corresponding epochs. With each important social change new economic questions have presented themselves; and the theories prevailing in each period have owed much of their influence to the fact that they seemed to offer solutions to the urgent problems of the age ... every thinker, however in some respects he may stand above or before his contemporaries, is yet a child of his time, and cannot be isolated from the social mechanism in which he lives and moves. He will necessarily be affected by the circumstances which surround him ...

The movement of economic thought is constantly and powerfully affected by the prevalent mode of thinking, and even the habitual tone of sentiment on social subjects generally ...

History of Political Economy, op. cit., p. 3.

The noted author of a relativist history of economic thought

published during the 'Forties also recognized the relativist strain in Ingram's writings and identified him as one of the founders of this approach to the subject:

... the view prevails that the connection between reality and thought, economic life and economic theory must be comprehended as a process of action and reaction. It was, above all, John Kells Ingram and Lewis Haney who developed this thesis ... (emphasis in original)

Werner Stark, The History of Economics, in its Relation to Social Development (London: Routledge and Kegan Paul, Ltd., 1944), p. 5.

7. The extremity of Ingram's epistemological relativism, as opposed to that weaker form of relativism involved in the interpretation of the history of social thought, is illustrated by the following:

It is of highest importance to bear in mind these relations of economic research both to external circumstances and to other spheres of contemporary thought because by keeping them in view we shall be led to form less absolute and thus juster estimates of the successive phases of opinion. Instead of merely praising or blaming these according to the degree of their accordance with a predetermined standard of doctrine, we shall view them as elements in an ordered series, to be studied mainly with respect to their filiations, their opportuneness, and their influences ...

History of Political Economy, op. cit., p. 4.

8. Ibid., p. 4. See also J. K. Ingram, "The Present Position and Prospects of Political Economy," contained in Essays in Economic Method, R. L. Smyth (ed.), with an introduction by T. W. Hutchison (New York: McGraw-Hill, 1963), p. 51 for similar comments regarding the economic writings of Adam Smith. In History of Political Economy, op. cit., p. 106, Ingram criticizes Smith's "system" for being "... too absolute in its character; it does not sufficiently recognize the fact that ... man, as a member of society, is a child of civilization and a product of history, and that account ought to be taken of the different stages of social development as implying altered economic conditions and calling for altered economic action, or even involving a modification of the actor." A relativistic critique of Montesquieu's Sociology also appears in the History of Political Economy, op. cit., p. 90.

9. History of Political Economy, op. cit., pp. 4-5. This historically motivated attitude of humility toward the "absolute validity" of one's theories is in some sense a healthy antidote to the

professional diseases of intellectual arrogance and narrowminded opposition to theoretical innovations. On the other hand, if one is not willing to forcefully assert the truth of his speculations, he is equally unlikely to test their truth in the manner and variety required by the enterprise of critical science.

10. "The Present Position and Prospects of Political Economy," op. cit., p. 43 (hereafter referred to as "Present Position").

11. Ibid., p. 44.

12. Ingram's beliefs concerning the role of deduction in "Sociological" inquiries are summarized in "Present Prospects," op. cit., pp. 59-60. This basic position is further clarified, with warnings against "excessive deduction," in A History of Political Economy, op. cit., pp. 132, 207. Of these sources there is at least one passage which bears quotation, not only for the light it casts on Ingram's basic attitudes towards induction, but also for his rather typical Nineteenth Century confusion of "deductivism" and a priorism and his emphasis on the need to test the deductive consequences of a theory against observations:

Deduction has indisputably a real and not inconsiderable place in Sociology ... though economists of the so-called orthodox school recognize no other method ... (it is really) ... available only in simple cases. Social phenomena are in general too complex and depend on too manifold conditions, to be capable of such a priori determination. In so far as the method can be used, the vital condition of its legitimate employment is the ascertainment of the consilience of the results of deduction with those of observation; and yet such verification from fact of the conclusions of theory, though essential to the admissibility of this process of inquiry, is too often entirely overlooked.

"Present Prospects," op. cit., p. 58.

13. See, for instance, Chapter V of this dissertation and Appendix B.

14. "Present Prospects," op. cit., p. 45. It is curious to note that although Ingram apparently regarded a law as no more than a "generalized fact" (viz., an empirical generalization), in his discussion of the relationship between economics and statistics he warned against considering facts apart from theories: "This search (after 'the realities of the material life of society') must, of course, be regulated by general principles, and must not degenerate into a purposeless and fortuitous accumulation of facts ..." ("Present Prospects," op. cit., p. 71.)

15. The distinction between dynamic and static laws is presented quite clearly in "Present Prospects," op. cit., p. 50, but it is also referred to in a prefatory note which Ingram composed for R. T. Ely's Introduction to the Study of Political Economy:

It has been shown that Economic science, like Sociology ... must be--to employ the useful terminology of Comte--not statical only, but also dynamical. It must not assume one fixed state of society and suppose that it has to deal only with laws of coexistence, ignoring those of succession. It is now universally acknowledged that societies are subject to a process of development, which is itself not arbitrary, but regular; and that no social fact can be really understood apart from its history.

W. A. Scott, The Development of Economics, op. cit., p. 516.

16. "Present Prospects," op. cit., p. 50.

17. This is at least one reasonable interpretation of Ingram's continual expressions of concern regarding the use of a prioristic methods in explaining the "complex" phenomena of social action. See, for instance, "Present Prospects," op. cit., pp. 55, 58.

18. J. K. Ingram, A History of Slavery and Serfdom (London: Adam and Charles Black, 1895).

19. J. K. Ingram, "Government Valuation of Ireland," printed as a supplement to his "Work and the Workmen" (London: Longmans and Company, 1880).

20. The quoted passage is found in the preface to Ely's Introduction to Political Economy, reprinted in Scott's The Development of Economics, op. cit., p. 516. This similar, if not more extreme, passage is from Ingram's "Present Prospects," op. cit., p. 60: "There is, indeed, no more important philosophical theorem than this: that the nature of a social fact of any degree of complexity cannot be understood apart from its history."

21. "Present Prospects," op. cit., pp. 60-61. Ingram apparently believed that even Ricardo had seen the necessity for Historical Laws (for Laws of Social Dynamics) in his analysis. Although this necessity was one "... which from his own point of view it was impossible to supply." (A History of Political Economy, op. cit., p. 121.) With perhaps more justification, Ingram also claimed to have discovered "dynamical elements" in Smith's writings; although he cautioned that Smith had also been infected by "the Nature hypothesis"

of an earlier age. (History of Political Economy, op. cit., p. 91.)

22. "Present Prospects," op. cit., pp. 50-51.

23. Ibid., p. 48.

24. Ibid., p. 49.

25. Ingram's praise for Comte's intellectual abilities and accomplishments was lavish to an extreme. See, for instance, A History of Political Economy, op. cit., pp. 159, 191, 192 and "Present Prospects," op. cit., p. 51.

26. "Present Prospects," op. cit., p. 50.

27. Ibid. Ingram's argument by analogy between the practices and reputations of medical doctors and the practices and reputations of social scientists was hardly propitious. It is somewhat amusing that one of the central points of this analogy was precisely that no one would think of trusting himself to the care of a medical doctor specialized in the care of only one organ or bodily system and, thus, no one should trust those social scientists (the Orthodox economists) who dealt solely with one class of human motivations.

28. "Present Prospects," op. cit., pp. 43,59.

29. Ibid., pp. 56-58, 60-62, 66-68.

30. Ibid., pp. 56, 58.

31. The "static" part of Ingram's proposed reform of orthodox methodology is summarized in the following excerpt from his address to Section F of the British Statistical Society:

The phrase desire for wealth represents a coarse and crude generalization; ... the several impulses comprised under the name assume altered forms and vary in their relative strength, and so produce different economic consequences, in different states of society; and therefore ... the abstraction embodied in the phrase is too vague and unreal for use in economic investigations of a really scientific character ... All these economic motors require to be made the subjects of careful and extensive observation; and their several forms, instead of being rudely massed together under a common name, should be discriminated as they in fact exist.

"Present Prospects," op. cit., pp. 56-57.

32. Ibid., p. 60.

33. Ibid.
34. A History of Political Economy, op. cit., pp. 191-192.
35. Ibid., pp. 4-5; "Present Prospects," op. cit., p. 70.
36. A History of Political Economy, op. cit., pp. 9, 15, 21.
37. Ibid., pp. 24-27.
38. Ibid., pp. 40, 42.
39. Ibid., p. 90.

40. The reference is, of course, to the ultra-Orthodox classicals such as J. E. Cairnes and N. W. Senior. See, for instance, A History of Political Economy, op. cit., pp. 136-137.

41. The idea that the role of the early Classical was to act as destroyers of the older Medieval and Mercantile policies (i.e., that were primarily "negative" thinkers tied to an age of criticism) was reiterated at several points in Ingram's writings. See, for instance, his History of Political Economy, op. cit., p. 31, 62fn, 102, and his "Present Prospects," op. cit., p. 68.

Of these various sources the following passage from his History of Political Economy summarizes, perhaps most concisely, Ingram's attitudes toward the historical role of the classicals and the continuation and extension of their influence into the late Nineteenth Century:

The tendency of the orthodox school was undoubtedly to consecrate the spirit of individualism, and the state of non-government. But this tendency, which may with justice be severely condemned in economists of the present time, was then excusable because inevitable. And, whilst it now impedes the work of reconstruction which is for us the order of the day, it then aided the process of social demolition, which was the necessary, though deplorable, condition of a new organization.

A History of Political Economy, op. cit., p. 62.

42. The idea of a "scientifically constructed society" is but one variety of what Karl Popper has characterized as "utopian social engineering." See Karl R. Popper, The Poverty of Historicism (London: Routledge and Kegan Paul, 1957), p. 67. For a more recent discussion of this and related issues, see Alan Donagan's "Popper's Examination of Historicism," in The Philosophy of Karl Popper, Book I, P. A. Schilpp (ed.) (La Salle: Open Court Publishing, 1974),

pp. 905-924, especially pp. 915-916. We refer in the next footnote and in the conclusion to this dissertation to the link which regularly arises between a belief in historical prophesy cum "scientific planning" and political activism. This link is quite apparent in the historicist-normative character of both Ingram's and Comte's Sociology. Of the modern era, Ingram stated:

Now, however, that synthesis (of the forces of science and industry) is becoming appreciable; and it is the effort toward it and toward the practical system to be founded on it, that gives its peculiar character to the period in which we live. And to this spontaneous nisus of society corresponds ... a new form of economic doctrine, in which it tends to be absorbed into general sociology and subordinated to morals.

"Present Prospects," op. cit., p. 32.

And in his conclusion to the History of Political Economy, he proclaimed:

It will be seen that our principal conclusion respecting economic action harmonises with that relating to the theoretic study of economic phenomena. For, as we held that the latter could not be successfully pursued except as a duly subordinated branch of the wider science of Sociology, so in practical affairs we believe that no partial synthesis is possible, but that an economic reorganization of society implies a universal renovation, intellectual and moral no less than material. The industrial reformation for which western Europe groans and travails, and the advent of which is indicated by so many symptoms (though it will come only as the fruit of faithful and sustained effort), will be no isolated fact, but will form part of an applied art of life, modifying our whole environment, affecting our whole culture, and regulating our whole conduct--in a word, directing all our resources to the one great end of the conservation and development of Humanity.

A History of Political Economy, op. cit., p. 300.

43. In illustration of his historical methodology and historical determinism, Ingram states that:

When our object is not the explanation of any past or present fact, but the prevision ... of the future, and the adoption of a policy in relation to that future, our guide must be the historic method, conceived as indicating, from

the comparison of successive states, the general tendency of society ... and the agencies which are in the course of modifying existing social systems ... We can by judicious action modify them in their special mode of accomplishment or in the rate of their development, but cannot alter in their fundamental nature ... An attempt to introduce any social factor which is not essentially conformable to the contemporary civilization will result, if not in serious disturbance, at least in a mere waste of effort.

"Present Prospects," op. cit., p. 61.

See also the material appearing on pages 239 and 240 of this chapter.

44. A History of Political Economy, op. cit., pp. 199-200, 207-208; "Present Prospects," op. cit., p. 71.

45. A History of Political Economy, op. cit., p. 255. I must admit to an error in the inclusion of this quotation within the body of this chapter. Upon more careful examination I have discovered that it is properly attributed to William A. Scott rather than to J. K. Ingram. I do, however, still believe that it expresses Ingram's sentiments at the time of his death in 1885. As evidence for that position I offer the following material, found in one of his last writings and the quotations referred to in the immediately preceding footnote:

There has been (in recent years) what Professor Gide, the ablest representative of the new School in France, has well described as un grand degel-- "a great thaw." A more humane and genial spirit has taken the place of the old dryness and hardness which once repelled so many of the best minds from the study of Economics, and won for it the name of "the dismal science."

William A. Scott, The Development of Economics, op. cit., pp. 516-517.

46. "Present Prospects," op. cit., p. 69.

47. Ibid., p. 52.

48. Ibid., pp. 53-54. In the conclusion to his A History of Political Economy Ingram speaks even more clearly of the close connection between economics and social ethics in the coming age:

Economics must be constantly regarded as forming only one department of the larger science of Sociology, in vital connection with its other

departments and with the moral synthesis which is the crown of the whole intellectual enterprise ... Especially must we keep in view the high moral issues to which the economic movement is subservient, and in the absence of which it could never to any great degree attract the interest or fix the attention either of eminent thinkers or of right-minded men.

A History of Political Economy, op. cit., pp. 296-297.

49. See Karl R. Popper, The Poverty of Historicism, op. cit., pp. 14-17, 49.

50. "Present Prospects," op. cit., p. 54.

51. "Work and the Workmen," op. cit., p. 4.

52. Marshall, as mentioned in the chapter on his writings, saw a utopia of "social chivalry," while J. S. Mill believed in an age of cooperation and "ethical socialism" bordering on syndicalism and reinforced by organizations seeking the social refinement and education of the working classes. In such an atmosphere Ingram's rather absurd utopian visions are understandable, if not wholly justifiable.

53. "Work and the Workment," op. cit., pp. 5, 7.

54. Ibid., pp. 10-11.

55. Ingram's attitudes toward the social role and significance of the working classes in the Positivistic Age are, in part, represented in the following passages:

One thing is plain, that the working classes will more and more become the great laboratory of public opinion. This would be inevitable from the fact that they compose the mass of society, and suffer most from the imperfections of the social system, even if they were not, as they now are, invested with political power. Hence the importance to other classes as well as to themselves of their being directed in their judgments by a true social doctrine--which, discountenancing all violence and oppression, will at the same time furnish just standards founded on rational ideas, by which the mode of discharge of every social function, whether public or private, can be tried and estimated.

... it is almost implied that the great question is not how to improve and ennoble the workman's

life, but how to enable the ambitious and energetic to escape from it. I think current ideas on this matter require a good deal of correction. The causes which determine the rise of some to the rank of directors of industry, whilst others remain in the position of workmen, are not always easy to trace; most frequently, accidental elements of situation or opportunity are involved. But so far as personal qualities are operative, it would be a great mistake to suppose ... that a rise of this kind is always or ordinarily connected with superiority of nature. A man who remains a workman all his life may be, and often is, in all the essential qualities of manhood, of far greater intrinsic value than another who raises himself to wealth and rank.

"Work and the Workmen," op. cit., p. 18.

56. It is of some passing interest that Ingram was concerned that the trends toward collectivism, which he favored in the main, should not lead overly far into state socialism of the traditional sort. See, for instance, his History of Political Economy, op. cit., pp. 122, 298 and his "Work and the Workmen," op. cit., p. 8. Ingram also opposed cooperative schemes as utopian and divertive of the working movement's true aims ("Work and the Workmen," op. cit., p. 8.

57. A History of Slavery and Serfdom, op. cit., pp. 3-5.

58. The theme of the necessity of a slave-owning stage in the development of civilization and of the morally relative character of such institutions is again discussed in A History of Political Economy, op. cit., p. 16.

59. Ibid.

60. Ibid., pp. 176-178.

61. For Ingram's use of these various concepts and categories of classical sociology see, for instance, A History of Political Economy, op. cit., pp. 8-9, 15.

62. W. A. Scott, The Development of Economics, op. cit., p. 517.

THE BRITISH HISTORICAL SCHOOL IN POLITICAL ECONOMY
ITS HISTORY AND SIGNIFICANCE
VOLUME II

A Dissertation
by
CRAIG JAY BOLTON

Submitted to the Graduate College of
Texas A&M University
in partial fulfillment of the requirement for the degree of
DOCTOR OF PHILOSOPHY

December 1976

Major Subject: Economics

CHAPTER VIII

ALFRED MARSHALL AND THE REFORM OF ECONOMIC ORTHODOXY

Alfred Marshall (1842-1924) was both a man of his time and a man who would set the tone for the future. He was one of the most cautious and thorough economists ever to set pen to paper. Yet his writings were often, for those very reasons, lacking in any hard empirical content. Rather than reorienting economic research towards matters of data collection and toward an increased emphasis on well-defined empirical problems, Marshall's example served to preserve the a prioristic character of Classical economics and to retard the future development of the discipline as an experimental or empirical science.

As a synthesizer of the pure theory of past economists Marshall has had few equals, and in matters of economic history of the literary variety, he was equally adept. He and his wife compiled several massive volumes recounting the long-term development and contemporary status of each of the major European economies, and in yet other works he himself examined the growth and functioning of money, credit and trade from the earliest times to the present. His concern with historical detail was unmatched among British economists, and his work was said to resemble the researches of the German Historical School more than the studies of his British colleagues. Yet Marshall's histories sometimes bordered on being little more than elaborate historical ledgers. While they contained references to a multitude of events, they connected these events, one with another,

only through "common-sense" homilies or historicist speculations concerning the "flow" of economic and cultural development.

Although Marshall continually admonished those who developed theories apart from facts or considered facts as a proper replacement for theory, he himself was seldom beyond reproach for those very failings. Virtually all of the material which he polished for publication is characterized by a systematic hedging of both facts and theories in a manner which renders its most critical aspects beyond either dispute or correction. His historical writings seldom touched upon definite empirical hypotheses, and the bulk of his significant theoretical discussions were hidden in footnotes or appendices.

Although he was undoubtedly a highly perceptive and conscientious scholar who labored with the best of intentions toward the reconstruction of a field fallen into disrepute and beset by a multitude of critics, Marshall's own accomplishments contained the seeds for a more basic disorientation of future economic research than that contemplated by the most extreme British historicist. His own reformulation of the economic Organon was tainted by the psychological reasoning popular in his time, by the peculiarities of his own personality and by his professional pride in economics and his desire to protect it against future assaults. What Freud would later do for (or to) psychology, Marshall did for Neoclassical economics. He created something which was less of an embryonic social science than it was a social metaphysics, i.e., a unified, unfalsifiable perspective on social events, which could be used to explain

any occurrence, or its opposite, but which would seldom yield definite and unambiguous predictions about the results to be expected from existing or proposed institutional arrangements.¹

In the following pages I have explored those features of Marshall's writings and professional conduct which were to have an enduring and unhealthy influence on the future development of economic methodology. While these considerations should not be allowed to detract from Marshall's many more positive achievements, they are a part of the historical record which has been unduly neglected. If I have committed the sin of allowing Marshall's defects to outweigh his merits, it is only because his defects were in matters connected with methodology. The sins of historical appraisal have been committed on the opposite extreme. The debt which modern economics has been examined with infinite variations of opinion since the nineteenth century; there need be little fear that the shortcomings in methodological matters are serious enough to overshadow his achievements in other areas of economic research.

Marshall's Intellectual Background

Alfred Marshall was neither typical of the insular English intellectual of his day, nor were his interests limited to the narrow specialty of political economy. We are told by reliable witnesses that he was personally familiar with all the major economic writings of both France and Germany, having read them all in their original languages, and that he had even perused and found some merit in the

neglected writings of American economists.² In his youth Marshall was noted for his mathematical abilities, and in his early career he made a thorough study of the fields of ethics, psychology (in its associationist form) and metaphysics.³ He attributed his first acquaintance with economics to ethical problems arising out of the existence of poverty and the inequality of income distribution,⁴ and he confided to a friend, shortly before his death, that the two greatest influences on his life had been his early acquaintance with Hegel's Philosophy of History and his first academic post as a lecturer in moral philosophy.⁵

Economics and Social Ethics

While it is easy for modern readers to discount the ethical and metaphysical aspects of Marshall's writings, these elements were crucial in shaping his goals and methods. Talcott Parsons has identified "two major streams of thought" in Marshall's "organization": "the nucleus of his economic theory proper" and a more pervasive "theory of the progressive development of human character."⁶ Each of these theories complemented the other by dealing with different aspects of the same subject: economics proper with the realities of hedonistic acquisition, and "the theory of activity" with the relationship between existing economic conditions and an individual's intellectual and moral development. Marshall himself believed that the "more important side" of economic inquiry was the study of "the influence of economic conditions upon (the developing) human character."⁷ He had relatively less regard for the technical

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apparatus which he himself had formalized for the study of maximizing behavior. Excessive poverty or riches too easily gained or too greatly enjoyed would, in Marshall's view, result in "stagnant, sluggish, wasteful and unreliable" personalities, personalities incapable of the steps necessary to insure continued ethical and economic progress. "Free industry and enterprise," with a satisfactory "standard of life," would, however, lead to the continued development of men's characters and generate a society characterized by "energy, industry, rationality, frugality and honorable dealing."⁸

Marshall's concern with what he believed to be the progressive evolution of the human character (or of "human culture") resulted in his adoption of certain postulates about social ethics which superficially resemble those paternalistic attitudes associated, in our own times, with writings such as Galbraith's The Affluent Society. Marshall distinguished sharply between "the standard of living" which would allow an individual to live and grow through the full exercise of his faculties and "a standard of comfort which might "suggest a mere increase of artificial wants [sic] among which, perhaps, the grosser wants may predominate."⁹ His solutions to the ills of "excessive" consumption were, however, the opposite of those later proposed by Galbraith. As Parsons has noted: "Marshall saw the field of business enterprise as the principal opportunity for the exercise of what he considered as the noblest traits of human character. The wealth acquired in the process was not the aim, but rather a by-product, and one which was not without its dangers (to the businessman)."¹⁰ While wealth (earned or unearned) could prove as a danger

to an individual's self-development, the opportunities for any degree of character development were directly related to those activities which led to the creation of ever-increasing amounts of wealth. The continued development of increasingly more steadfast individual characters was thus dependent upon increasing opportunities to put wealth to work in productive ways, rather than allowing it to be dissipated for purposes of individual indulgence. "Good" men could not be created by eliminating the opportunities for them to become "bad," but only by increasing the incentives and opportunities for them to engage in progressively more exertive enterprises.

Despite the well-developed character of his system of social ethics, Marshall always veiled his public expressions of these views in his typically obscure literary style. As Viner has pointed out: "Marshall ... never explicitly discussed these ethical ideals, and in fact sought deliberately to avoid being entrapped into open discussion thereof and into formal statement of his position by using as colorless and as irenic terms as were available to express the ethical implications and presuppositions of his economics. Without surrendering or completely concealing his position, he thus succeeded fairly well in escaping the necessity of ever having to defend it."¹¹ We shall see that Marshall's reluctance to assume an attackable stance on "controversial" subjects extended beyond his positions in the area of ethics and social evolution. Whether the issue was ethical, theoretical or factual, he always sought to express his opinions in a form which was so nebulous or obscure as to be unobjectionable.

Marshall as an Hegelian

The influence of Hegel on Marshall's thought was more indirect than that of Victorian standards of "right conduct," but it was, perhaps, no less important in the formation of his ultimate views concerning the character of social science. Of the philosopher's many works, Marshall was only familiar with The Philosophy of History, and his understanding and interpretation of that volume was undoubtedly influenced by his associate, Benjamin Jowett, Master of Balliol College.¹²

The biological analogy to social evolution, which Marshall apparently associated with Hegel, is considered at length in other sections of this chapter (see pages 281-285, 288). It is with reference to this imputed analogy that Schumpeter was correct in stating that, "Marshall did not understand Hegel at all."¹³ There is, however, another aspect to Hegel's writings: an abiding respect for the "Historical process," for facts which, properly interpreted, are the story of the advance of the "objective spirit of the World and of Man" (i.e., the Weltgeist). It was in this latter way, as a philosopher of the "historical method," that Marshall appreciated Hegel all too well and learned a great deal from his example.

Parsons, who was among the first to comprehend Marshall's persistent preoccupation with history, characterized the impact of this concern on Marshall's analytic style and his scientific methodology:

... he always refuses to define precisely the field of his investigations as a whole, and to work out his leading ideas to their ultimate logical consequences. On the contrary, he prefers to take up

all of what he considers the important aspects of each particular concrete problem as he comes to it, without figuring out the implication beyond the particular problem. The result is a lack of clarity as to what his position really is ... leading to a particularly insidious form of the "fallacy of misplaced concreteness."¹⁴

Yet even Parsons, who is rightfully acknowledged as a master historian of sociological thought, neglected the close and obvious connection between Marshall's methodological procedures and the example of Hegelian philosophy. Marshall's attention to the concrete particulars of each case, with a presumption that the interrelationships between the particulars would form themselves and his "misplaced concreteness,"¹⁵ which insisted upon detail to the detriment of interpretative clarity, were both characteristics of a typically Hegelian approach to social investigations. Marshall's choice of the Hegelian slogan "The many in the one and the one in the many"¹⁶ as the motto for his major work in economic history, Industry and Trade, and his opening discussion of the implications of "economic evolution,"¹⁷ beginning on the first page of that volume, reemphasize the Hegelian character of what he meant by "history" and "an attention to the particular." To place Marshall's "thirst for facts" within the British empiricist tradition of science is to commit a grievous error in interpretation.

Marshall's Attitudes Toward "Critical Science"

Marshall's views concerning the relative roles of history and theory in social investigations were also to mold his opinions of the type of "empiricism" which was appropriate to the social sciences.

Marshall's vivid awareness of the "complexities of the actual world" and his corresponding emphasis on the importance of detailed historical research has sometimes been interpreted as an anticipation of more modern attitudes toward social investigations. Yet even a superficial examination of Marshall's writings would dispel the idea that he was a precursor of modern "critical empiricism." Nothing could be more alien to the tone of his thought than the notion of an empiricism which feeds upon, and develops through, a process of critical discussion and well-defined test procedures, an empiricism which seeks to continually subject "established" theories to new and different tests carried out by new research teams under new and different test conditions.¹⁸

Marshall's attitudes toward economic theory were anything but critical. He was exceedingly intolerant of those who were not properly respectful to the writings of the Classical economists,¹⁹ and he assumed a similar, though usually more subtle, stance toward his own critics. His attitude toward the founders of economics was one of extreme deference. He seldom claimed to find any error in their works more serious than a certain vagueness of expression or a failure to complete a thought, and he was often known to attribute to them achievements more advanced than any they could have imagined.²⁰ Marshall was similarly protective of and sensitive about his own ideas and writings,²¹ leading to an increasing insularity of his thoughts and his eventual isolation from the intellectual world outside of his circle of admiring friends.

In a letter to John Bates Clark, Marshall wrote that he "scarcely

ever read controversies or criticisms. I have not read even a quarter of those which have been written about myself."²² On only the rarest occasions did he reply in public to his critics.²³

Marshall's rejection of all public controversy was applied equally to matters of pure theory and to the social issues of his day. He would frequently refuse to communicate any statements of his policy conclusions through the press, even when he informed friends and associates that he had developed a firm position on what he considered as an important issue.²⁴ On those few occasions when he resorted to the printed word to further normative purposes, his messages were phrased as "an appeal for ..."²⁵ rather than as a statement of his arguments in favor of or in opposition to a particular proposal. In policy matters Marshall always chose to rest his case upon grand ethical principles rather than upon a cool-headed analysis of the problems at hand.²⁶

Although many historians have attempted to justify Marshall's more dogmatic attitudes toward both positive and policy economics by reference to his attempts to increase unity within the economics profession²⁷ or by reference to his endorsement of a wertfrei economics,²⁸ their case in Marshall's behalf seems quite thin. While Marshall was demonstrably disturbed by the disunity and often outright bitterness which prevailed in economics during the 'Sixties, 'Seventies and early 'Eighties, and especially by the resulting damage to the reputation of the discipline, his own "solution" to this state of affairs was as much a function of his own personality as it was a consequence of his desire to "set things right." Two

questions demand our attention in assessing Marshall's vision of a properly reconstructed economics: What did Marshall regard as the "essential character" of economic theory? And what did he believe to be the proper relationship between economic theory and public policy?

The Creation of the New Economics

The questions involved in the unity of the economics discipline and in the validity of the Classical's system were of central importance to Marshall in the reconstruction of his new "economic organon." His plan for the "reform" of economics was not to rebuild its structure on the basis of well-tested empirical theories but rather to reconstruct it as a system of social maxims. He hoped to develop an economic philosophy so grounded in the study of history and in common beliefs about the "nature" of man and his world as to become impervious to fundamental criticism.

In his classical evaluation of Marshall's Principles of Economics, G. F. Shove has uncovered the core of Marshall's intent in his principal work:

Marshall set himself out to rehabilitate it (the reputation of economics) in the general esteem. The Principles is an apologia for economics as well as an exposition of it: a kind of counter-Reformation ... directed at doubts within and denunciations from without the fold.²⁹

Dogmatism was, however, more than an unpleasant element of Marshall's literary style. It was an intrinsic part of his personality. A contemporary who was commissioned to paint his portrait

has left us this description of Marshall in his memoirs: "Marshall I was told had a broad outlook on economic subjects, but on other subjects his views were angular, his opinions all corners ... not a gleam of humor lighted his talk ... he was a vain man."³⁰ Even Marshall's respectful and admiring nephew, C. W. Guillebaud, was driven to admit that "... Marshall was lacking in a sense of humor; and this was especially marked when any kind of moral issue was involved."³¹ The moralist in one sphere is often the true believer in others, and it is difficult to imagine a more dedicated and unwavering devotee to the notion of an absolutist economics than was Marshall.

Normative Social Theory

The notion that Marshall believed in a purely positive social theory or in a wertfrei economics is simply untrue, and no serious attempt has ever been made to justify this characterization of his views. Although he was fearful that the discipline would again become immeshed in the tangles of an unpopular ideology as it had during the mid-Nineteenth Century, Marshall had his own vision of a future utopian state based upon an ethological theory similar to Mill's and a theory of global evolution like that of Bagehot's.³²

According to his view, the institutions of British industry were destined to spread ever more extensively over the surface of the globe, finally engulfing even those "more phlegmatic races" and the inhabitants of the tropical regions. At the same time, however, the more advanced countries would have attained the social state or

customary arrangements which Marshall referred to as "economic chivalry." The static state, even the optimistic static state envisioned by J. S. Mill, was, for Marshall, an "uninteresting speculation." Rather, an ordered progress toward an ever more perfect human character and toward a set of economic customs compatible with this new social man was the natural course for the "gradual and continual unfolding of "economic evolution."³³ The precise implications of this process for the immediate future of British society might remain somewhat obscure to the uninitiated, and there has been some suggestion that Marshall desired to keep them so. He himself, however, saw their outline clearly enough to advise the government on matters of monetary policy, foreign trade, education, unemployment, taxation, land policy and the conduct of the war.³⁴ "It is a great thing," said the Pall Mall Gazette in 1890, "to have a Professor at one of our old Universities devoting the work of his life to recasting the science of Political Economy as the Science of Social Perfectibility,"³⁵ and it is undoubtedly true that little less than this was Marshall's actual goal. Marshall's hesitancy to express his policy opinions even more openly than he did and to engage in open debate over them was clearly a consequence of his aversion to controversy and his insistent avoidance of public displays, rather than a symptom of scholarly concern for the separation of positive theory and personal opinion. To equate his perspective on social investigations with those of Twentieth Century economists, or even with the quasi-positive studies of Nineteenth Century economists like Ricardo or Mill, is to

distort the historical record and to miscategorize Marshall's literary efforts.

Marshall's Empiricism

A taste for uncritical and undefined research procedures and a tendency toward the obscure have frequently been associated, in intellectual history, with anti-empirical approaches to social inquiry.³⁶ Marshall was but another example of that ancient association. Marshall's stance on many issues connected with empirical methodology (i.e., with the use of statistical data in economic studies) may seem ambiguous or even superficially encouraging to those who would like to interpret his works as the forerunners of modern economics. His positions concerning these issues were often, however, not so simplistic as the modern reader might at first presume.

As early as 1885 Marshall had professed to an enthusiastic interest in proposals to construct and issue statistical tables and graphs of both national and international economic data.³⁷ Yet his private correspondence and the example of his own writings are perhaps more telling than his public pronouncements. In correspondence with A. L. Bowley in 1906, he wrote that: "... since many (social or economic) ... causes have either no statistical side at all, or no statistical side that is accessible practically for common use, therefore the statistical element must be kept subordinate to general considerations ..."³⁸ And, in discussing problems in the published comparisons between real wages received by British and

German agricultural workers, he commented that: "I rely more on my 'field work' in the workmen's quarters of many German towns, and on my conversations with Germans in the Tyrol, than I do on Statistics."^{39,40} The methodology of relying mainly on "field work" would be admirable if systematically pursued according to predefined procedures. Marshall's "field work," however, relied solely on "casual observation" and casual conversation. He was seldom at a loss to describe the "typical" characteristics of any given set of economic phenomena, but his published works rarely referred to the masses of statistical data already available on many of these same topics.⁴¹ His "empiricism" was best expressed by his holiday stroll through a local factory,⁴² and he believed that the empirical scientist's primary skills, "a shrewd mother-wit, a sense of proportion and ... a large experience of life,"⁴³ were best developed by such first-hand "experiences."

Marshall's defenders have excused the absence of any significant amount of statistical corroboration in his numerous "factual" studies as a consequence of his desire to "write for businessmen" and his continual struggle to keep his works within readable proportions.⁴⁴ A more plausible explanation is, however, quite different. The assertion of definite numerical matters-of-fact invites, at two different levels, the very types of controversies and criticisms which Marshall had hoped to avoid: controversies about the accuracy of the "facts" and their relevance to the hypotheses proffered for testing, and controversies about the falsification of key economic hypotheses. That is, the use of statistical data may threaten not

only the application of certain economic hypotheses to certain situations but also the "validity" of the hypotheses themselves.

In pressing for the development of international economic statistics, Marshall was clearly not interested in providing improved means for the testing of economic relationships, not, at least, in the sense of critical testing. His proposal was, rather, aimed at the provision of additional source material which he hoped would be (psychologically) suggestive of new economic relationships or extensions of his own theories.⁴⁵ Marshall was undoubtedly aware that the availability of such international tables of economic statistics would provide him with expanded means of illustrating the uses to which his "engine of analysis" could be put, although he certainly would have been dismayed had it been suggested that any data could ever be used to refute his analytic structure.

The suggestion that Marshall omitted statistical data from his most important writings in order to please his business readers is quite probably the opposite of the truth. Marshall was undoubtedly aware of the favorable impression which a generous display of statistical data would have upon the numerically-oriented minds of the better class of businessmen, and had other factors not outweighed his desire to write in a manner pleasing to this class he undoubtedly would have drawn more frequently upon the available statistical sources.

Marshall's continual reliance upon platitudes, historical fables and the results of casual observation was primarily a result of his own methodological preconceptions. He saw the world through the

eyes of Hegel and Darwin: a dynamic, changing, infinitely complex, evolving system, the "essence" of which could only be caught in a glance from the corner of one's eye, rather than being faced full on. "Economic causes," he wrote, "are intermingled with others in so many ways, that exact scientific reasoning will seldom bring us very far on the way to the conclusion for which we are seeking ... it would be foolish ... to suppose that science can do all the work, and that nothing will remain to be done by practical instinct and trained common sense."⁴⁶ Statistical facts, illustrating the present state, or even the developmental course, of social life could only be a supplemental aid in the construction of more realistic theories. The social scientist was most likely to obtain satisfactory results, in Marshall's view, if he relied first of all upon "the powerful machinery of thought and knowledge that has been gradually built up by past generations"⁴⁷ as well as upon his own well-experienced professional instincts.⁴⁸

German and British Historicism

Alfred Marshall and J. N. Keynes have been known as the great peacemakers of Nineteenth Century British economics,⁴⁹ and the impression that they incorporated elements of both "orthodox" and "historical" economics in their writings has become an integral part of the professional folklore.⁵⁰ In a sense their writings were rightly viewed as an eclectic combination of Classical orthodoxy and historical economics, but not in the sense in which the term "Historical" has been used in most of our discussion (refer to

Appendix B for a discussion of the distinction between the various senses of the term "historical" and "historicism.")

"Historical economics" in Marshall's writings referred to the works of the early and late German Historical School, including the writings of Marx and Lassale, along with those of Roscher, Hermann and Schmoller.⁵¹ In an 1885 essay, Marshall summarized his assessment of the German Historical writings, as follows:

It would be difficult to overrate the importance of the work that has been done by the great leaders of this school in tracing the history of economic habits and institutions. It is one of the chief achievements of our age, and is an addition of the highest value to the wealth of the world. It has done more than almost anything else to broaden our ideas, to increase our knowledge of ourselves, and to help us to understand the central plan, as it were, of the Divine government of the world ...⁵²

It is apparent that his opinion did not change much over the next three decades, for the same passage was reprinted in the last edition of his Principles which appeared shortly before his death.⁵³

Schmoller was quoted with approval in both the later editions of the Principles and in Marshall's Industry and Trade.⁵⁴ He was also defended against his English critics in the first edition of the Principles,⁵⁵ although most of those passages were deleted from subsequent editions. Marshall's main advice to Keynes, upon reading the proofs of The Scope and Method of Political Economy, was that he should be "more favorable to Schmoller."⁵⁶ As Shove commented in his evaluation of Marshall's writings, "If any school left its mark on the Principles it was the (German) Historical School rather than the marginal utility school ..."⁵⁷

Marshall's relationship with the British Historicists was similarly cordial but less enthusiastic than his romance with their German colleagues. Leslie reviewed Marshall's Economics of Industry in a friendly, if somewhat reserved, tone, but cautioned his readers that: "This book ... makes greater changes in economic method and doctrine ... than might be perceived at first sight; for they are made without the sound of trumpet, and for the most part without controversy. Sometimes, indeed, they seem to us made without sufficient warning to call the student's attention."⁵⁸

Marshall had munificent praise for Jones' writings,⁵⁹ although he was never clear about the reasons for his admiration, and he was known to speak highly of Bagehot's economic efforts for their lofty literary style.⁶⁰ Beyond one-line references he entirely ignored the other British Historicists, however, undoubtedly applying his policy that silence was preferable to conflict.

What little controversy did exist between the Marshallians and the British Historicists was limited to the rather harsh reception accorded Marshall's early writings in Ingram's History of Political Economy⁶¹ and the totally unjust distortions of Leslie's methodological views in Keynes' Scope and Method of Political Economy.⁶² Although Marshall's passing feuds with Cunningham and Ashley would be of some interest in an expanded consideration of the decline of the British Historical School after the 1880's, they will not concern us here.

The difference in the treatment accorded to the British and German Historical Schools in Marshall's writings was not an isolated

or chance phenomena and was certainly not due to Marshall's ignorance of the extent of Historical writings in English. Rather, Marshall's attitude was partly reflective of his outlook on the general character of economic inquiry and partly due to his aversion to the opening of old wounds. The Germans were to be praised because they valued history and historical facts and because they recognized the importance of social evolution and its impact on economic theorizing. Their only fault was that they were sometimes extravagant in the claims they advanced for historical studies: they ignored the organon of analytic economics which was itself an historical product⁶³ in favor of a less tightly developed analysis of "economic facts."

The case of the British Historicists was far different. At best, they might have possessed valuable and productive traits, such as Bagehot's mastery of English style. Basically, however, they were upstarts, intolerant of the traditions of economic thought and eager to create further divisions and controversies within the discipline. Even while praising Leslie, along with a number of other deceased economists⁶⁴ in a spirit of De mortuis no nisi bonum, Marshall avoided any recognition of Leslie's methodological doctrines or of his attacks upon the Classics. In mentioning Leslie's contributions to the analysis of land systems, Marshall stated that "The whole history of land tenure is a most important study."⁶⁵ Yet in his own works we see that the emphasis of that statement is properly upon the phrase "history of land tenure," to which he himself would devote an appendix⁶⁶ of the Principles, rather than upon a

specialized theory of property structures which might result from such an historical study. Once again, Marshall's preoccupation with a "misplaced concreteness" and with the absolutist forms of economic theory were the determining factors in his attitudes toward the domestic variety of economic Historicism.

Mathematics, Abstract Reasoning and the Marshallian Method

Marshall's reputation as a mathematician is a prominent part of the legend that surrounds his name. Some authors even have claimed that the central character of his work was mathematical (that it was "disguised mathematics"⁶⁷) and that all of his principal writings were composed according to his own formula of "(1) Use mathematics as a shorthand language, rather than as an engine of inquiry. (2) Keep to them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real life. (5) Burn the mathematics. (6) If you can't succeed in (4) burn (3)."⁶⁸ Whatever the truth of that contention, it is certainly true that Marshall's skills in mathematical reasoning were notable while he was still a youth⁶⁹ and that his Mathematical Appendix to the Principles was a significant advance over the writings of earlier British authors (Walras being, of course, a non-British exception).

While omitting extensive mathematical excursions from his writings, Marshall was always careful to express his own admiration for and involvement with mathematical procedures. At one point in

his life he reported that he was better able to think in mathematics than in English,⁷⁰ whatever that may mean, and he repeatedly mentioned that his first endeavor in political economy had been an attempt to translate Ricardo and Mill into mathematical form.⁷¹ In fact, however, Marshall's mathematical skills may have somewhat waned toward the end of his life, a fact which brought him much discomfort, and his attitudes at even an earlier date were not so unconditionally favorable to the development of a purely mathematical economics as his early interpreters seem to indicate. In his Principles Marshall wrote that:

The chief use of pure mathematics in economic questions seems to be in helping a person to write down quickly, shortly and exactly, some of his own thoughts for his own use ... It seems doubtful whether anyone spends his time well in reading lengthy translations of economic doctrines into mathematics, that have not been made by himself.⁷²

Many years before his death, he had also confided to his friend, Francis Bowley, that he could not fully grasp the meaning of the basic formulas in correlation analysis.⁷³

Marshall's "misplaced concreteness" was again key to the attitudes which he developed toward mathematical economics. He was continually wary that any attempt to substitute mathematical reasoning for literary reasoning "might lead us astray in pursuit of intellectual toys, imaginary problems not conforming to real life; and further, might distort our sense of proportion by causing us to neglect factors that could not easily be worked up in the mathematical machine."⁷⁴ As a result of these considerations, he

hesitated to construct anything resembling a unified mathematical treatment of economic theory. Mathematics, for him, was only properly used to illustrate isolated points, never to substitute for the organon of intuition and innumerable economic facts.⁷⁵ (Extensive supplementary material regarding this issue is contained in the footnote.)

Marshall's fear of departing too far from "the facts" (facts in the sense of historical details rather than in the sense of demonstrably important institutional constraints) eventually led him to reject all "long trains of reasoning," whether mathematical or not.⁷⁶ He found the trait of extensive speculation alien to British thought and specifically singled out the "Semitic character" of Ricardo's mind as an explanation for its introduction into the Classical system.⁷⁷ Marshall had a passionate attachment to "facts," and he obviously believed that his own writings were carefully empirical in a way opposed to the extreme a priorism of the Ricardians.⁷⁸ It is one of the ironies of intellectual history that his example would serve as the inspiration for generations of Neoclassical theorists primarily interested in the construction of elaborate mathematical models⁷⁹ and too frequently convinced of the irrelevancy of "facts."

The Marshallian Style

Many classic works in social theory have been the subject of a perennial debate concerning their true worth. "Has this work survived and retained a meaning for a new generation because of

its profundity or because of its obscurity?" is the question which the intellectual historian must constantly reconsider. Marshall's most important writings have seen more than their share of this type of controversy, partly because they have been misinterpreted as prototypes for modern economic writings and partly because they share qualities of both the profound and the obscure.

While defending the worth of his contributions to economic theory, Marshall's nephew (and the editor of the critical edition of his Principles) C. W. Guillebaud remarked that:

... there is a difficulty of bringing Marshall to a focus. The sentences flow evenly and smoothly and each one by itself is perfectly intelligible and apparently simple; but having read a paragraph, a section or a chapter the problem remains--what was Marshall driving at, what does it all amount to ...?⁸⁰

Marshall's most stalwart American defender, F. W. Taussig, noted in a somewhat similar vein that "Marshall's style at its best is not luminous. He shrank from a plain and simple statement as from positive pain."⁸¹ Taussig was more than a mere critic of the Marshallian style, however. He sought with some success to identify the source of Marshall's rather peculiar mode of expression and finally concluded that it was, in large part, the product of his "anxious desire to prevent misunderstanding, the constant endeavor to forestall objection ... [which] had some consequences beyond caution and precision. The substance of [those] things [which he discussed] is left still obscure."⁸²

It is to Taussig's credit that he recognized the connection between Marshall's cautious style and his characterization of

economics as a science of "tendencies" and "approximations," a subject which dealt with an especially "complex subject-matter."⁸³ Yet the roots of Marshall's obtuse style run much deeper than this Millian perspective on economic science^{84,85} or his desire to anticipate any possible criticisms, although the path to a more complete explanation is a tortuous one.

Mary Paley Marshall identified a major influence on Marshall's social epistemology and, more indirectly, on his literary style, when she remarked that her husband "did most of his hardest thinking" during the long Alpine walks which filled his summer vacations. During these walks, she reported, he took along the works of "Goethe or Hegel or Kant or Herbert Spencer,"⁸⁶ all either evolutionist or "historical" philosophers. And, at a later date, his lectures began to reflect these readings, being composed of extensive commentaries on "the History of Economics, Hegel's Philosophy of History and Economic History from 1350 onward"⁸⁷ as well as more conventional material on economic theory. This tradition of interweaving historical and theoretical discussions was carried on into the later editions of the Principles, which contained many references to the complexity of economic relationships and the importance of both economic and intellectual evolution to a "proper" understanding of social science.⁸⁸ It is apparent that Marshall was gradually becoming convinced that economic reasoning was due for a radical reformation much like that which had previously transfigured geology or biology. The most unmistakable expressions of his belief in this coming transformation and the clearest

statements of the methodology which he believed would dominate the more advanced stages of economic reasoning are to be found, not in his lecture notes, written asides or appendices, but in his carefully polished contributions to professional journals. In an 1898 article entitled "Distribution and Exchange," Marshall devoted some ten pages to the future of economic theory. He expressed admiration for the doctrine of gradual and pervasive evolution which had revolutionized the theories and reasoning of biologists,⁸⁹ and he then proceeded to outline the course which he believed economic theory would eventually follow:

There is a fairly close analogy between the earlier stages of economic reasoning and the devices of physical statics. But is there an equally serviceable analogy between the later stages of economic reasoning and the methods of physical dynamics? I think not. I think that in the later stages of economics better analogies are to be got from biology than from physics; and, consequently, that economic reasoning should start on methods analogous to those of physical statics, and should gradually become more biological in tone.

The Mecca of the economist is economic biology rather than economic dynamics.⁹⁰

Similar statements were incorporated into the Principles where, as early as the first edition, Marshall had paid homage to "The notion of continuity with regard to development [which] is common to all modern schools of economic thought, whether the chief influences acting on them are those of biology, as represented by the writings of Herbert Spencer, or of history and philosophy, as represented by Hegel's Philosophy of History ..." ⁹¹ In the preface

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There is a great deal to be learned from the earlier stages of the development of the devices of physics. There are an equally large number of the later stages of the methods of physics which I think not. I think that in the future economics better analogies will be got from biology than from physics; and, consequently, that economic reasoning should start on methods analogous to those of physical statics, and should gradually become more biological in tone.

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which he appended to still later editions of that work, Marshall acknowledged that, "These two kinds of influences (those of continuity in history and of historical evolution) have affected, more than any other, the substance of the views expressed in the present book." In one of his later treatises, Industry and Trade, Marshall again returned to the notion of an economic science concerned with historical and "dynamic" (i.e., growth oriented) continuity. That volume can, in fact, be interpreted as an attempt to provide the raw materials from which a theory of economic evolution would eventually be constructed.⁹²

While influenced by Hegel and other German writers, Marshall's theory of evolution was not Hegelian. As noted by both Schumpeter and Parsons, it was "linear rather than dialectical."⁹³ The motto of the Principles, "Natura Non Facit Saltum," was the essence of Marshall's creed. Progress, whether in knowledge or in the development of the human character, took place through a process of gradual accretion; it could not be forced through revolutionary change.⁹⁴

The most critical question about Marshall's evolutionary outlook is not, however, its relationship to Hegel. What is of real importance is the impact of his evolutionism on the rest of his theoretical system. Schumpeter hinted at a possible relation between the theoretical content and evolutionary intent of Marshall's writings when he wrote that Marshall's thought "ran in terms of an organic, irreversible process," a process of which he had imparted "some of the flavor of ... to his theorems and concepts and still more to the factual observations with which he presented them."⁹⁵

Marshall himself was still clearer regarding his own views concerning social evolution. He wrote in one of his most revealing essays that:

... "Progress" or "evolution," industrial and social, is not mere increase and decrease. It is organic growth, chastened and confined and occasionally reversed by the decay of innumerable factors, each of which influences and is influenced by those around it; and every such mutual influence varies with the stages which the respective factors have already reached in their growth.⁹⁶

The consequences of a process of social evolution thus result in real differences in the types of phenomena which face the social scientist. Such a process renders these phenomena as unique events: unrepeatable occurrences not subject to the same type of laws which governed the "static" world of the physicist.

As if to emphasize the distinction between economic and physical laws and the further differences to be expected in the development of the two types of studies, Marshall wrote in his Principles that:

As the centuries wore on, people were getting clearer ideas as to the nature of organic growth. They were learning that if the subject-matter of a science passes through different stages of development, the laws which apply to one stage will seldom apply without modification to others; the laws of the science must have a development corresponding to that of the things of which they treat.⁹⁷

In this way, Marshall effectively closed the door on any hopes for an economic theory which was spatially and temporally general.

Marshall's aversion to "long chains of thought" and to the

extensive use of mathematics to model the world is easily understood if one accepts the Hegelian notion that there is no one world which can be modeled: that there are no fundamental static relationships in the social world, that things must be fundamentally and irreversibly different tomorrow in many ways which can only be incompletely predicted today. The lack of unity and order which characterized Marshall's style were, therefore, not the fault of some flaw in his education. They were a direct reflection of the philosophic spectacles through which he perceived the world. In Marshall's view it was possible to "predict" (or prophesize) the broad trends of historical development and to discuss these trends in a general and un-presuming manner. But to formulate theories which purported to describe unchanging cause and effect relationships without regard for the history of a society was, for him, the ultimate presumption.

Partial Equilibrium and General Equilibrium

Marshall's opinions regarding the role and relative importance of partial equilibrium and general equilibrium analyses in economics have often been misconstrued by historians of economics in an attempt to "modernize" his thoughts. Most contemporary economists acknowledge the usefulness of partial equilibrium tools and rely heavily upon that form of analysis. Although general equilibrium systems are admired for their mathematical elegance or as constructs which may be more closely descriptive of the way in which market economies may actually function, there are seemingly insurmountable difficulties in constructing general equilibrium systems for purposes of

prediction.⁹⁸ It was perhaps inevitable that similar views would be imputed to Marshall as the primary molder of the Neoclassical system in the English speaking countries, and the growth of such interpretations was in no way thwarted by Marshall's use of partial equilibrium welfare constructs such as consumer's and producer's surplus. Some authors have gone so far in their interpretation of Marshall as an unyielding advocate of partial equilibrium methods as to accuse him of unduly prejudicing the profession against general equilibrium models by turning to a consideration of their merits only in his later and lesser known work on Money, Credit and Trade.⁹⁹ The truth is otherwise, but it is not a simple truth.

It should first of all be noted that Marshall did not conceive of the same distinctions or divisions in economic theory which have become familiar to us today. General equilibrium analysis in his writings was not the equivalent of a closed economic model with fixed "structural" characteristics. It was, instead, the vision of an infinitely complex organic process in which each individual or institution affected all others and was in turn affected by them: where each action had consequences which permanently altered the environment in which all future actions would be formed.

Marshall's interest in partial equilibrium tools and in examining a variety of partial equilibrium situations resulted from a number of different motives. He, of course, believed that such tools were useful in the analysis of policy issues (i.e., taxation policy and income distribution), but he also hoped that the results of a developed program of partial equilibrium research could be

used to increase the understanding of and appreciation for the organic workings of the economy in general. In his critique of the Comtian program for constructing a universal science of social phenomena, Marshall attacked with great fervor the view that society could (and should) be analyzed as an irreducible gestalt: as a seamless entity of which economic activity, family, political and religious relations were inseparable aspects. Marshall asserted, to the contrary, that every science proceeded by breaking down complex phenomena into their component parts, analyzing each of these parts separately and then synthesizing the results of these partial analyses into a prediction about the behavior of the whole.¹⁰⁰ His discussion of partial equilibrium economic analysis, its relationship to general equilibrium analysis and the goals to be advanced by the progressive expansion of partial equilibrium cases toward increasing complexity corresponded exactly to this pattern. If partial equilibrium models of each important sector of the economy were conscientiously constructed and continually updated, Marshall believed they could then be used as the basis for a general equilibrium analysis of the entire economy. The partial analyses of the separate aspects of the economy were not eliminable in this process. Indeed, they provided the only possible building blocks from which a general equilibrium analysis of the entire economy could be derived.¹⁰¹

Marshall's view of the proper course for economic inquiry thus supplied a persuasive justification for what promised to be both productive and extensive studies of existing economic phenomena.

He was far from endorsing the notion that general equilibrium analysis was irredeemably unrealistic, a view often taken of the tradition which had been founded by Walras. Despite his long-term vision of a model describing the entire economy, Marshall was able to maintain that the immediate work incumbent upon economists was to plod along the path of cautious and detailed partial analysis. Only by painstaking construction, bit by bit, of models of each of the important sectors of the economy would economists take the first steps toward a final model of societal evolution.

Despite its great virtues in carefully balancing the theoretical concerns of statics and dynamics and of providing a spur to continuing research, Marshall's program for the development of economic theory did contain certain critical flaws. It implied that the job of synthesis was trivial once the components of the social system were clearly defined, and it drew no real distinction between a static general equilibrium system, in the modern sense, and an evolutionary-historical description of a society's development. In short, Marshall may have created an appealing vision capable of providing inspiration to himself and his colleagues, but his vision was operationally vague and probably unattainable in practice.

"Theoretical and Empirical" Ceteris Paribus Conditions

Marshall had early recognized and corrected the Classics' error of leaving unspecified the ceteris paribus conditions of their theories. He was conscientious in supplying an explicit list of the variables which might act as "disturbing causes,"¹⁰² and he seemed to

realize that certain items in such a list must be considered as more empirically significant than others (in the sense that changes in their values would have a "large" impact on the predictions of the analysis).¹⁰³ In repetition of a familiar pattern, however, Marshall offered no intersubjective procedures for separating significant from insignificant variables, for distinguishing those disturbing causes which should be listed explicitly in the ceteris paribus clause of an economic theory from those which were only "theoretical" sources of disturbance and which would have expanded such a list to unmanageable proportions. In this regard he once again seems to have fallen back on intuition and casual empiricism as a means to distinguish the two categories.

An intuitive approach to the determination of empirically significant disturbing causes would have been speedily recognized as inadequate had Marshall or his immediate successors been interested in operationally meaningful issues rather than in questions which were "testable" and empirical only in the broadest sense of those terms. In the interpretation given economics in the late Nineteenth Century, however, intuitive procedures filled a certain gap. Late Nineteenth Century economics was envisioned as a study providing explanations for all conceivable social events, as opposed to those more predictive sciences which attempted to differentiate the conceivable and the expectable. Because of its special character as a "social metaphysics," economics was composed primarily of commonsense "truisms," the contents of which were suggestive of various notions of positive truth, but which were sufficiently vague so as to be

open to quite different empirical interpretations. A man, for instance, might be said to be maximizing utility if he stepped in front of or out of the path of a speeding carriage, depending, of course, on the structure of the variables contained in his unspecified and, perhaps, unspecifiable, utility function. Indeed, any man necessarily must be maximizing utility, or intending to do so, in whatever he does, for that is the way in which the definitions of our economic system constrain us the the description of his behavior.

This intuitive and tautological approach to social issues left unresolved or arbitrarily resolved a number of important issues. For instance, the frequency of sunspots may affect the price of corn, as might the position of Mars or an infinite number of other things. The question of whether sunspots are as important to a determination of the future price of corn as is the prevailing or expected price of wheat is, however, a question which is necessarily empirical--which depends upon well-defined speculations and observations taken according to well-defined observation rules. As Marshallians, our immediate impulse is probably to discount the influence of the former factors in favor of the latter, and in so doing we may have excellent empirical support. But whether Marshall himself based his determination of the empirically important ceteris paribus conditions on a knowledge about past relationships between sunspots, the price of wheat and the price of corn, or whether he was again acting from intuition and a "keen mother-wit" is important to the place assigned him in the history of economic methodology.

Marshall as a Micro Theorist

Even though Marshall is usually regarded as the father of modern partial equilibrium micro theory of the sort taught to every undergraduate economics student, his works seldom contained tools appropriate to the analysis of the individual firm or to the analysis of the individual consumer. His demand curves were constructed for "different classes of consumers" or for the market as a whole, and his supply curves were usually for an industry or a "representative firm" rather than for any particular firm.¹⁰⁴ Even his discussions of social issues were couched in terms of Scholastic speculations about the nature of man or the role of classes¹⁰⁵ rather than being based upon the interests of particular men or the effects of particular types of institutional structures. His approach to economics was, in short, grossly aggregative, from start to finish.

The reasons for the aggregative quality of Marshall's "micro-theory" are fairly obvious in the light of the previous discussion. If economics is an evolutionary science, then it must concern itself with changes in the species (or in the culture) rather than with changes in the behavior of particular individuals or particular institutions. As Marshall himself stated: "For our present purposes the pliability of the race is more important than the pliability of the individual."¹⁰⁶

An additional influence of importance in molding Marshall's approach to economic questions was the traditionally aggregative character of the writings of Smith, Ricardo and the later Classics.

The very quality of "abstraction," the tendency to assume away all critical parameters in a social setting, which the British Historians had condemned in the writings of Classical authors, was preserved in a new and more subtle form in Marshall's most important works. It was precisely because Marshall rejected all laws of economics which were not either laws of thought or laws of history, and because he replaced empirical categories with singular facts, that he was unable to advance to a more significant form of economic analysis. That he insisted on constructing his pure economic theory in terms of aggregates while writing his history in terms of particulars (as opposed to using it to illustrate general social laws) was but the consequence of the anti-individualistic and historicist bias which Marshall had absorbed from Hegel.

Fortunately, Marshall was to have less of an impact in the delineation of the scope and subject-matter of economics than he had had in considering other methodological issues. The combined efforts of Jevons and the Austrians complemented his analysis of groups and weakened his prescriptive authority. As a result of their efforts, post-Marshallian economic researchers were able to select the degree of aggregation which seemed to best serve their various purposes and attack issues of intra-industry structure and intra-group relations as well as problems of a more collective character.

Marshall's Impact Upon the Profession:
An Evaluation of its Scope and Intensity

To determine the influence of one man's thoughts is a difficult

task, perhaps impossible. But it is at least possible to record what others have written about him and to note the kind and number of students who felt his hand.

Perhaps the modest evaluation of Marshall's impact on the future study of economics was penned by a noted sociologist and historian of ideas, Talcott Parsons. Despite obvious reservations concerning the validity of Marshall's social speculations, Parsons wrote that "... he is overwhelmingly the most eminent representative in his generation of the orthodox school, so that their case may be almost said to stand or fall with his work."¹⁰⁷ Many of Marshall's contemporaries and many later British and American economists were even less restrained in expressing praise for his efforts.

Soon after Marshall's death in 1924, the prominent American economist, Frank Taussig, wrote that: "None among the English-speaking will question his [Marshall's] primacy; and I doubt whether on the Continent or elsewhere a name could be mentioned that would dispute his title ... [he was] ... economic sciences' ... most distinguished representative."¹⁰⁸ Two years before, on the occasion of his eightieth birthday, the Royal Economic Society had sent Marshall greetings which contained the following passage: "But it is as a master of method and pathbreaker in difficult regions that we ... desire especially to greet you. Through you British economists may boast among their foreign colleagues that they have a leader in the great tradition of Adam Smith and Ricardo and Mill, and of like stature." Many "foreign colleagues" of the British obviously endorsed these sentiments, for the foremost economists of America, Germany,

France and Sweden also signed their names to the document.¹⁰⁹

For many years after his death Marshall's influence grew ever stronger. As late as 1942 David Macmillan could write in the Economic Journal that: "Through his Principles or by personal tradition as his pupils or his pupils' pupils, down to what is now the fourth generation, all British economists have been brought up under the pervasive influence of his thought,"¹¹⁰ and Joan Robinson would note, somewhat later, that "the search for Marshall's hidden assumptions has occupied a whole generation."¹¹¹

Marshall's indirect influence through his students was nearly as great as the authority granted to him by the popularity of his writings. In 1888 Foxwell noted, in the Quarterly Journal of Economics, that: "Half of the economic chairs in the United Kingdom are occupied by his pupils, and the share taken by them in general economic instruction in England is even larger than this."¹¹² As the Twentieth Century came into its own, many of the most prominent contributors to the discipline, Keynes, Pigou, Robinson and Sraffa, as well as numerous "lesser lights," were found to have been Marshall's students at Cambridge.¹¹³

Conclusions

Marshall's impact upon economic methodology and upon the patterns of economic investigation was, for many years, a substantial force molding the character of economic research. Yet it remains unclear whether his influence upon the decision of non-technical issues (i.e., those concerned with matters outside of the "core"

of economic theory) was for good or for ill.

Just as the intent and characteristics of Marshall's writings have been misinterpreted by many historians, so also has the full spectrum of his authority remained obscure. Methodological questions or issues of scientific procedure have seldom interested historians of economics, and the economic theorist has often shared with the historian a certain uneasiness over these problems. It is, indeed, difficult to locate any major methodological writings in English during the period from Keynes' Scope and Method of Political Economy (1890) to Lionel Robbins' The Nature and Significance of Economic Science (1932), and until very recently the methodological orientation of the profession was undoubtedly more reflective of the former work, and of Marshall's own views, than of any more modern contributions.

One cannot help but agree with Robinson's assessment that "the search for Marshall's hidden assumptions has occupied a whole generation" or with Taussig's observation that "... the beginner who has been introduced into the subject ... will get an impression (when reading Marshall's works) similar to that of the youth who read Hamlet for the first time: he did not think much of it because it was so full of quotations."¹¹⁴ Yet, despite the detailed attention devoted to Marshall's writings in economic theory, there have been few attempts to assess the impact of his analytic style, his counsel in settling the meta-economic disputes of the late Nineteenth Century or the example of his attitude regarding the sanctity of the profession and of its past.

Marshall might reasonably be credited with the restoration of the scholarly and popular reputation of economics, a reputation greatly tarnished by ideological reactions to the presumed political stance of the Classics and by the methodological attacks of the Historical school. He must certainly be recognized as a remarkably diligent and industrious scholar who drew upon many sources to construct a theoretical apparatus more sophisticated than that of any other social science and more successful (in terms of its flexibility and appeal) than any other form of social analysis yet invented. Yet there were costs as well as benefits in the discipline's wholehearted acceptance of Marshall's views.

Marshall himself was the product of an age obsessed with the doctrines of social evolution, intuitive certainty and historical sociology, all of which he integrated into his writings. His impressive achievements in economic theory and his wide knowledge of facts recommended these writings to professional economists, while his attempts at maintaining a conversational tone and a non-technical style rendered them influential among the more educated members of the public. In both cases, however, the depth and extent of his influence served not only to revive and popularize the Neoclassical system but also to extend the faulty notions characteristic of social speculation in his age into the social thought of the next century.

The "misplaced realism" which led Marshall to consider every possible situation without asserting anything definite about existing situations bred a generation of admiring economists who would copy his style without fully comprehending its original motivation.

Even today, over sixty years after the death of Marshall and over eighty years since the first edition of the Principles, there are still too few economists who can write a paragraph without hedging and qualifying their pronouncements to the point where they end up by asserting little or nothing.

The most questionable features of Marshall's meta-economics, as transmitted to future generations, were, however, his attachment to the notion of superior insight into the "real" nature of an economic problem and his mistrust of methodological controversy. Marshall's reliance upon a "shrewd mother-wit, a sense of proportion and a large experience of life," instead of upon established procedures for inter-subjective testing and the painful process of collecting accurate data, are methods which have only recently come into disrepute. Even today there are probably many pseudo-Marshallians who would claim that certain empirical relationships "have to be" solely on the basis of their own presumed intuitions and casual experiences.

Present attitudes toward methodological studies and the extreme defensiveness of the economics profession are, however, the most direct and unambiguous consequences of Marshall's views. A professional attitude which legislates against critical debates over "established" theories and which regards methodological research as ill-tempered, if not ill-mannered, can only lead to a long-run scientific revolution or to gradual stagnation. Even when new theories contain substantial contributions from the pens of past writers, it may be more in the interests of professional development

if their original aspects are emphasized and debated while their more conventional elements are left to the evaluation of a more settled future. Karl Popper has observed that a science advances most dramatically through the introduction of and controversy over bold and elemental speculations.¹¹⁵ If he is correct, then Marshall's influence has paradoxically served as a barrier to more rapid progress in economic analysis by imbuing the profession with a too great regard for conventional approaches to the explanation and prediction of human behavior and too fundamental a faith in the virtues of building on, rather than superceding, previous economic models.

Although Marshall is well-deserving of his reputation as a brilliant synthesizer of advances in theoretical economics and as an original theorist in his own right, his justified fame in one study should not be considered as sufficient proof of his skills in another. History has shown that great physicists and great actors are not necessarily great political theorists, and neither are great economists necessarily great philosophers, even in matters concerning their own subject.

Footnotes to Chapter VIII

1. This passage should not be interpreted as asserting that the Marshallian system was without empirical consequences. Rather, I have meant to imply that the ability to predict on the basis of the model depended upon certain functional shapes and relationships which could only be determined by assumption. Marshall never attempted to define the empirical procedures for measuring the elasticity of a demand curve or the amount of consumer's surplus, to say nothing of the slope of a firm's long-run supply curve; and, for the purposes to which he put these concepts, it would often be quite difficult to define such procedures. The consequences of an acceptance of the Marshallian system were, thus, conflicts between those who made different a prioristic assumptions about purportedly empirical matters (i.e., conflicts between those economists with different "intuitions" or different "casual observations" about the same classes of economic phenomena).

2. L. L. Price, "Notes on a Recent Economic Treatise," Economic Journal, Vol. 2 (March, 1892), pp. 20-21.

3. A. C. Pigou (ed.), Memorials of Alfred Marshall (New York: Kelley and Millman, Inc., 1956), pp. 3-7. (Hereafter cited as Memorials.) See also C. W. Guillebaud (ed.), Alfred Marshall's Principles of Economics, Vol. II (London: Macmillan, 1961), p. 4. (Hereafter cited as Variorum Principles II.) P. T. Homan has described Marshall at the early stages of his career as "A brilliant mathematician, a young philosopher carrying a somewhat undigested load of German metaphysics, Utilitarianism and Darwinism; a humanitarian ... eager to lighten the burdens of mankind, but sobered by the barriers revealed to him by the Ricardian Political Economy." (Variorum Principles II, p. 5.) Pigou noted that "for some years after taking his degree his interest was centered in philosophy." (Memorials, p. 82.) And Marshall himself wrote that "If I had to live my life over again I should have devoted it to psychology. Economics has too little to do with the ideal." (Memorials, p. 37.)

4. The well-known passage detailing how the concern of poverty brought Marshall to the study of economics is quoted in Robert B. Ekelund and Robert F. Hebert, A History of Economic Theory and Method (New York: McGraw-Hill, 1975), p. 338.

In 1893 Marshall stated in testimony before the Royal Commission on the Aged Poor that: "I have devoted myself for the last twenty-five years to the problem of poverty; and very little of my work has been devoted to any inquiry which does not bear upon that." (Memorials, p. 70.)

5. Memorials, op. cit., p. 11fn.

6. Talcott Parsons, "Wants and Activities in Marshall,"

Quarterly Journal of Economics, Vol. 46 (1931), p. 102.

7. Ibid., pp. 102, 111.
8. Ibid., p. 107.
9. Ibid., pp. 111-113.
10. Ibid., p. 115.
11. Jacob Viner, "Marshall's Economics, in Relation to the Man and his Time," American Economic Review, Vol. 31 (January, 1941), pp. 230-231.
12. For evaluations of the influence of Jowett and of the Victorian English Hegelian, T. H. Green, upon Marshall's economic system, see Anastasios Petridis, "Alfred Marshall's Attitudes to and Economic Analysis of Trade Unions," History of Political Economy, Vol. 5 (Spring, 1973), pp. 183-184 and John Whittaker's "Alfred Marshall: The Years 1877-1885," History of Political Economy, Vol. 4 (1972), pp. 12-18. Jowett once wrote to Mary Paley Marshall, regarding the recently published Principles, that: "I am glad to see that there is a considerable element of Hegelianism in the book." (Whittaker, op. cit., p. 18.) Marshall's memorial appreciation of Jowett appears in Memorials, op. cit., pp. 292-294.
13. Joseph Schumpeter, History of Economic Analysis (New York: Oxford University Press, 1954), p. 780fn.
14. Parsons, op. cit., p. 140.
15. Schumpeter refers to this quality of Marshall's works as "his bent toward misplaced realism." (Schumpeter, op. cit., p. 1046.)
16. Alfred Marshall, Industry and Trade, 4th edition (New York: Augustus Kelley, 1970). The first edition of this volume was published in 1919.
17. Marshall stated of the motto "The Many in the One and the One in the Many" that "This motto supplements the motto of my Principles which is: "Natura non facit saltum": i.e., economic evolution is gradual and continuous on each of its numberless routes." (Industry and Trade, op. cit., pp. v-vi.)
18. For an excellent discussion of modern "critical empiricism" see Karl R. Popper's Conjectures and Refutations (New York: Harper and Row, Inc., 1963), pp. 33-59 and Karl R. Popper's "Normal Science and its Dangers" in Imre Lakatos and Alan Musgrave (eds.), Criticism and the Growth of Knowledge (London: Cambridge University Press, 1972), pp. 51-58.

19. See Memorials, op. cit., pp. 99-100 for Marshall's own description of his reaction to criticisms of the Classics.

20. Memorials, op. cit., p. 47, 47fn. See also Alfred Marshall, Principles of Economics, 8th edition (New York: Macmillan, 1948), p. V. (The page numbers to this edition of the Principles and to the edition reprinted as Volume 1 of the Variorum edition are identical.) Marshall's harshest appraisal of the Classics, or more precisely, of the Ricardians, is to be found in Memorials, op. cit., pp. 154-155.

21. Pigou correctly noted that Marshall was "...very thin skinned" about a variety of subjects. See A. C. Pigou, Alfred Marshall and Current Thought (London: Macmillan, 1955), p. 26. Marshall's sensitivity to criticism is also mentioned by Edgeworth (Memorials, op. cit., p. 68) and John Maynard Keynes (Memorials, op. cit., p. 37). Although replying politely to those few of his critics that he considered worthy of notice, Marshall was more influenced by the subtle persuasion of his colleagues at Cambridge and by the writings of the German Historical School than he was by any of the critics of his writings.

22. Memorials, op. cit., p. 217.

23. An exception to his rule of remaining silent in the face of personal attacks is Marshall's reply to Cunningham's criticisms of his scholarship and his economic history. This reply first appeared in the Economic Journal for September, 1892, and was subsequently reprinted in Variorum Principles II, pp. 735-750.

24. See, for instance, Memorials, op. cit., pp. 397, 400-401, 432, 469, and George J. Stigler's "Alfred Marshall's Lectures on Progress and Poverty," Journal of Law and Economics, Vol. 12 (April, 1969), pp. 184fn-185fn for Marshall's expressions of hesitancy when asked to publish his views concerning controversial issues closely connected to economic theory.

25. See Memorials, op. cit., pp. 500-508, items numbered 22, 51, 66, 68, 72, 74, 75, and 77 for Marshall's articles and "public communications" concerned with matters of public policy. It is to be noted that Foxwell felt that Marshall had supported Pigou in preference to himself as the successor to the Chair of Political Economy at Cambridge primarily because he (Marshall) had been angered by a letter Foxwell had sent to the Times, a letter which exposed disunity within the economics profession over the issue of free trade and which openly "took sides" on a controversial policy matter within the arena of public debate. The letter expressing Foxwell's bitterness over this incident and stating his suspicions is printed in A. W. Coats, "The Appointment of Pigou as Marshall's Successor: A Comment," Journal of Law and Economics, Vol. 15 (October, 1972),

p. 490. Coats refers to Marshall's "customary rule of abstaining from public declarations on controversial policy issues" (Ibid., p. 487), and he states, regarding the tariff controversy, that "... there is no doubt that he (Marshall) deplored the fact that the economists had exposed their disagreements to the public" (Ibid., p. 488). Despite his policy of caution in dealing with controversial issues, Marshall did become somewhat more rambunctious in his old age. In his declining years he became less and less hesitant to take up the pen in defense of what he interpreted as "the public good." See Memorials, op. cit., pp. 500-508, items numbered 37, 73 and 78 as examples.

26. For comments regarding the strain of Victorian moralizing which ran throughout Marshall's works, see G. F. Shove, "The Place of Marshall's Principles in the Development of Economic Theory," Economic Journal, Vol. 52 (1942), p. 310 and C. W. Guillebaud, "Some Personal Reminiscences of Alfred Marshall," History of Political Economy, Vol. 3 (1971), p. 4.

For a particularly ghastly example of moralism run rampant in Marshall's writings, see his "Social Possibilities of Economic Chivalry," reprinted in Memorials, op. cit., pp. 323-346. Marshall was so absorbed by "the ethical ideal" that he even allowed it prominence over his central theme of social evolution. Perhaps in response to late Victorian and Marxian philosophies, he maintained that the simple fact that a certain organism or institution was the evolutionary result of a certain environment was not sufficient to justify its existence as "right" (see Industry and Trade, op. cit., pp. 175-176).

27. See Coats, op. cit., p. 488 and Stigler, op. cit., p. 185fn.

28. Memorials, op. cit., p. 88.

29. Shove, op. cit., pp. 3-4. John Neville Keynes was even less inhibited in expressing his opinion of Marshall when he believed that he was writing only for his own personal reference. His diary entry for December 11, 1899, reads: "Marshall is the most exasperating talker I know. He will agree with nothing you say and argues and dogmatizes so as to drive one wild." (Quoted in R. H. Coase, "The Appointment of Pigou as Marshall's Successor," Journal of Law and Economics, Vol. 15 (October, 1972), p. 474.) John Maynard Keynes, in his biographical sketch of Marshall's life and writings, remarked that "... a missionary he remained all his life." (Memorials, op. cit., p. 27) That would certainly seem to be confirmed by his attitudes toward economic theory and social reform.

30. Guillebaud, "Some Personal Reminiscences ...," op. cit., pp. 3-4.

31. Ibid.

32. See Marshall's essay on the "Social Possibilities of Economic Chivalry," op. cit. Also of interest are Shove, op. cit., pp. 310-311 and Parsons, op. cit., p. 107.

33. It is interesting to note that Marshall still clung to the doctrine of acquired characteristics (Industry and Trade, op. cit., pp. 163-164). He maintained that although new evidence had led to its disrepute in biology, it at least described the process of learning and the accumulation of knowledge and custom within a culture. Marshall's views on evolution and process analysis are the subject of a comprehensive survey by Bruce Glassburner entitled "Alfred Marshall on Economic History and Historical Development," Quarterly Journal of Economics, Vol. 69 (1955), pp. 577-595.

34. Memorials, op. cit., pp. 500-508, items numbered 11, 17, 21, 27, 49, 56, 57, 71, 74, 75 and 79.

35. Ibid., p. 41.

36. See Karl R. Popper's The Open Society and its Enemies, Vol. 2 (New York: Harper and Row, 1967), pp. 220-223.

37. See Marshall's "Graphical Method of Statistics," first presented as an address to the International Statistics Congress and later reprinted in the Journal of the Royal Statistical Society and in Memorials, op. cit., pp. 175-187. See especially Memorials, op. cit., p. 177 for Marshall's proposal for the collection and compilation of world-wide economic data.

38. Memorials, op. cit., pp. 428-429. Of this quote Pigou said, "This I think we may fairly take as Marshall's considered view." (Alfred Marshall and Current Thought, op. cit., p. 17.)

39. Ibid.

40. Although Marshall had been enthusiastic about the compilation of international economic data in 1888, by 1905 he was warning Irving Fisher that such a plan might be premature because of insufficient standardization in the empirical interpretation of many terms and because of the differences in collection methods between the various countries of Europe and North America (Memorials, op. cit., pp. 474-475). Marshall further expressed an opinion that such statistical studies would "perplex the ordinary man, even if ... really complete," and that the "main purpose of the studies," i.e., policy actions by the State, "ought not to wait for further calculations by methods as crude as the best which are within our reach today." (Ibid., p. 475.)

41. Minor exceptions to Marshall's general avoidance of statistical data are to be found in Industry and Trade, op. cit., pp. 154fn, 757 and 848.

42. Memorials, op. cit., pp. 358-359.
43. Principles of Economics, op. cit., p. 778.
44. Memorials, op. cit., p. 26.
45. Ibid., p. 177.
46. Principles of Economics, op. cit., p. 779.
47. Ibid.

48. The elitist aspects of Marshall's thought, his belief that certain individuals were gifted with minds specially suiting them to economic reasoning, are exemplified in Memorials, op. cit., p. 47fn and in Principles of Economics, op. cit., p. 779.

49. Adolph Wagner, in his "Marshall's Principles of Economics," Quarterly Journal of Economics, Vol. 5 (1890-1891), singled out both Marshall and John Neville Keynes as shining examples of the spirit of reconciliation which was then beginning to make itself felt in the economics profession (see p. 321 of that article). Paradoxically, Wagner also expended much ink and effort condemning Schmoller and other "more extreme" members of the German Historical School (pp. 317-320 of the same article). See also Shove, op. cit., p. 5, Price, op. cit., pp. 18-19, 22-23 and Variorum Principles II, op. cit., p. 765 for other appraisals of Marshall as the great conciliator of Nineteenth Century economics.

50. Shove, op. cit., p. 305.

51. For Marshall's generally favorable evaluation of German Historicism see his Principles of Economics, op. cit., pp. 768, 783-784 and Variorum Principles II, op. cit., pp. 764-766. Further corroboration of Marshall's attitudes toward German Historicism are to be found in Wagner, op. cit., p. 321, Price, op. cit., p. 20 and Memorials, op. cit., p. 20.

52. Memorials, op. cit., p. 165. Marshall did engage in rather veiled criticism of what he believed to be the German Historical School's unwarranted neglect of economic theory. See his Principles of Economics, op. cit., pp. 774-775.

53. Principles of Economics, op. cit., p. 768.

54. Ibid., p. 29. Numerous citations can also be found in Industry and Trade, op. cit. See the index entry on "Schmoller" appearing on pp. 869-870.

55. Variorum Principles II, op. cit., pp. 764-765fn.

56. T. W. Hutchison, A Review of Economic Doctrines, 1870-1929 (London: Oxford University Press, 1953), p. 22.
57. Shove, op. cit., p. 305.
58. T. E. C. Leslie, Essays in Political Economy (New York: Augustus Kelley, 1969), pp. 73-74.
59. Memorials, op. cit., p. 296 and Hutchison, op. cit., p. 66.
60. Marshall edited a special low-cost edition of Bagehot's Postulates of English Political Economy for his Cambridge students. He found Bagehot to be "a master of literary form" and "a leader of affairs" and frequently expressed the hope of one day being able to imitate his style. Marshall, however, entirely misinterpreted Bagehot's remarks on the problems of economic semantics (Hutchison, op. cit., p. 67), and it is unclear whether he sufficiently comprehended any of his other meta-economic discussions.
61. Ingram praised Marshall's Principles as "a new and important contribution to the science" (J. K. Ingram, A History of Political Economy (London: Adam and Charles Black, Publishers, 1923), p. 275), and he expressed the belief that Marshall was "fair-minded and catholic in his sympathies and habits of thoughts and exhibits great skill in distinguishing between the essentials and non-essentials of principles and theory and tracing their application throughout the entire realm of economic phenomena" (Ibid., p. 274). Yet Ingram was also aware of certain defects in the work which Marshall's Cambridge colleagues had chosen to ignore: "In attempting to restate old doctrines in such a manner as to make them fit the conditions of modern life, and harmonize with newer theories, he (Marshall) has deprived them of a part of their vitality and considerably diminished their usefulness. In some instances he had avoided issues ... by simply omitting to discuss them." (Ibid.) It was perhaps not surprising that Marshall lobbied in favor of the deletion of the compulsory history of thought requirement from the curriculum required of economics students at Cambridge (Coase, op. cit., p. 478fn).
62. John Neville Keynes, The Scope and Method of Political Economy, 4th edition (London: Macmillan, 1930), pp. 318fn-319fn.
63. Principles of Economics, op. cit., pp. 754-769.
64. Memorials, op. cit., p. 152.
65. Variorum Principles II, op. cit., p. 769.
66. Principles of Economics, op. cit., Chapter Five, "Land Tenure," pp. 637-655.

67. Pigou, Alfred Marshall and Current Thought, op. cit., p. 6.
68. Memorials, op. cit., pp. 427-428.
69. Refer to the references in footnote 2 above.
70. Variorum Principles II, op. cit., p. 15.
71. Hutchison, op. cit., p. 64. Hutchison also mentions that Marshall's attempt to translate Smith and Ricardo into mathematics had been earlier undertaken by William Whewell, op. cit., p. 340.
72. Quoted in Ekelund and Hebert, op. cit., p. 340.
73. Memorials, op. cit., pp. 422-423. One of Marshall's most devoted students reported that: "What he aimed at in all this was to get ... the direct feel of the economic world, something more intimate than can be obtained from reading mere descriptions" (Memorials, op. cit., p. 95).
74. Ekelund and Hebert, op. cit., p. 340.
75. Marshall wrote in his essay on "Distribution and Exchange" that "the most helpful applications of mathematics to economics are those which employ few symbols and which aim at throwing a bright light on some small part of the great economic movement rather than as representing its endless complexities" (Memorials, op. cit., p. 313). He had earlier commented that "We owe several valuable suggestions to the many investigations in which skilled mathematicians, English and Continental, have applied their favorite method to the treatment of economical problems. But all that has been important in their reasonings and results has, with scarcely an exception, been capable of being described in ordinary language ... as clearly as that of the mathematic." (Pigou, Alfred Marshall and Current Thought, op. cit., p. 8.) Pigou's own assessment of Marshall's attitudes toward the application of mathematics to economic problem solving is itself revealing, the observation of a close and intimate associate of Marshall's of many years standing: "The key to this attitude of Marshall's was, I think, his feeling that elaborate mathematical analysis applied to economics was bound to be unrealistic; that the influences at work in real economic situations are so numerous and so intimately inter-related that, if they were all brought in, the mathematics would be unworkable, whereas, if, as is inevitable in practice, many of them are left out, we are not studying actualities ... Moreover, in his view a mathematical treatment of economic problems tends to focus attention on mechanical analogies and to keep it away from more important biological analogies ..." (Ibid., pp. 9-10). H. M. Robertson in his "Alfred Marshall's Aims and Methods Illustrated from his Treatment of Distribution," History of Political Economy, Vol. 2 (1970), was even more thorough than Pigou in his consideration

of the roots of Marshall's aversion to long chains of mathematical reasoning. According to Robertson's evaluation, Marshall was hesitant to employ mathematics more extensively than he had because he believed that the available statistical techniques were inadequate to determine the true shapes of certain key mathematical functions, because mathematics could not be used to describe "the motion" of economic variables, because mathematical presentations could easily lead to the presumption that economists actually possessed more knowledge than they actually did, and because "in economics every event causes permanent alterations in the conditions under which future events can occur" (Robertson, op. cit., pp. 9-10).

Marshall's attitudes toward the extensive use of mathematics were further illustrated in his correspondence with Bowley when he wrote that: "I think you should do all you can to prevent people from using Mathematics in cases where the English Language is as short as the Mathematical" (Variorum Principles II, op. cit., p. 775). He displayed the same prejudices in his review of Edgeworth's Mathematical Psychics in 1881 when he commented that: "... it will be interesting ... to see how far he succeeds in preventing his mathematics from running away with him; and carrying him out of sight of the actual facts of economics" (Memorials, op. cit., p. 26). And eleven years earlier in his review of Jevon's Theory of Political Economy, he had written that: "... the book before us would be improved if the mathematics were omitted, but the diagrams retained" (Memorials, op. cit., p. 25). See also p. 419 of Memorials.

Keynes speculated that Marshall's fear of alienating businessmen had caused him to suppress the more quantitative aspects of his work (Memorials, op. cit., p. 26), and Viner agreed that "Marshall ... was anxious for a wide audience, and the fact that the bulk of his potential readers were both unable and unwilling to read economics in mathematical form no doubt was an additional consideration" (Jacob Viner, "Marshall's Economics in Relation to the Man and to his Time," American Economic Review, Vol. 31 (June, 1941), p. 231). Viner, however, believed that the primary factor prejudicing Marshall against the extensive use of mathematics in his economic investigations was the fact that he had always found the study of mathematics pleasurable, and, as a good Victorian who had been warned by his father against the "frivolity" of the study, he naturally equated the pleasurable with the sinful (Ibid.).

76. The secondary sources dealing most directly with Marshall's distaste for "long chains of abstract reasoning" are Parsons, op. cit., p. 105 and Edgeworth. Edgeworth wrote in his memorial appreciation of Marshall that: "These characteristics--supreme skill and extreme caution in the application of abstract reasoning--may be traced in most of Marshall's writings" (Memorials, op. cit., p. 67).

Much more extensive are Marshall's own references to the subject. In his Principles of Economics he wrote that: "It is obvious that there is no room in economics for long trains of deductive reasoning" (Principles, op. cit., p. 781). See also

Variorum Principles II, op. cit., pp. 770-771, for an earlier and somewhat more lengthy rendering of the same thought. Marshall also added that "... even in mechanics long chains of deductive reasoning are applicable only to the occurrences of the laboratory" (Principles of Economics, op. cit., p. 771).

The parallel between long chains of deductive reasoning and complex mathematical models was explored at some length in Marshall's Mathematical Appendix to the Principles, especially in pp. 850-852; and his objections to the one form of reasoning were closely tied to his rejection of the other (Ibid., pp. 850-851).

77. Marshall was well known among friends for his nearly neurotic antipathy to Jews (Memorials, op. cit., p. 76). While he was more cautious about exposing his prejudices in print than he was in private confidences, he did go so far as to insinuate that the strain of irresponsible socialism in German thought, of which he disapproved, was primarily the doing of Jewish intellectuals (Principles of Economics, op. cit., p. 769). Also see Memorials, op. cit., p. 153 for his remarks on Ricardo's "semitic mind" and his appraisal of Ricardo's impact on Classical economics.

78. While criticizing the Classicals for their extreme deductive techniques in some of his published essays, Marshall was more subdued in his major writings. In his Principles, for instance, he considered their views as merely a first, rather simplistic, approximation to the ultimate corpus of economic theory. Although the Classicals had underestimated the complexity of the problems with which they dealt, they had provided, in Marshall's evaluation, the tools which, properly modified by historical research and the evolutionary doctrines of biology, could be used to construct the ultimate economic edifice. (Principles of Economics, op. cit., p. 777.)

79. Curiously, Marshall contrasted his own position to that of Foxwell by stating that: "It seems to me that our differences in temper cause you to lay greater emphasis upon accuracy as regards facts, and me to insist on their (the students at Cambridge) wrestling with difficult analysis and reasoning" (Coase, op. cit., p. 447). This seeming contradiction between Marshall's "insistence on their wrestling with difficult analysis and reasoning" may be resolved by noting that Marshall meant by "reasoning" what he called "many short chains and single connecting links" (Principles, op. cit., p. 773), and he meant by "difficult analysis" primarily the analysis of historical evolution. Foxwell, on the other hand, would have interpreted "facts" as actual statistical data or well-corroborated historical accounts, rather than casual empiricism or a "feeling for" the subject.

80. C. W. Guillebaud, "Marshall's Principles of Economics in the Light of Contemporary Economic Thought," Economica, N.S., Vol. 19 (1952), p. 113.

81. Taussig, op. cit., p. 8. Marshall's lecture style apparently bore many similarities to his style of writing in the sense that it was equally as obscure. His students reported, however, that it did not possess the virtue of being well organized (Memorials, op. cit., p. 78).

82. Taussig, op. cit., pp. 7-8.

83. Ibid., p. 8.

84. The reference is, of course, to the use of the term "tendencies," a favorite ploy of Classical economists. Marshall, however, used this term in a manner which was even less definite and more viciously anti-empirical (because of its evolutionary undertones) than the usage of either Cairnes or Mill. See, for instance, Memorials, op. cit., p. 360.

85. Marshall argued vigorously against Mill's conception of economics as the study of purely economic motives and against the extreme abstraction of Mill's model of an economic man. For Marshall's views in opposition to this portion of the Classical methodology, see "The Present Position of Economics," reprinted in Memorials, op. cit., especially pp. 160-161, 281-282. For a further discussion of the same topic, see Principles of Economics, op. cit., p. 17fn.

86. Memorials, op. cit., p. 13. Each of these philosophers is, of course, known as an advocate of an a prioristic and evolutionary outlook upon questions of epistemology and social development, and each is noted for his hostility to "merely empirical" knowledge.

87. We are also told that Marshall, in his lectures of this period (the early and mid-'Seventies) "... would give half an hour to theory and half an hour to history" (Hutchison, op. cit., p. 66).

88. Principles of Economics, op. cit., pp. xiv-xv, 46-47 and 772.

89. The relevant section of the essay on "Distribution and Exchange" is reprinted in Memorials of Alfred Marshall, op. cit., under the title of "Mechanical and Biological Analogies in Economics" (see especially pp. 312-318). In the course of this selection Marshall explains, in part, what he meant by the terms "dynamic," "static," "Caeteris paribus," and "evolution." In most cases there is a significant variance between his meaning and the more modern usages of these terms in mid-Twentieth Century economics.

90. Memorials, op. cit., pp. 314, 318.

91. Principles of Economics, op. cit., p. ix.

92. For a similar interpretation of the role of Industry and Trade in Marshall's overall schema for the development of an evolutionary economics, see Glassburner, op. cit., p. 578.
93. Schumpeter, History of Economic Analysis, op. cit., p. 780fn, and Parsons, op. cit., p. 578.
94. Industry and Trade, op. cit., p. v and Glassburner, op. cit., p. 582.
95. Schumpeter, "Alfred Marshall's Principles," op. cit., pp. 242-243.
96. Memorials, op. cit., p. 317.
97. Principles of Economics, op. cit., p. 764.
98. Schumpeter, History of Economic Analysis, op. cit., p. 990, and C. E. Ferguson, Micro-Economic Theory, 3rd edition (Homewood: Richard D. Irwin, 1972), p. 13.
99. Schumpeter, History of Economic Analysis, op. cit., pp. 990-991.
100. Principles of Economics, op. cit., p. 770; Industry and Trade, op. cit., pp. 676-670; Memorials, op. cit., pp. 163-164; and Variorum Principles II, op. cit., p. 762. This view of the steps involved in the construction of a comprehensive social theory is, of course, identical to the procedures defended by J. E. Cairnes in his attack upon the Comtists.
101. Variorum Principles II, op. cit., pp. 48-50; Memorials, op. cit., pp. 314-315; Principles of Economics, op. cit., pp. xv, 336.
102. Principles of Economics, op. cit., pp. 37, 369-370 and 379fn-380fn.
103. Ibid., pp. 774-775.
104. The aggregative or collective character of Marshall's economics is made especially clear in Principles of Economics (Ibid., pp. 25-26) where Marshall discusses the types of entities with which economics is properly concerned.
105. Memorials, op. cit., pp. 306-309, 329-334, 339-342.
106. Principles of Economics, op. cit., p. 772.
107. Parsons, op. cit., p. 101; Taussig, op. cit., p. 1.

108. Taussig, op. cit., p. 1.
109. Among the famous foreign economists to sign the Royal Economic Society's greetings to Marshall were Gustav Cassel, Charles Gide, E. R. A. Seligmann, Joseph Schumpeter, R. H. Tawney and F. W. Taussig (Memorials, op. cit., pp. 497-499).
110. David Macmillan, "The Centenary of the Birth of Alfred Marshall," Economic Journal, Vol. 52 (1942), p. 289.
111. Quoted in Hutchison, op. cit., p. 74.
112. Ekelund and Hebert, op. cit., p. 339.
113. Ibid. Among the "lesser lights" of Marshall's students were B. L. Hutchins, Barbara Wooten and D. H. Robertson.
114. The Robinson quote appears in Hutchison, op. cit., and the Taussig quote appears in Taussig, op. cit., p. 5.
115. Karl R. Popper, "Back to the Pre-Socratics," reprinted in Conjectures and Refutations (New York: Harper and Row, 1965). See especially pp. 148-157 for Popper's discussion of critical rationalism and the role of bold conjectures in advancing science.

CHAPTER IX

THE METHODOLOGICAL "SCHOOLS"--RETROSPECT AND PROSPECT

Introductory Comments

In previous chapters we concentrated our attention upon the distinctly individual characteristics of writers within both the Historical and Orthodox traditions. In this section, however, our purposes are to (1) identify the unifying features within each of these opposing perspectives and (2) to trace the continuation of the traditions of Orthodox and Historical economic methodology to the present day.

There is no presumption in what follows that considerations connected with the history or historical-sociology of a discipline are the principal determinants of the discipline's intellectual development. All we have hoped to demonstrate is that unconscious or unexamined professional beliefs about what is "obvious" are too often the source of fundamental and persistent errors. In a rapidly expanding field such as Twentieth Century Economics, it is all too easy to take for granted the seemingly inconsequential and innocuous methodological rhetoric handed down from the Nineteenth Century. What once was regarded as "word-games" or "useless philosophic quibbling," however, is once again assuming importance in the modern clashes between traditional and "alternative" economics. Those economists who hope to preserve some portion of a positive economic science from the rising tides of ideological reaction would be well advised to take a fresh look at their own methodological foundations

rather than blindly relying on shop-worn phrases concerned with methodological topics.

"Orthodoxy" and "Historicism": A Summary

Nineteenth Century Orthodoxy

In the foregoing sections we have considered three major formulations of the "Orthodox" approach to matters of economic methodology: that of J. S. Mill, that of J. E. Cairnes and, most importantly, the neo-Orthodoxy of Alfred Marshall. Although it would be possible to locate earlier expressions of similar views (e.g., in the works of Senior or Whately), these three authors were the direct inspiration for nearly all subsequent Orthodox writings.

From their works it is possible to extract both the common core and particularized variants of the Orthodox view as a basis for its comparison with the empirical or Historical tradition. The main doctrines forming this core of economic Orthodoxy in the Nineteenth Century were:

(1) A trust in the "right intuitions," "professional insights," or "casual experiences" of "expert" economists as a means for both arriving at and "testing" economic theories.

(2) A strong distinction between the "validity" of economic theory, resulting from its a priori derivation, and the "hypothetical" or "incomplete" character of all attempts to "apply" the theory for reasons of prediction.

(3) A belief in the power of economics as a tool for social "explanation" or as an aid to the "understanding" of social events

(viz., the interpretation of economics as a "perspective upon society" or a way of "seeing" social relations as opposed to its interpretation and use as a tool for the prediction of social events).

(4) A belief in the "absolutism" or unconditional universality of economic theories (similar to Friedman's belief that the "realism" of a theory's "assumptions" is irrelevant to its "usefulness"). The support of this "absolutism" was usually by means of "unspecified caeteris paribus conditions" and vague discussions regarding the possibility of "correcting" economic predictions for "special circumstances."

(5) A rejection of all experimentation as "impossible" in the social sciences.

(6) A belief that empirical events ("observations") were "relevant" to economic theory solely as a source for the (psychological) "suggestion" of "interesting" modifications in the theories, connected with a concomitant rejection of all tests of economic theories as either "irrelevant" or "inconclusive."

(7) A tendency to stress the specially "complex character" of social as against physical phenomena.

(8) The use of "mental experiments" or "thought experiments" to "prove" economic hypotheses.

(9) The deduction of empirical relationships from uninterpreted ("tautological") theories through the use of implicit, unstated and often deliberately obscured auxiliary hypotheses (e.g., the use of "the maximizing principle" in ways which could only be justified via

a specification of the function being maximized).

The Doctrines of Mill and Cairnes

Complementary to the above common characteristics of Orthodoxy were specialized aspects of the writings of Mill, Cairnes and Marshall; many of which have reappeared in more modern writings. Mill was the most explicit of the Orthodox economists, always cautious to spell out the assumptions of his analysis. Economics was for him nothing more nor less than the conceptual consideration of the likely actions of an "economic man" (i.e., a man moved solely by "economic motives"). Ordinary social actions were, however, only partially determined by economic influences, and, thus, a predictive social science would require the development of other, complementary, areas of social study (e.g., sociology, government and social ethics) for its success. Mill was further interested in the prediction of future social states (both in the Ricardian and the Utopian-historicist sense), and for these purposes he hoped to one day combine with economics an evolutionary science of Ethology, or "a study of the development of the human character."

Mill's successor, J. E. Cairnes, added a modification to the doctrines of Orthodox methodology which was to serve as a perpetual source of both puzzlement and "insight": his characterization of economics as the study of "valued matter." Whereas Mill had viewed economics as a branch of applied psychology, Cairnes believed that it depended jointly upon the principles of psychology and of physical mechanics. Mill had hoped to firmly establish economics as a branch

of a universal evolutionary psychology (his Ethology). Cairnes believed that it was properly an autonomous study, drawing upon related fields but improving upon their findings in a non-reducible way. In addition to this rather important modification in Mill's Orthodox view, Cairnes intermingled in his own writings elements of the extreme axiomatic a priori of Senior and a mistrust of mathematical methods. Of equal importance with his methodological innovations was his systematic and semi-popular presentation of Orthodox doctrines in his widely read Character and Logical Method of Political Economy. This volume both established his stature as an "expert" on methodological questions and served to inculcate the perspective of the Orthodox School into a new generation of economists.

Marshall's New Methodology

Mill and Cairnes lived in an age which was only beginning to display its disenchantment with the dogmas of Orthodox economics. Marshall, however, was faced with a crisis in the Orthodox tradition, and he responded accordingly. By combining elements of German Historicism with the Mill-Cairnes tradition in economic methodology, he arrived at a position which was nearer to Popper's description of the scientific ideology of "historicism" than it was to the rationalistic or Kantian conceptions of previous Orthodox theorists. Marshall abandoned Mill's characterization of economics as the study of an "economic man," but he substituted for this rather definite conception one much more broad and vague: "... the study of mankind in the ordinary business of life." He extended, yet softened, Cairnes' rejection of

mathematical methods, transforming it into a general suspicion of all "long chains of deductive reasoning," whether mathematical or literary.

By attempting to integrate Hegelian philosophy with economic theorizing, Marshall inspired a generation of economic writings noted for their obscurity. While responsible for the introduction of many of the terminological distinctions still vital in modern micro-analysis (e.g., the distinction between the various "market periods" and the conceptions of increasing, constant and decreasing cost industries), Marshall was also a master of psychologistic reasoning and the impressive, but vacuous, phrase.

Although encouraging research into economic history, he did everything possible to confine such research to the collection of singular "economic facts" and to speculations concerning the broad trends of economic evolution. Nominally an empiricist, interested in the relevance of existing constraints to the construction and meaning of theory, Marshall was himself convinced that "facts" were merely illustrative of certain fundamental truths. He placed more confidence in "a shrewd mother-wit," the intuitions of experts, and vacation visits to factories and foreign lands than he did in statistical studies or carefully documented historical accounts.

We will see in the following survey of contemporary methodological writings that many of the convictions of the earlier "Orthodox" writers have been reproduced, with only slightly different justifications, in recent years. Orthodoxy is far from extinct. At best, it has learned to adopt a more indirect statement of its convictions in

right intuition and absolute truths.

The Early British Historical School

The Victorian Age in England was not, however, lacking in either methodological skeptics or in true empiricists. Not everyone possessed the dogmatic certainty of Cairnes' convictions or agreed with Mill's reduction of economic science to the contemplation of Homo economica. Beginning in the writings of Richard Jones and William Whewell there occurred a multi-fold reaction against the Scholastic speculations of Orthodox economics. While there was little agreement as to the character of alternative methodologies, there was near unanimity concerning the features of the Orthodox perspective requiring repair or replacement, i.e., those same features we have listed above. The fact that the Historical view was itself not so well-structured nor so nearly monolithic as the Orthodox School is not, however, to be regretted. Each author added his own contribution to the tradition, thus strengthening the case against Orthodoxy and increasing the number of possible alternatives.

Whewell was the philosopher of the School, anxious to overturn the naive-intuitionist view of economic research and the Orthodox extension of their theories to cases where their applicability remained untested. Whewell had, perhaps, a better appreciation for the distinction between higher and lower level hypotheses than did the other economists of his day, and he also had a more detailed appreciation for the processes involved in empirically testing a theory.

Jones was the first of the Historical writers to combine

empirical research with a demand for the reform of Orthodox (Ricardian) theory. His research methods were crude, being based upon an exhaustive examination of available accounts of foreign and ancient economies; but these methods were perhaps the best available at the time and certainly strengthened his appreciation of the effects of different institutions and different social mores. Although primarily interested in pursuing the pleasures of "research" in his quest for "all the facts," Jones was responsible for the development of an early (rather crude) classification system for differentiating wages, profits and rents upon the basis of different types of social structures.

T. E. C. Leslie, who did his main work over two decades after the deaths of Jones and Whewell, is to be credited for a synthesis of many of the desirable elements in previous Historical writings, as well as for the introduction of many new Historical doctrines. Among his contributions we may mention his endorsement of direct empirical procedures (including first-hand investigation of the geographic region or empirical phenomena being analyzed, conjoined with extensive correspondence with those in positions of long-term familiarity with the phenomena), his use of arguments based upon a comparison between varying systems of claims and liabilities (viz., different "property structures"), his attempts to operationalize Classical terminology and to point out the tautological character of much of economic theory as it then existed, and his formulation and defense of clear-cut alternative explanations for many contemporary doctrines (e.g., his discussion of the connection between regional price fluctuations and the

introduction of improved forms of transportation). Given the constraints of his age, Leslie was probably the best all-around representative of the early Historical tradition in Britain.

Walter Bagehot, Leslie's contemporary, made few original contributions to the Historical view, but his presentation and organization of important features of perspective had a profound impact on the popularity of the School. In his "Postulates of English Political Economy," he isolated the major assumptions of Ricardian analysis (the free mobility of capital, the maximization of profits, etc.) and demonstrated their inapplicability to previous ages and to large segments of the globe as it then existed. Bagehot's own views were not those of spatial or temporal "relativism," however, despite charges of his critics. He wished only to limit the analysis of a competitive and capitalistic economy to those cases where it was really "fitted to the facts."

What Was British Historicism (in General)?

The British Historical School has been a matter of interest to historians of economic thought for over forty years. From Grossman's distorted portrayal of the movement in the 1940's to the essays on Jones and Leslie which have appeared only recently, historians have puzzled over the writings of these authors and have arrived at sharply varying interpretations. Few are still so naive as to group the School, at least in its earlier stages, with the intellectual movements which later dominated German economics, yet despite a continued interest in a re-examination of the Historicist's views, little has

been definitely settled. One account of the School portrays its members as radical inductivists of a naive Baconian sort, while another places them within the context of the Irish Reform Movement and attempts to reduce their differences with Orthodox theory to disagreements concerning policy.

Although in the opening chapter of this dissertation we were compelled to remain content with a conventionalist definition of the School--applying the term freely to those who were commonly said to fall within that tradition--we are now in a better position to assess the character of these thinkers taken as a whole. Given the restrictions on the temporal scope of this study, from the 1830's to the mid-1880's, British Historicism can be identified almost entirely with philosophic arguments or research practices grounded in the reinterpretation of economics as an empirical study. That is, the British Historical economists were the first to break free from the Scholastics' reduction of social science to speculative humanism (or the contemplation of "man's nature").

"Social Empiricism"

To assert that the British Historicists were advocates of an empirical or "operational" economics is, of course, to assert nothing very specific. Nor was it possible for many of the Historical economists to get very specific within the context of the conditions which faced them. Each of the authors considered in this study did believe in the desirability of a scientific study of social phenomena, and each believed that this science had to be tied to observable phenomena

both through its "premises" and its "conclusions." There, however, the similarities between the various British Historicists came to an end. It is impossible to identify the members of this tradition with any one political perspective: Jones and Whewell were orthodox Tories while Symes and Ingram were collectivists, and Bagehot and Leslie were reformed liberal individualists. Similarly, it would be extremely distortive of the actual record to group all the Historicists as "radical inductivists," uninterested in "theory." Although both Jones and Ingram expressed contempt for any theories constructed on an intuitional or a prioristic base, the former was a "theorist" in his own right, and the latter believed that economics would one day reach the "deductive stage." All the other early British Historical writers were quite explicit in stating their goal as the reconstruction of economic theory.

The contention of many historians of economic thought that the Historicists were primarily evolutionists is also in error. Although most British Historical authors expressed sympathy with studies of social evolution, their aims in such studies were to determine the economic consequences of institutional change and were generally unconnected to the formulation of a doctrine of social evolution. (This same retort is, however, inapplicable to Marshall's writings and, to a lesser extent, to Mill's. Neither of these Orthodox theorists hid their belief in a science of social evolution or in the ethical purposes of such a science.)

The one and only common property binding together the views of the various Historical writers was their rejection of introspection and

their mistrust of expert intuition as investigatory tools primary to the study of society. The full consequences of this rejection were not entirely clear in their own day, however, for the methods required to carry out the opposing research programs of empirical inquiry were still unknown. It was not until the Twentieth Century exchanges between Knight and Hutchison and between Friedman, Rotwein and Machlup that the methodological camps divided into clearly defined advocates of methodological a priorism and clearly defined "hypothetical-deductive" empiricists. As a necessary background for the appraisal of these extensions of Historicism and Orthodoxy into the Twentieth Century, we now turn to a sketch of recent methodological positions.

Recent Methodological Contributions

The Foundations of Modern Economic Methodology

J. E. Cairnes' volume, The Character and Logical Method of Political Economy, supplemented by Mill's Essays on Some Unsettled Questions of Political Economy, served as the methodological bible for Nineteenth Century Orthodox economists. This same role was fulfilled during the last decade of the Nineteenth Century and the first decades of the Twentieth Century by John Neville Keynes' The Scope and Method of Political Economy.¹ Keynes' treatment of the subject was trebly blessed: by appearing after the death of the last of the early British Historicists, by being composed in a style so moderate as to raise little opposition and by being so detailed and comprehensive as to virtually overwhelm any contenders. The volume was

consequently so successful that it was reprinted repeatedly from its first appearance in 1890 down to 1930. It was required reading in the better economics graduate programs in both the U. S. and Britain and remained so at least as late as the mid-1920's. Although a complete appraisal of Keynes' methodology is beyond the scope of this dissertation, a consideration of those of his views concerning key issues in the British Historical debates will prove of value in our consideration of more contemporary meta-economic writings.

The Function of Economic Methodology

Keynes' opinions concerning the role of the economic methodologist are clearly expressed in his 1890 "Preface" to The Scope and Method of Political Economy and in the subsequent first chapter of that work. The methodologist, according to Keynes, is properly a healer of wounds and a solver of pseudo-puzzles, whose job it is to go about unraveling the opposing positions in past intra-disciplinary controversies so as to demonstrate that the differences between the opposing parties were not so great as they at first seemed. The methodologist is thus a conciliator of disputes involving non-theoretical issues, one who frees the economist from the waste involved in "playing word games" and allows him to get on with the business of "doing economics."² While the methodologist does have some non-conciliative function, especially in a field whose subject phenomena were "more complex and less uniform than those with which the natural sciences are concerned,"³ he is well advised to "be upon ... guard against allowing any such (methodological) discussions

to obscure the greater importance of actual economic investigations."⁴ Above all, the methodologist must avoid the position of a partisan, anxious to defend one methodological procedure to the exclusion of others. His first principle should be that each of the proposed forms for methodological inquiries has a role to play in "assisting ... in the investigation (of) the phenomena of wealth."⁵ In light of such open-ended views, it is easy to understand how Keynes himself felt called upon to defend social evolutionism as a doctrine just as legitimate as "the deductive method." It is also possible to excuse his rejection of British Historicism since he clearly believed that the British Historicist's attempts to replace "deductive" by "inductive" methods were based upon what would later become known as a "category mistake."

The Character of Economics

Keynes distinguished three basic categories into which economic studies might fall: a positive science, a normative or "regulative" science and an art.⁶ The first he defined as "a body of systematized knowledge concerning what is," the second as "a body of systematized knowledge relating to criteria of what ought to be concerned ... with the ideal," and the last he defined as "a system of rules for the achievement of a given end."⁷ Although Keynes admitted that the distinction between a positive science and an art had frequently been obscured in the past (i.e., by Adam Smith),⁸ and was sometimes himself unfaithful to the distinction between an art and a normative science,⁹ this tri-fold classificatory schema was vital to his developed

methodological views.

In substance, what Keynes attempted in The Scope and Method of Political Economy was a partition of economics into two distinct and operationally separate realms--a purely "theoretic" positive science and an applied art. The former was, with some modifications, psychologistic,¹⁰ absolute,¹¹ abstract (in the sense of seeking "pure knowledge" rather than the answers to "practical" issues),¹² and logically independent of experience¹³ (although capable of gaining "insights" from nature). The latter, the applied art, was "realistic" (i.e., empirical), "hypothetical" (i.e., conditional),¹⁴ issue-oriented and totally dependent upon the institutional and customary context of society for supplying the datum which it was to analyze.

By drawing a distinction between "theory" and "art" in exactly this way, Keynes' classification was destructive of the concept of an empirical social science, a science conceived as a deductive system of higher-level and lower-level "hypotheticals," empirically interpreted and conjoined with a series of observation statements for purposes of testing. To compound the anti-empirical character of his methodology still further, however, Keynes persisted, against the authority of his own distinctions, to reinterpret the "art" of economics as a hypothetical-normative study of the possible courses for economic legislation. As he himself expressed this view:

... it seems likely to conduce to clearness of thought to regard the branch of inquiry under consideration as forming the economic side of political philosophy, or of the art of legislation or of social philosophy, as the case may be, rather than as constituting a distinct art of political economy.¹⁵

In considering the question of "whether the aim of the economic art is individual or social and whether it is national or cosmopolitan,"¹⁶ Keynes was willing to admit the legitimacy of all possibilities, excepting only the application of the art to problems of individual decision-making.¹⁷ Economics was at best a science for determining the possible paths or likely patterns of social or collective action; it had virtually nothing to say about the behavior of individuals faced with different systems of constraint. Marshall's study of "the ordinary man in his everyday pursuits of making a living" was somehow relegated to ethics or the dogmas of "economic chivalry," as, in fact, it had been in Marshall's own writings.

Keynes' Treatment of the Historical School

Keynes' perspective on the divisions and scope of political economy was a major factor molding his treatment of the Orthodox and Historical Schools. In concluding the final chapter of his work, he had written that:

As to the doctrine expounded in the following pages ... great importance will be attached to the place of the deductive method in economic inquiry ... (although) ... no one method will be advocated to the entire exclusion of other methods. It will, on the contrary, be shewn that, according to the special department or aspect of the science under investigation, the appropriate method may be either abstract or realistic, deductive or inductive, mathematical or statistical, hypothetical or historical.¹⁸

Yet Keynes was not so perfectly unbiased as to ignore the existence of "two broadly distinguished schools" in economic thought, "one of which describes political economy as positive, abstract and deductive,

while the other describes it as ethical, realistic and inductive."¹⁹

The "positive, abstract and deductive" form of economic speculation Keynes correctly identified with J. S. Mill, J. E. Cairnes and Naussau Senior. The opposing trend, of an "ethical, realistic and inductive" political economy, was, however, the exclusive property of "the dominant German School."

The reasons for Keynes' concern with the German Historical School²⁰ and his near neglect of the British Historical authors are several. First, it must be admitted that the age of the early Historical School in Britain had already passed by the 1890's. Although a revised edition of Leslie's Essays appeared in 1888, it was the last major work of a truly Historical character, the author himself having passed away the year before. Secondly, Keynes' own conception of the controversy between Historical and Orthodox economists turned upon the worth of historical vs. a priori methods. What he meant by "historical" was in turn dependent upon his conception of economic history. For Keynes believed that the "historical method" was an attempt to replace traditional economic theory with a new "evolutionary" theory based upon a study of "the facts," or, in its more extreme forms, an attempt to substitute for any theory a pure study of past economic events. The latter program of historical imperialism was related directly to the character of the opposition which Keynes' believed to exist between theoretical and historical studies. As he expressed this dichotomy:

The former [the study of economic history] describes the economic phenomena existing at any given period in the past, and traces the

actual progress of such phenomena over successive periods; the latter [the study of economic theory] seeks to determine the uniformities of coexistence and sequence to which economic phenomena are subject. The propositions of economic history are accordingly statements of particular concrete facts; economic theory, on the other hand, is concerned with the establishment of general laws.²¹

While theories and facts had some connections with each other in Keynes' methodology, their relations were quite as nebulous as the Cartesian ties between mind and matter. History, in principle, might be used to exemplify theories; yet there were major difficulties, in practice, with any particular connection. As Keynes explained:

A theory may be satisfactorily tested and confirmed by an historical record taken in its entirety, and yet it may be difficult to point to any separate portion of the record as constituting by itself an adequate illustration or exemplification. Illustrations avowedly fictitious are preferable to historical illustrations that require to be doctored in order to serve their purpose; and on the whole, while it is desirable to have recourse to historical illustrations wherever suitable ones present themselves, it is chimerical to expect that such illustrations can wholly supersede and replace illustrations of a hypothetical character.²²

Similarly, though history might be suggestive of the proper limits of a theory's application,²³ there is never any suggestion in Keynes' writings that it could be used to critically test theories or "theorems."²⁴ It might, at best, be used to refute "economic precepts," by which Keynes meant "rules for the guidance of actions."

The only really legitimate interactions between history and theories were to be found in the case of "theories of economic growth and progress."²⁵ Where "the direct comparison of successive stages of society (is the only means through which) we can reasonably hope to discover the laws, in accordance with which economic states tend to succeed one another or to become changed in character."²⁶ Although Keynes believed that the study of societal evolution was a legitimate branch of economic studies, he objected vigorously to the German Historical School's attempt to reduce all of economics to a study of economic factors in the evolutionary process.²⁷

In Keynes' view, then, the debate between Historical and Orthodox economists centered upon two issues: a confusion between falsifiable "economic precepts" and non-falsifiable "economic theorems," and an imperialistic claim by Historical writers for the dominance of an evolutionary economics and/or a systematic study of "economic facts" to the exclusion of economic theory of the static variety.

Since the writers of the British Historical School had ceased to be a viable intellectual movement by Keynes' time, and since they did not neatly fit into either his general classificatory schema or his division between the Historical and Orthodox viewpoints, he saw fit to ignore their existence. There were, however, additional reasons for rejecting the British Historical tradition which were "philosophically" sound according to Keynes' perspective, and it is to one of these--his rejection of falsifiable-empirical economic theories in the form of a study of alternative property structures--to which we now will turn our attention.

In one of his few references to a British Historical author, Keynes spent considerable space quoting and criticizing Leslie's discussion of the tautological character of the Classical's' version of "wealth maximization." After quoting a passage in which Leslie cited the different forms assumed by wealth in different ages and nations and the quite different behavior pattern reinforced by an attempt to "maximize wealth," Keynes commented that:

The whole of the above argument is very persuasively put, but it does not establish the conclusions that Cliffe Leslie desires to establish. By the desire for wealth is meant the desire for general purchasing power, that is, the desire to increase one's command over the necessities and conveniences of life in general; and nothing that Cliffe Leslie says proves it to be either an illegitimate or barren assumption that in their ordinary economic dealings men are in the main influenced by this desire, and that, in consequence, a greater gain is to be preferred to a smaller. That there are enormous variations in men's ideas, as to the particular things that constitute the necessities and conveniences of life, is nothing to the point. For, as observed in the text, the immediate effects of the desire of wealth may be the same, although the ulterior objects had in view are very different."

We may add that it is also not to the point that, under different conditions, the desire of wealth may lead to very different lines of conduct. The assumption that men are actuated by this desire is, in economic reasonings, combined with other assumptions--as, for example, the absence of force and fraud--which circumscribe within certain limits the modes in which the desire can operate.²⁸

It is obvious, however, that this entire critique is merely self-contradictory. If the "desire for wealth" is not only a "desire for general purchasing power" (i.e., money) but also a desire for

things partly obtainable through money and partly not, a point regarding which Keynes is quite ambiguous,²⁹ then what is to be considered as "wealth" (or should we rather say "well-being"?), and in what relative amounts, is very important to an operational economic theory. Similarly, it will simply not do to continually retreat into an unspecified collection of implicit background conditions bounding the theory or into an equally uncertain claim that "surely" a "due account" will be taken of "non-economic" factors when they are "important" in predicting human actions. Yet these are Keynes' common practices.³⁰ Keynes was, in fact, so blinded by his own conviction in the truth of Orthodox theory and his own conception of the Historical alternative that he expressed amazement at Leslie's use of inferential reasoning and his application of market principles to market situations.³¹ He, after all, believed that these were un-Historical practices.

An Evaluation of Keynes' Influence

We have previously discussed the extent of Keynes' influence on the methodology taught to early Twentieth Century economists, and by now the character of that influence should be clear. Keynes' example served only to extend the Orthodox paradigm for economic research into the Twentieth Century. While his treatment of methodological subjects was considerably more detailed and sophisticated than that of Cairnes, it was also infected by Marshall's "misplaced concreteness" and the evolutionary outlook of Comtists and German Historicists. What superficially appeared as a conciliatory strain in his writings was but the

deepening of previous methodological errors.

Frank Knight and American Orthodoxy

American economics never developed any strong singular methodological tradition prior to the Twentieth Century, yet elements of both the Historical and Orthodox Schools sometimes made their appearance in the curriculum of American Schools. Mill's Principles was widely distributed in this country, usually in a pirated and abridged edition which omitted his "historical" discussions. Only a few decades thereafter, Symes' Outlines was widely used "as a textbook in elementary political economy ... in U. S. colleges and schools"³² and received a favorable review from the pen of Henry Carey. German influences became even more evident after the founding of the American Economic Association by Richard Ely, and Orthodoxy did not stage a revival until at least the third decade of the Twentieth Century. America's first two Marshallians were hardly typical of the group which surrounded Marshall at Cambridge. Frank Taussig was known to have definite Austrian sympathies despite his appreciation for Marshall's work, and was not at all interested in sanctifying Marshall's every word. Herbert Davenport, although unquestionably more devout, was himself somewhat of a heretic to the Marshallian system. His writings were, in any case, so difficult to interpret that he found few followers among his colleagues.

The mantle of Orthodox economics in Twentieth Century America eventually came to rest upon the shoulders of Frank Knight, whose monumental Risk, Uncertainty and Profit (1921)³³ was one of the first

economic classics to be produced by an American author. If it was accurate to describe Marshall at an early stage in his career as "a young philosopher carrying a somewhat undigested load of German metaphysics, Utilitarianism and Darwinism,"³⁴ it would be just as accurate to describe the young Knight as a young philosopher carrying a somewhat undigested load of Pragmatism, Kantian metaphysics, Millian epistemology and a belief in the introspective approach to social inquiry.

Knight's methodological writings are scattered over half a dozen books and as many essays, spanning nearly thirty years (1921-1951). Since a comprehensive and detailed consideration of the whole of his methodological thought would be out of place in a note of this character, we have chosen only a few representative essays to illustrate the structure and changes of his views.

The earliest and most cautious expressions of Knight's meta-economics are to be found in Chapters 1 and 4 of his Risk, Uncertainty and Profit. In the first paragraph of the text we are met by Menger's distinction between exact and empirical sciences and by Knight's own estimation that "Economics, or more properly, theoretical economics, is the only one of the social sciences which has aspired to the distinction of an exact science."³⁵ Also, on the same page, we are warned that "artificial experiments" are denied the economist, and he is thus thrown either upon a reliance on historical data, derived under variable circumstances, or he must "rely upon intuitive knowledge of general principles and follow through the workings of individual chains of sequence by logical processes " (i.e., Cairnes'

"mental experiments").³⁶ The "analytic method," however, yields only "an approximation to the laws of the situation as a whole ... statements of what 'tends' to hold true or 'would' hold true under 'ideal' conditions, meaning merely in a situation where the numerous and variable but less important 'other things' which our laws do not take into account were entirely absent."³⁷ Knight was also surprisingly clear concerning the nature of those "other things" or "disturbing causes." They were "simply anything not included in the specifications (of empirical application conditions), and their elimination is probably equally impossible, and, again, equally necessary to assume."³⁸ A clearer statement of the traditional Orthodox position or of the contradictions within that view would be difficult to imagine.

Although all science was, in Knight's view, of this same "incomplete" character, "theoretical economics has been much less successful than theoretical physics ... largely because it has failed to make its nature and limitations explicit and clear."³⁹ It was, of course, the role of Knight's Risk, Uncertainty and Profit to explicitly delimit economics and make the assumptions of its competitive model quite "explicit." That the model thus derived contradicted both systematic observation and "ordinary experience" did not seem to disturb him in the least.

Although it is obvious from the first page of Knight's major work that he was very much in the lineage of Orthodox methodology, the particular cast of his views were not spelled out until he turned to "epistemological" considerations. In what is virtually a quote from Mill's Logic, he stated that: "We shall see that there is ultimately

no such fact [sic] as deduction as commonly understood, that inference is from particulars to particulars, and that generalization is merely tentative, a mere labor-saving device."⁴⁰ In this same spirit he later cautions his reader that "This writer is in fact a radical empiricist in logic, which is to say, as far as theoretical reasoning is concerned, an agnostic on all questions beyond the fairly immediate facts of experience."⁴¹ The radical dichotomy between "the facts" and "the theory" is also present in Knight's earliest writings, for shortly after proclaiming that there was ultimately no such thing as "deduction as commonly understood," he turned to an endorsement of introspection and an examination of "the facts of consciousness."

In examining the correct methods for the social scientist, he wrote:

It is to be noted ... that our common-sense generalizations have a very high degree of certainty in some fields, giving us, in regard to the external world, for instance, the "axioms" of mathematics. Even more important in the present connection is the role of common sense or intuition in the study of human phenomena. Observation and intuition are, indeed, hardly indistinguishable operations in much of the field of human behavior. Our knowledge of ourselves is based on introspective observation, but is so direct that it may be called intuitive. Its extension to our fellow human beings is also based upon ... interpretation ..., far more than upon direct observation of behavior, and this process of interpretation is highly instinctive and subconscious in character. Many of the fundamental laws of economics are therefore properly "intuitive" to begin with, though of course always subject to correction by induction in the ordinary sense of observation ...⁴²

And in his examination of the meaning of "risk" and "uncertainty," he

observed that the "first datum for the study of knowledge and behavior is the fact of consciousness itself."⁴³

Although Knight was willing to concede a great deal to behavioral observations, and occasionally strayed across the boundaries separating "radical inductionism"⁴⁴ and intuitive procedures, he was fundamentally enslaved by the Cartesian interpretation of "intentionality" as the acts of a mind which controlled the physical body but which itself existed in a non-material world of "ideas" and future plans.⁴⁵ As if to emphasize the non-material character of the real subject of the economist's study, Knight wrote in his examination of risk and certainty that: "... we mean that things not present to sense are operative in directing behavior, that reason, and all consciousness, is forward-looking; ..." And also:

... as already pointed out, it is always theoretically possible to ignore the form of the conscious relation, and interpret the reaction as a mechanical effect of the cause actually present. But it remains true that practically we must regard the situation present to consciousness, not the one physically present, as the controlling cause. In spite of the rash statements by over-ardent devotees of the new science of "behavior," it is preposterous to suppose that it will ever supersede psychology (which is something very different) ...⁴⁶

The precise significance of these views for "the science of rational decision-making" was never explicitly spelled out in Knight's most famous work. For no sooner had he built up a systematic "view of the world," in accord with his own "functionalist" bias,⁴⁷ than he turned away from metaphysical and epistemological considerations in favor of truly functional discussions concerning the behavioral impact

of different forms of risk distributions.⁴⁸

It would be easy to overlook Knight's methodological views against the background of his sound theoretical work if it were not for the fact that he persistently returned to the subject in literally dozens of future books and articles. In the year immediately following the publication of Risk, Uncertainty and Profit, for instance, he published an essay entitled "Ethics and the Economic Interpretation" [sic]⁴⁹ in which his stated purpose was to determine whether and to what degree economics could be considered a normative study. The essay, in fact, extended far beyond these limits and is best viewed as a rewrite of Marshall's evolutionary methodology interlaced with a variety of neo-Pragmatism. It is here that we meet with what was later to become a major theme in Knight's methodological writings (and had already been a major consideration in the writings of both Mill and Marshall)--economic activity conceived as a process of evolving wants:

Wants and the activity which they motivate constantly look forward to new and "higher," more evolved and enlightened wants, and these function as ends and motives of action beyond the objective to which desire is momentarily directed.

Life is not fundamentally a striving for ends, for satisfactions, but rather for bases for further striving; desire is more fundamental to conduct than is achievement, or perhaps better, the true achievement is the refinement and elevation of the plane of desire, the cultivation of taste ...⁵⁰

Whether these doctrines were derived from Mill, Marshall or Dewey is ultimately inconsequential. Their importance lies not in their

source but in the manner which Knight employed them in order to circumscribe economic inquiry:

... the conscious felt needs of men are not directed toward nourishment, protection from the elements, etc., the physiological meaning of the things for which money is spent. They desire food, clothing, shelter, etc., of the conventional kinds and amounts. (emphasis in original)

One of the most serious defects of economics as an interpretation of reality is the assumption that men produce in order to consume. Except for those very low in the economic scale the opposite is as near the truth, and the motives of a large part of even "lower-class" consumption are social in their nature.⁵¹

Knight's references to the importance of "conventional standards" in consumption⁵² and his consideration of various "non-economic motives" for "economic acts" point the way to his ultimate conclusion.

There is no definable objective, whether subsistence, gratification of fundamental impulses or pleasure, which will serve to separate any of our (economic) activities from the body of conduct as a whole. Nor, we aim especially to emphasize, is there any definable objective which properly characterizes any of it. It simply is not finally directed to the satisfaction of any desires or the achievement of any ends external or internal which can be formulated in propositions and made the subject of logical discourse.⁵³

An empirical science which separated out the "economic causes" for human action from other possible causes was, thus, in Knight's view, an impossibility. In drawing this conclusion, however, he does not commit himself either to the abandonment of the economic enterprise or to the absorption of economics into a general theory of human

behavior. Quite the contrary. The alternative to an empirical science of economic action was, for Knight, an "abstract" or "intuitive" economic "science," a science of the "economic man." After extolling the virtues of this "much mistreated" concept and assuring his readers that it was a mere approximation to reality, for men "neither know what they want--to say nothing of what is "good" for them--nor act very intelligently to secure the things which they have decided to try and get,"⁵⁴ Knight clearly defined the consequences of this view.

A science of conduct is, therefore, possible only if its subject-matter is made abstract to the point of telling us little or nothing about actual behavior. Economics deals with the form of conduct rather than its substance or content. We can say that a man will in general prefer a larger quantity of wealth to a smaller ... because in this statement the term "wealth" has no definite concrete meaning; it is merely an abstract term covering everything which men do actually (provisionally) want ...

Such laws are unimportant because they deal with form only and say virtually nothing about content, but it is imperative to understand what they do and what they do not mean.⁵⁵

In a 1925 essay entitled "Economic Psychology and the Value Problem,"⁵⁶ Knight appeared little concerned with the purely formal character of economic "principles." He instead had turned his attention to a direct confrontation with economic behaviorism, a consideration of the arguments necessary in order to establish a role for an "understanding" superior to mere observation of physical acts, and the meaning of what Nagel would later call "theoretic terms" (i.e., "force" in physics or "desire" in economics). In explaining his

version of "the problem of other minds" and the "paradox" resulting from this view, Knight stated that:

... logic notwithstanding [sic], we do recognize some behavior as communicative from other minds [sic] and do not so interpret other behavior ... no human being really disbelieves in the reality of force as an element in the external existent world, any more than he doubts the fact of consciousness in his fellow human beings. The contradiction between logic and common sense is aggravated rather than resolved by appealing to the analogy between mechanics and human behavior.

In mechanics there can be no discrepancy between forces and their effects, because we have no source of information regarding the forces except the effects themselves.

... in human behavior we have two sources of information in regard to desire (which is the analogue of force), and the two sources disagree ...

... and yet we cannot disbelieve in the validity of either of them. We "know" that there is a causal relation between desire and conduct, and we "know" also that the causes do not accurately or closely correspond with the effects.⁵⁷

The obvious conclusion, that the "dilemma" could be avoided if we were to admit that our "knowledge" of the "inner world" was no different than our "knowledge" of a dream, apparently never occurred to Knight; primarily, one would suspect, because he was willing to accept the "vividness" of a conception as the test of its truth.

Knight did, however, distinguish a definition of "desire" which would have been totally acceptable to any "behaviorist" of Ryle's type from the intuitive and introspective conception so prominent in Nineteenth Century economics.⁵⁸ He also recognized that: "...

scientific economics is restricted in its data to behavior-facts. It cannot deal with feeling facts except as a mode of expressing behavior-facts ... [for] ... the facts of desire and satisfaction cannot be accurately observed and measured ..."⁵⁹

Yet he insisted that "scientific economics" was an absurdity, resting upon an indefensible exclusion of those things which we "know" (i.e., that our feelings exist whether or not they have any behavioral consequences).

The question of how it is that we can connect unobserved "feelings" with observed actions was, for Knight, at the most obvious level, a trivial problem--we do it "through analogy." That he believed the triviality of this problem to be more apparent than real was, however, obvious from the extensions of his discussion. Interpretation of the behavior of others was, for Knight, the result of social training, just as all "... observation itself ... is a power socially developed and trained in the individual, and produced in the course of history by the accumulation of communicated and compared experiences ... always we see largely what we expect to see, what fits into our organized knowledge of the world ..."⁶⁰ "So far from our knowledge of the consciousness of other persons being an "inference" from a "perception" of their behavior, it turns out that the very capacity to perceive is developed through and dependent upon intercommunication between minds as conscious centres."⁶¹

It is hardly necessary to point out that if we mean by "minds" the entities that "exist" in secret or purely private "worlds" of their own, and we mean by "consciousness" the secret acts of these

minds, acts which are somehow prerequisite to the physical acts of the bodies which the minds "manipulate," then it is logically impossible for one person ever to teach another how to "organize perceptions" or how to "be conscious." Acts are only repeatable (or exemplifiable) if they are first intersubjectively observable. Yet only the gross physical movements of bodies, the indirect results of consciousness in Knight's sense of the term, are observable. The actual conscious processes are necessarily hidden from view. Hence, we can only teach someone how to act "as if" he were conscious, never how to be conscious. It is this type of absurdity which must necessarily result from philosophic dualism applied to the description, explanation or prediction of human behavior, and it is this type of absurdity with which Knight felt perfectly comfortable. In describing the character of static economic analysis, for instance, he stated that:

There are no data for a science of conduct ... The data of conduct are provisional, shifting and special to individual, unique situations in so high a degree that generalization is relatively fruitless ... an individual acts (more or less) as if his conduct were directed to the realization of some end more or less ascertainable ... [Yet] The person himself is usually aware that it is not really final, not really an "end"; it is only the end of the particular act, and not the ultimate end of that. (emphasis in original)⁶²

Knight's hostility toward behaviorist interpretations of social science was carried on into his "The Limitation of Scientific Method in Economics" (1924), where he wrote that:

It is impossible to argue at length in this paper the issues involved in behaviorism; we can only state briefly our own position, which is that consciousness is useful, and its recognition necessary and inevitable, in the interpretation of human behavior. The reason is simply that we cannot help ourselves ...

Logically the behaviorist is right; we do not perceive consciousness, in any other person at least; we cannot prove or verify it; we only infer it from behavior. But in spite of logic we all recognize that as a matter of fact we know consciousness more surely and positively than we know the behavior from which theoretically we infer it.

The behavior of human beings depends upon their previous history, and the history of no two individuals is the same or closely similar, in essential respects. (Hence, no behavioral laws are possible unless they can be based on a complete history of each individual's past experiences.)⁶³

Although his introduction of the element of psychological uniqueness is a new twist upon an old theme, Knight's conclusions are unchanged. "Human phenomena," he concludes, "are not amenable to treatment in accordance with the strict canons of science."⁶⁴

Knight published prolifically in the years following 1925, yet there were few modifications in his basic methodological position.⁶⁵ The main changes, if any, were involved with his increasing bitter denigration of scientific economics and his growing involvement with issues of social ethics and psychologistic sociology. A characteristic piece of this period was his "Social Economic Organization" (1939)⁶⁶ which, despite a certain popularity among economic theorists, had little to contribute to the technical development of economics and was little more than an attempt to systematically debase its

importance.

Despite the uniformity of Knight's methodological perspective over the years, it may be somewhat instructive to take note of his reactions to the methodological "essays" of Robbins and Hutchison (discussed below) and his attempts to delineate the "social uses" of economic theory.

Regarding Robbins' The Nature and Significance of Economic Science Knight wrote:

... only with qualification can the book ... be endorsed as a satisfactory treatment of its topic, for in spite of much insistence, of a rather too-much-protesting sort, on "precision," and on being "scientific," many of the positions taken fall considerably short of being thought out to definiteness and accuracy.⁶⁷

One of the main "defects" which Knight attacked in Robbins' methodology had to do, however, with his (Robbins') attachment to a strict interpretation of ends as givens (a view which is, of course, traditional in economics and is only illegitimate from the standpoint of process-philosophy or Knight's own version of an evolutionary Pragmatism). The remainder of Knight's objections revolved around Robbins' attempt to view human action as positive data useful in the construction of a positive science:

Human "facts" are essentially and primarily both purposive and evaluative. In addition, they are essentially historical--"dynamic" in a sense, in comparison with which any mechanical or mathematical dynamics belongs definitely in the field of statics. It is simply impossible to describe empirically or analyze theoretically in the human field

and be "objectively correct" without being clear as to the bearings upon one's statements of fundamental conceptions of individual and social policy in a broad sense. The categorical distinction between judgments of "is" and "ought" which Robbins stresses so often simply cannot be maintained.⁶⁸

It is, of course, possible that there can be no such thing as a positive science of human behavior, but Knight in no way demonstrates its impossibility. It is hardly enough to assert that human action displays no empirical regularities, for men are free to assert whatever may strike their fancy (and few hesitate to do so). The probable explanation for Knight's normative and "philosophic" view of social investigation is simply that he was known to enjoy speculative philosophy, especially medieval metaphysics, and these pursuits undoubtedly were more stimulating if mixed with asides about the future of the social organism.

That Knight's own aims in social investigations were ideological as well as "philosophic" can hardly be doubted.⁶⁹ He frequently emphasized that "agreement" or "consensus" was the goal of all social action as well as all social theorizing.⁷⁰ He quite explicitly posed the "instrumental attitude of investigation ... of an external subject matter" against the grounds for ordinary action, i.e., for social organization and for all our "important" pursuits, and it would seem simply unjust not to take him at his word.

The Latter Days

The last major expression of Knight's social methodology is

contained in his massive critique⁷¹ of T. W. Hutchison's The Significance and Basic Postulates of Economic Theory (1940) (discussed below). After a particularly vicious collection of observations on the worth of methodological inquiries, which is rather reminiscent of a man swearing at his own image, Knight proceeded to a forty-page summary of his own views concerning methodological matters. In this discussion, we are almost immediately greeted by his old bias against "purely objective" hypotheses and his conviction in the necessity of "purposefulness" in economic discussions: "... if one is talking about a check system ... one is certainly concerned with purposes aimed at and results achieved as well as with the existence and path of printed pieces of paper, or any physical event."⁷² Yet a few sentences further we are reminded that "tests" of an hypothesis are largely a matter of professional convention, and that they depend for their success upon the "basic postulates" of economic theory:

While there are or can be "definite, agreed and relatively conclusive criteria for the testing of propositions, solutions, and theories," there are no very serious intellectual problems, and no methodological problems whatever. The problem of truth in Mr. Hutchison's subject matter is not one of finding such tests; any tests which can be proposed would rather themselves have to be tested by the propositions of economic theory as already understood.⁷³

Knight for the first time seemed willing to explicitly identify the "fundamental propositions" of economics with Kant's synthetic a priori basis for science, yet he persisted in denying this same basis to the natural sciences:

The fundamental propositions and definitions of economics are neither observed nor inferred from observation in anything like the sense of generalizations of the positive natural sciences or of mathematics [sic], and yet they are in no real sense arbitrary. They state "facts," truths about "reality"--analytic and hence partial truths about "mental" reality, of course--or else they are really "false." Economics and other social sciences deal with knowledge and truth of a different category from that of the natural sciences, truth which is related to sense observation--and ultimately even to logic--in a very different way from that arrived at by the methodology of natural science. But it is still knowledge about reality.⁷⁴

Descartes' distinction between "the objective reality" of the physical world and the subjective reality of "men's minds" is still a fundamental element of his discussions,⁷⁵ yet it is modified by Kant's qualification--that the structure of thinking, "the way in which minds work," is also the "structure of reality."⁷⁶ Knight seemed, in fact, to have been completely converted from his previous "radical empiricism" to a Kantian perspective on epistemological questions. In his rebuttal to Hutchison's positivism and critical empiricism, he was willing to accept both a Gestalt theory of perception and the notion of a basic structure of things which is known instinctively.

Not surprisingly, Knight was still wedded, in his 1940 essay, to a purely "analytic" economics and was still averse to the development of an empirical-predictive economic science. Instead of his previous references to "uneconomical" motives which must be considered along with the economic motives or to Mill's model of the economic man, he now based the deviations between analytic expectations and the

observed behavior of men to "ignorance, error and "prejudice" in innumerable forms [which] affect real choices."⁷⁷ Most important of these disturbing factors were, of course, the lack of both "perfect knowledge" (i.e., knowledge of the current status of all relevant decision variables) and "perfect foreknowledge" (i.e., knowledge of future changes in decision variables). Foreknowledge of the responses and behavior of one's fellows is especially necessary for "rational action" of the type described by the economist's model.

"Ignorance, error and prejudice" necessarily result in problem-situations which a "pure positivist view" (i.e., Hutchison's) must reject as "unreal, transcendental, or mystical." In Knight's characterization, the "positivist" is compelled by his own premises to reject "... all conception of any process of problem-solving in any sense."⁷⁸ This curious notion, that "problem-solving" is a process occurring only in the domain of mental ghosts, without any distinguishable behavioral "manifestation," is at the heart of Knight's late methodological views. If it is true that we can never "really" know what motivates another person or "really" know anything at all about the processes by which he lives his life, then there are numerous other implications which Knight does not consider. For instance, how is it that we "know" that one person is clever and another dull? How do we "know" that a particular person does something "intentionally" and that another person does it "by accident"? How do we "really" know that one person is in agony, but is reasonably stoical, while another is only in slight pain, but "needs" attention and sympathy? It is insufficient to argue, as Knight does, that we

know of the existence and fundamental properties of "other minds" as a result of some compulsive intuition. If this was a true explanation of how it is that we gain our knowledge about others, we would never be able to make judgments as specific as the preceding ones. No one has ever imagined that our synthetic a priori knowledge is specific enough to discriminate "maximizing action" from "successful action," yet we regularly speak of one person "succeeding by his merits," another "succeeding by luck" and a third "not trying." No one has ever suggested (for it is, in Knight's sense, "intuitively absurd") that we know only that there exist other minds and that they perceive according to Euclidian geometry and act according to the "maximizing principle." Yet this is all that we can know under Knight's view of how we arrive at our social understanding. It is not behaviorism which strips individuals of everything which makes them individuals, but, rather, the Kantian intuitive approach to social inquiry.

A Concluding Note

Although Knight published many articles concerning economic topics during the remaining thirty years of his life, his final methodological stance is well summarized in his 1951 Presidential Address to the American Economic Association. In this address, appropriately entitled "The Role of Principles in Economics and Politics," he appealed to what seemed to him to be the vital distinction between the mental and observable acts of men: "One reason why a science of human behavior, in the literal sense, is impossible is that, in contrast with physical objects, our behavior is so saturated with varied make-believe

and deception, not clearly separable from the 'realities'."79 Also:

As to a science of human behavior, I have mentioned some difficulties, notably that one of the most distinctive traits of man is make-believe, hypocrisy, concealment, dissimulation, deception. He is the clothes-wearing animal, but the false exterior he gives to his body is nothing to that put on by his mind.⁸⁰

This was then followed by what Knight obviously believed to be the ultimate refutation of the behaviorist view: "The simple fact is that we commonly recognize and describe human behavior forms as expressions of some feeling, intent, belief, not as bare acts. And our terms often contain an inseparable value judgment as well; there is no specific intent, not to mention a specific act, of murder or theft."⁸¹

It apparently never occurred to Knight that terms such as "hypocrisy" or "dissimulation" refer to the observable acts of men, that we do not condemn our fellows as hypocrites or liars because of what goes on in their "unknowable" and inaccessible inner worlds any more than a jury finds a man guilty of "murder" or "theft" because they don't happen to like his personality. The characteristics of men or their acts are determined on the basis of observations of their past or present actions in the context of some system of constraints. We would not normally call someone a liar if he lied under extreme duress (i.e., if he was instructed to lie by someone immediately threatening his life), nor would we call someone a thief who recovered his own property. There are, however, perfectly "objective" standards (which is not to say "certain" standards) for the determination of these "background conditions" and (conventionally) appropriate or

inappropriate ways in which to apply terms. That the same term may be appropriately applied to "different acts" is explicable by reference to the range of its conventional uses and to certain accompanying circumstances. If we accept Knight's analysis at face value, then it would be impossible ever to speak to one another since it "seems likely" that we all have (somewhat) different "values" and would connect different "value-judgments" to the same term.

One suspects that all of the quibbling over the "possibility" of a behavioral science comes back to Knight's belief that "we know" that men are "free," and thus their actions can never be predicted. This is certainly confused, however. A man can no more be said to be "unfree" because he is predictable than he can be said to be "free" if he acts in a purely random fashion. The problem of the "freedom of the will" should in any case be separated from questions of behavior since "the will," as Knight continually insists, is only contingently connected to the observable world.

Lionel Robbins on the Nature and Significance of Economic Science

While Knight was attempting to build up an American Orthodox School, economists in both Britain and Germany were busily undermining their own Orthodox methodologies. The two most significant attempts to strike out along new pathways were Lionel Robbins' Essay on the Nature and Significance of Economic Science (1932; second revised edition, 1937)⁸² and T. W. Hutchison's The Significance and Basic Postulates of Economic Theory (1938).⁸³ We will consider each of these works in turn.

Robbins' Essay was a startling advance over previous orthodox writings, yet it is apparent that he was concerned to defend many of the primary orthodox tenets.⁸⁴ Unlike Marshall, Keynes and Knight, Robbins believed that methodological investigations were vital to the continuing development of a well structured empirical economics. They were to serve not so much as the shield of an already completed edifice, which found itself in the position of fending off the attacks of the ignorant, but as spurs to the consideration of new and difficult problems in the body of the discipline.⁸⁵ Although Robbins acknowledged a debt to both Ludwig von Mises and Phillip Wicksteed,⁸⁶ it is questionable whether he actually gained more from their writings than his engaging literary style. Despite an opening flourish in which he declared that "The efforts of economists during the last hundred and fifty years have resulted in the establishment of a body of generalisations whose substantial accuracy and importance are open to question only by the ignorant or the perverse,"⁸⁷ Robbins' treatment of the subject was well-reasoned and remarkably free from dogmatism.

The broad outlines of Robbins' arguments are known to every economics undergraduate, and are undoubtedly believed by many to have been handed down from the pen of Father Adam. Robbins defined economics not as the study of economic motives,⁸⁸ nor as the study of scarcity per se,⁸⁹ but as a consideration of "the forms assumed by human behavior in disposing of scarce means."⁹⁰ The scope of economic studies and the proper definition of the economic, as opposed to the non-economic, were not matters of arbitrary professional standards,

but resulted from the activities of economic scientists in the pursuit of particular goals.⁹¹ In general, Robbins believed that "to speak of any end as being itself 'economic' is entirely misleading."⁹² All ends, the achievement of which involved a choice between alternatives, are necessarily economic. "Economics is not concerned at all with any ends as such. It is concerned with ends in so far as they affect the disposition of means. It takes the ends as given in scales of relative valuation, and inquires what consequences follow in regard to certain aspects of behavior."⁹³ Stating this same point from a slightly different perspective later in his argument, Robbins wrote: "... the subject-matter of Economics is essentially a series of relationships--relationships between ends conceived as the possible objectives of conduct, on the one hand, and the technical and social environment on the other. Ends as such do not form part of this subject-matter. Nor does the technical and social environment. It is the relationships between these things and not the things themselves which are important for the economist."⁹⁴

The implications of this methodological perspective were, in Robbins' opinion, sweepingly different from prior beliefs. Economic history was now transformed into the testing ground for economic theory, and any event which could be explained as an interplay between scarcity, human desires and particular institutional constraints was fair game for the economic historian.⁹⁵ The later German Historical School (that of Schmoller) was criticized for the "intrusion of all sorts of sociological and ethical elements which cannot, by the widest extension of the meaning of words be described as Economic

History."⁹⁶ Yet in every other sense the definition of the study was to be expanded.

While Robbins believed that the growth of "sectional studies in the economic field" was a healthy sign of an increased appreciation for applied problem-solving, he cautioned, however, against a confusion between business-oriented studies of the methods for minimizing costs and studies of the economic behavior of firms.⁹⁷

True to his expressed roots, Robbins did advance an Austrian formulation for the concept of wealth,⁹⁸ for the determination (or "imputation") of factor prices,⁹⁹ and for an Austrian explanation of business cycles.¹⁰⁰ That he was never slavish in his devotion to that tradition is, however, obvious from the fact that he correctly identified the most advanced elements in value theory with the Hicks-Allen indifference analysis,¹⁰¹ discovered a quote from Samuel Bailey which strongly endorsed "substitutibility" as a replacement for "utility" and clearly stated the primary elements of the "Scitovsky Paradox" many years before Scitovsky himself.¹⁰² Robbins was never the defender of a holy doctrine; he was an innovator and a critical spirit willing to embrace new concepts.

Among other contributions and achievements, Robbins meticulously demonstrated the unspecifiability of macro concepts without micro foundations¹⁰³ and demanded that all lower-level economic concepts must be at least "exact" (i.e., operationally tie-able and quantitative).¹⁰⁴ He spoke out forcefully against Marshall's dynamics of social evolution¹⁰⁵ and noted the key flaw in any unconditional theory of social evolution, viz., its inability to take account of original

technological developments.¹⁰⁶ He anticipated some of the more sophisticated refinements of micro-economic theory over the following decades involving problems of the costs of decision-making and the costs of attaining complete "rationality" (i.e., transitivity) in a consumer's preference function.¹⁰⁷ He restated Jevon's still-neglected problem of the "minimum sensible,"¹⁰⁸ and he advocated a comparative study of property structures.¹⁰⁹

It is unrealistic to expect that a complete break with Orthodoxy could be achieved by any one author, however. Accordingly, Robbins apparently felt the need to pay lip-service to many of the Orthodox principles which he had so skillfully superseded. Although he forthrightly rejected the naive inductivist view of deriving "correct hypotheses" from a study of historical facts,¹¹⁰ he was less decisive in passing a negative judgment on "the stuff of our everyday experiences."¹¹¹ Nor was he willing to consider the possibility of controlled experimentation in economics.¹¹² Robbins was obviously ignorant of the true character and the context of the feud between the Historical and Orthodox Schools in Britain. For although he upheld the Orthodox School over the Historical,¹¹³ his references are mainly to the German Historical School and to the Austrian critique of its views.

Like Knight, Robbins was unfortunately enthralled with the notion that behavioral economics must be wrong since it omitted references to the "inner world" of motivations:

Valuation is a subjective process. We cannot observe valuation ... Our business is to explain certain aspects of conduct. And it is very questionable whether this can be done in terms which involve no psychical

elements. It is quite certain ... that, we do in fact understand terms such as choice, indifference, preference, and the like in terms of inner experiences. The idea of an end, which is fundamental to our conception of the economic, is not possible to explain in terms of external behavior only.¹¹⁴

In support of these considerations Robbins raised what is perhaps the most weighty possible argument--that of "expectations" and their effects upon price determination.¹¹⁵ Yet his arguments say more, perhaps, against a post hoc, ergo propter hoc explanation of human behavior based on unspecified "expectations" than they do for the notion of "expectations" or for a hierarchy of "secret" human ends. If "expectations" is simply the term used to refer to the residual error in our predictions, then we should describe it as such. If, however, it refers to "mental acts" which are in principle outside the realm of intersubjective observation, then it should be classed with Kant's "thing-in-itself" and summarily discarded.

The roots of Robbins' methodological bias were grounded in the same soil as his achievements--Austrian economics and its Kantian orientation. His writings often display a struggle between his understanding and appreciation for British Orthodoxy, certain aspects of behaviorism and the dogmas of writers such as Wicksteed and von Mises Mises.¹¹⁶ In one passage he would argue that the "postulates" of economics were reflective of universal empirical regularities, and in the next that they were supportable only through intuition and common experience, i.e., that they were similar to, if not identical with, Kant's synthetic a priori foundations for a science.¹¹⁷ Only a few pages further into the same discussion, Robbins retracted his previous

position, arguing, instead, that economic postulates were merely probabilistic, although we have "substantial" evidence of the intuitive and conventional sort for their "validity."¹¹⁸

Robbins displayed a strong and consistent bias against scientific naturalism (see Appendix B),¹¹⁹ but he was equally as insistent that economic laws were of the same general character as physical laws. In his most extreme moments he attempted to circumvent any thought of critically testing economic theories by stating that there were probably no empirical economic regularities¹²⁰ to discover; and, thus, that economic laws could "explain" human action but never predict it in any but the most general way. There is some indication, however, that he may not have judged this situation to be too different from that faced by the physical scientist, and may have believed that the vital distinction between the physical and social studies lay in the availability of controlled experimentation and the ability of the physical scientist to quantify the variables with which he was working.

In summary, Robbins' methodological views were an unquestionable advance over those of the Orthodox economists, but they were still flawed by major defects regarding the possible empirical character of economic theory and the separation of the definitional core of economics from its interpreted "applications." The modern version of an "Historical" or empirical economics was yet to come.

T. W. Hutchison on The Significance and Basic Postulates of Economic Theory

The last of the pre-contemporary writers of relevance to this

sketch is T. W. Hutchison, the author of a superior history of the Neoclassical era and an important and consistent contributor to debates concerning meta-economics. Much of Hutchison's most comprehensive and important work, The Significance and Basic Postulates of Economic Theory, was concerned with clarifying basic concepts or "fundamental assumptions" in economic discussions, i.e., "equilibrium," "expectation," "utility," etc., and is, therefore, somewhat beyond the scope of our interests. There are, however, many passages in this work which touch upon the types of methodological issues which have previously concerned us, and these we will briefly consider.

Hutchison introduced his methodological views by drawing a fundamental distinction between two exclusive and exhaustive classes of economic statements: those which belong to the class of "pure theory," and those which do not.¹²¹ Statements of "pure theory" were tautologies, or definitionally true, thus:

Being unconditionally true and neither confirmable nor contradictable by an empirical synthetic proposition, propositions of pure theory cannot tell us anything new in the sense of telling us new facts about the world. But they call attention to implications of our definitions which might otherwise have escaped our attention, and reveal unexpected relations between our definitions which are thus explained and clarified.¹²²

This dichotomy between the statements of "pure theory" and other economic propositions had several important implications recognized by Hutchison. First of all, the empirical worth, or "usefulness" of economics, rested not upon its pure theory but upon its other propositions (the empirical interpretations of this theory). Hutchison was,

however, willing to weaken the full impact of this conclusion by allowing for "conceivably" testable theories, rather than demanding that all non-tautologous theories were actually testable:

... if the finished propositions of a science, as against the accessory, purely logical or mathematical propositions used in many sciences, including Economics, are to have any empirical content, as the finished propositions of all sciences except of Logic and Mathematics obviously must have, then these propositions must conceivably be capable of empirical testing or be reducible to such propositions by logical or mathematical deduction. They need not, that is, actually be tested or even be practically capable of testing under present or future technical conditions or conditions of statistical investigation ...¹²³ (emphasis in original)

That the position of "conceivable falsifiability" was probably intended to appease the a priorist, rather than to represent Hutchison's preferred stance, is apparent from his discussion of "philosophic problems" as definitional disputes unbounded by conventional rules, and by his conclusion that, "If inter-subjective tests (of a theory) could not satisfactorily be made, there could be no science." (i.e., there would be no way of settling questions not concerned with the "pure theory" or definition system of the science.)¹²⁴

Of related interest is Hutchison's use of the criterion of falsifiability, a term which he may have encountered in Popper's Logik der Forschung,¹²⁵ and his concern with the "explanatory scope" of a theory, defined in terms of what it forbade:

We propose this as a "division by dichotomy," as it is called, or exhaustive twofold classification of all propositions which have "scientific" sense. According to our definitions of the terms ... either a proposition which has sense is conceivably falsifiable by

empirical observation or it is not. If it is not thus falsifiable it does not, if true, forbid any conceivable occurrence ... Propositions obtain their empirical content simply in so far as ... they exclude, restrict, or forbid something ... Therefore a proposition with empirical content ... must, by definition, be conceivably falsifiable, that is, ... exclude some conceivable possibility. Conversely, a proposition with sense, the validity of which does not depend on any empirical observation, cannot, by definition, exclude any conceivable possibility, and is therefore devoid of empirical content. The price of the unconditional necessity and certainty of propositions ... is, therefore, [a] complete lack of empirical content.¹²⁶

To be certain that he was not misunderstood, Hutchison considered in a footnote the objection of the "Kantian economists" that his dichotomy did not allow for synthetic a priori's, and argued that (1) this question had already been resolved (in a manner unfavorable to Kant) by professional philosophers, (2) that no economist had ever clearly formulated an example of a synthetic a priori proposition in economics, and (3) that the "maximizing principle" (which had sometimes been referred to as a possible example of such a proposition) had been rejected as such by Kant himself.¹²⁷

That Hutchison's "twofold classification of all propositions" was intended only as (what Popper has called) "a demarcation principle" and not as a criterion of "meaningfulness" in general (although he frequently used that term) is clearly expressed in the "Introduction" to his volume:

We are not attempting here to exalt "scientific" propositions or problems above "non-scientific" ones. We do not argue that the meteorologist's "knowledge" of a sunset is

either ... superior or inferior to the poet's or artist's "knowledge." Nor do we insist that this is necessarily exactly the most suitable defining line between these two planes of science and of non-science--though we prefer it to any other terminological suggestion we have seen ...

It is these particular scientific problems (of economic usage and research methodology) which are our concern rather than general methodological issues.¹²⁸

Having established a framework within which his argument would be developed, Hutchison turned to the main issues of his study: What was the actual status of most economic propositions? And what, correspondingly, was the status of economics as an empirical science? Although Hutchison's discussion of these issues spans nearly two hundred closely reasoned pages, leading to inevitable distortions in any compact summary, it is necessary to attempt some characterization of his response for purposes of our methodological survey.

Hutchison's central argument rests upon the ambiguity present in many arguments which seem empirical but are easily interpreted as tautologies. As he remarked, " 'All swans are white' might have been defined as an inductive generalisation, conceivably falsifiable, or as a definition that creatures that were not white were not to be called 'swans'."¹²⁹

It was Hutchison's general contention that the overwhelming bulk of all economic arguments were either formal or interpreted tautologies, that although there was no inherent difficulty in formulating an empirical economics (so long as the hypotheses of the study were formed with due regard for "what statistics may or may not be

available"),¹³⁰ in fact, very few empirical economic relationships had been either formulated or tested. Conjoined with his assessment of the present state of economic inquiry was his distinction between the pursuits of the scientist and those of the philosopher:

The reason why scientists, unlike philosophers, can build on and advance their predecessors' work rather than each being simply "influenced" by it and starting afresh right from the beginning at the same problems with some completely new system, is that "scientists" have definite, agreed, and relatively conclusive criteria for the testing of propositions according to definite criteria which is the source of that steady secular piecemeal agreement and advance of "science," and its cumulative, international, impersonal and "coral-reef-like" growth.¹³¹

It was thus apparent from Hutchison's discussion that, in his estimate, economics had seen little real progress in its empirical aspects since the days of Adam Smith.¹³² The necessity for continually justifying an enterprise which claimed empirical import but rejected empirical methods had, in Hutchison's view, led economists to prefer some degree of obscurity in their concepts, at least in those concepts dealing with the "postulates" or "foundations" of their analytic system:¹³³

... it (is not) the case that the foundations of economic science have been found necessarily to be precarious, but rather that it is not at all clear precisely what they are. To this lack of fundamental clarity can be attributed to [sic] a certain extent the ferocious and interminable character of the many controversies that rage among economists themselves on the one hand, and on the other hand much of the uncertainty as to the significance of their results with which economists face the outside world.¹³⁴

The three main props of Orthodox methodology, which had allowed for the persistence and superficial plausibility of speculative

economics, were the notions of a mental experiment,¹³⁵ the introduction of unspecified ceteris paribus into every situation in which the theory was in need of saving,¹³⁶ and the idea of introspective examination of "motives" as a legitimate form of economic research.¹³⁷ Since Hutchison's comments concerning each of these topics are substantially in accord with the views expressed throughout this dissertation, they do not require a detailed examination. It might be said in summary, however, that Hutchison totally rejected the notion of mental experiments as replacements for well-structured empirical experiments,¹³⁸ did not believe that the notion of ceteris paribus was unredeemable though he did consider it to have been perpetually abused in most economic discussions, and rejected introspection as an intersubjective scientific procedure while reserving comment on the quite separate issue of the existence of motives and "consciousness."¹³⁹

Concluding Evaluation of Hutchison's Early Work

In many of the aspects we have considered, then, Hutchison's main work is a classic of modern economic methodology. Many contemporary economic theorists could still benefit from its wise counsel on methodological matters. There are several respects, however, in which the volume was open to justifiable criticism, although perhaps not to the extent which it in fact received. Its discussions of theories of monopoly and competition, welfare economics and the economics of a socialist economy were less than enlightened, and too frequently were positively in error. Despite rather copious footnotes, Hutchison might also be faulted for an insufficient acknowledgement of his debt to

both positivist and non-positivist Austrian philosophers. Many of his passages were virtually lifted from these writers with little notice to the reader of their original source.¹⁴⁰ Finally, one might suppose that a great deal of the vituperous, and in the main quite unjust, condemnation which met the work was inspired not solely by its content but also by its "tone." To some extent the conditions under which Hutchison composed his volume account for its overly blunt (one would not want to say "overly simplistic") and somewhat "hurried" style. As Hutchison himself mentioned in his reply to Knight, the work was in part intended as a response to and a defense against "the color-shirted champions of some mass persecution creed (by whom I was surrounded when I wrote my book)." Historical or psychological justifications aside, however, it might have been preferable, both from Hutchison's personal point of view and from the perspective of the future advancement of the discipline, if Hutchison had been more concerned about explaining his position and less concerned about its advocacy.

Milton Friedman and the Reform of Orthodoxy

Despite the numerous and vicious attacks directed toward Hutchison's methodology,¹⁴¹ it was apparent to those who closely followed the controversy that his views had gained the upper hand. Orthodoxy of the old style seemed (temporarily) moribund, although it would not be long before new revitalized forms of the old doctrines would again make their ascent.

In 1953 Milton Friedman penned an essay on "The Methodology of

Positive Economics"¹⁴² in which he formulated a position which has since become, for many orthodox micro-theorists, little less than a faith. Friedman's methodological article is so well known in the profession that it seems almost pedantic to once again review its contents. Yet such a review reveals that there is considerably more to his methodological position than is commonly believed (i.e., much more than his "as if" doctrine and his demand for theories which yield testable consequences).

The orientation of Friedman's essay is detectable in his opening line where he refers to John Neville Keynes' "admirable book" and to Keynes' tripartite distinction between positive sciences, normative sciences and arts.¹⁴³ After equating a "positive science" to any well-structured Wertfrei study of cause and effect, Friedman moves to consider the meaning of "scientific objectivity," concluding (in my opinion mistakenly)¹⁴⁴ that it must be connected with the attitudes of the scientific observer toward the phenomena he is observing. In the same sentence, he somewhat contradictorily refers to the social scientist's possession of "a class of data (i.e., introspection) not available to the physical scientist" and seems to assert that this special class of knowledge places him both in a superior and less "objective" position than the inquirer into physical phenomena. These issues have previously been noted in connection with the writings of Cairnes, Keynes and Knight and do not require reexamination. The only thing remarkable in Friedman's references to them is that he considered (and still considers)¹⁴⁵ them as so transparently obvious as to be unworthy of explicit defense.

Instrumental Economics

One of the fundamental principles of Friedman's methodological views is discoverable in his classification of scientific hypotheses and his characterization of their role in scientific inquiry:

The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed. Such a theory is, in general, a complex intermixture of two elements. In part it is a "language" designed to promote "systematic and organized methods of reasoning." In part, it is a body of substantive hypotheses designed to abstract essential features of complex reality.

Viewed as a language, theory has no substantive content; it is a set of tautologies. Its function is to serve as a filing system for organizing empirical material and facilitating our understanding of it ...

Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain."¹⁴⁶

Just as Robinson's Essay had its roots in the conception that economics was meant to be purely explanatory, with prediction being possible only in a general and not strictly dependable sense, so Friedman's "Methodology of Positive Economics" was based upon the notion that economics should be purely predictive, that it could really only "explain" in terms of the increased predictive scope of "better" theories. Friedman best exemplifies the principle of "explanation through testability" by discussing the preferability of a theory which predicts the movement and placement of leaves on a

tree through "survival traits" rather than by reference to the deliberate action of the leaves based on some process of "intentional" thought. He clearly states that the only reason we would select the former theory over the latter is that "This alternative hypothesis ... is part of a more general theory that applies to a wider variety of phenomena ... has more implications capable of being contradicted, and has failed to be contradicted under a wider variety of circumstances."¹⁴⁷ That its " 'assumptions' are more 'realistic' " is, in Friedman's view, irrelevant.

Although it is difficult to fault Friedman's position concerning the correct interpretation for "explanation" in an empirical science, he extends this principle (in the altered formulation of "a theory which explains more contradicts more") to what Samuelson subsequently labeled the "F-Twist"¹⁴⁸ (i.e., the idea that "To be important, therefore, an hypothesis must be descriptively false").¹⁴⁹

The extremity of Friedman's F-Twist position was in response to the methodological doctrine of common or casual refutation of a theory's "assumptions," which he believed to be too common among economic theorists of his time. Probably his main goal in "The Methodology of Positive Economics" was to undermine the notions that "... hypotheses have not only 'implications' but also 'assumptions' and that the conformity of these assumptions to 'reality' is a test of the validity of the hypothesis different from or additional to the test by implications"¹⁵⁰ (emphasis in original). Friedman believed, with undoubtedly some justification, that arguments of the naive refutationist sort had been used to justify empirical laziness

in economic research and to circumvent the often difficult procedures required in the testing of a theory's "implications":

"The difficulty in the social sciences of getting new evidence ... (concerning the empirical implications of hypotheses) ... makes it tempting to suppose that other, more readily available evidence is equally relevant to the hypothesis."¹⁵¹ And it must indeed be admitted, in retrospect, that he was quite correct in his criticism. "Tests" of the "assumptions" of economic theories based upon casual observation were in vogue during the 'Fifties, and numerous books and articles had appeared and continued to appear which criticized the basic assumptions of micro-analysis (profit and utility maximization and the foundations of perfect competition theory) on the basis of their "realism."¹⁵² Such criticisms were seldom justified by reference to well-defined studies of market-structure, however. Their goal was not to "clarify" or tie micro-analysis to observable parameters (e.g., the institutional framework of a market), but, rather, to replace it with a new body of untested speculations concerning what was believed to be "significant." Although the wave of "new" theories was thus little advancement over the Orthodox system (and may have even been regressive due to the underdeveloped character of many of the proffered "alternatives"), Friedman's own reaction to the controversy was equally excessive and equally flawed. His underlying efforts to solve certain difficulties in the application of micro-theory went unheeded,¹⁵³ while his more glamorous methodological discussions were universally acknowledged. By arguing against the casual empiricism of the neo-Marshallians from his own neo-Orthodox

perspective, Friedman only muddled the debates of his time and seriously befuddled the "conventional wisdom" concerning methodological issues. The grounds for this judgment of Friedman's position are present in his own article. By considering why he believed that hypotheses were "untestable by their assumptions" and why he maintained that theories were preferable if they were "descriptively false," we will arrive at the roots of his methodological perspective and at the confusions upon which it was based.

Friedman justified his views concerning the untestability of a theory's "assumptions" and the preferable falsity of these assumptions by reference to three considerations. First, he considered how the applicability of a theory could be determined, or, put differently, how it could be determined that one situation (that which was believed to exist in the world) was "significantly" different from another (that specified in the "assumptions" of the theory). Friedman's response to this issue was fully in accord with his exclusive devotion to "predictability" as the sole criterion for assessing the correctness of an hypothesis. A theory, for Friedman, could be considered as correct, or as substantially correct, if its predictions were not "significantly" different from observations of the world.¹⁵⁴ Friedman was willing to soften this initial stance somewhat by conceding that "experiments" or "tests" of economic theories, in the sense of controlled experiments, were seldom possible¹⁵⁵ and by adding that crucial tests (i.e., those capable of strictly ruling out one of a pair of conflicting, but not fully contradictory, hypotheses) were virtually never achieved.¹⁵⁶ Friedman was further willing to admit

those cases where the degree of significant deviation between a prediction and an observation of the predicted event was determined after the fact, i.e., cases where the acceptable accuracy of the prediction was not specified until after the occurrence of the event predicted.¹⁵⁷ (While this procedure is permissible in a scientific investigation, and in some cases is actually preferable to the prior specification of a "level of significance," it is a procedure which is, in itself, not definitive. Even though "something is better than nothing," there may be some hypotheses which yield predictions which are less accurate than a random guess within some preestablished range of alternatives or values. A strict adherence to Friedman's proposal would not, however, rule out hypotheses yielding even "very high" levels of error in their predictions. In the extreme of a "pure case," therefore, the procedure of identifying permissible error after the fact seems highly questionable.)¹⁵⁸

Further, Friedman's basic contention, that testing is the only way to determine the similarity of situations (and thus the "applicability" of an hypothesis) is in itself deficient. Such a practice leaves unsettled, for instance, the question of how to decide whether a future and past situation are themselves "sufficiently similar" to warrant confidence in the application of the same hypothesis to both. In attempting to evade criticisms of this type, Friedman stated that the specification of the scope of an hypothesis' applicability "is not one thing and the hypothesis another. The specification is itself an essential part of the hypothesis ..."¹⁵⁹ Yet this identification of an hypothesis and its "empirical specifications," coupled

with Friedman's recognition that "(the specifications of applicability) are a part (of the hypothesis) particularly likely to be revised and extended as experience accumulates," blatantly contradicts his separation of "assumptions" and the situation in the world. What are the specifications of applicability if not those very "assumptions" to which Friedman continually refers? It is apparent from his own discussion that he must reject one or the other part of his argument. If the specification of test conditions is "an essential part of the theory," then theories must be modified with regard to conditions to which they are applied. If, however, we distinguish between the "pure theory" and its "empirical interpretation," theories may be invulnerable, but the "application specifications" of the theories become something separate from the theories themselves. Friedman seemed willing to accept this separation at one point in his article,¹⁶⁰ but, as we have seen, he later contradicted himself.

A second way in which Friedman sought to justify the alleged untestability of a theory's assumptions and the preferable falsity of these assumptions was by appeal to a two-pronged Darwinian argument, aimed first at the world of market competition and secondly at the enterprise of economic science. Given the assumption of natural selection, Friedman believed that it was possible to deduce that "acceptance of the hypothesis (that firms engage in behavior consistent with a maximization of "returns") can be based largely on the judgment that it summarizes appropriately the conditions for survival."¹⁶¹ He also maintained that theories which had survived the

longest had the greatest claim to "worth":

... the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect evidence to its worth. The evidence for a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its nature is difficult to document at all comprehensively. It tends to become part of the tradition and folklore of a science revealed in the tenacity with which hypotheses are held rather than in any text-book list of instances in which the hypothesis has failed to be contradicted.¹⁶² (emphasis added)

It is apparent, however, that both of these contentions rest upon unstated, and, in many instances, questionable presumptions about conditions in the marketplace and the academy.

The identification of those firms which maximize returns with those which survive is only true if the firms referred to are operating in perfectly competitive markets or if we strip the term "maximize returns" of any definite empirical content. Firms operating under the threat of anti-trust action, for instance, may not act to maximize returns, in any classic sense, nor may regulated firms want to maximize their identifiable profits. Friedman, however, felt free to assume away such disparate cases under the authority of his "as if" principle and did his best to muddy the analytic import of his contentions by proposing an empirically empty (i.e., tautologous) interpretation of "returns."¹⁶³ The force of his remarks is to defeat his own goal of constructing a testable body of (predictive) economic theories. If we are allowed to resort to ad hoc redefinition

of our terms whenever it seems convenient, then we will arrive at an economics which "explains" everything but can exclude nothing.

The portion of Friedman's natural selection argument which rests upon the survival of a theory within the framework of a professionalized discipline depends upon conditions which he himself rejected. The persistent acceptance of a particular "point of view" (or body of theories) within a discipline is an indication of the "fitness" of this perspective in passing empirical tests only if (1) the "point of view" is well-defined and empirically specified according to some standardized set of rules and (2) it is "really" intended as a device for predicting events in the world rather than as an "interpreted tautology" (i.e., an extension of the definition system). While such theories certainly play a prominent role in many sciences, they depend for their empirical status upon well-defined rules for testing and rejecting an hypothesis. Since there is little to indicate that economics has developed such standards, and much evidence that it is still enmeshed in pre-scientific speculations of a primarily ideological or game-like character, the faith which Friedman puts in the traditional acceptance of certain hypotheses within the discipline seems misplaced. The standards for the continual acceptance of hypotheses in a setting such as economics has traditionally found itself have little to do with the hypotheses' ability to accurately predict events in the world and much to do with their fruitfulness in spawning interesting "puzzles" or their abilities to mystify the uninitiated.

It need hardly be remarked, in any case, that the continual

acceptance of a theory, even within well-constituted empirical sciences, is insufficient to add to its logical "validity."¹⁶⁴ There have been many theories (e.g., Ptolemaic astronomy) which were accepted for generations or even centuries only to meet with eventual rejection. Perhaps more relevant to the continued acceptance or rejection of an hypothesis is Friedman's suggestion that even a "bad" hypothesis may be retained if there is no "better" hypothesis (in terms of accuracy and/or predictive scope) to be adopted in its stead.¹⁶⁵

The final and weakest of Friedman's defenses for "unrealistic assumptions" was that it was frequently impossible to distinguish in a definitive manner between "assumptions" and "implications" of economic theories. He supported this contention by two types of considerations. First, he maintained that the "implications" of one theory are often employed as "assumptions" of another theory;¹⁶⁶ and secondly that "assumptions can (sometimes) themselves be regarded as implications of the hypothesis, and hence their conformity with reality as a failure of some implications to be contradicted ..."¹⁶⁷

It is obvious, however, that the intent of these assertions does not at all correspond with their effect; they in fact constitute either irrelevancies or they directly contradict Friedman's main thesis. The idea that the "implications" of one hypothesis can serve as "assumptions" for others is, for instance, transparently true. It would be difficult to understand Friedman's enthusiasm for this thesis, however, if it were not for the fact that he apparently believed he had both supported his own position and allowed for the "indirect"

testing of "assumptions" by means of this diversion. Obviously, however, he had accomplished neither of these goals and had only displayed the weakness of his own arguments. The possibility of overlapping "assumptions" and "conclusions" is without any interesting implications unless we assume that the different theories in which these "assumptions" and "conclusions" appear are part of an overarching theory of economic behavior in general (Friedman's "the hypothesis"?). If the various hypotheses are not part of such a structure, then it is presumptuous to assert the "implications" of one theory are "really" "the same" as the "assumptions" of another theory, for the same term will usually have different meanings in different theoretical systems. We have, in any case, determined nothing about the "validity" of the "assumptions" of the first theory upon which the "validity" of its "implications," and thus the "validity" of the assumptions of the second theory, must necessarily depend. Friedman's fundamental position is, apparently, that if we push the foundations of our theories far enough back, we will eventually arrive at obvious or intuitive truths. But that was, of course, the very doctrine which he set out to refute.

The second of Friedman's arguments for "indirect testing"--that the "assumptions" of a theory could also be interpreted as "implications," and were testable as such--is (in one sense, at least) quite true. This rather simplistic observation, however, directly undermines Friedman's belief in the empirical irrelevance of "assumptions." If one of the possible implications of an hypothesis is its own assumptions, then the hypothesis is falsified if these assumptions are

false (but not, of course, if they are merely "incomplete").

In summary then, it seems fair to assess Friedman's methodological position as an attempt to evade certain basic characteristics of any system of inference; namely, the impossibility of deducing particular statements (predictions) about states of the world from universal-conditional statements (laws) without the use of other particulars (the empirical test conditions or "assumptions" of the theory), and the fact that the truth-value of premises can only be preserved, not improved upon, via inferential reasoning. Friedman's view of the irrelevance of assumptions leads to a "science" which is no more than a series of unconnected speculations about particular cases. If an "accurate" prediction is arrived at by using an hypothesis conjoined with a statement of existing conditions which is "inaccurate," then we cannot assert that the hypothesis has been tested. Put differently, this situation is indistinguishable from the case where a "prediction" is "guessed at" and later proves true. That is, the "test" of the hypothesis does not provide a usable rule for formulating further predictions or ruling out possible future alternatives.

Friedman was, of course, correct in objecting to the informal methods used to "refute" the theories of early Neoclassical micro-theory (i.e., the belief that it was meaningful to assert that industry A was "obviously" non-competitive or that firms "obviously" did not seek to maximize their returns). Yet his own ill-defined discussion of "significant difference" was but one step removed from appeals to the "obvious." In order to really know whether a theory

of perfect competition is "applicable" to a particular industry, we must first have defined a standardized measure or "scale" of "competitiveness" and a way of weighting the differences in "competitiveness" which may characterize the various "segments" of the industry. It may then be possible to discuss the question of whether an industry is "significantly" non-competitive.

The Post-Friedmanian Period

Although Friedman's essay on "The Methodology of Positive Economics" was widely regarded as an authoritative source during the decade after its publication, it was far from the only such source, and its claim to authority was frequently questioned. In this section, we turn to some of the complements to and criticisms of Friedman's work, as found in the writings of Fritz Machlup, Eugene Rotwein, Ernst Nagel and Paul Samuelson. Although these were far from the only contributors to the methodological controversies of the post-Friedmanian era, the constraints of space have resulted in the consignment of other contributions to a later work.

Fritz Machlup and the Resurrection of Orthodoxy

The first of the papers to attempt an extensive review of economic methodology after Friedman's own essay was Fritz Machlup's "The Problem of Verification in Economics" (1955).¹⁶⁸ Machlup prefaced his presentation of methodological opinions by an erudite and well-documented discussion of various issues concerned with the character and testing of economic hypotheses. He then turned to his primary

concern, that of distinguishing and discussing the "Ultra-Empiricist" and "Extreme A Priorist" traditions in economic methodology. Although his discussion is, as a whole, highly suggestive and meticulously referenced with respect to both the economic and philosophic literature, only a few of Machlup's concerns are of importance within our present study. It is notable, first of all, that Machlup apparently classed Friedman with the a priorists,¹⁶⁹ a designation which he himself was willing to adopt in a somewhat modified form. Secondly, he drew a distinction in his methodological view between lower and higher level "assumptions," maintaining that the former were subject to varying degrees of testing while the latter were not directly testable.¹⁷⁰ Although willing to admit to the logical possibility that higher-order "assumptions" might be falsified, Machlup stressed the high costs involved in such a falsification and the (sociological) requirement that they be replaced by alternative "assumptions":

Economists who are still suspicious of non-verifiable assumptions, and worry about the legitimacy of using them, may be reassured by this admission: The fact that fundamental assumptions are not directly testable and cannot be refuted by empirical investigation does not mean that they are beyond the pale of the so-called "principle of permanent control," that is, beyond possible challenge, modification or rejection. These assumptions may well be rejected, but only together with the theoretical system of which they are a part, and only when a more satisfactory system is put in its place; in Conant's words, "a theory is only overthrown by a better theory, never merely by contradictory facts."¹⁷¹

While there is nothing indefensible in these views, Machlup's characterization of the "Ultra-Empiricist" alternative was obviously

nothing more than a straw-man, set up for an attack from his own perspective. The character of his own perspective is apparent when he discusses the role of explanation in a science,¹⁷² when he stops barely short of endorsing synthetic a priori propositions as the base for all social inquiry,¹⁷³ and when he concludes by stating that "thus the fundamental assumptions of economic theory are not subject to a requirement of independent empirical verification, but instead to a requirement of understandability in the sense in which man can understand the actions of fellowmen."¹⁷⁴

Machlup's subsequent consideration of the problems involved in "disconfirming" probabilistic hypotheses and of testing those in which "it is not possible to check the fulfillment of all the conditions specified"¹⁷⁵ led him yet further into a conventionalist view of social inquiry. His concluding position seemed to be reducible to the view that social science was no more than a body of widely accepted explanations for social events wherein tests are "of the character of illustrations (rather) than of verifications ..."¹⁷⁶ In support of this position, Machlup had what he believed to be excellent evidence. He relied most heavily upon (1) the inability of social scientists (or economists) to test each of their assumptions separately (meaning by this that hypotheses of social science contain "high-level" constructs not testable except in conjunction with auxiliary hypotheses)¹⁷⁷ and (2) the inaccessibility of the methods of controlled experimentation in economics, these methods being required in order to isolate the effects of one factor from other factors.¹⁷⁸ (In order to appease econometricians and other

advocates of empirical investigations, Machlup was, however, willing to distinguish "a large body of economics apart from its theoretical or "hypothetico-deductive" system, namely, "the empirical relationships obtained through correlation of observations, but not derivable, or at least not yet derived, from higher-level generalizations."¹⁷⁹⁾

The similarities between Machlup's and Marshall's methodological views suffer only from Marshall's own hesitancy to clearly detail his methodological position. Every identifiable element in Marshall's meta-economics, excepting only his evolutionary views, was reproduced in Machlup's writings. The emphasis upon explanation and "understanding" (in the Weberian sense), the willingness to accept casual observation as the "test" of "fundamental assumptions,"¹⁸⁰ the rejection of experimentation and of empirical studies aimed at critical testing, the grotesque characterization of the "ultra-empiricist" alternative, and, finally, the dichotomy between "historical" and "theoretical" inquiries (with the accompanying assumption that both are important--in their separate spheres) were major elements of both men's methodologies. If Friedman may be said to have muddied the waters of methodological dialogue by cloaking his endorsement of intuitive certainty in the rhetoric of critical testing, Machlup's influence was to once again set Orthodoxy in its proper framework. Leslie's comment concerning Cairnes, that "he said all that was possible in defense of the Orthodox position and demonstrated everything which could conceivably be 'gotten out' of it," is equally applicable to Machlup.

Rotwein's Positivism

One of the most detailed and significant critiques of Friedman from an "empiricist" perspective is contained in Eugene Rotwein's "On 'The Methodology of Positive Economics'."¹⁸¹ Rotwein's method for examining Friedman's paper involved a step-by-step consideration of each of the points which Friedman had raised in his discussion, for, as he stated:

Friedman develops much of his argument through a series of analogies. For this reason it is difficult, while considering his analysis in its own context, to proceed as directly to a general treatment of his position as one would like for purposes of criticism.¹⁸²

Rotwein's mode for organizing his own discussion, however, makes any summary of his own views quite combersome. We are, therefore, constrained to a consideration of only a select sample of his more original points.

Friedman had illustrated his endorsement of those hypotheses with "unrealistic" or "false" assumptions by reference to the example of gravitational laws which were formulated for a situation of a "pure vacuum" but which remained "valid" for far different situations (viz., for those where there was a considerable amount of atmospheric pressure). In part, Rotwein agreed with Friedman's discussion and assented to his reasoning concerning this case. The probability of an hypothesis being "successful" was, in Rotwein's view, determinable only on the basis of numerous previous tests of the hypothesis (viz., Rotwein believed in a "frequency view" of probability).¹⁸³

Rotwein was further willing to agree that the "realism" of an hypothesis' assumptions rested on the basis of the empirical "significance" of deviations from these assumptions (viz., that "realistic assumptions" depended on the degree to which the accuracy of the theory suffered when it was applied to different cases).¹⁸⁴ Rotwein believed, however, that Friedman's own position concerning methodological matters extended far beyond either of these fundamental points.

Rotwein's own interpretation of Friedman's vacuum example was that it constituted both an argumentum ab ignoratum and a confusion of what is "conceivable" with what can ("reasonably") be expected. On the one hand Rotwein maintained that Friedman had confused the conceivable limits of an hypothesis' applicability, before any tests of the hypothesis, with the limits to be expected on the basis of the results of numerous tests.¹⁸⁵ On the other hand, while endorsing Friedman's rejection of intuition as proper for determining the significance of "air pressure" or of any other "assumption" of a theory before tests had been performed, Rotwein noted that an admission of ignorance regarding the initial significance of an assumption did not imply (as Friedman obviously believed it did) that an "assumption" was necessarily insignificant. If we don't (yet) know, then we don't know, and nothing further follows from our ignorance.¹⁸⁶

The consequence of Friedman's attempt to deduce insignificance from ignorance was, as Rotwein pointed out, that "there is then no reason for ever observing any relations between matters of fact."¹⁸⁷ In Rotwein's own terms, if "conceivability" is identical to

"probability" (viz., if it is tantamount to "reasonable expectation"), we are at liberty to maintain that our "assumptions" and their consequences are always to be considered "as if" they were "true," regardless of observations. That Friedman would have never been willing to assent to this formulation of his views and was, of course, very concerned with observation, is only one indication of his imperfect perception of the full implications of his own illustrations.¹⁸⁸

One final contradiction which Rotwein discerned in Friedman's discussion of the vacuum example arose from Friedman's own emphasis upon the importance of precisely specifying those conditions to which an hypothesis was ultimately found to be applicable. As Rotwein noted, regarding Friedman's point, "This amounts to an abandonment of his own position. The only reason we would ... be interested in specifying these (real) circumstances is because we wish to use them to tell us when, in the future, we may expect satisfactory conformity with the theory."¹⁸⁹ Although Rotwein's criticism overlooked Friedman's distinction between "specifying" and "determining" the conditions to which theories apply, we have already demonstrated that that distinction can only be used against the view of "verbal realism."¹⁹⁰

In a number of other instances Rotwein's criticisms of "The Methodology of Positive Economics" were both perceptive and devastating. In examining the "extreme case" where a theory's predictions were perfectly in accord with observations, but its empirical specifications were not, he, for instance, noted that it was unwise to

"accept" the theory as a proper tool of scientific inquiry. Theories are, after all, intended to provide rules for the prediction of future events, yet the predictions of a theory which has been previously tested only upon the basis of "false assumptions" are "like the messages from a crystal ball [since] it (the hypothesis) would (in varying degrees) leave us with 'chance' or 'unreal factors' instead of 'explaining' the behavior of firms."¹⁹¹

Rotwein also found fault with Friedman for "arguing that unless the critics of orthodox economics produce a better theory, their criticisms are in some sense impertinent."¹⁹² While one might disagree with Rotwein's reasoning concerning this point and support Friedman's position if the "theory" being scrutinized were the Neoclassical "paradigm" as a whole, an opposite stance would become appropriate if "criticisms" were being directed against the traditional methodology connected with this paradigm or against its non-specific character. The distinction between "paradigms," their parts and the details of their methodology is, in fact, central to the issue of whether or not we would want to "judge 'an hypothesis' (strictly) on its own intrinsic merits."¹⁹³ It has been recognized by Popper, for instance, that in order for a science to exhibit identifiable growth, it must (1) start somewhere, (2) be willing to accept and even actively encourage criticism of its starting position, but (3) demand that theories not be abandoned unless they can be replaced by other theories with greater predictive scope and/or accuracy. The tightrope strung between dogmatism and nihilism is often difficult to balance upon, but science cannot progress by becoming a faith or by

giving itself up to the void.

Concerning other issues, Rotwein criticised Friedman for confusing "assumptions" which were "descriptively false" with those which were descriptively incomplete. Although he held that it was desirable to have theories which require very few conditions for their application, for they would then possess the widest possible "scope," this desire for "abstractness" in no way justified empirical specifications which conflicted with the situations to which the theory was to be applied. Incompleteness, in other words, is distinct from contradictoriness.¹⁹⁴

In constructing a sketch of Friedman's views in their most general form and proffering his own alternative to these views, Rotwein endorsed a position similar to that later expressed by Ernst Nagel (but which he himself attributed to Norman Campbell).¹⁹⁵ In Rotwein's view, a science (any science) is composed of a hierarchy of lower-level hypotheses ("Law Hypotheses") and higher-level hypotheses ("Theory Hypotheses"). Theory hypotheses are formulated at a "much higher level of generality" than law hypotheses and can be used, in connection with certain empirical tie-rules, to deduce the pre-existing law hypotheses and new law hypotheses.¹⁹⁶ While the "entities" referred to in the antecedent clause of a law hypothesis are always observable, those referred to in the antecedent clause of a theory hypothesis are not observable, at least "at the time the Hypothesis is framed." (emphasis added)¹⁹⁷ The latter qualification connotes an important distinction between Rotwein's and Nagel's philosophy of science. For Rotwein all "theoretic terms" refer to

entities which are "potentially capable of being 'seen' (to mention one literal [sic] mode of detection)." ¹⁹⁸ Until these entities are "seen," however, "a scientific Theory ... is in an important sense the equivalent of 'religion'." ¹⁹⁹ The differences between the pursuits of science and the pursuits of religion are also important in Rotwein's view, of course. These he connects with the scientist's continued striving to empirically isolate the "presumed entity" and his refusal to accept any "assumption" which cannot, in principle, be decided (viz., "falsified") by observation procedures. That the theoretic entities of Theory Hypotheses are not immediately observable forms the critical distinction between these Hypotheses and Law Hypotheses, ²⁰⁰ but it is a distinction which, for any given Hypothesis, should be ephemeral.

Other Developments in Economic Methodology

The 1963 annual meetings of the American Economic Association hosted one of the most recent professional discussions of economic methodology. In addition to a delightful paper by Andres Papandreou, intended mainly as a summary and extension of his 1958 Economics as A Science, ²⁰¹ and an enlightened but overly terse comment by G. C. Archibald, ²⁰² the session heard Paul Samuelson's comments concerning Friedman's "F-Twist" (considered below) and Nagel's defense and correction of Friedman entitled "Assumptions in Economic Theory." ²⁰³ Although a consideration of Papandreou and Archibald would disclose truly solid advances in economic methodology, we will restrict our attention in the body of this chapter to Nagel's and Samuelson's more

popular contributions.

Nagel on Friedman

Nagel's evaluation of Friedman's "Methodology of Positive Economics" rested upon his own beliefs concerning the structure of scientific investigations and the character of scientific theories. He divided the component statements "in a given codification of a theory" into "three sub-groups": the "basic hypotheses," theorems deducible from these hypotheses and statements of the correspondence between "theoretical terms" used in the hypotheses and the "observable traits of things."²⁰⁴ The concept of "theoretical terms," its role in hypothesis formulation and its interpretation, was central to Nagel's description of the scientific enterprise: "Theoretical terms signify either various entities that cannot be specified except by way of some theory which postulates their existence, or certain ideal limits of theoretically endless processes ..."²⁰⁵ Although Nagel recognized that "a sufficient number of theoretical terms (but not necessarily all)" should be tied to the observable characteristics of things,²⁰⁶ he clearly believed that such terms were expressive of more than the observations to which they were "coordinated":

It must be emphasized, however, that these statements (of the correspondence between theoretical terms and observations) do not define theoretical terms by way of terms signifying observable traits, so that theoretical terms cannot be eliminated from formulations in which they occur ...²⁰⁷

It was by use of this concept of a "theoretic term" that Nagel was able to defend a highly restricted form of Friedman's "as if"

doctrine.

Nagel's position concerning the "Methodology of Positive Economics" was a carefully constructed combination of cautious criticism and cautious accord. While he fully agreed with Friedman's observations concerning the difficulties involved in testing "assumptions" (or fundamental hypotheses) of a theory against "directly perceived descriptive inaccuracies," Nagel also noted Friedman's somewhat contradictory stress upon the difficulty of separating "assumptions" and "implications" into general categories (i.e., the difficulty of distinguishing the components of these categories apart from some given formulation of the hypothesis in which they were included). He also dissented sharply from Friedman's position concerning the acceptable falsity of "fundamental statements," and he held that if "assumptions" were interpreted to refer to the "antecedent clause of some theoretical statement," the falsity of the theory's "assumptions" was equivalent to the admission of its inapplicability for that particular "domain."²⁰⁸

However, Nagel's own perspective did allow for the existence and central importance of certain statements "of dependence between phenomena" which were formulated as: pure cases, ideal types, theoretical statements (or laws) "which formulate relations to hold under highly 'purified' conditions between highly 'idealized' objects or processes ..."²⁰⁹ These "objects or processes" were of a peculiar type such that "none (of them) ... is actually encountered in experience ...,"²¹⁰ and the "theoretical statements" were also somewhat extra-scientific, "For they are not distinguished by their failure to

provide exhaustive descriptions nor are they literally false of anything; their distinguishing mark is the fact that when they are strictly construed, they are applicable to nothing actual."²¹¹

The mainline of Nagel's defense and criticisms of Friedman thus rested upon his own case for the use of "theoretical statements" and "theoretical terms" in empirical science. In so far as Friedman had recognized the importance of certain irreducible concepts in providing a foundation upon which a theoretical structure could be constructed, he was due both praise and commendation. To the extent, however, that he insisted upon the direct or indirect reduction of all terms to observable conditions, he was either "ambiguous" or contradictory.²¹² Nagel's concern was with "cleaning up" certain logical difficulties in Friedman's view. He was definitely not interested in refuting Friedman's defense of Orthodox methodology.

Samuelson's Instrumental Economics

The proportion of Samuelson's writings concerned with questions of economic methodology is so slight as to be easily overlooked. Yet his views are certainly of some importance considering his position of leadership within the profession and his well-deserved reputation for analytic clarity.

Of the three major sources of Samuelson's methodological opinions, the best organized, the earliest and the most nearly complete is his Foundations of Economic Analysis (1948).²¹³ In the Foundations, Samuelson assumed a methodological position similar to, but even more "extreme" than, Hutchison's. On the first page of his discussion, for

instance, is found an appraisal of the state of economic science, and an appraisal of the causes for this condition, which is emphatically less than a complement to the Orthodox tradition:

... only the smallest fraction of economic writings, theoretical and applied, has been concerned with the derivation of operationally meaningful theorems. In part at least this has been the result of the bad methodological preconceptions that economic laws deduced from a priori assumptions possessed rigor and validity independently of any empirical human behavior.

... Literally hundreds of learned papers have been written on the subject of utility. Take a little bad psychology, add a dash of bad philosophy and ethics, and liberal quantities of bad logic, and any economist can prove that the demand curve for a commodity is negatively inclined.²¹⁴

The claim that economic theorems would "one day" become operational if only we were patient enough, a claim which dated back to the neo-Ricardians and which would later be revived in the writings of Klappholz and Agassi,²¹⁵ was treated by Samuelson with equal severity:

The economist has consoled himself for his barren results with the thought that he was forging tools which would eventually yield fruit. The promise is always in the future; we are like highly trained athletes who never run a race, and in consequence grow stale. It is still too early to determine whether the innovations in thought of the last decade will have stemmed the unmistakable signs of decadence which were clearly present in economic thought prior to 1930.²¹⁶

Although some of Samuelson's attempts to operationalize economic terms were based upon a faulty characterization of professional usage

(e.g., his discussion of "returns to scale"),²¹⁷ his research in the Foundations was, on the whole, a well-merited effort to eliminate "intuitively defined" or introspectively based economic concepts (e.g., his discussion of "utility" and "preference").²¹⁸

It is notable that nearly five years before the publication of Friedman's essay on "The Methodology of Positive Economics," Samuelson had already stated the case against his doctrine of untestable assumptions:

It is clear that every assumption either places restrictions upon our empirical data or is meaningless. A price must be paid for any simplifications introduced into our basic hypotheses. This price is the limiting of the field of applicability and relevance of the theory because of the extra empirical restrictions to be imposed on the data. Many writers do not appear to be aware of this; in any case few have indicated the costliness of their assumptions or have adduced any evidence to support a presumption of their admissibility.²¹⁹

In a strict sense, of course, this passage itself is untrue (for a definition is "an assumption" of sorts but does not, in itself, "place restrictions upon our empirical data" and is not, in any ordinary sense, "meaningless"). Generosity and a due regard for Samuelson's other statements, however, dictates that the word "meaningless" should be interpreted with a modifier such as "operationally" or "empirically," that Samuelson was attempting to establish a "demarkation principle" between empirical science and non-science rather than a criterion for distinguishing sense from nonsense.

It is somewhat ironic in the light of the above considerations that the same work which inspired a flurry of mathematical model-

building in Twentieth Century economics would be liable to the charge of an excessive Logical Positivism. There is certainly nothing explicit in the Foundations which would lend support to a deepening of the gulf between the "pure" theorist and the "applied" economist. Yet it seems equally certain that it had for many years that very effect.

In part, the anomalous effects of the Foundations were mirrored in Samuelson's own methodological evolution. In his contribution to the 1963 A. E. A. session on "Problems of Methodology,"²²⁰ Samuelson expressed a growing distaste for methodological questions, commenting that:

When Maxwell's Demon rank orders scientific disciplines by their "fruitfulness" and by their propensity to engage in methodological discussion, he finds a negative correlation and a strong inverse relationship. It is if a science could lift itself by its own bootstraps: by maintaining a superlative silence on method ... Like many "as if" statements this is nonsense. It is more correct, albeit not very informative, to say that soft sciences spend time in talking about method because Satan finds tasks for idle hands to do. Nature does abhor a vacuum and hot air fills up more space than cold. When libertines lose the power to shock us, they take up moral pontification to bore us.²²¹

He then, of course, proceeded to demonstrate his abilities as a former libertine.

Although Samuelson expressed approval for Friedman's reduction of scientific "explanation" to the predictive accuracy and predictive scope of the theories which dealt with the phenomena to be explained, and although he complimented Friedman for refusing to engage in speculations concerning ultimate realities, Samuelson was quite

critical of what "was special and distinctive in Friedman's methodology," what he termed the "F-Twist."²²² The "F-Twist," in Samuelson's eyes, was not restricted to Friedman's belief in the preferability of empirically false assumptions (as opposed to assumptions which were merely incomplete empirical specifications). Rather, Samuelson chose to attack the much weaker claim that "the unrealism of the theory 'itself' or of its 'assumptions' is quite irrelevant to its validity or worth."²²³

In carrying through his criticism of the "F-Twist," Samuelson devised a lengthy set of complex, not to say obscure, arguments. Of these, but three require our attention. First, Samuelson seemed to want to argue that it was strange to speak of assumptions, hypotheses and consequences as three distinct sets of statements. The "assumptions" and "hypotheses" taken together do, after all, imply the conclusions; and implication is simply a process of making explicit what is already contained in the premises. Secondly, the same set of "conclusions" are not derivable from the original hypotheses conjoined with a different but "similar" set of "assumptions." Nor is the observation of "a tolerable degree of approximation" to the predicted state of things acceptable, unless the prediction was probabilistic and/or a range of "tolerable" error had previously been specified. Finally, "assumptions" will always be capable of implying themselves and, thus, must be "realistic" (viz., non-falsified) so long as the theory with which they are conjoined is intended to apply to existing conditions.²²⁴ It is unclear to what extent Samuelson's criticisms were based upon an ambiguous interpretation of economic

theories as simultaneously probabilistic and deterministic (viz., as "interpreted" and "uninterpreted"), but it does seem that he located several ambiguities, if not outright contradictions, in Friedman's paper.

Samuelson's concluding comments regarding Friedman were perhaps his most decisive. He maintained, in contradiction to Nagel, that a discussion of "ideal types" was only useful in the realm of the psychologically-suggestive, and he concurred with Nagel in asserting that models which contained demonstrable "empirical falsities" must be "jettisoned."²²⁵

In what is perhaps his most recent article to deal with methodological considerations, a semi-popular speech delivered to "The Twelfth Annual Conference on the Economic Outlook,"²²⁶ Samuelson's views seem to have moved much closer to the Orthodox paradigm. It is possible, however, that because of the setting and purpose of the speech, he was simply less concerned with meticulously constructed and well-substantiated claims.

Almost at the outset of his paper, Samuelson drew a sharp distinction between the predictions yielded by a properly constituted economic (or econometric) theory and the procedures to be recommended in arriving at predictions about future values of macro-economic variables. His opinions regarding the judgment of economic (or econometric) theories remained largely unchanged--their operational meaningfulness was still to be determined by their capacity for testing. The relevant distinction between the predictions of such theories and "good predictions" about the economy lay, however, in

the fact that "Nature has simply not performed the controlled experiments that enable us to predict as we should wish."²²⁷ Although willing to concede that "the men who develop topnotch judgment have an analytical framework within which they try to fit the facts" and that such individuals keep a constant watch upon "the evidence of economic time series; the evidence of cross-sectional data; [and] the evidence of case studies,"²²⁸ Samuelson appeared to be willing to accord equal weight to "anecdotes," conjectures about psychological role-playing and "all the hunches and experiences" which the "master economist" has "ever had."²²⁹

In all fairness to Samuelson we should note that he, in fact, never demanded that a theory need be more than "conceivably falsifiable" in order to be "operationally meaningful." In his reply to Donald Gordon's "Professor Samuelson on Operationalism in Economic Theory,"²³⁰ for instance, he quite explicitly stated that the mark of a "meaningful" statement was that "it is conceptually an empirically refutable proposition" (emphasis added).²³¹ He then proceeded to clarify what was his purpose in the Foundations (and elsewhere?):

Gordon's criticism seems to me to be concerned not so much with the sufficiency of my reasoning but rather with the quite different problem of how we go about deciding that a conceptually meaningful proposition is or is not a fruitful hypothesis and whether as a result of any particular observation we are to decide that it has or has not been refuted ...

I do not think that I discussed this issue anywhere in Foundations, and in retrospect I feel little urge to have done so. To test, refute, or "verify" a meaningful proposition is a tough empirical job and no amount of flossy deduction can obviate this ...²³²

Despite his brave declaration of devotion to an "operationally

meaningful" theory, it is apparent that Samuelson never, in fact, was willing to engage in the "tough empirical job" of demonstrating how his theory was actually operational.²³³ Although he would eventually publish many studies which were nominally "empirical," the task of assisting in the establishment of professional standards for tying economic concepts to well-defined observations procedures was beyond his interests. While there will always be room for the "pure theorist" in any science, it is always preferable that such specialists clarify their intentions at the outset of their studies, rather than referring to their research as something which it is not.

More Recent Methodological Writings

The previous discussion of recent contributions to economic methodology is far from exhaustive. Certain recent works have been excluded simply because they fall outside the rather narrowly defined scope of this study²³⁴ while others, often of great importance, were excluded, not because of any lack of relevance, but because of their rather "technical" character.²³⁵ Included in this latter group are considerations of the formal (logical) structure of an economic theory or considerations of specialized issues involved in the application of economic theories to certain classes of phenomena. These latter types of studies (dealing either with the formal character of economic theories or with their application) are themselves very encouraging, for they demonstrate a willingness to build upon the notion of economics as an empirical science. Yet they are, in one sense at least, too optimistic. We have already seen, for instance,

that many economists are simply unwilling to assent to the conception of a refutable body of economic theories. And given such attitudes the construction of extensions (or clarifications) of empirical methodologies seems to be both a thankless and premature task.

Some Miscellaneous Methodological Topics

Synthetic A Priori Statements

The concept of a statement which is necessarily true (i.e., which is a tautology) but which is also informative about classes of non-linguistic events or "causal sequences" in the world (i.e., which is "synthetic" or "empirical") was first explicitly considered in an organized fashion by Immanuel Kant in his Critique of Pure Reason and Prolegomena to All Future Metaphysics. It was Kant's belief that the axioms of Euclidean geometry were, in fact, synthetic a priori's, that is, that they were true of the world but were knowable as true apart from or prior to any experiences of the world. Kant also believed that it would ultimately be possible to ground each of the existing sciences upon some set of such synthetic a priori axioms.²³⁶ This Kantian idea has long met with a favorable response in economics, beginning, in a restricted form, in the writings of William Whewell and culminating, in a much less refined and cautious formulation, in the Orthodox methodologies of Cairnes, Keynes, Knight and Machlup.

The issues surrounding the status and use of synthetic a priori statements are as numerous and varied as the divisions of philosophy itself. There have been attempts to organize virtually every branch

of human inquiry in terms of such statements, but none of the purportedly "self-evident" axioms proposed for these projects have ever proved very useful in organizing the observations of empirical studies.

The issue of a synthetic a priori base for empirical science has recently been replaced by inquiries concerned with the scope and the extent of the methodological conventions binding given sciences (viz., by inquiries into the extent to which the terms of a science are "theory-laden"). This newer approach is, undoubtedly, a more productive perspective on the problems of "unfalsifiable assumptions," for if one is willing to accept a strict distinction between analytic and contingent statements then the key question must become "How far do we want to interpret the terms of our discussion as purely analytic?" Although a single answer cannot be formulated for all possible types of empirical investigations or even for all the types of investigations carried on within a given science, it is possible to say something in general about this question.

If a theory is to be empirical (viz., if it is to assert something as true of the world which is not entirely dependent upon the rules for linguistic usage already established within a given study), it must: (1) rule out certain types of non-linguistic events; (2) be stated in such a way that its conditionality (i.e., its conditional truth or falsity) is dependent upon certain observation statements; and (3) be "applied" or tested according to standardized application criteria and standardized test methods (that is, the observation statements against which the theory is tested must be derived in some

standardized and repeatable fashion).

"Theories" which do not meet the first criterion are purely "analytic" and can, if true, tell us only about linguistic usage. While it is permissible for a theory to "assume" conditions which are at present unobservable (although "potentially" observable), it must also be the case that (1) these "assumed" conditions are either inconsequential (and could thus be eliminated from the theory without effecting its predictive consequences), or (2) that they are really a disguised part of the theory's definition system or the methodological rules for the discipline, or (3) that they are founded in "psychological assumptions" rather than being explicitly defined, or (4) that the theory, given our present tools and/or circumstances, cannot be fully tested, i.e., that a "test" of the theory is always conditional upon circumstances which we in fact know nothing about.

Finally, a failure to formulate and proceed according to standardized "application criteria" or "test methods," accompanied by failures to explicitly specify the initial test situations and the tools, measures and procedures for testing, will inevitably lead to unrepeatable, and, thus, to largely futile "test" results. Issues of this type are of the same character as Hutchison's concern with "unspecified ceteris paribus assumptions." As the example of modern empirical economics has shown, it is as easy to become uncritical about our "tests" by ignoring the details of the test methods and the test situation as it is to become "tautological" in our testing by reliance upon an unlimited, and therefore unspecified, set of possible "structural disturbances." Although a complete specification of the test situation

and data requirements for any experiment is just as impossible as a totally unambiguous technical language, the example of other sciences, even some of the other social sciences, seems to indicate that economics is unnecessarily backward in these areas.

"Mental Testing" and Casual Empiricism

Of the two strains in early Orthodox thought, that founded by J. S. Mill and that founded by J. E. Cairnes, one has always tended toward the "empiricism" of the British psychological tradition (i.e., associationism) and the other toward a modified form of the Kantian view of science as a system founded upon synthetic a priori propositions. Within the former tradition the dominant means of "testing" hypotheses has been not to deduce them from "first principles" but to rely upon common sense and common experience, interpreted by a body of "experts." The justifications for these procedures are, however, neither so naive nor so commonsensical as they might at first appear.

The British associationist-empirical tradition was constructed upon Hume's doctrine of the association, through repetition, of sequences of sense impressions. Although "causality" was thus a matter of "mentally" imposed relations, and induction, in the Baconian sense of "a movement from the particular to the general (or the universal)," was non-logical and dependent upon "habit," we could, in Knight's apt expression, "not help ourselves." That is, according to those in the orthodox tradition founded by Mill, human beings as such must believe in certain causal connections if exposed to the same stimuli because of the mechanical processes of their minds.

On this same basis, then, it would seem reasonable to assert that "experts" in a given field of study, viz., those "most familiar" with the phenomena itself and/or with descriptions of it, are in a privileged position to judge hypotheses concerned with their field. "Testing," according to this view, is thus possible, and even preferable, by reference to the associations built up in the minds of "experts"; and the way to become an "expert" is to have a "wide experience" (Marshall's phrase) of the phenomena.

The "operational significance" of this Millian view is, of course, no different than the significance of neo-Kantianism. In both cases we must end up by relying upon "tests" which are non-intersubjective. The Millian characterization of science is, if anything, even more pernicious than the Kantian, for by virtue of it we do not even have an identifiable set of first principles open for inspection. For the Kantian philosopher a man who would not assent to the a priori character of a science's "first principles" was either "mad" or did not understand the language being spoken. For the Millian "scientist," however, the dissenter had proved his own ineligibility for entry into the class of "experts."

Unspecified Ceteris Paribus Assumptions

One of the favorite charges of British Historical writers, later resurrected by Hutchison, was that Orthodox economists justified their empirical "predictions" by reference to a seemingly endless list of "implicit" or unspecified ceteris paribus assumptions. This procedure was, of course, devastating to the possibilities of any real empirical

testing of economic theories since in any uncontrolled situation there would inevitably be several things, changes in which "conceivably" could affect a theory's predictions. There neither was nor is, however, any way to curb such ad hoc reinterpretation of a theory's predictions under uncontrolled conditions. As both Kuhn and Popper have argued, there is no critical science possible if scientists refuse to be critical of their own theories and methods. It is only when controlled testing of economic theories becomes more frequent and more standardized that we will see some curb to Bagehot's social theorist who can "always argue that he was right."

Concluding Remarks

In the opening paragraphs of this dissertation we decried the neglect of methodological issues by historians of economic thought. In this chapter we have seen the fruits of that neglect, best summarized by the oft-abused aphorism, "Those who are ignorant of history are doomed to repeat it." Knight, Machlup and, to a lesser extent, Friedman are symptomatic of the failure of economic historians to root out the methodological errors of the past. It would be mistaken to say that their views are indistinguishable from those of Mill, Cairnes or Marshall, but they are certainly much closer to that camp of writers than to the more modern perspectives of Rotwein or Hutchison.

A totally pessimistic interpretation of the development of economic methodology over the past hundred years is no more justified, however, than a purely idyllic view. There are significant differences in the

views of Friedman and those of Cairnes, even though many of these differences are restricted to their stated goals or apparent "intentions" rather than extending to the ultimate consequences of their views. The visible differences between the writings of modern methodologists and those of Nineteenth Century Orthodox economists revolve around the justifications which are offered in support of their respective positions. Although many economists may still believe that economic theories are best "tested" via "expert intuition," most realize that it is no longer acceptable to say so in public. Yet the gradual elimination of a priorism in economic research has occurred despite the dominance of explicit a priorists in methodological circles. Given the structure of the economics discipline and its role as the handmaiden for political policy-making, it is perhaps not surprising that the works of authors such as the British Historical School and of such later empiricists as Rotwein and Hutchison would have less of an impact on explicit methodological positions than the positions of writers who promised immediate "relevance."

Despite political pressures for instant solutions and instant science, however, there has been substantial progress within the last fifty years in the transformation of economics from social mythology to social science. What used to be considered as the exile of the unworthy--the "applied" fields such as agricultural economics or the economics of regulation--have rightly gained new respect as the harbingers of a truly empirical economic science. Perhaps one day economic methodologists will awake to find that their "intuitively obvious" study has been replaced by a body of well-tested empirical theories,

and historians of economics will awake to find an entirely new set of authors who "anticipated" this new development.

Footnotes to Chapter IX

1. John Neville Keynes, The Scope and Method of Political Economy, 1st edition, 1890; 4th edition, 1917 (New York: Macmillan, 1930). The 1930 reprint of the 4th edition is used as the source for the following citations and is hereafter referred to as Scope and Method.

2. Ibid., pp. vi, 9-10.
3. Ibid., p. 6.
4. Ibid., p. v.
5. Ibid., pp. 10, 29-30.
6. Ibid., p. 34.
7. Ibid., pp. 34-35.
8. Ibid., p. 35fn.
9. See, for instance, Keynes' comments in Scope and Method, p. 56.
10. Ibid., p. 44.
11. Ibid., pp. 65, 310-311.
12. Ibid., pp. 48, 118. It is of some interest that Keynes quoted Bacon's Novum Organum in support of his contention that science should be the "pure study" of causes and effects. The passage from Bacon's work, however, contains the same addendum regarding the "practical impact" of "the pure study of cause and effect relationships" as was used by both Keynes and Cairnes in support of their position: "... Axioms rightly discovered (i.e., through an inductive process) and established supply practical uses not scantily but in crowds; and draw after themselves bands and troops of effects." (Quoted in Scope and Method, p. 49fn.)
13. Ibid., pp. 144, 311.
14. Ibid., p. 57.
15. Ibid., p. 58.
16. Ibid., p. 74.
17. Ibid., p. 74.
18. Ibid., p. 30.

19. Ibid., pp. 9-10.
20. For examples of Keynes' excessive absorption with the views of the German Historical School to the virtual exclusion of the British Historical views, see Ibid., pp. 20, 22fn.
21. Ibid., p. 268.
22. Ibid., p. 277.
23. Ibid., p. 278.
24. Keynes discussed at some length the possibility of "establishing" or "supporting" economic hypotheses by reference to "history" or the "facts of common experience," but he considered all attempts to refute economic hypotheses as a confusion resting upon the incomplete separation of "theorems" (i.e., deductions from the "postulates" or first principles of economic reasoning) from "precepts" (i.e., rules of conduct derived in part from economic and in part from social or ethical considerations).
25. Scope and Method, op. cit., pp. 283-284.
26. Ibid.
27. Ibid., pp. 321, 322.
28. Ibid., p. 122.
29. Keynes' attempts to narrowly circumscribe the scope of "the economic" are well illustrated in the following passage:
- Utility may be defined as the power of satisfying, directly or indirectly, human needs and desires; and the possession of utility is the one characteristic that all writers are agreed in ascribing to wealth. It seems clear, however, that we cannot from our present standpoint identify wealth with all sources of utility whatsoever, since there are many means of satisfying human needs, such as family affection, the esteem of acquaintances, a good conscience, a cultured taste, which have never been included within the scope of political economy, and the laws of whose production and distribution have hardly anything in common with the laws that are as a matter of fact discussed by economists.
- Ibid., p. 94.
30. On pp. 130-131 of his Scope and Method, Keynes offers a

lengthy list of those things which "might" act as disturbing causes, but are seldom explicitly mentioned in the usual analysis of an economic condition. He, of course, notes that there may be other such influences which he himself has neglected to include in the list.

31. Ibid., p. 318fn.

32. J. A. La Nauze, Political Economy in Australia (London: Cambridge University Press, 1949), pp. 10, 11.

33. Frank Knight, Risk, Uncertainty and Profit (New York: Harper and Row "Torchbook Series," 1965; first published in 1921 by Hart, Schaffner and Marx). All references are to the Harper and Row reprint which is hereafter referred to as Risk, Uncertainty and Profit.

34. See page of Chapter VIII (on Marshall) of this dissertation.

35. Risk, Uncertainty and Profit, op. cit., p. 3.

36. Ibid., pp. 3-4.

37. Ibid., p. 4.

38. Ibid.

39. Ibid., p. 5.

40. Ibid., p. 7fn.

41. Ibid., p. 201fn.

42. Ibid., pp. 7-8fn.

43. Ibid., p. 200.

44. Ibid., pp. 8fn, 201.

45. See, for instance, Knight's remarks in Ibid., p. 201.

46. Ibid., p. 203.

47. Ibid., p. 200. For a summary sketch of Knight's epistemology, see Ibid., pp. 207-208.

48. Ibid., p. 212fn.

49. Frank Knight, "Ethics and the Economic Interpretation," Quarterly Journal of Economics, Vol. 34 (1922), pp. 454-481, reprinted in Frank Knight, The Ethics of Competition and Other Essays (New York: Harper and Brothers, 1935), pp. 19-40. All citations are to the

reprint which is referred to as Ethics.

50. Ethics, op. cit., p. 23.

51. Ibid., p. 26fn.

52. Ibid., pp. 22, 26.

53. The passage quoted in the text of this chapter is from Ethics, p. 33.

54. After asserting that individuals "neither know what they want--to say nothing of what is 'good' for them--nor act very intelligently to secure the things which they have decided to try to get," Knight continued, in a footnote, to explain that "From this point of view again the animals are superior to man, in that they are more intelligent, sensible; a hog knows what is good for him and does it." (Ibid., p. 35fn.)

55. Ibid., p. 36.

56. Frank Knight, "Economic Psychology and the Value Problem," Quarterly Journal of Economics, Vol. 39 (1925), pp. 372-409, reprinted in Ethics, op. cit., pp. 76-104. All references are to the reprint.

57. Ethics, op. cit., p. 84.

58. Knight's distinction between the two types of desire is clarified in the following passage from his "Economic Psychology and the Value Problem":

In saying that conduct is always controlled by the strongest desire, we may mean either of two things, either desire as the analogue of force in physics, or desire as felt ...

Scientific economics must keep sharply separate and distinct the two conceptions of desire, namely, the desire which explains observed behavior and is the analogue of force in mechanics, and desire as known by the individual human being in himself directly, and in other individuals through language and social discourse.

Ibid., p. 85.

59. Ibid., p. 86.

60. Ibid., p. 96. The recent controversy in the philosophy of science over the issue of paradigms or "ways of seeing the world" is

critically examined by Israel Scheffler in his "Discussion--Vision and Revolution: A Postscript on Kuhn," Philosophy of Science, Vol. 39 (1972), pp. 366-374.

61. Ethics, op. cit., p. 96.

62. Ibid.

63. Ibid., p. 122.

64. One of the most characteristic and revealing of Knight's methodological essays during the 'Twenties is his "The Limitations of Scientific Method in Economics" contained in Rexford G. Tugwell's The Trend of Economics (New York: F. S. Crofts and Company, 1924), reprinted in Ethics, op. cit., pp. 105-147. Special attention is due pp. 129-130 of this essay and the following passages:

There are no laws regarding the content of economic behavior, but there are laws universally valid as to its form. There is an abstract rationale of all conduct which is rational at all, and a rationale of all social relations arising through the organization of rational activity. We cannot tell what particular goods any person will desire, but we can be sure that within limits he will prefer more of any good to less, and that there will be limits beyond which the opposite will be true. We do not know what specific things will be wealth at any given time and place, but we know quite well what must be the attitude of any sane individual toward wealth wherever a social situation exists which gives the concept meaning. (emphasis in original)

Ethics, op. cit., p. 136.

The way in which economic laws are arrived at and their "necessary" character is also discussed in the following:

It is not necessary to regard the general, a priori laws of mathematics or economics or such mechanical principles as inertia as being "intuitively" known in any inscrutable way. They may all be more advantageously treated as mere facts of observation, characteristics of the world we live in, but characteristics so obvious that it is impossible to escape recognizing them and so fundamental that to think them away would necessitate creating in the imagination a different type of universe.

The "necessary" character of axioms is undoubtedly due, not to their being created or given to experience by the mind, but rather on the contrary to the fact that the mind has not the creative power to imagine a world fundamentally different from that in which we actually live.

Ibid., p. 136.

65. Some examples of Knight's continued interest in methodological questions are to be found in the following: Frank Knight, "Economics at its Best," American Economic Review, Vol. 14 (1926), pp. 51-58; -----, "Review: David Atkins, The Measurement of Economic Value," Journal of Political Economy, Vol. 35 (1927), pp. 552-557; -----, "Statik und Dynamik," Zeitschrift fur Nationaloekonomie, Vol. 2 (1930), pp. 1-26; -----, "Relation of Utility Theory to Economic Method," in Stuart Rice (ed.), Methods in the Social Sciences (Chicago: University of Chicago Press, 1931), pp. 59-69.

66. Frank Knight, "Social Economic Organization," in The Economic Organization (New York: Augustus Kelley, 1951; originally published in 1933), pp. 3-30.

67. Frank Knight, "Review: Lionel Robbins, The Nature and Significance of Economic Science," International Journal of Ethics, Vol. 44 (April, 1934), p. 359.

68. Ibid., p. 361.

69. For a most enlightening discussion of Knight's political views, see Don Patinkin, "Frank Knight as Teacher," American Economic Review, Vol. 63 (1973), pp. 798-808.

70. Knight's attitudes toward the grounds for "social control" and the role of social science in this process are clearly delineated in the following:

I doubt whether "scientific" method will ever be very important to the ordinary man or the scientist as a means for understanding or controlling his own behavior ...

Surely the study of social phenomena must be to a considerable extent identical with and dependent upon self-knowledge; and social control will be meaningless or monstrous if not associated with individual self-control, and unless the main ingredient is mutual understanding and regulation of action through discussion, which is a very different thing from observation and inference. It is to be noted,

too, that social "science" itself is a social phenomenon of the most typical and important sort, communication aimed at agreement. And it seems doubtful whether the term "science" would be used without warning qualifications, to characterize discourse which must explain the discourse itself, and which is addressed to its own subject matter, and must change, and is primarily intended to change, that subject-matter.

Relevant discussion of social phenomena, within a moral society itself, must strive to minimize the scientific-instrumentalist attitude of investigation of techniques of manipulation of an external subject-matter, ... and must aim at the mutual establishment of a consensus in the making of rules.

Frank Knight, "Economic Science in Recent Discussions," American Economic Review, Vol. 24 (June, 1934), pp. 236-237.

71. Frank Knight, " 'What is Truth' in Economics," Journal of Political Economy, Vol. 48 (1940), pp. 1-32. Knight's opening observations on the worth of methodological inquiries are most revealing:

It seems that a great many thoughtful people in the world are like Pontius Pilate in that they ask the question of our title, but "do not wait for an answer." But a considerable number differ from him in the interesting respect that instead of asking others the question they volunteer to give the answer themselves, to others, and to the world, without waiting to be asked. This leads to the writing of books of varying character and size which one suspects are more interesting on the average to their authors than they are to any considerable number of readers. And to the many of those who do read them this may be a comforting thought, since it means that books on methodology probably do not do much damage. The chief reservation would be that they are most likely to be read and taken seriously by the young.

Ibid., p. 1.

72. Ibid., p. 3.

73. Ibid.

74. Ibid., pp. 5-6.

75. Ibid., p. 6.

76. Ibid., pp. 8-9, 10-11.

77. Ibid., p. 20fn.

78. Ibid.

79. Frank Knight, "The Role of Principles in Economics and Politics," American Economic Review, Vol. 41 (March, 1951), pp. 1-29. See p. 8 for this citation.

80. Ibid., p. 11.

81. Ibid., p. 12.

82. Lionel Robbins, An Essay on the Nature and Significance of Economic Science (London: Macmillan, 1st edition, 1932; 2nd edition, 1935). All references are to the second edition which differs from the first in the addition of several explanatory passages and notes and in the softening of an accusatory tone which pervaded the criticisms of the first edition. The second edition of Robbin's Essay has long been more available and has certainly been more influential than the first edition.

83. T. W. Hutchison, The Significance and Basic Postulates of Economic Theory (New York: Augustus Kelley, 1960; first published in 1938). The 1960 printing is identical to the 1938 printing except for the addition of a lengthy and informative preface.

84. See Robbin's explicit declaration of this goal in his Essay, op. cit., p. xv.

85. Ibid., p. xiv.

86. Ibid., p. xvi.

87. Ibid., p. 1.

88. For an extensive argument against the separation of economic actions and other human actions, see Ibid., pp. 7-12.

89. The distinction between the "condition of scarcity" or the "condition of something being scarce" and scarcity as "a relationship between wants and supply" is seldom explicitly considered in more modern discussions. The former conception does seem to possess the connotations of a social or non-subjective standard of scarcity, and that was unquestionably the reason that Robbins found it objectionable. However, it should be noted, for balance, that the

latter conception is somewhat more difficult to "tie" operationally. For Robbins' discussion of these two senses of "scarcity," see his Essay, op. cit., pp. 12-13.

90. Ibid., p. 15.

91. Ibid., pp. 2-5fn.

92. Ibid., p. 24.

93. Ibid., p. 30.

94. Ibid., p. 38.

95. Ibid., pp. 38-39.

96. The following passage seems somewhat odd in the light of Robbins' expressed desire not to restrict "the economic" to that which involved monetary considerations:

It is clear that the influence of the German Historical School was responsible for the intrusion of all sorts of sociological and ethical elements which cannot, by the widest extension of the meaning of words, be described as Economic History. (emphasis in original)

Ibid., p. 40fn.

It almost seems that by objecting to the introduction of "sociological elements" in economic history that Robbins has regressed to the definition of economics by the character of its subject matter (i.e., the "economic" as opposed to "the social," "the political," etc.). This conclusion may be an exaggeration, however, for but a few paragraphs later we discover that one of Robbins' main concerns was with the confusion of economic history and historical materialism.

97. Ibid., p. 42fn.

98. Ibid., pp. 47-48.

99. Ibid., pp. 50-52.

100. Ibid., p. 53.

101. Ibid., p. 56fn.

102. Ibid., p. 58. See also pp. 75-76 for a more direct discussion of welfare economics.

103. Ibid., pp. 63-66, 68.

104. Ibid., p. 66.

105. Ibid., p. 103.

106. The passage in Robbins' Essay appears on p. 133. This was the precise flaw which Popper later identified as the Achille's Heel of historicism. See the Preface to the later editions of his Poverty of Historicism.

107. The question of the costs of decision-making (in the sense of the time and effort required to examine all available alternatives) and the costs of formulating a complete transitive ordering of alternatives in accord with preference-indifference relationships are discussed by Robbins in his Essay, op. cit., p. 92.

108. The Jevonian concept of a "minimum sensible" is considered in Robbins' Essay, op. cit., p. 99.

109. Robbins' comments in endorsement of an analysis of the legal framework and of "the subject-matter of political science" in terms of constrained maximization (scarcity) can be found on p. 134 of his Essay.

110. Ibid., p. 74.

111. Ibid., p. 79. When discussing the quantity theory of money and prices, Robbins states that "This proposition is deducible from the most elementary facts of experience of the science, and its truth is independent of further inductive test." (Ibid., p. 117)

112. Ibid., p. 74.

113. Ibid., pp. 82-83, 114-116.

114. Ibid., pp. 87-88. In what was almost a direct quote from Knight, Robbins stated that:

It is really not possible to understand the concepts of choice, of the relationships of means and ends, the central concepts of our science, in terms of observation of external data. The conception of purposive [sic] conduct in this sense does not necessarily involve any ultimate indeterminism. But it does involve links in the chain of causal explanation which are psychical, not physical, and which are, for that reason, not necessarily susceptible of observation by behaviourist methods.

Ibid., p. 90.

115. Ibid., pp. 88-89.

116. Robbins' debt to von Mises and Wicksteed is acknowledged in An Essay, op. cit., p. xvi. For a representative sample of Mises' "economic epistemology," see his Human Action, Revised Edition (New Haven: Yale University Press, 1963), pp. 1-11, 30-72. For a translation of the earlier epistemological articles cited by Robbins, see Ludwig von Mises, Epistemological Problems of Economics (Princeton: D. Van Nostrand, 1960).

117. Robbins characterized the "postulates" of economic theory as follows:

It is characteristic of scientific generalizations [sic] that they refer to reality ...

In this respect ... the propositions of Economics are on all fours with the propositions of all other sciences. As we have seen, these propositions are deductions from simple assumptions reflecting very elementary facts of general experience ...

It may be admitted that our knowledge of the facts which are the basis of economic deductions is different in important respects from our knowledge of the facts which are the basis of the deductions of the natural sciences ... But it does not follow in the least that its generalisations have a "merely formal" status ... it may be urged, on the contrary (that) there is less reason to doubt their real bearing than that of the natural sciences. In Economics we have seen, the ultimate constituents of our fundamental generalisations are known to us by immediate acquaintance.

An Essay, op. cit., pp. 104-105.

118. Ibid., p. 110.

119. Ibid., p. 111-112.

120. Ibid., p. 107.

121. T. W. Hutchison, The Significance and Basic Postulates of Economic Theory (New York: Augustus P. Kelley, 1960), p. 3 (hereafter cited as The Significance).

122. Ibid., p. 34.

123. Ibid., p. 9.

124. Ibid.

125. For Hutchisons' references to the German edition of Popper's Logic of Scientific Discovery, see Ibid., pp. 48-49 n, 126 n.

126. Ibid., pp. 26-27.

127. Ibid., pp. 26-27 n.

128. Ibid., p. 12.

129. Ibid., p. 26.

130. Ibid., p. 31.

131. Ibid., p. 7.

132. To assert that the empirical methods of economics had not changed significantly from 1776 to the 1930's does not, of course, deny the significant improvements in its definition system.

133. If an economic theory is axiomatized, it will necessarily include some "primitive terms" used in the definition of other terms but themselves undefined, e.g., "wants" or "desires." The existence of "primitive terms" does not necessarily lead to "a lack of clarity," however. Hutchison's claim is better interpreted to mean that economists were themselves unclear about the different (distinct) senses of the terms they used, and that they often "reasoned" psychologically.

134. The Significance, op. cit., p. 4.

135. Ibid., pp. 36-40.

136. Ibid., pp. 40-46.

137. Ibid., pp. 131-143.

138. Hutchison did not, however, rule out mental experiments as a "psychologically useful" way for an investigator to imagine and describe to himself the working of some particular model community representing an extreme case. He quickly added, however, that "this cannot be anything more than a preliminary thought-clearing exercise ... it would be fantastic to suggest that one could thus achieve the concrete results obtainable from laboratory experiments for which this procedure constitutes a (supposed) replacement." (Ibid., p. 38.)

139. Ibid., p. 143.

140. Although Hutchison had been remiss in acknowledging the extent of his indebtedness to German philosophers and scientists in the original edition of his volume, he attempted to atone for this offense in the Preface added to the 1960 reprinting of this volume.

In this Preface he was especially solicitous of Popper's reputation and achievements.

141. In addition to Knight's "'What Is Truth" In Economics," supra, Hutchison's essay was also criticized in Machlup's "The Problem of Verification In Economics," Southern Economic Journal, Vol. 22 (July, 1955), in Klappholz and Agassi's "Methodological Prescriptions In Economics," Economica, N. S., Vol. 26 (February, 1959), pp. 63-65, and in A. W. Stonier's "Review," Economic Journal, 49, pp. 114-5.

142. Milton Friedman, Essays In Positive Economics (Chicago: University of Chicago Press, 1953), pp. 3-43. (Hereafter cited as Positive Economics.)

143. Ibid., p. 3.

144. Ibid., p. 4.

145. Ibid., pp. 4-5; for a recent restatement of Friedman's belief in introspective techniques, see "An Interview With Milton Friedman," Reason, Vol. 6 (December, 1974), p. 8.

146. Positive Economics, op. cit., pp. 7-8.

147. Ibid., p. 20.

148. American Economic Review Proceedings, Vol. 53 (May, 1963), pp. 233-236.

149. Positive Economics, op. cit., pp. 14-15.

150. Ibid., p. 14.

151. Ibid.

152. Ibid., pp. 21, 21 fn, 25, 30-31.

153. It is, of course, true that economic inquiry is becoming superficially more concerned with empirical tests of theories. It is somewhat unclear, however, whether Friedman's essay aided this effort or, to a great extent, helped to subvert it.

154. Positive Economics, op. cit., pp. 18-19.

155. Ibid., p. 10.

156. Ibid., pp. 10-11.

157. Ibid., p. 18.

158. It is interesting that this way of looking at hypotheses

does provide some grounds for rejecting an hypothesis even though no other explicit alternative is available to replace it.

159. Positive Economics, op. cit., p. 18.
160. Ibid., pp. 24-25.
161. Ibid., p. 22.
162. Ibid., p. 23.
163. Ibid., pp. 21-22, 21fn.
164. Ibid., pp. 22-23.
165. Ibid., p. 31.
166. Ibid., pp. 26-27.
167. Ibid., p. 28.
168. Fritz Machlup, "The Problem of Verification in Economics," Southern Economic Journal, Vol. 22 (July, 1955), pp. 1-21.
169. Ibid., p. 17.
170. Ibid., pp. 8-9.
171. Ibid., p. 11.
172. Ibid., pp. 9-10, 17.
173. See Fritz Machlup's "A Rejoinder to a Reluctant Ultra-Empiricist," Southern Economic Journal, Vol. 22 (1956), p. 486 for another instance of Machlup's appeal to an "intermediate category of propositions, which are neither a priori or a posteriori in the strict sense ..."
174. Machlup, "The Problem of Verification ...," op. cit., p. 17.
175. Ibid., p. 19.
176. Ibid.
177. Ibid., p. 9.
178. Ibid., pp. 18-19.
179. Ibid., p. 19.
180. Ibid., p. 18.

181. Eugene Rotwein, "On 'The Methodology of Positive Economics'," Quarterly Journal of Economics, Vol. 73 (November, 1959), pp. 554-575.

182. Ibid., p. 556.

183. For a concise discussion of several different senses of "probability" ("metalinguistic," "propensity theory" and "frequency theory"), see J. J. C. Smart's Between Science and Philosophy (New York: Random House, 1968), pp. 40-51. Rotwein's comments related to this discussion appear in his paper (supra, pp. 558fn.).

184. Rotwein, op. cit., pp. 559, 561.

185. Ibid., pp. 558-560.

186. According to Rotwein's interpretation, then, Friedman's argument constituted a curious reversal of more traditional forms of "linguistic realism."

187. Rotwein, op. cit., p. 560.

188. Ibid., pp. 560-561.

189. Ibid., pp. 559-560.

190. The distinction between "specifying" and "determining" the conditions to which an hypothesis applies is discussed by Friedman in his "Positive Economics," op. cit., p. 19. This distinction does tend to undermine the traditional forms of linguistic or verbal realism by clarifying the fact that we need not have complex or comprehensive models in order to predict certain types of events.

191. Rotwein, op. cit., pp. 562, 563-564.

192. Ibid., p. 562fn.

193. Ibid. This is simply to say that theories, especially well-developed and well-elaborated theories, should not, as a rule, be given up simply because they are neither perfectly accurate or as comprehensive in scope as we might ideally desire. "Criticisms" of established theories are, of course, never "impertinent" so long as they aid in the refinement of the theories or present well-developed alternatives to them. Purely "destructive" criticisms may, however, be "impertinent."

194. Rotwein, op. cit., p. 565.

195. Ibid., p. 569fn.

196. Ibid., pp. 572-573.

197. Ibid., p. 571.

198. Ibid.

199. Ibid., p. 573.

200. Ibid.

201. Andres G. Papandreou, "Theory Construction and Empirical Meaning in Economics," American Economic Review (Proceedings), Vol. 53 (May, 1963), pp. 209-221. See also, Andres G. Papandreou, Economics As A Science (Chicago: J. B. Lippincott, 1958). Although Papandreou's work is presented in a form which I am not competent to judge (i.e., mainly in topology and other forms of advanced set theory), his English commentary does indicate a profound understanding of and appreciation for the most subtle distinctions in economic methodology.

202. G. C. Archibald, "Comment on Papandreou," American Economic Review (Proceedings), Vol. 53 (May, 1963), pp. 227-229. In his comment Archibald was mainly concerned with clarifying the notions of a "generic anchorage" and/or a "space-time anchorage" for economic theories. In his discussion concerning the steps in arriving at such a clarification, however, he considered many of the questions (e.g., problems in the identification of an instance of a generically specified economic concept, the meaning of "tolerable" error in predictions and the relationship between "explanation" and "prediction") which we have discovered to be of central importance in modern methodological controversies. For an earlier statement of Archibald's concern with the vagueness of many of the terms bandied about in these discussions, see his article in the May, 1959 issue of the British Journal for the Philosophy of Science.

203. Nagel's article became rather popular after its first appearance and was frequently reprinted in readings books concerned with microeconomic topics. Our citations to the article are to the readily available reprint appearing in William Breit and Harold Hochman's Readings in Microeconomics (New York: Holt, Rinehart & Winston, 1968), pp. 60-66.

204. Nagel, op. cit., pp. 61-62.

205. Ibid., p. 61.

206. Ibid., p. 62.

207. Ibid., p. 62.

208. Ibid., p. 63.

209. Ibid., pp. 63-64.

210. Ibid., p. 64.
211. Ibid.
212. Ibid., p. 66.
213. Paul A. Samuelson, Foundation of Economic Analysis (Cambridge: Harvard University Press, 1947).
214. Ibid., pp. 3-4.
215. K. Klappholz and J. Agassi, "Methodological Prescriptions In Economics," Economica N. S., Vol 26 (1959), pp. 60-74 argue that many modern methodological writings are to be faulted for their impatience, that by demanding too much from the new science of economics too soon they actually retard the progress of the discipline. This same theme was reiterated in Klappholz and Agassi's "Rejoinder to Hutchison," Economica N. S., Vol. 27 (1950), pp. 160-161.
216. Samuelson, Foundations, op. cit., p. 4.
217. Ibid., p. 84.
218. Ibid., pp. 91-92.
219. Ibid., p. 172.
220. Paul A. Samuelson, "Discussion," American Economic Review (Proceedings), Vol. 53 (May, 1963), pp. 231-236.
221. Ibid., p. 231.
222. Ibid., p. 232.
223. Ibid.
224. Ibid., p. 234.
225. Ibid., p. 236.
226. The speech, entitled "Economic Science and Forecasting," first appeared in print in 1956 and was subsequently reprinted in Robert C. Merton (ed.), The Collected Scientific Papers of Paul A. Samuelson, Vol. III (Cambridge: M. I. T. Press, 1966), pp. 774-780. All citations are to the reprint.
227. Ibid., p. 775.
228. Ibid.
229. Ibid., pp. 775-776.

230. Donald Gordon, "Professor Samuelson on Operationalism in Economic Theory," Quarterly Journal of Economics, Vol. 69 (May, 1955), pp. 305-310. Samuelson's reply appeared in the same issue of the Quarterly and was subsequently reprinted in Joseph E. Stiglitz (ed.), The Collected Scientific Papers of Paul A. Samuelson, Vol. II (Cambridge: M. I. T. Press, 1966), pp. 1767-1771. All citations are to the reprint.

231. Ibid., p. 1768.

232. Ibid.

233. These comments are not meant in any way to belittle Samuelson's achievements as an economic theorist of the greatest merit. I simply wish to point out that the pursuits of the "theorist" are not necessarily those of the "applied economist"; and even when they are the same, it is best to be explicit about what enterprise one is currently engaging in.

234. I have not, for instance, discussed the recent controversy over "situational determinism" in economics. For the primary sources to this debate, see Spiro J. Latsis, "Situational Determinism in Economics," British Journal for the Philosophy of Science, Vol. 23 (1972), pp. 207-245; Fritz Machlup's "Situational Determinism in Economics," British Journal for the Philosophy of Science, Vol. 25 (1974), pp. 271-284; and A. W. Coats' "Situational Determinism in Economics: The Implications of Latsis' Argument for the Historian of Economics," Ibid., pp. 285-288.

235. For articles which are "technical" in this sense but which still have a great deal to offer to those seriously interested in economic methodology, see R. L. Basmann, "The Role of the Economic Historian in the Predictive Testing of Proffered 'Economic Laws'," Explorations in Entrepreneurial History, Second Series, Vol. 2 (1965), pp. 159-185; -----, "Modern Logic and the Suppositional Weakness of the Empirical Foundations of Economic Science," Schweizerische Zeitschrift für Volkswirtschaft und Statistik, Vol. III (1975), pp. 153-175, especially pp. 153-156; and H. Wold, "Causality and Econometrics," Econometrica, Vol. 23 (1954), pp. 162-177.

236. Kant probably had not conceived of social phenomena as organizable into a "science" of the same character as the physical sciences.

APPENDIX A

THE ROLE OF J. S. MILL IN THE DEVELOPMENT OF
BRITISH META-ECONOMICS

The role played by J. S. Mill in the methodological controversies of Nineteenth Century political economy has been clouded by the twists and turns of his own mental development and by his unfortunate intellectual and personal association with the worst elements of both Orthodox and Historical economics. Mill's early education under the heavy-handed supervision of his Benthamite father has been a favored topic in surveys of the cultural and social development of Victorian England. His later intellectual and emotional "crisis" and subsequent conversion to the Romanticism of Coleridge and the social theories of Comte have also been documented and appraised by scores of competent historians. His political speculations and economic theorizing were praised and damned by his contemporaries and excited significant interest for many generations after his death. Yet his contributions to the one field in which he felt himself most competent--logical theory and its application to scientific method¹--have received only a few, rather half-hearted, appraisals.²

Perhaps one significant reason for the neglect of Mill's methodological writings is the ambiguity in interpretation to which they are subject. Mill was regularly regarded as an Orthodox economist by writers of the Historical School and was both worshiped and mistrusted by Cairnes.³ He was at the same time a champion of induction, an advocate of research into a science of historical development and

a proponent of the "geometric" or a prioristic approach to economic problem solving.

In what follows we do not attempt to consider the full range of Mill's thoughts regarding epistemology, the theory of logic and "pure" philosophy of science. A task of those dimensions is far beyond the scope of a minor appendix. The following comprises only the briefest sketch of Mill's methodological views concerning economics, but it is, hopefully, extensive enough to illustrate some of the major influences he exerted on his own time and on the future, and some of the major themes and conflicts in his own beliefs concerning social theory.

Economics as a Psychology Without a Subject

Mill's views as an economic methodologist have often been portrayed as little more than an explicit expression of the practices and procedures of James Mill and David Ricardo.⁴ More recent research has, however, disclosed numerous respects in which Mill actually deviated from the Ricardian tradition, both in his characterization of the scope and nature of economic theory and in his opinions concerning the methodology of and results to be expected from any social science.

Robert B. Ekelund, Jr. and Robert H. Hebert, in their recent History of Economic Theory and Method, have noted the Comtist orientation of those sections in Mill's Principles devoted to the scope of economic theory. They quote the following passage in their assessment of his views as evidence for this bias:

For practical purposes, Political Economy is inseparably intertwined with many other branches of Social Philosophy. Except on matters of mere detail there are perhaps no practical questions, even among those which approach nearest to the character of purely economical questions, which admit of being decided on economical premises alone.⁵

Pedro Schwartz in his New Political Economy of J. S. Mill has concisely summarized three additional ways in which Mill was untrue to the traditional doctrines of economic Orthodoxy.⁶ Mill originated the division of economic laws into those which were "absolute," that is, beyond human modification, and those which were "relative," or subject to modifications arising from Law and Custom. He explicitly limited the applicability of economic theory to those cases where its model of an "economic man" was approximately true of the world, but allowed that the predictions of the theory might represent "tendencies" even when its assumptions were not precisely met. Finally, he reintroduced the "historical" or illustrative form of economic writing first popularized in Adam Smith's Wealth of Nations,⁷ spurning the purely axiomatic or "geometric" format which had been favored by James Mill and, to a lesser extent, by Ricardo.

While Schwartz's distinctions between Mill's economic perspective and that of the Orthodox Ricardians are certainly of some importance to a complete evaluation of his impact on subsequent economic writers, more significant aspects of Mill's philosophy of economic investigation had previously been isolated by historians whose research had apparently escaped Schwartz's attention. Of the many economists who have puzzled over Mill's social and methodological writings none have been more perceptive than Jacob Viner.

In a 1917 article first published in the Journal of Political Economy,⁸ Viner identified Mill as the individual most responsible for a rejection of experimental techniques in economics, and for the longstanding professional prejudice against more informal empirical techniques. Viner then proceeded to systematically debase Mill's sharp distinction between the pursuits and methods of the physical and moral sciences and to critically examine the remaining differences in the procedures by which the two types of enterprises went about their respective research programs. Viner noted, first of all, that Mill "considered political economy to be a psychological science," but that he also believed that "the immense multitude of influencing circumstances" which molded each person's life in any complex social setting effectively barred the path to a direct consideration of individual psyches. In order to say anything about economic questions Mill was driven to what he considered as second-best procedures.⁹ Among the several alternative techniques open to economic problem-solvers he soon fell back upon the methodological faith of his childhood (viz. a modified version of Ricardian a priorism).

Even though Mill may have believed that he was essentially in agreement with the basics of Ricardian methodology, the qualifications and defenses which he introduced into the tradition of Orthodoxy were of overwhelming significance. He both undermined the original supports for the Orthodox approach to economics and then reintroduced similar if not identical procedures based upon new and more subtle defenses. Schwartz and many other of Mill's commentators

have, for instance, recognized the key role assigned to the explanatory model of an "economic man" in his writings. Yet, they have failed to understand the reasons motivating Mill's endorsement of this doctrine and the particular taint which he, himself, imparted to it. Classical methodology prior to Mill had implicitly assumed both the model of an economic man and an institutional framework of "competitive" capitalism, i.e., a framework of economic institutions similar to those which prevailed in Great Britain during the early half of the Nineteenth Century.¹⁰ Those assumptions were, in fact, the very points most frequently criticized by Historical economists who were seeking to refute the supposed "universal applicability" or "absolutism" of Classical theory. Mill strengthened the Historians' case against the generic universality of Classical theory by explicitly identifying and analyzing the assumptions and limits implicit within the Classical's system. Yet he also added certain provisions to his own reformulation of economic a priorism which moved economic theory further from the status of an empirical enquiry and transformed it into little less than a set of interrelated tautologies.

In addition to championing the notions of a purely hypothetical economic man, and a purely hypothetical economic environment in which he operated, Mill also introduced the idea that all predictions based upon economic theories were necessarily "hypothetical" (that is, that economics could only predict social "tendencies" rather than classes of social events). Mill further believed that the only tests which were meaningful for "hypothetical" social theories were

"mental" or "psychological" tests (i.e., "tests" resting upon introspective examination of the contents of one's own mind).¹¹

The a prioristic and "deductive" approach of the pre-Millian Classical was, for Mill, a self-justifying necessity, based upon the fundamental character of economic theory as a psychological and hypothetical science. The world of social action, according to Mill could be examined only through a prioristic models resting upon " ... assumed premises ... premises which might be totally without foundation in fact, and which are not pretended to be universally in accord with it."¹² Premises of this type were not, of course, testable by reference to existing conditions since they were not intended to refer to any conditions whatsoever. "Political economy ... is conceived by Mill," stated Viner, " as a study of human psychology. But not of all human psychology, or even of real human psychology, but only of an assumed psychology (of an "economic man") ... A hypothetical psychology and a hypothetical environment," he continued, "cannot, of course, be subjected to anything but hypothetical examination or experimentation."¹³ The premises of economic theory, then, could only be tested against those "first principles" of human psychology which were essential to man qua man, i.e., the same presumptions about universal "psychological laws" from which they had first been derived.¹⁴ The facts of the world were, according to Mill, "no part of the business of science at all," for facts had no necessary relationship to the content or the conclusions of economic speculation. Empirical prediction, insofar as it was required for public and private decision-making was relegated to the less-worthy

practitioners of "applied economics" (an obvious contradiction in terms).¹⁵ Mill apparently had little esteem for most "applied" social "theorists," finding that they commonly substituted sloganizing and theoretical chicanery for the arduous and less appealing task of the conditional application of economic theory.

The doctrines of hypothetical prediction and the support which they lent to the characteristic "unrealism" of economic assumptions proved extremely useful to future Orthodox theorists. Although the "hypothetical view" recognized that real social situations were too varied and too complex to serve as direct checks on the assumptions made by economists, thus freeing them from the tiresome duties of empirical testing, it also allowed that many social situations might be "explainable" in terms of "economic tendencies." The application of political economy was not a flawless procedure, for many motives influenced real men which would not concern an economic man, but this was not, in itself, a disadvantage. When economists guessed wrong about the consequences of legislations or business policy it was certainly because the economic tendencies present in the given situation were outweighed by other types of "non-economic motives." There was never any question of possible flaws in the economist's analysis of a situation "in general" or of flaws in his basic theoretical structure.¹⁶

Mill and those less scrupulous political economists who succeeded him were, however, not so willing to place a straight-jacket on their practical impact as their characterization of the limits of economic analysis might initially indicate. Mill and the post-

Millians maintained that the analysis of political economy was applicable even to those cases where the desire for wealth did not hold exclusive sway. If only the predictions of the economist could be "corrected" for the effects of disturbing influences then they would become perfectly accurate and universally applicable.¹⁷ The continued occurrence of this argument from ignorance is a topic considered more fully in the later sections of this dissertation. It should be noted, however, that no Orthodox economist, including Mill, ever sought to develop the idea of "correcting" economic predictions or ever was able to suggest a systematic procedure for attaining acceptable "corrections." It is obvious that the attempt to investigate these questions would have involved just that sort of methodological inquiry into the foundations of "economic epistemology" which most post-Millian Orthodox economists were anxious to avoid.

The Forming Up of the Troops

The various reactions to Mill's methodology among British social theorists played a major role in the formation and later revitalization of the Methodological Schools. Mill's proclivity to divorce theory from fact and the forthright style of his presentation¹⁸ led to a number of rather extreme reactions.

In an address to Section F of the Royal Statistical Society L. L. Price suggested that economists had been both mistaken and overly restrictive in viewing their study as a properly constituted (or properly constitutable) positive science. Economics, according to Price, was best conceived as a means for correcting and strengthening

"common-sense" notions about the conduct of public policy.¹⁹ Its purpose was therapeutic rather than informative. The apparently widespread sentiment in favor of removing the economics section from the program of the Royal Statistical Society, and the continuing attacks on British political economy in the intellectual journals of the time seem to indicate that Price was not alone in concluding that the study was an "art" or social perspective rather than a science.²⁰

While those trained in the methodology of the physical sciences reacted with revulsion to Mill's prescription for the ills of political economy, the defenders of Orthodoxy seized upon his "hypothetical" reinterpretation of the subject as a reassuring defense for their own pursuits. Cairnes and those who followed him in both the Nineteenth and Twentieth Centuries were able to deny any direct role for "fact-gathering" in either the construction or the testing of economic hypotheses. The Orthodox economist's ability to regard his own studies as purely positive, without any necessary connection to observable cases, but still of relevance to the formulations of public policy, left him in an enviable position. Unlike politicians he could choose to act as a social prophet or as a disinterested and impartial observer, just as his tastes and the situation demanded. And in neither of these roles could he be held responsible for the accuracy of his predictions, for all such predictions were merely "hypothetical."

A third and final type of reaction to Mill's methodology expressed itself in the writings of the British Historical School. Despite the sea of psychologistic, evolutionary and historicist fads

which surrounded them in the intellectual climate of Victorian England, many of these writers, and most of those considered in this dissertation, managed to steer a middle-path between the "abolutism" of the Classics and the theoretical nihilism of authors like Price and the German Historicists. Although rejecting Mill's tautological reformulation of economic theory, which they misleadingly labelled as "the deductive view," the British Historicists attempted to construct an alternative theoretical structure based upon a recognition of the impact on acting men of different institutional and customary social structures.

Although the true intent of the British Historical movement and the actual character of the doctrines to which they were reacting was subsequently obscured in later historical accounts of the period, Viner was able both to reconstruct and cleverly defend their central views. He also pinpointed the most important influences affecting the methodological evolution of Orthodox political economy and he proffered several useful suggestions regarding the steps necessary in order to free economics from these regressive methodological influences. Like Symes, Viner wished to liberate economic speculation from the clutches of a "mentalistic" perspective, i.e., one which stated explanantions for human actions in terms of "motives." Like the early Historical economists in Britain, he attacked the idea of unconditioned and unspecified "disturbing causes" which could be used to justify the most fallacious social conceptions.²¹ Oddly enough, Viner's scant citations to the Victorian critics of economic Orthodoxy may indicate that his views were largely original. He does

indicate some passing familiarity with the writings of Jones and Ingram, but his understanding of these authors is clearly insufficient to explain the extent to which his own writings parallel those of Whewell, Leslie and the other competent writers in the Historical tradition.

As a result of Viner's extensive analysis and criticism of Mill's psychologistic political economy it becomes possible to more clearly define his (Mill's) role in the controversies which perturbed Nineteenth Century economics. Past historians have correctly evaluated Mill's part in weakening the core of Classical theory (i.e., the Classical "paradigm"). Significant in this respect are (1) his distinction between the laws of production and the laws of distribution, (2) his abandonment of the Wages-Fund doctrine and (3) his expressed doubts about the unconditional validity of Ricardian welfare economics. Current and past attempts to evaluate Mill's influence on Classical and Neoclassical methodology have been, however, generally inaccurate. Even though Mill's formalization of the assumptions implicit in Classical theory elicited an eventual uproar over the "relevance" of political economy, his analysis of the meaning and applicability of economic speculations formed the foundation for all subsequent Orthodox discussions of these issues. Cairnes' Character and Logical Method of Political Economy, for many years the standard reference on the subject, is little more than an expansion of Mill's central methodological positions, liberally sprinkled with tidbits from Senior and the medievals. Frank Knight's "What Is Truth In Economics?" (1940) is mainly a condensation of Mill and Cairnes

interlaced with Kant and Pragmatism. The even more modern writings of Coats, Machlup and Friedman are similarly Millian in their attempt to maintain the positions of intuitive certainty and the virtual unfalsifiability of basic economic models.²² The practice of economics as an empirical science may have made apparent advances in the last hundred and fifty years, but the way in which most economists perceive and practice their enterprise has altered only superficially.

Mill and the Science of History

While the character and consequences of Mill's psychologism are fairly evident there remains another aspect to his thought which worked its influence along more devious and indirect routes, that is, his belief in a theoretical or "scientific" history. It is most important, first of all, to distinguish Mill's concern with a science of historical change from the superficially similar beliefs of later "social evolutionists" and/or "social Darwinists." Although Mill acknowledged the existence of Darwin's Origin of the Species in the fifth edition of his own Logic (1862), and while he was well acquainted with his younger contemporary, Herbert Spencer, there is no evidence to indicate that he was impressed by, or fully understood, the notion of Darwinian evolution and "natural selection."²³ Insofar as Mill's belief in a science of history was anything more than the product of the Victorian idea of Social Progress, or a reflection of certain vague impressions which he may have extracted from the works of Coleridge and the essays of Macaulay, its roots probably are to be found in Mill's appreciation for the writings of the French social

historians, Guizot and Michelet,²⁴ and in his brief, but significant, infatuation with Comtist philosophy. In Mill's review of Michelet's History of France we find one of the earliest and most complete statements of his belief in a science of historical change:

There is a third, and ... highest stage of historical investigation, in which the aim is not simply to compose histories, but to construct a science of history ... All history is conceived as a progressive chain of causes and effects ... The facts of each generation are looked upon as one complex phenomenon, caused by those of the generation preceding, and causing, in its turn, those of the next order. That these states must follow one another according to some law, is considered certain: how to read that law, is ... the fundamental problem of the science of history.²⁵

In this same work Mill also expressed his evaluation of what had so far been accomplished in this "ultimate" social science as well as his appreciation to those most responsible for its accomplishment:

" ... this greatest achievement is rather a possibility to be one day realized, than an enterprise in which any great progress has yet been made. But of the little yet done in this direction, by far the greater part has hitherto been done by French writers."²⁶

These scattered endorsements of a science of history neither exhaust nor fully explicate Mill's views concerning the study of Social Progress, however. His peculiar perspective on the possibility of constructing a theoretical history were but another, albeit important, branch of his overriding belief in a science of developmental psychology. As Karl Popper has observed: "Society being (in Mill's view) the product of interacting minds, social laws must ultimately be reducible to psychological laws, since the events of

social life ... must be the outcome of motives springing from the minds of individual men."²⁷ As Mill himself expressed a similar thought in a passage which Popper overlooked: "... we are justified in concluding, that the order of human progression in all respects will mainly depend on the order of progression in the intellectual convictions of mankind, that is, on the law of the successive transformations of human opinion."²⁸

It would be a mistake, however, to assume that Mill's use of the term "psychology" was limited to a study of "the laws of thought" for that phrase better defines the narrower sub-field of psychology which the Victorians called "Logic." "Psychology," in the Victorian sense of the term, was concerned both with the temporary and passing sequence of thoughts that were immediately evident in men's words and actions and also with the more permanent "association of ideas" which led to the formation of a man's habits and his future reactions to various situations.

It was upon a foundation provided by the study of the (historical) determinates of an individual's reactions and propensities to act, upon a proposed discipline of "Ethology, or the Science of the Formation of Character," that Mill hoped to construct his own sciences of History and Social Progress.²⁹ Yet even though he was firmly convinced that: "there exist universal laws of the formation of Character"³⁰ (and, thus, that it should be possible to formulate laws of historical development which were equally universal), Mill eventually realized that a knowledge of these relationships would be attained only at the expense of prolonged and laborious efforts.

"Before we can trace the filiation of states of society one from another," he cautioned, "we must rightly understand and clearly conceive them, each apart from the rest."³¹ The clear conception of each individual social state was itself a complex process, resulting from a synthesis of the knowledge gained from many separate forms of social study.³²

While in a burst of initial enthusiasm over Comte's philosophy Mill had envisioned the rapid composition of a multi-volumed treatise on "ethology." He eventually concluded, after more extensive consideration, that he must first content himself with a more limited contribution to that area of social study with which he was most familiar, viz., political economy. The "historical character" of Mill's Principles was, therefore, far more than a consequence of his admiration for Adam Smith or his desire to exemplify as well as "clarify" the Ricardian system. The Principles was intended as a sketch of the historical evolution of one aspect of Social Progress-- the accumulation and use of wealth. As he stated in his introduction to the Principles, however, the goal of the volume was not to consider the topic of wealth in isolation from the remainder of social and moral philosophy or in isolation from the various non-economic factors influencing social life, it was rather to trace out the interconnections between the principle and the motive of wealth maximization and the numerous other social and political influences which molded men's behavior.³³ In pursuing this goal Mill was probably just as true to Smith as he was to Comte, for Smith himself was interested in the results of self-interest not as the only spur goad-

ing men to action but as one of the most persistent and reliable motives around which a dependable social system might be constructed. Neither Smith nor the Comtian Mill, then, were true believers in what Mill, himself, would later construct as a pure and hypothetical science of the economic man.

Mill's Influence: A Concluding Note

From the foregoing discussion it may appear that Mill's philosophy has few qualities to recommend it to advocates of a critical empiricism, and that, in those instances where he influenced the development of economic methodology his influence was, at least, unhealthy. That impression is not at all weakened by Mill's obvious disregard for any real attempts to operationalize economic theory, or by his own history of political crusades on the basis of "economic considerations." Concerning the one topic of his day which approached a modern version of property-rights analysis, Mill's own record was less than admirable. As Cairnes noted in examining Mill's contributions to the science of political economy: "... there is no portion of the economic field in which Mill's originality is less conspicuous than in that which deals with land."³⁴ And in perhaps the major instance where Mill seemed close to making a significant contribution to the study of alternative land systems and the differential incentives which they generate, he deleted the relevant passages from a later edition of his essay dealing with the question.³⁵

Although the methodological aspects of Mill's economic writings

closely resemble the methodological views later expressed by Alfred Marshall, who may, in fact, have modeled his own reflections after Mill's,³⁶ there is at least one respect in which Mill's methodological writings were superior to those of Cairnes, Marshall or any of the later Orthodox methodologists, viz., he was always impatient with obscurity and intolerant of attempts to avoid clear-cut methodological positions. Being of often generous spirit Mill would frequently excuse his contemporaries for what he believed to be elements in their styles which revealed a weakness of character or an indecisiveness of expression. He did, after all, believe that his own age was characterized by a spirit of transition, i.e., that he was living through a period in which old opinions and patterns of thinking had been rejected while their newer replacements remained yet imperfectly formed.³⁷ Even so, however, Mill himself expressed a continued devotion to strong opinions and a continued admiration for those who were willing to take their positions "to an extreme." This willingness to submit his own opinions to the vicissitudes of critical debate was one of the most consistent elements of Mill's intellectual perspective. It is explicitly argued for in his earliest Comtist essays and in one of his last social works, On Liberty.³⁸

Although we are told by Cairnes, in his essay on Mill's political economy, that:

The character of his intellect ... led him to strive to connect his thoughts ... with the previously-existing body of speculation, to fit them into the same framework, and exhibit them as parts of the same scheme; so that ... he was at more pains to conceal the originality and independent value of his contributions to the stock of knowledge than most

writers are to set forth those qualities ... "39

It seems certain that these attributes of Mill's literary style were inspired more by his honest intellectual modesty than by a motive of "professional trade-unionism" of a desire to protect economics from criticism, both of which were, however, prime motives for similar literary characteristics found in the writings of Cairnes and Marshall. Even though Mill's philosophic perspective on matters of economic methodology proved as a faulty guide for many of his successors, his beliefs were always honestly pursued to their ultimate conclusion and presented in a forthright manner. The faith of the early Liberals in the victory of "truth" through the mechanism of open and unrestricted debate remained alive in the man who was among the first to abandon the creed of liberalism.

FOOTNOTES TO APPENDIX A

1. In writing his friend, John Sterling, in 1831 Mill remarked that: "If there is any science which I am capable of promoting, I think it is the science of science itself, the science of investigation--of method." Quoted in Bruce Mazlich, James and John Stuart Mill (New York: Basic Books, 1975), p. 403.

2. The evaluation of Mill's Logic and his other methodological writings in Joseph Schumpeter, A History of Economic Analysis (New York: Oxford University Press, 1954), pp. 451-452 can only be ranked as one of Schumpeter's least perceptive writings. According to this interpretation, Mill was no more than an eclectic philosopher who combined elements of the traditional a prioristic methodology of the early classicals with the "inverse historical" method of Comte. Somewhat more perceptive are R. P. Anschutz, "The Logic of J. S. Mill," contained in J. B. Schneewind (ed), Mill: A Collection of Critical Essays (Garden City, New York: Anchor Books, 1968), pp. 46-83 and Anschutz's book-length The Philosophy of J. S. Mill (New York: Oxford University Press, 1953). The best examination of Mill's philosophy of logic and philosophy of social science is, however, Alan Ryan's John Stuart Mill (New York: Pantheon Books, 1970).

3. Ingram held that: "Mill's effort is usually to vindicate his master (Ricardo) while others have criticized him and to palliate his admitted laxities of expression." (J. K. Ingram History of Political Economy (London: A & C Black, 1923), p. 145.) For the even less sympathetic assessments of Leslie and Bagehot see T. E. C. Leslie, Essays In Political Economy (New York: Augustus Kelley, 1969), pp. 56-57 and Walter Bagehot, Economic Studies (Stanford: Academic Reprints, 1962), pp. 180-181.

J. E. Cairnes leaned heavily on Mill's Logic and his Unsettled Questions of Political Economy in developing his own methodological viewpoint (see J. E. Cairnes, The Character and Logical Method of Political Economy, Second Enlarged Edition (London: Macmillan, 1875), pp. 27-31, 64-65 and 66-68; yet he also found Mill's methodological comments to be somewhat "inconsistent" and invented the concept of "valued matter" to remedy what he believed to be their central flaw--the characterization of economics as a study concerned only with the "laws of mind," Ibid., p. 30.

4. For a more or less typical appraisal of Mill's methodology as merely an extension and explication of the methodology of earlier classicals see Lewis H. Haney, History of Economic Thought, Fourth Edition (New York: Macmillan, 1970), pp. 473-474. An explanation for this interpretation may lie in Mill's propensity to deemphasize his own originality in matters of both economic theory and economic methodology. For comments on and evaluations of this trait see Ingram, op. cit., pp. 145-146 and the material in Note 39 below.

5. Robert B. Ekelund and Robert F. Hebert, A History of Economic Theory and Method (New York: Harper and Row, 1975), p. 117.

6. Pedro Schwartz, The New Political Economy of J. S. Mill (London: Weidenfeld and Nicholson, Ltd., 1972), pp. 62-66.

7. In a passage quoted in Mazlich, op. cit., p. 353 and in the sub-title to his Principles Mill makes it clear that the "historical" discussions contained within his writings were not intended as attempts to test or "verify" economic theory but were, rather, "illustrations" of the applicability of the theory. The speculation that Mill may have been influenced by Comte, rather than by the example of Smith, to give up the purely "geometric" discussions of his father and return to a more "historical" mode of writing is discussed in Robert B. Ekelund, Jr. and Emile Olsen, "Comte, Mill and Cairnes: The Positivist-Empiricist Interlude in Late Classical Economics," Journal of Economic Issues, Vol. 7 (September, 1973), pp. 389-390, 411, and is illustrated in a quote from Mill in Mazlich, op. cit., p. 357.

8. Jacob Viner, "Some Problems of Logical Method in Political Economy," Journal of Political Economy, Vol. 35 (1917), pp. 236-260 reprinted in Earl L. Hamilton (ed.), Landmarks In Political Economy, Vol. 1 (Chicago: University of Chicago Press, 1962), pp. 101-124. The Hamilton reprint is used as the source for all following citations and is referred to simply as "Hamilton."

9. For Mill's rejection of experimentation in the social sciences see Hamilton, op. cit., pp. 107-109, Karl Popper, The Poverty of Historicism (New York: Harper & Row, 1964), p. 85fn and the quote from Mill's Logic in J. E. Cairnes, op. cit., pp. 67-68.

10. The completely "hypothetical" character of Mill's "economic man" and the similar status of the political and social environment in which he lived and acted is discussed in Hamilton, op. cit., p. 109. For additional comment on Mill's "economic man" and the relationship of this concept to his economic theory, strictly speaking, see Popper, "The Autonomy of Sociology," op. cit., p. 429.

11. Hamilton, op. cit., pp. 108-109, 111.

12. Quoted from Mill's Unsettled Questions of Political Economy in Hamilton, op. cit., p. 109.

13. Ibid.

14. Mill divided the sciences not only on the basis of their use of controlled experimentation as a possible investigative device, but also on the basis of the character of their subject matters. The following is a passage from his Logic illustrating just that view: "... laws of matter and laws of mind are so dissimilar in their

nature that it would be contrary to all principles of rational arrangement to mix them up as part of the same study. In all scientific methods, therefore, they are placed apart." Quoted in Mazlich, op. cit., p. 410.

15. Hamilton, op. cit., p. 110.

16. Ibid. See also the chapter on Cairnes in this dissertation for further refinements upon the idea that economic predictions were necessarily true, but conceivably could be inapplicable to certain situations.

17. Hamilton, op. cit., p. 110. The doctrine of "disturbing influences" played a major role in the future development of orthodox economics, especially in the writings of Cairnes and Marshall. It would also, eventually, develop into the unspecified caeteris paribus clauses of early neoclassical economics, discussed more fully in the concluding section of this dissertation.

18. As is noted in the text of Chapter I and in Note 38, below, Mill always attempted to clarify his position so far as possible by considering "pure" or "extreme" cases. He was also quite hostile to those who valued good will over honest debate or attempted to confuse the obscure with the profound.

19. "The Relation of Economic Science To Practical Affairs," reprinted in Essays In Economic Method (New York: McGraw-Hill, 1963), pp. 146-148. The issue involved in the separation of "positive" and normative economics (or in the separation of positive and normative assertions by economists) has agitated controversy almost since the beginning of the discipline. For two recent discussions of the issues involved in this controversy see T. W. Hutchison's "Positive" Economics and Policy Objectives (Cambridge: Harvard University Press, 1964) and a recent paper, yet unpublished, by Alfred F. Chalk of Texas A&M University, entitled "Myrdal and Schumpeter On the Role Of Value Judgments In Classical Economics." The paper by Professor Chalk contains a brief but insightful examination of the differences between the early Ricardians and J. S. Mill concerning the issue of a "normative economics," see especially pp. 9-11 and 14-16.

20. For a discussion of the attempt to disband the economics section of the Royal Statistical Society see J. K. Ingram, "The Present Position and Prospects of Political Economy," printed in Essays In Economic Method, op. cit., pp. 41-42, note especially the reference in fn. 1 to page 41 and the reference to the work by Bonamy Price of Oxford on p. 44.

For one example of the Comtist reaction to orthodox political economy see W. Cunningham, "The Comtist Criticism of Economic Science," printed in Essays In Economic Method, op. cit., pp. 98-111. See also the reference to the writings of Harrison in Ekelund and

Olsen, op. cit., p. 415 fn.

21. Hamilton, op. cit., pp. 114-117, 123-124.

22. These various methodological writings are discussed in the concluding chapter to this dissertation.

23. Mazlich, op. cit., p. 424.

24. See John M. Robson (ed), John Stuart Mill: A Selection of His Works (New York: The Odyssey Press, 1966), pp. 436-437.

25. Ibid., p. 436.

26. Ibid., p. 437.

27. Quoted in Mazlich, op. cit., p. 417.

28. J. B. Schneewind (ed.), Mill's Essays on Literature and Society, (New York: Collier-Macmillan, 1965), p. 10.

The close connection between logic and psychology in the Nineteenth Century was the basis for Mill's belief in the "inductive character" of all human knowledge. (See the essay on "His (Mill's) Work In Philosophy" by J. H. Levy printed in John Stuart Mill: His Life and Works (Boston: James R. Osgood and Company, 1873), pp. 55-61).

29. Mazlich, op. cit., pp. 404, 414.

30. Ibid., p. 415.

31. Robson, op. cit., p. 437.

32. For a lengthy definition of the different components or aspects of a social state and the methodological difficulties involved in investigating each of these so as to obtain a well-rounded picture of the whole see passages from Mill quoted in Mazlich, op. cit., pp. 418-419.

33. Mazlich, op. cit., p. 357.

34. J. E. Cairnes, "His (Mill's) Work In Political Economy," as contained in John Stuart Mill: His Life and Works (Boston: James R. Osgood and Company, 1873), p. 70.

35. The passage occurs in Mill's essay on "The Claims of Labour," contained in Mill's Essays On Economics and Society (London: Routledge & Kegan Paul, 1967), pp. 387-389.

36. The resemblances between the general methodological and

social perspective of Mill and Marshall are truly remarkable. Both were interested primarily in the influences effecting the formation of human character (see the chapter on Marshall, especially those sections dealing with Parson's evaluation of his social writings), both had the same view of the role of history and the character of induction in the investigations of a social science, both believed in a utopian future state which would arise due to the improved character of mankind, and both had a well-developed theory of history which they attempted to support by a loose and generalized form of "historical research."

37. That portion of Mill's theory of history which was concerned with the effects of different historical periods on the types of opinions held by men of those periods ("the spirit of the age") is illustrated by quotes from Mill's works in Robson, op. cit., pp. 15-18.

38. For examples of Mill's impatience with anything less than clearly stated opinions and rigorous logic see his "Spirit of the Age," in Schneewind, op. cit., p. 34 and his essay "On Liberty" in Robson, op. cit., p. 28.

39. Cairnes, "His Work In Political Economy," op. cit., p. 65.

APPENDIX B

THE TERMINOLOGY USED IN THIS WORK

Much of the confusion which has characterized studies of economic methodology is attributable to the varied meanings which different authors have assigned to the "same" technical term and to their apparent obliviousness to the different senses given these terms by other authors. Although any discussion of fundamentals must necessarily involve some undefined or "primitive" concepts, the explicit definition of other important terms can often reduce further controversy to a minimum. The definition of key terms to be used in this study is the purpose of this appendix.

Concerning "Historicism"

The term of central importance to this dissertation is, of course, "historicism" itself. Yet this concept is fraught with many ambiguities and has been used in at least three separate senses in past studies of British Historical economists.

The most searching investigation of historicism in the social sciences is found in Karl Popper's Poverty of Historicism (later supplemented by material appearing in recent editions of his Open Society and Its Enemies, his Logic of Scientific Discovery and his Conjectures and Refutations). Popper divided the various strains in the historicist outlook into those which were pronaturalistic (viz., favorable to a unified scientific method) and those which were anti-naturalistic (viz., favoring separate methodologies for social and

natural sciences).

Among the pronaturalistic doctrines of historicism he included the following ideas or orientations:

- (1) A concern with large scale forecasts, on the model of research carried on in speculative astronomy or speculative geology: predictions resulting from such research usually being expressed in a temporally unbounded form (i.e., given these conditions we can expect _____ to eventually arise).
- (2) A concern with history as the only observational basis for social science, and a reorientation of all social investigations toward large-scale predictions about future "social stages." That is, the transformation of social science into a "theoretical history" or a study of societal development.
- (3) An exaltation of dynamics (the theory of social development as determined by "historical forces") over statics (the investigation of the interrelationships between social units at any one instant of time).
- (4) A search for laws of historical development which are spatially and temporally limited to a particular society (or a temporally and spatially defined set of such societies), as

opposed to a search for social laws which are "universal" in the sense of being applicable to any time or place (but not, of course, to any social structure).

- (5) A concern with historical prophesy rather than with the technology or mechanics of social structures.
- (6) A relegation of every social science to a mere branch of a general science of historical change. Although allowing for the legitimacy of a division between social studies on the basis of their subject matter (i.e., on the basis of the "type" of phenomena which they studied--the "political," the "economic" or the "social"), any historicist view of social science would require that each field be considered as only one component of a more general investigation into the influences effecting the "motion of human society."
- (7) An interpretation of the policy studies and policy recommendations of social scientists as examples of "social midwifery." That is, as mere means for hastening that which is already inevitable rather than as attempts to conscientiously improve social structures according to well-defined standards of "betterness."

The antinaturalistic doctrines of historicism (those connected with

arguments for the uniquely specialized character of investigations within social science) included the following:

- (1) An advocacy of "policy-activism" rather than an emphasis on positive studies (as represented by Marx's comment, "The philosophers have only interpreted the world, the point is to change it").
- (2) An opposition to experimentation in the social sciences.
- (3) A belief in the essential uniqueness of each social event, rather than an attempt to group any such event into an analytic category.
- (4) An emphasis on the complexity of social phenomena and the relative simplicity of physical phenomena.
- (5) A concern with factors which hinder "objectivity" in the observation of social events.
- (6) A belief in the imprecise character of social predictions as contrasted with the precision of predictions in the physical sciences.
- (7) An emphasis on the "organic" or "dialectic" development of social structures, rather than upon their mechanical functioning.
- (8) A belief in the importance of "familiarity with" (or "a feeling for") the subjects of social investigations. That is, a reliance

upon the "experienced intuitions" of "expert" social investigators rather than upon well defined problems and test methods.

- (9) An opposition to the use of quantitative or mathematical methods in the social sciences.
- (10) An endorsement of "methodological essentialism" (i.e., the doctrine that there are historical essences or "spirits of an age," which are both discoverable and meaningful once discovered).

Popper's discussion of the "historicist" outlook does not require that a methodological perspective possess all of the above features in order to properly be classed under this label. It is sufficient that some of these characteristics be present and that the advocates of the position adopt an uncritical attitude toward the results of their inquiries. If "scientists" of any variety become more interested in a defense of their theories as "absolute" truths than they are in the testing of these theories they have taken the major step on the road to historicism, according to Popper's definition.

Another and quite different sense of the term "historicism" was advanced by Frederick A. Hayek in his Counter-Revolution of Science. According to Hayek's treatment "historicism" is closely allied to and often intermingled with what he has variously referred to as "false rationalism" or "the religion of the engineers" (i.e., the belief that society is essentially a machine subject to the conscious control of social designers). It is further reinforced by the

influences of French "scientism" (the application of the "inductive" methodology of the natural sciences to the social sciences) and is itself a derivative of the confusions of German Hegelism. The major characteristics of an historicist view according to Hayek are a reliance on "dialectical reasoning" (in which he does not believe) and a faith in historical fatalism. The doctrines of social evolution and the attempts to develop a science of social history are also recognized by Hayek as important components of the historicist view, but his main interests revolve around the combination of rationalist scientific and historicist perspectives into an ideology advocating social prediction and control.

Although Hayek was to eventually become an admirer of Popper's work in the philosophy of science, and was to modify his antinaturalistic views accordingly, his original critique of the tripartate view of rationalism-scientism and historicism exerted a major influence on A. W. Coats in his 1954 article, "The Historical Reaction In English Political Economy, 1870-1890." That Coats apparently overlooked the distinction between historicism per se and the other two components of the ideology of social control in no way detracted from his wholehearted acceptance of Hayek's antinaturalism or his distorted interpretation of the goals of the leading British Historicists.

A third and final major sense given to the term "historicism" gained its popularity from the writings of J. N. Keynes and Alfred Marshall. "Historicism" in their view was predominately concerned with the reduction of all economic inquiry to economic history or

economic statistics without regard for (and, in fact, with much contempt for) the theoretical implications of such empirical studies. (This same view was later differentiated from the mainstream of historicism by Karl Popper, and was given the label of "historicism.") Although Marshall's definition of "historicism" might be accurately applied to the views of the later German Historical School, and the various reformed Comtists and neo-German "economists" who bedeviled British economics during the closing decades of the Nineteenth Century, it would be totally inaccurate to use this sense of the term to refer to the writings of Jones, Whewell, Bagehot, Symes or Leslie.

"Historicism" in this Study

The term "Historicism" as used in this study shares some of the features of Popper's use of the term, but does not include his central doctrine of uncritical or "devout" adherence to a body of doctrines. Although some of the authors writing during the declining years of the movement (e.g., Ingram and to a lesser extent Leslie) employed evolutionary notions, sought after large-scale social predictions and stressed the importance of a unified social science, these views always seemed secondary to their more central involvement with empirical methods and empirically enriched social theories (again, Ingram may be an exception to this general rule). I have used the term "Historicism" (always capitalized when referring to this usage) to refer to matters more concrete than those which were the concern of either Popper or Hayek. Specifically, "Historicism" in this dissertation refers to the body of doctrines and the group of writers

opposed to the a priori of orthodox economic methodology and offering in its place some sort of empirically supported or empirically enriched economic theorizing. By defining the term in that way the term becomes partly, but not completely, stipulative. If a particular economic writer claimed to be an Historicist and was recognized as such by his contemporaries then he was, for our purposes, a proper representative of the view. The main exceptions to this stipulative and conventional rule for defining the term "Historicism" are those derived from the temporal scope of this dissertation. That is, although there were many economic writers during the last decades of the Nineteenth Century and the early decades of the Twentieth Century who were recognized as "historicists" our concern is strictly with the British Historical School of the period 1830-1880. Although this limitation is in part imposed by the constraints of time and size for this dissertation an even more important factor influencing this decision is the rather radical alteration in the character of Historical economics in England which took place in the last years of the ninth decade.

Other Terms of Importance

Several other terms frequently encountered in this dissertation require explicit definition, both because their usage is somewhat non-standard and because the ways in which they are employed may otherwise seem ambiguous. The terms are, as follows:

Absolute - incontestable: applicable to any situation without regard for particular constraints or institutional

structures; orthodox methodologists sometimes maintained that economic theories were "absolute" (i.e., that they were contextually as well as spatially-temporally non-specific).

A priori - logically or epistemologically prior to the organization or identification of experiences; proffered without regard for established "facts." Although the former meaning is common in philosophy, the latter means was frequently used by the British Historicists in referring to the process by which Orthodox economists arrived at and justified the basic relationships of economics.

Empiricism -

Baconian - the view that science is a systematically arranged body of "generalized facts," that its role is not to explain or predict the sequence of occurrences, but merely to describe the components of this sequence. (See also "Induction.")

Modern - the view that scientific theories or hypotheses should be testable by reference to facts (or of observation statements), that they should assert something about the world which can be determined to be either true or false through some well-defined intersubjective test procedures.

Evolutionism - a doctrine which is concerned with the pattern of

changes occurring during the "life history" of an individual, a species, or a society, which seeks to formulate "laws of development" to describe this sequence, and which, when applied to social investigations, considers "time" or "historical stage" to be the primary variable effecting the likelihood of any type of social occurrence.

Fact - an observation statement derived on the basis of standardized and accepted observation rules. Not identical to generally agreed upon, or "common sensical" notions about relationships which "seem reasonable."

Induction -

Baconian - a psychological-epistemological doctrine concerned with the derivation of "correct" theories; the belief that "correct" theories can be derived from the examination of a body of "facts." Connected with a belief in "insightful," "gifted" or "expert" intuitions about the "true" relationships in the world.

General - a concern for "fact-gathering" as a means for developing a base against which theories may be tested and suggested modifications to theories may be judged as "productive" or "unproductive."

- in the writings of J. S. Mill--a movement from the "specific" or "particular" to the general; a set

of rules for deriving correct or complete hypotheses under conditions of controlled experimentation.

Intuition - a logical insight into the "true" or "essential" nature of things; usually used with reference to some relation, state or property of things which either cannot be determined on the basis of intersubjective procedures or which has not been determined.

Methodology - the investigation of the "scope and limits" appropriate to a science and of its research procedures.

Metaphysics - any investigation involving assertions about properties of or relationships between "things in the world," based upon speculations which are untestable or procedures which are unrepeatable.

Observation

(rule) - any well-defined process by which the observations undertaken to test particular types of hypotheses may be carried out.

As previously alluded to, there is at least one additional terminological convention used in this dissertation which might cause unnecessary confusion, i.e., the convention concerning the capitalization of the terms "Historicism" and "Historical." In those cases where these terms are used to refer to a methodological procedure or perspective of the type discussed by Karl Popper they remain

uncapitalized. When they are, however, used to refer to the members or views of the British Historical School they are capitalized. This distinction is required both because Popper's description does not apply to early British Historical economics and because many of the practices of the Orthodox School were historical, in Popper's sense of the term.

Although the above clarifications may help to avoid many unproductive misunderstandings which might otherwise arise in the interpretation of various sections of this dissertation, I am certain that there are a number of issues remaining which will engender real disagreements. I also believe, however, that it would be a mistake to attempt to "soften" these issues so as to avoid any truly substantive criticisms.

Progress within any area of knowledge is not the same thing as change, even if that change is both systematic and constructive. Questions about which discussion and debate are proscribed by custom and tradition often involve issues which, rather than being transparently obvious, are unnecessarily obscure. Economic methodology has been and is one such area, and the only cure for its degenerate state is a new wave of constructive criticism and open debate. So long as the methodological foundations of economic inquiry are regarded either as barren ground for further research or as a "sensitive and embarrassing area," best avoided for the sake of professional unity, we can be assured that the future of economics as a science will be no brighter than its past.

APPENDIX C

HISTORICISM AND THE SCIENCE OF HISTORY¹

One of the most influential and destructive doctrines ever to be spawned in the history of social thought was that of Societal Progress and the accompanying attempt to describe and control this Progress through a Science of Historical Change. A well developed appreciation for the meaning of this doctrine is essential to an accurate appraisal of the darker side of social thought in the Nineteenth and early Twentieth Centuries and to the distinction between the positive contributions of the earlier British historicists and the often regressive influences exercised by later historical writers in both Germany and Britain. Yet the distinction between attempts to construct a science of history and the perfectly legitimate pursuits of the economic historian have been too frequently blurred in past discussions of economic methodology.

What then is the meaning of "a law of historical change"? And what is the distinction between the discovery of laws of this type and the more standard quest after universal social laws? A historical law is a means of forecasting the dominant features of the successive stages of a society's development. It does not claim to predict the details of future developments or even the particular events which will bring these development to fruition, but is only concerned with the "broad strokes" in the movements of societal development. Historical laws are derivable only on the basis of expert examination of a society's past, examination directed at ascertaining the "pattern"

of past developments and providing a basis by which intuition (or "dialectical reasoning") can determine the path of future changes. The rationalization for this type of enterprise is itself appealing and superficially convincing, and demands examination.

Institutions are continually evolving in any but the most primitive societies yet static laws require a fixed institutional structure as a condition for their applicability. The "relevance" of social investigation, its capacity to become more than an ex post explanation for past events, thus seems to depend upon the determination of a constant sequence of social change or a constant pattern of social development. Constancy is not, however, to be equated with logical consistency, for logic itself is a tool applicable only to static situations. It is not that societies develop "deterministically" in accord with measurable changes in growth-related variables (i.e., the size of the capital stock or the skill and education of the labor force), it is rather that they evolve dialectically as a result of the interaction of contradictory forces. Such are the basic arguments for a science of historical change.

Scientific laws of the more standard type differ from laws of historical change in a number of respects. They are, first of all, universal in scope. That is, they do not specify any particular time or place to which they are applicable and do not contain any proper names (although they may contain the names of classes). Historical laws must, however, name the time and place to which they refer or they remain too vague to specify any events (or social developments) of any type.²

This difference between historical laws and the usual laws of science has rather important consequences which may not be immediately apparent. If a "law" is space-time specific then it is not subject to successive tests. That is, if it refers to a particular sequence of events or social stages then it either correctly forecasts the occurrence of these events or it does not. In either case we are left with no basis on which to judge the truth or falsity of future predictions and with no logically derivable rule which can be applied in making future predictions. Historical laws are thus equivalent to a series of guesses about what forms a society will take at particular times in the future. In contrast, the standard laws of social science are universal in form; they are applicable to any situation which corresponds to the initial conditions specified in the statement of the law. Since no term in the statement of a universal social law may be a proper name such laws are typically applicable to a large number of different times and places. They are thus subject to repeated testing and do yield rules by which future predictions may be made.

A second difference between historical laws and universal social laws arises from their different sources. It has been argued that since historical laws are "derived" from historical research they are at least indicative of "obvious" historical trends and may, in fact, suggest patterns which are non-logical but highly "useful." This type of contention is, however, open to question in more than one respect. First, history is not merely a statement of all past occurrences. It is an interpretation of those particular events or

aspects of events which a certain historian has found interesting, informative, or of relevance to one of his own speculations. Even if an attempt were made to write a "comprehensive" history of future events it would be foredoomed to failure. Everything which occurs in even a very small and simple society is simply unrecordable, and without some decision about what is important it is impossible to decide what should be left out of the record. Second, it is impossible to derive or infer universal statements from particular statements or particular statements from other particular statements. Any assertion of a "trend" is thus no more than a disguised assertion of a historical law and must be treated as such.³ In fact, any attempt to uniquely connect a particular universal hypothesis with a finite set of historical facts is similarly foredoomed to failure, for it is impossible to derive a universal statement from a finite set of particular statements which do not exhaust the domain of discourse to which the universal statement refers.⁴

In order to clarify the remarks in the last sentence, and set the stage for a final point of criticism directed against the historical outlook, we must distinguish between the statement of a universal law and an empirical generalization. If it is possible to actually examine each item in our domain of discourse and determine whether or not a universal statement correctly describes some relationship between items (or between the qualities of particular items) then we can formulate a universal statement about this relationship which is either true of all items (or associations of qualities) or is false, (i.e., if we have available all rubies we can determine whether the

generalization "if anything is a ruby then it is red" is true of each item or if it is false of some⁵). Universal scientific laws, however, differ from empirical generalizations in that they refer not only to items or events which are available for our inspection but also to events which may be beyond our spatial or temporal reach--that is, they are empirical statements about class relationships which assert something beyond the immediately available evidence. This distinction, between universal statements which serve as empirical generalizations and universal statements which serve as scientific laws, suggests another possible criticism of historical laws, but we must lead up to this criticism by taking a somewhat roundabout path.

Let us assume that in some fashion we can state a particular historical law in the form of a universal proposition (perhaps by proffering a path of development along which each society must necessarily evolve). In postulating such a "law," however, we are still faced with one uncontrolled element: that of innovation. Although it is undeniably true that discoveries in the physical or biological sciences, or even in the science of economic or political organization, will have some impact on the future course of societal evolution, such occurrences are, by definition, unpredictable. If a scientific breakthrough is predicted then the prediction must contain some account of the specific nature of the breakthrough, in order to correctly evaluate its impact. But such a prediction would virtually constitute the discovery itself, and thus would not be a "prediction" of a "future" event. This seeming paradox flows from the basic character of a historical law and is completely avoidable in the case

of the more usual type of scientific relationships.

Since a historical law claims to definitely predict what will occur without condition it must necessarily account for all those events which actually will occur. (We limit our consideration to socially endogenous events such as discoveries, but there is actually no reason to do so. Hurricanes, earthquakes, volcano eruptions and other acts of nature should also be included.) The only evidence on which such a claim can possibly rest is the historicist's claim to have actually examined, or to actually have knowledge of, all future events. That is, the construction of a historical law is tantamount to a claim of prophetic insight. This problem does not arise with the usual scientific law, since the application of these proffered relationships always requires a fulfillment of the test conditions specified within them. That is, they are conditional statements to begin with and may well become simply inapplicable to certain situations as an alternative to being either true or false.

It seems rather likely that the above description of what is properly implied by the notion of an "historical law" reduces this concept to the point of absurdity--in the sense that no social scientist would care to defend it in its full and explicit formulation. That is, in fact, the case, as is clear from the reaction of a leading Marxist theoretician to Karl Popper's attack upon the Marxist form of historicism.⁶ What is of more interest, however, is the way in which the notion of "historical laws" is reworked so as to become acceptable. The usual behavioral pattern (for it cannot properly be referred to as an "argument") which is engaged in by those who endorse

the concept of historical laws is to (1) assert the "obvious intuitive appeal" of the notion of a law of historical change, (2) to proffer an example of such a relationship, coached, of course, in vague and undefined macro-terms (i.e., "class," "historical stage," "racial spirit," et. al.), and then (3) to immediately hedge against predictive failures of the "law" by noting that its conclusions refer to "historical necessities" not to temporally bounded predictions. This omission of any time frame within which predictions can be tested is the most frequent ploy of the historicist. It is especially out of character for any scientific inquiry to assume this form, however, for it implies no less than the fatalism of the ancient pre-scientific (and, indeed, pre-philosophic) Greeks. Nevertheless statements about necessary future social stages have increased in their popularity since at least the time of Comte and the English evolutionists, and it seems that we will "eventually" all be engulfed by the future utopia, given some indeterminately long period of time whether or not any of us care for the prospect.

FOOTNOTES TO APPENDIX C

1. This appendix is based primarily on the writings of Karl Popper, including The Poverty of Historicism (New York: Harper and Row, 1957), Sections 14-16, The Open Society and Its Enemies, Vol. ii, Second Edition (New York: Harper-Torch, 1967), pp. 264, 268 and Conjectures and Refutations, (New York: Harper-Torch, 1969), pp. 312-335, 336-340.

2. This is precisely the difficulty with Marxist and Christian (Augustinian) forms of the philosophy of history. While any type of historical inquiry is necessarily non-scientific, in the sense of proffering non-repeatable relationships, most do at least attempt some vague estimate of the time-frame within which their "predictions" are placed. An historical prediction without any specification of the time limits on predicted events is not only bad science but also bad prophecy.

3. It should be noted that Popper disagrees with this characterization of "trends," but it seems that his acceptance of the procedure, as within the bounds of scientific research, springs from his non-standard usage of the term.

4. This is not, of course, to assert that known facts are irrelevant to theory formulation. They may be "psychologically suggestive" of certain hypotheses, or, more important, they can serve to rule out certain hypotheses which would be immediately contradicted (or falsified) if proffered for testing. We must, however, assume, in this way of looking at things, that the rules for determining what are or are not facts (the "test-rules" of the subject-area) have been determined prior to the suggestion of any particular hypotheses. If this is not the case then we have no pre-theoretic facts at all for the testing of any proffered hypothesis.

5. It should be recognized that I am not, in this example, concerned with definitional issue of whether a stone must be red in order to be a ruby. The question is, rather, can we "verify" or confirm any conditional statement whose domain of reference is an inexhaustible class.

6. The interested reader should refer to Herbert Marcuse's "Karl Popper and the Problem of Historical Laws," Partisan Review, Vol. 36, No. 1 (Spring, 1959), reprinted in Studies In Critical Philosophy (Boston: Beacon Press, 1972), pp. 191-207, especially pp. 197-198 for Marcuse's denial of the existence of any social doctrine resembling historicism (in Popper's sense).

APPENDIX D

THE METHODOLOGY OF WILLIAM WHEWELL

In Chapter II we examined the views of Richard Jones with special emphasis on the "Prefatory Note" to his Literary Remains by William Whewell. This "Note" to the collection of Jones' economic writings is certainly the best known of Whewell's writings on the subject of political economy, but it is neither exhaustive of his contributions to that field nor does it begin to illustrate the breadth of his interest in and research concerning questions of economic and scientific methodology.

The number of secondary sources which mention Whewell's economic and meta-economic investigations can be counted on the fingers of one hand, but what little research has been done has been done well. Hutchison, for instance, in his Review of Economic Doctrines, 1870-1929, devotes a lengthy footnote to Whewell's early investigations into mathematical (or mathematicized) economics. He quotes lengthy passages from Whewell's writings concerning the long run and short run determination of price, the elasticity of demand for various commodities and his caveats concerning the overuse of mathematics in economic investigations. Hutchison, in fact, recognizes the close parallel between Whewell's attempt to translate Ricardo (and Smith) into mathematics and Marshall's similar endeavors some twenty years later.¹ The only glaring defects in his account of Whewell's writings are matters of omission rather than commission. His concern is unduly limited to the Cambridge Philosophical Papers on mathematical

economics, his selection of quotes from Whewell's writings might be improved in order to better corroborate his discussion, and he seems to have simply bitten off more than it is possible to fully digest within the limited space he allots to the topic.

Another popular source to the history of economic thought, Schumpeter's History of Economic Analysis, has the merit of at least recognizing Whewell's existence and mentioning his major works. Schumpeter accords Whewell lavish praise, both as regards his personal influence on academic contemporaries and his writings in the history of science, yet he displays little familiarity with the precise content of Whewell's philosophic writings and still less with his economic contributions.² Although defending his demand analysis against a contemptuous dismissal by Jevons, Schumpeter clearly believes that there was little original merit in Whewell's economic writings, and seems to classify him as a well intentioned but amateuristic dabbler in fields beyond his concern or competence.

A more recent and more appreciative evaluation of Whewell's economics is to be found in James Cochrane's "The First Mathematical Ricardian Model," appearing in the Fall, 1970 issue of the journal History of Political Economy.³ Although the author modestly states that his paper "concentrates solely on his (Whewell's) exposition of Ricardo" and looks forward to "a thorough evaluation of the quality of his (Whewell's) work and his influence upon the development of economics," this paper in fact, constitutes a major and important step in any more comprehensive evaluation of Whewell's writings. In addition to a carefully developed appraisal and critique of Whewell's

mathematical models it contains an extensive bibliography of secondary and primary sources which can only be considered as the definitive listing of material concerning his economic views. Cochrane also displays a more than passing knowledge of and appreciation for many of Whewell's non-mathematical contributions to the science, although these are, of course, relegated to a place of secondary importance in the context of his study.

Works concerned with Whewell's career as an academic administrator or his contributions to fields outside economics are numerous. Of these we need only mention the major studies by Todhunter and Douglas⁴ and the prefaces to reprints or collections of his writings authored by Herival and Butts,⁵ most of which are referred to at the appropriate point in the following discussion.

Whewell's Life and Writings

An excellent biography of Whewell's life and bibliography of those of his works published during his lifetime is to be found in Volume Twenty of the Dictionary of National Biography.⁶ For our immediate purposes, however, let it suffice to note that he was a friend of Faraday's, whom he advised on electrical terminology, and of Maxwell's teacher David Forbes.⁷ He was the author of "one of the earliest books written in English to make use of the calculus in solving dynamical problems" in mechanics and "laid the foundation of mathematical crystallography" in a paper read before the Royal Society of Science. He was knighted for his numerous studies on the tides, and made substantial contributions to the study of ecclesiastical

architecture and the "new mechanics" applied to engineering. He was also interested in and worked within the fields of theology, moral philosophy and meteorology (for the study of which he invented the "first self-registering anemometer").⁸

In this chapter we are primarily interested in and have restricted our discussion to Whewell's writings in mathematical economics, the theory and methodology of political economy and to his voluminous contributions to the history and philosophy of science. Of these works we may mention his History of the Inductive Sciences (three volumes)⁹ which first appeared in 1837 and was revised in 1847, his Philosophy of the Inductive Sciences (two volumes) which went through various editions from 1840 to 1860, his On the Philosophy of Discovery, 1856,¹⁰ which contains a chapter on "Mr. Mill's Logic," "M. Auguste Comte" and "Political Economy as an Inductive Science," and his "Six Lectures on Political Economy,"¹¹ prepared for the edification of the Prince of Wales at the request of his father, Prince Albert. Of Whewell's "lesser works" his three articles on the application of mathematical methods to political economy, which first appeared in the Cambridge Philosophical Society's Transactions for 1829, 1831, and 1850,¹² and his "Prefatory Note" to the Literary Remains of the Rev. Richard Jones have each provided valuable material. His correspondence, appearing in the second volume of Todhunter's William Whewell's Writings and Letters, has also been both a rich and entertaining source for the preparation of this appendix.

The following discussion is divided into three major sections. The first presents an abbreviated sketch of Whewell's philosophy of

scientific discovery and scientific investigation, concentrating upon his historical antecedents and the relation of his views to competing perspectives (i.e., those of J. S. Mill and Auguste Comte). Part two focuses in upon his more specific pronouncements concerning economic methodology and his differences with Richard Jones. A comparatively short concluding section then deals with a reappraisal of the substance and significance of his work within the realm of economic theory. While one can only concur in Cochrane's desire to see a "thorough evaluation" of Whewell's writings, it is hoped that this less pretentious outline of his economic and meta-economic views will help to fill a gap within the history of the Historical School in England.

Whewell's Philosophy of Science, An Introduction

Because of the volume of his writing concerned with the history and method of science and his differences with leading intellectuals of his day (i.e., J. S. Mill, John Hershel and Augustus De Morgan), Whewell's views concerning these issues were the subject of both contemporary controversy and successive reevaluation, continuing to the present day. Todhunter noted and commented upon numerous Nineteenth Century critiques and appreciations of Whewell's philosophic views, and himself added to this literature in the first volume of his William Whewell's Writings and Letters.¹³ In more recent times, Butts has compiled a list of nearly a dozen Twentieth Century books and articles devoted in whole or in part to a consideration of Whewell's views,¹⁴ and has undertaken to reevaluate Whewell's philosophy of

science in an introduction to a collection of his papers. What follows is based mainly upon Butts interpretation, corrected by the material in Whewell's more obscure essays and by his comments in correspondence with academic contemporaries. While a more thorough survey of Whewell's views is desirable, it is not possible within the confines of this paper.

In approaching Whewell's writings on the philosophy of science it is important to note that he meant by that term something much broader than do philosophers like Karl Popper. Rather than being restricted to points in the logical form and testing of scientific hypotheses Whewell's philosophy sprang directly from his more general positions in the areas of epistemology and metaphysics. He was concerned with the sociological process of the "discovery" (formulation) and acceptance of new hypotheses as much as he was with their testing and interrelationship. He, in fact, authored an entire volume to illustrate how previous scientists had gone about the work of constructing their speculative systems.

Whewell's doctrines concerning the philosophy of scientific inquiry are perhaps best understood if approached against the backdrop of his comments on and criticisms of other philosophers, of whom we may mention Comte, Mill, Hegel and Kant as of special importance. This way of obtaining an understanding of and appreciation for Whewell's views is specially appropriate since he himself devoted major parts of his philosophic works to the history of science and to philosophers concerned with science.¹⁵

Whewell on Mill

Mill earned Whewell's disfavor at an early stage for his connection with the London and Westminster Review and his association with the circle of Benthamite reformers.¹⁶ Although we are told by Butts that "Whewell's History of the Inductive Sciences furnished John Stuart Mill with most of the material for his third book of the Logic, a work Mill claimed could never have been written without Whewell's preparatory work,"¹⁷ and by Schumpeter that "as regards logical fundamentals, Mill leaned heavily on Whewell...,"¹⁸ Whewell's own opinions of Mill were somewhat different. The most generous thing he could find to say in correspondence with Hershel, who had assumed the task of Mill's defender, was that "I agree with you that the Logic is logical; also ... it is deadly dull."¹⁹ And in the same correspondence, dated March and April of 1843, he went on to remark that "He (Mill) is quite subjugated by one who I think a very bad philosopher, Comte" and "... he does not appear to me to be an ally to set much store by; for, although acute and able, he is ignorant of science and still entangled in the prejudices of a bad school (an obvious reference to the views of James Mill and Bentham)."²⁰

Some six years following this exchange Whewell published a rather detailed critique of Mill's Logic in a pamphlet entitled "Of Induction, With Especial Reference to Mr. J. Stuart Mill's System of Logic."²¹ This work remains as one of the key sources to his (Whewell's) philosophy of science in the more modern meaning of that term.

In brief summary, Whewell was concerned that Mill had used the term "induction" in an ambiguous and unclear manner, to refer both to the rough and ready psychology of "practical action" and to the formal procedure (for so he believed it to be) of scientific generalization. He also objected that Mill confused what today has been called "knowing how," i.e., "practical skills" or "animal tendencies to action" with the quite different "knowing that," i.e. that part of knowledge which is "especially and distinctly human."²² "Induction," Whewell protested, could not be applied to mere "learning from experience" in the sense common to both men and animals. Rather, it "must be confined to cases where we have in our minds general propositions" of some given "inductive science."²³ Induction, he maintained is "experience of observation consciously looked at in a general form (and) this consciousness and generality are necessary parts of that knowledge which is science."²⁴(emphasis in original)

Although he also took issue with Mill's views concerning the importance of hypotheses, his "four methods" of inquiry, his use of the term "facts" and his attitudes toward deduction and prediction, Whewell's most important objection to the Millian system was concerned with the process of induction (as opposed to the scope of that term). For Mill, the inductive process was a psychological movement of thought from certain particular facts (or sense impressions) to expectations concerning other particular facts, without any explicit intermediation of general or universal propositions ("principles") concerning the relationship of the first class of facts to the second. His conception was thus very much in line with the tradition of

associationist psychology, to which his father had been a major contributor, and was, as Whewell pointed out, based upon instinct rather than "reason."

Whewell's view of the inductive process was much more modern, although, like Mill's, it contained elements of the psychological along with the logical. For Whewell, induction was the process by which separate facts were successfully and explicitly "bound together" by "superadding upon them the conception of an ... Idea" (emphasis in original).²⁵ His "Ideas," which closely resembled a scientist's version of Kant's "forms," were primarily those "of Time, of Force, of Number, of Resemblance, of Elementary Composition, of Polarity and the like." The knowledge requisite to apply these correctly to any particular case "required a special preparation, and a special activity in the mind of the discoverer," i.e., it might be a priori, but was not innate or instinctual.

Although Mill's concern with the importance of facts was justified in the context of an age obsessed by absolutism, he had gone too far in maintaining that the introduction of new "Conceptions," and the new terminology which accompanied them, "superadded nothing to the facts which they served to bind together" (a philosophic point of view later known as "instrumentalism").²⁶ Whewell countered that the formation of "Conceptions" (or "the one true relation" best suited to bind together the facts of a field of scientific study) was the necessary subject of "controversies (which) make up a large portion quite as important as the study of facts ..."²⁷ He further believed that the introduction of new terminology, such as Kepler's

"elliptical orbit" or Newton's "gravitation," was a clear reflection of the real differences in the ways in which facts were organized by the new hypothesis, or "Conception," proffered for testing: that such terminology was a tool "enabling men to proceed from each ... discovery to other discoveries more general."²⁸ In response to Mill's particular characterization of the Keplerian theory as merely the "sum of different observations ... the separate parts of which had been separately observed" Whewell stated that "Kepler ... did not find (his theory) ... by merely adding together the observations. The fact of the elliptical orbit (of the planets) was not the sum of the observations merely; it was the sum of the observations, seen under a new point of view ..."²⁹ The only real ambiguity in this position follows from Whewell's failure to distinguish between logic and "right thinking," a subject considered further on page 487 of this section. Due to this defect in his perspective we are constantly left wondering whether his view is more akin to Popper's "critical rationalism," where "conceptions" must be subject to falsification. Or whether it is more like Kuhn's "normal science" and "paradigm formation," where central ("higher order") hypotheses and supporting terminology are subject to minor growth and modification, but can only be abandoned for sociological reasons--are never rejected on the basis of critical tests. The evidence for associating Whewell with either of these views is present in nearly all of his writings. In his critique of Mill, for instance, he clearly states that the restructuring and development of scientific hypotheses is primarily a linguistic game:

All the discussions and controversies respecting Ideas and Conceptions of which I have spoken, may be looked upon as discussions and controversies respecting the grammar of the language in which nature speaks to the scientific mind. Man is the Interpreter. The study of the language, as well as the mere sight of the characters, is requisite in order that we may read the inscriptions which are written on the face of the world.³⁰

As we have already noted, he believed it was the duty of science to seek for the "one true relation" in every type of phenomena and to work toward "absolute truth" about the world. Yet Whewell's dogmatic view of scientific goals was laced with a healthy skepticism regarding the distance already traveled and the distance yet to be traversed on the path to "absolute truth." In a paper published just before his death he warned those who were ready to make a social fetish out of science, those who sought to proclaim it as the guide for a complete reordering of all social relations, that "no science is yet complete,"³¹ and the social sciences were probably the least complete of them all.

Whewell on Auguste Comte

An interesting extension of Whewell's strictures on the Millian perspective and his general hostility toward epistemological empiricism (or "sensationalism") is to be found in his 1866 article, "Comte and Positivism" and his remarks on Comte, first added to the 1847 edition of his Philosophy of the Inductive Sciences. Whewell opened his criticism of Comte by noting that "It is plain ... from the whole course of his work that he holds, in their most rigorous form, the

doctrines to which the speculations of Locke and his successors led; ... which tended ... to the exclusion of all ideas except those of number and resemblance."³² That is, it was Whewell's opinion that Comte traced all knowledge to sense experience and memory and believed that the essence of these lay in associationist psychology and mathematical models of the world.

Having established the fundamental difference between Comte's philosophy and his own Whewell widened his criticism on three fronts: two philosophic and one factual. In matters of the history and present state of the various sciences it is apparent that Whewell regarded Comte as a virtual illiterate. Of his "theory" of the three stages in a science's development he stated: "That M. Comte's hypothesis is historically false is obvious by such examples as I have mentioned."³³ And he then proceeded to quote, at great length, Comte's most absurd misinterpretations of contemporary theories and to debunk his more plausible expositions.

Surprisingly, however, Whewell's central objection to Comte's Three Stages was that he (Comte) was obviously ignorant of the true relation between metaphysics and the progress of science. It was Whewell's belief "that the discussions concerning Ideas (by which he meant higher-order constructs) and real discoveries, have in every science gone hand in hand ... There is no science in which the discoveries of the laws of phenomena, when once begun, have been carried on independently of discussions concerning ideas (there is no science) ... in which the expression of the laws of phenomena can at this time dispense with ideas which have acquired their place in

science in virtue of metaphysical considerations."³⁴

Among the greatest of these "metaphysical" notions was one which Comte has specifically wished to excise from the scientific body-- that of "Cause." Whewell's understanding of "Cause" was precisely equivalent to the concepts of "higher level theories" or "hypothetical constructs": the stock and trade of Twentieth Century philosophers such as Ernst Nagel. For him causes took the variant forms of "force," "the notion of light," "atoms,"³⁵ etc. according to the particular science.

Whewell may also be interpreted, with some justice, as a forerunner of Popper's view regarding the productivity of daring metaphysical speculations in fostering scientific theories³⁶ and the continuing spur to physical speculations provided by conflicting metaphysical conceptions of the universe and the "essence" of the physical world. As Whewell noted, "Kepler's discoveries would never have been made but for his metaphysical notions. These discoveries ... did not lead immediately to Newton's theory, because [emphasis in original] a century of metaphysical discussions was requisite as a preparation."³⁷ We are thus, once again, thrown back on the core of both Whewell's and Popper's explanations for scientific advancement: the power of critical discussion.

Mill and Comte Reconsidered

Nineteen years after the appearance of his first critique of Comte's philosophy and sixteen years after his critical essay on Mill's Logic Whewell again returned to a consideration of their

respective philosophies. It is clear in this, one of his last articles, that he had altered few of his basic attitudes concerning matters of philosophy but concerning personalities he was of a somewhat different mind.

Mill especially is accorded greater respect. Whewell speaks of him as "the great authority" ... "no authority of our time could be greater" ... And states with apparent sincerity that "Besides Mr. Mill's profound philosophical thought and wide sphere of knowledge, the dignity of his position naturally makes us look where he points. His love of truth and fearlessness of consequences have given him an eminence which all must rejoice to see generally acknowledged."³⁸ Yet all is not yet forgotten or forgiven for in the very next column we read that "I have always regarded Mr. Mill's opinions with respect, and considered them interesting and important subjects of discussion, but that on many subjects I have held them to be erroneous, and have not scrupled to publish my reasons for thinking so. I must still keep the same attitude."³⁹ And of all these subjects the chief offender against Whewell's assent was "Mr. Mill's admiration for Auguste Comte ... (which is now, however) limited in many points, and balanced by something very like contempt as to his more recent doctrines ... "⁴⁰

Comte, himself, fares even worse than before, for Whewell is much more frank concerning his opinions of him. He was and is, we are assured, "a person whose want of knowledge and of temperate thought caused his opinions on the philosophy and history of science to be of no value."⁴¹ Many of the previous strictures on his

philosophy are repeated verbatim and to these are added a rather lengthy discussion of fixity in scientific perception⁴² (of what Kuhn has called "normal science" and Popper has called "bad science"), aimed, of course, at what Whewell saw as Comte's own brand of scientific religiosity.⁴³ Of other original points we may note that the list of "causes" has grown to include "chemical bonding" and "vital power," and that Whewell has resorted to many effective, if rather unprofessional, "pot-shots" at Comte's personal life, mental health and positive religion.⁴⁴ Whewell's more sophisticated appraisal of Comte's competence in discussing scientific matters, is, however, summarized at the conclusion of a lengthy discussion concerning the corpuscular theory of light: "I am," he says, "not going to trace M. Comte's views of the other sciences. He is, I conceive, very superficial in all and in some grossly erroneous."⁴⁵

The Metaphysicians--Hegel and Aristotle

Lest Whewell's remarks concerning the usefulness of metaphysical presumptions to the advancement of science be interpreted in a manner in which he did not intend, it is necessary to attend, for a moment, to his appraisal of two of the greatest metaphysical philosophers, Aristotle and Hegel. Of Aristotle's science, Whewell states "his fundamental error ... is very nearly the same as one of Francis Bacon's leading mistakes. Aristotle says that Science consists in knowing the causes of things, as Bacon aims at acquiring a knowledge of the forms or essences of things" (emphasis in original).⁴⁶ Yet as Whewell notes, from the perspective of his own "system": "sciences

do not begin with such knowledge, and ... in few cases only do they ever attain to it." Although not beyond human knowledge (as we have already noted in the previous section on Comte) the discovery of causes (that is, of broad and simple fundamental laws) "is a triumph reserved for the later stages of each Science, when the knowledge of the laws of phenomena has already made great progress."⁴⁷

In his correspondence with De Morgan, Whewell goes even further to attack the Aristotlian notions of "the Infinite," which he believed was better translated as "the Indefinite" and the idea of "the Absolute," of which he says "any assertion about it must be foolish nonsense" for the concept itself is unintelligible.⁴⁸

Whewell's opposition to "metaphysics," in the sense of either a priori knowledge or muddled thinking, is further illustrated in his examination of Hegel's philosophy. Although Whewell restricted himself to what he believed to be major inaccuracies in Hegel's understanding of Kepler and Newton, and to Hegel's adherence to the ancient division between celestial and terrestrial physics⁴⁹ in his public criticisms of that philosopher, he was much less reserved in his private correspondence. He there spoke of "Hegel's vagaries" with thinly veiled contempt, and even went so far as to state that "There is nothing which so entirely deprives me of all respect for German heads in the matter of reasoning as the way in which they have allowed Hegel to domineer over them. It appears to me that on every subject he is equally fanciful and shallow, though he may not be so demonstrably wrong as in the matter of Newton."⁵⁰

From the foregoing it should be amply clear then, that when

Whewell admonished Jones for his total rejection of metaphysics and himself defended its use in the development of scientific theories⁵¹ he had in mind something quite different from the speculative philosophy of Hegel or Aristotle. To see just what that something was, however, it is necessary to examine what Whewell had to say of the philosopher, Immanuel Kant, and what others have said of the relationship between Kant's philosophy and Whewell's own.

Immanuel Kant and William Whewell, Similarities and Distinctions

Despite Whewell's infatuation with Bacon during his undergraduate days at Cambridge his later philosophy is generally acknowledged to have moved away from Bacon and toward the views of Immanuel Kant. As Butts has stated in his excellent introduction to Whewell's Theory of Scientific Method:

Fundamental Ideas are what the activity of mind contributes to knowing. Whewell likens some of them, notably space, time, and number, to Kant's forms of intuition. Others ... play for Whewell something akin to the role of Kant's categories, though he does not use Kant's term to designate them. Furthermore in its treatment of some of the Fundamental Ideas ... Whewell's account of their Epistemological status deviates very little from the Kantian theory ... "52

Whewell's association with the great philosopher was also widely recognized, and even exaggerated much out of proportion, during his own lifetime.

Although wishing to avoid the impression that he was merely an English disciple of Kant's, Whewell himself was generous in acknowledging his debt. In his Philosophy of Discovery he wrote that

" ... I had adopted some of Kant's views, or at least some of his arguments. The chapters on the Ideas of Space and Time in the Philosophy of the Inductive Sciences, were almost literal translations of chapters in the Kritik der Reinen Vernunft."⁵³ Whewell was further adamant in the defense of Kant's efforts in and contributions to philosophy against the charge, leveled by Dugald Stewart, that all of his ideas had been fully anticipated--"by Price, by Cudworth, and even by Plato ... "⁵⁴

Even so, it seems reasonable to accept at face value Whewell's dissent from the characterization of his views as purely Kantian.⁵⁵ Our reasons for acknowledging this perspective, rather than relegating the entire controversy to professional vanity, lie in two distinct areas. First, Whewell consistently maintained that his position was significantly different from Kant's, and, as evidence, he quoted a contemporary Kantian's judgment that: his writings on the philosophy of science were "clearly incompatible with the ... views of Immanuel Kant."⁵⁶ Second, certain fundamental differences between Kant's and Whewell's philosophies are evident from even a superficial survey of his writings, and even more deep distinctions have been identified by modern commentators on his works. In order to more completely grasp the nature of Whewell's epistemological and meta-scientific perspective we will briefly consider the major deviations in his view from a pure Kantian paradigm.

It is true, as Butts has charged, that Whewell believed his "Fundamental Ideas" to be necessary truths, in the sense that the trained intuition of scientists would be incapable of "clearly and

distinctly" conceiving of another state of affairs.⁵⁷ Yet this assertion is not identical, as Butts has wrongly maintained, to the assertion of synthetic a priori truths. That is, Whewell, unlike Kant, is not so readily charged with the conclusion of logical and linguistic truth.⁵⁸

In a flight of fancy Butts assumed that since Whewell believed that no experience (for which we may read "experiment") was capable of confirming a "Fundamental Idea," then he must also have believed that no experiment was capable of falsifying one of these conceptions. There is some grounds for this confusion in Whewell's unfounded "extension" of the principle that "... facts cannot be expressed without theory ..." to support the quite different claim that there is ultimately one theory best suited to organize the data of any given field.⁵⁹ Yet there are also points in Butts own interpretation of Whewell's philosophy which run counter to this interpretation. For instance, Butts "resolved" a contradiction between Whewell's belief that Ideas cannot be proved by experience, and his quite contrary notion that Ideas are gained from and shown to be "necessary" by experience, by separating out two senses of the term "experience." Sometimes, he believes, Whewell had used the term to denote the un-systematic occurrences of everyday life and sometimes to refer to formal scientific experimentation.⁶⁰ While Butts is able to muster some support for this distinction from Whewell's writings,⁶¹ by adopting it he is left with two other "loose-ends" yet to be explained. How is it that the "clear and distinct" intuition of the "necessary truth" of the "Fundamental Ideas" was "a rare and difficult

attainment"⁶² if these ideas were like Kant's a priori conceptions of space, time and number? How is it that such truths were both "progressive" in their "clarity,"⁶³ and ultimately dependent for their truthfulness not on any clarity of perception but on their ability to "fit" the "constituent nature" of the phenomena under examination?⁶⁴

We have already noted Whewell's preoccupation with the use of "right terms" (the higher-order constructs associated with the introduction of a new paradigm) and his discussion of how such "technical terms" developed and were modified through a process of continuing debate within the scientific community. Yet it should be obvious that this same analysis provides the answers sought to the three questions above. The more casual type of experience, the "experience" different from formal experimentation, was, for Whewell, just this continuing informal debate between scientists already in possession of critical facts, but as yet possessing theories too underdeveloped to fully explain these facts. We have already mentioned, in a different connection, that this process of debate over new terms, "Conceptions" or "fundamental truths" is exactly what Popper had in mind when he spoke of "critical realism," just as the conditional hardening of a theory into a "necessary-seeming" truth is what Kuhn has described as "normal science." In a certain sense then, Whewell may be said to have anticipated both these points of view.

Butts' misunderstanding of these rather basic characteristics of Whewell's perspective would be much more forgivable had he not correctly identified many closely related features of his view. Butts, for instance, recognized that Whewell's theory of induction

"in its full form" was nothing less than an anticipation of the "hypothetical-deductive" view of science (a Twentieth Century doctrine) and that his "inductive tables" were attempts to trace out the relations between higher and lower order theories in the more well-developed sciences.⁶⁵ Even more remarkable, however, is Butts's exceedingly clear interpretation of what Whewell had meant by the increasing "clarification" or "advancement" of an Idea, i.e., an expansion in the generality of a scientific hypothesis and an increase in its simplicity (in the sense of increased freedom from restrictions on the class of phenomena to which it could be applied), and greater accuracy or consistency in yielding accurate predictions. Somewhat in contradiction to his original belief concerning the non-falsifiability of "Ideas" in Whewell's system, Butts also recognized that the "clarification" of an "Idea" is not a once-and-for-all change but a result of ordered modification.⁶⁶

Some of the difficulties regarding Butts' seeming contradictions can be resolved by reference to his own conception of induction and scientific method. In his comparison of Mill's "logic of confirmation" with Whewell's strictures concerning the impossibility of induction (in Mill's sense of that term) Butts clearly disclosed that his own view of science was basically "inductivist." This was further supported by his charge that Whewell was basically a "Platonic" philosopher who was concerned with the "classification of concepts"⁶⁷ rather than observations of the world. The fact that this view contradicts a passage from Whewell's writings, which he quotes several pages⁶⁸ later did not seem to be worthy of note.

More Valid Criticisms of Whewell's Philosophy of Science

There are, however, several critical points in Butts' evaluation of Whewell that did not go completely astray, and for the sake of balance and further insight into Whewell's most-difficult philosophic writings it is well to consider some of these. Butts is certainly justified in his scorn for Whewell's rather archaic conception of "Logic." As Whewell himself stated his position in controversy with De Morgan: "Logic has got her name by being supposed to have something to do with discovering truth" and should properly be concerned with 'the Art of Discovery' as well as syllogistic reasoning."⁶⁹ That is, according to Whewell, Logic is properly considered as "a schematic presentation of what had already been thought in more complex matters" (Butts' characterization).⁷⁰

It is also true, as Butts pointed out, that Whewell did not admit for discussion a sense of the term "induction" different from "the formal testing of hypotheses" and the study of their mutual inter-relationships. More precisely, he used the term to refer to the psychological process by which "concepts not before apparent" are "suggested" to the scientist.⁷¹ This, it must be admitted, is quite odd in the context of Whewell's other writings. He had specifically criticized Bacon, and others, for just such vagueness in the use of the term "induction."⁷² Yet the documentation for his "backsliding" is, however, unquestionably sound.

Summary Comments Concerning Whewell's Philosophy

The basis of Whewell's philosophy of science and of his epis-

temology should now be clear: his view was much like that of the modern philosopher Karl Popper as regards the role and testing of scientific hypotheses, and much like that of Thomas Kuhn as regards their sociological function within a science and the tendency for them to be built upon, undisturbed, for long periods of time. His system of analysis was not, however, developed to the extent of either of these modern points of view: it was overlaid with still unclear notions concerning both "induction" and the character of a "logical" analysis of scientific problems. We shall see, however, that these defects in Whewell's more general views about the philosophy of science had few negative consequences for his meta-economic analysis and in no way served to distort his views concerning economic theory. Certainly, Whewell's philosophy, even when it was in error, did not lead to anything as pitiable as the presumptuous and bumbling views of the late German Historical School. While his orientation toward questions of scientific methodology was considerably more "empirical" than that of the Orthodox economists, he exercised a healthy restraint over Jones' tendency to reduce all theorizing to historical studies. That Jones was induced to engage in the fruitful enterprise of formulating alternatives to the Ricardian orthodoxy, rather than interminably gathering data about national institutions, was probably a consequence of Whewell's advice and encouragement.

Research Into Meta-Economics

The central feature of Whewell's economics and meta-economics was that he considered the science of political economy to be an "inductive" rather than a "deductive" study. To fully appreciate

his economic methodology we must thus turn to the definitions he offered for these central terms ("inductive science" and "deductive science") and to the differences which arose between his views concerning the character of economic studies and the views of other "inductive economists."

Firstly, it must be noted that Whewell, unlike Jones, was quite willing to apply rigorous deductive methods to existing economic theories in order to trace out their necessary consequences. This was, in fact, his avowed purpose in his three lectures on the application of mathematics to economics, the mathematics itself serving only as a handy and widely recognized tool for achieving this purpose.⁷³ Whewell thus recognized the distinction between "proving assumptions" and "deducing conclusions" from these assumptions,⁷⁴ no mean achievement in his day, and he clearly held the former enterprise in greater esteem than the latter.⁷⁵ He himself, however, remained content to pursue the mundane enterprise of checking on the consistency of theories and predictions while leaving the more glorious task of empirical study to Richard Jones. The modesty which he expressed regarding his own achievements in the field of political economy and the praise which he lavished upon Jones' work was thus an honest expression of his true assessment concerning the worth of their relative labors.

While it is clear what sense the term "deduction" had in Whewell's writings on the philosophy of science and on the topic of mathematical economics, his use of this same term in application to the non-mathematical writings of the Ricardian School has served as

a constant source of confusion for both his contemporaries and modern historians. The intent of his criticisms of the "deductive school" are not that unclear, however. They can be found tersely summarized in his published writings and are easily reconstructed from his available correspondence. In Whewell's Six Lectures On Political Economy, for instance, he states that: "Their (the Ricardians') method consists in taking definitions and reasoning downward from them, as is done in geometry ... We take the definition of Rent (for instance) ... and we come to the proposition that Rent is the excess of good soil above the worse."⁷⁶ To rephrase this quote: The Ricardians are purporting to say something about the world as it exists, or the part of that world known as the use and distribution of land, by thinking of some arbitrary definition of "Rent" and seeing what follows from this definition. In his "Prefatory Note" to Jones' Literary Remains Whewell notes an additional feature of the "deductive view": "... certain definitions were adopted (by the Ricardians) as of universal application to all countries upon the face of the globe and all classes of society; and from these definitions and corresponding axioms was deduced a whole system of propositions, which were regarded as of demonstrated validity."⁷⁷ Whewell's criticism of the "deductive school" is thus summarizable as two interrelated propositions or methodological rules. First, terms (or, we might say "data categories") should be constructed as to reflect the major problems with which the scientist is concerned and the types of evidence which he will have available. This was not, however, the procedure of the Ricardians in dealing with the

causation of rent nor of Malthus in dealing with population. Second, those terms or data categories which may be appropriate under one set of institutional and customary constraints may not be appropriate in a dissimilar setting, i.e., it is useless to talk about the free-market price of land services if all land is State owned. In such a situation "Rent" would refer to the price set by the governing body and "the causes for rent" to the incentives faced by the governing body to raise or lower this price or to distinguish different types of land.

Viewed in the above manner Whewell's concept of and objections to deductive economics are not so outlandish as they might superficially appear. It is, therefore, unfortunate, that he was unsuccessful in fully defining the correlative concept of inductive economics, i.e. his own alternative to the orthodox view. A possible justification for this is, however, found in his correspondence with Jones: that is, it seems that Whewell believed that "induction" could not be well-defined in economics at that stage in the discipline's development, since the fundamental concepts around which the inductive process revolved had not yet been fully settled upon.⁷⁸ Until the Ideas of Fundamental Concepts of economics were fully enunciated there was room only for a systematic collection of facts, and not for "induction" in the stricter sense of that term.

Whewell did, however, express a preference as to the particular type of research which would be most likely, in his estimation, to provide the material from which the fundamental categories of economics would eventually arise, as well as a pattern to be adopted by

future researchers in the social sciences. In his description of the "inductive course" of Jones writings he says: "He took a survey of the tenure of land, and of the conditions on which it is and has been cultivated, carrying out his examination by bringing together the accounts of all countries and all ages. And the result of this survey was, that the rent of land--the payment which the cultivator makes for its use--cannot be described by any one single definition from which its amount can be deduced, and can be understood only by dividing such payments into certain large classes; the effect of such a separation ... being, that we can then point out the bearing--very different in the different classes--which these payments have upon the comfort, prosperity and progress of each society."⁷⁹ Thus the "inductive economists" starts from problem to be solved, i.e., the determinates of national development, and then proceeds according to the dictum of Lao Tsz: "The beginning of wisdom is the ability to make distinctions." The ultimate aim of the economist is not, however, to collect data and arrange it into different classes, but rather "to see what propositions can truly be asserted concerning each class."⁸⁰ The same caveat against provincial attitudes which was leveled against the deductive method also applies in the case of induction: "We must classify facts which we observe and take care that we do not ascribe to the facts in our immediate neighborhood or specially under our notice, a generality of prevalence which does not belong to them ... We must ... be sure that we have obtained the narrower generalizations [concerning individual institutional structures] before we attain to the widest [concerning "rent" or "wages"

or "population," in general]" (explanatory phrases added).⁸¹

If these basic ground-rules of inductive inquiry had been applied to economics early in its history Whewell believed that "they would have saved the earlier speculators on this subject from some splendid errors ..."⁸² But he was liberal enough to admit that certain psychological advantages had resulted from the deductive course of early economics: "if these earlier speculations had not been thus bold," he said, "the science could not so soon have assumed that large and striking form which made it so attractive, and to which it probably owes a large part of its progress."⁸³

Somewhat surprisingly, the only example, other than Jones, which Whewell could find to illustrate the inductive mode of economic inquiry was the later editions of Malthus' Essay On Population, "excluding, of course, his anticipatory thesis" (of the numerical increase of food supply and geometric increase of population).⁸⁴ Malthus may have been a bad selection in light of the availability of other authors such as Petty and Hume, especially since Whewell had harshly criticized his "anticipatory thesis" in other writings and quite clearly understood that he had an axe to grind (or a point of view to defend) even in the later editions of his Essay.⁸⁵ The only reasonable justification for this choice seems to be that Whewell was not all that familiar with the history of economics before Smith and believed Malthus to be a good example of the breakdown of a deductive thesis (his admission of "moral restraint" to his previous list of checks to population growth) in the face of contradictory evidence.

Other Aspects of "Inductive Economics"

It is clear from the foregoing discussion that Whewell's "inductive method" involved a significant degree of respect for the impact of custom and institutions, "the different forms of society," on the "principle motives which operate on men regarded as masses."⁸⁶ There are, however, certain aspects of the "inductive view" which are not so immediately discernable from his basic methodological prescriptions. We now wish to consider a number of these refinements on his basic position which were never fully developed and too often were confined strictly to his correspondence.

Whewell, first of all, was not so naive as to expect that all institutional or customary differences in the various societies would prove of interest to economists. He noted that when dealing with complex phenomena it was necessary to include "all the predominant causes which really effect the result" (emphasis in original) but recognized that one could legitimately neglect "those agents" the effects of which were of "small amount or short duration."⁸⁷ This procedure would, it was true, yield only "first approximations" to existing conditions and related predictions, but in the present state of sophistication concerning social inquiry little more was attainable.⁸⁸

Second, the operational or empirical criteria which Whewell used to judge all attempts at economic theorizing lead him to adopt a rather unpopular limitation on the scope of economic inquiry. Quoting with approval a passage from Malthus' Definitions In Political

Economy he maintained that: "if Political Economy were to embrace a discussion of the production and distribution of all that is useful and agreeable, it would really be the best treatise on Political Economy."⁸⁹ Such a limitation would, of course, exclude what eventually would become known as "utility theory," but for Whewell the importance of including within economic inquiry only those variables which were readily identifiable through intersubjective techniques⁹⁰ far outweighed the importance of gaining an all encompassing explanation for any conceivable human act.

We have already seen (pages 489 and 490) that Whewell's empirical orientation also led him to distinguish between appropriate and inappropriate types of terminology, especially as regards the "fundamental concepts" of an "inductive economics." Yet we have not explored specific example of what he believed to be "the correct sense" in which to use economic terms. His doctrine at first seems but a superficial reworking of medieval ideas concerning natural concepts (or "right definitions"), especially in passages like the following: "The science of Political Economy does not rest upon Definitions. It rests upon facts. But facts are to be described in a general manner--that is, by means of terms. And these terms should be chosen so as to enable us to assert true (i.e. "factual") Propositions."⁹¹ Yet a little deeper probing reveals that Whewell's actual view resembled more closely the position arrived at by Ludwig Wittgenstein in the mid-Twentieth Century than it did any medieval or Scholastic conception. That is, Whewell believed that the factual or "True" sense of a term is its sense in "ordinary usage" (his own

description).

In applying this criterion of correct linguistic usage Whewell argued that the distinction between productive and unproductive labor was firmly grounded in the ordinary language, had an identifiable (operational) significance in the theories of economics and thus, on both counts, should be retained. He also argued that the Ricardian usage of the term "rent" conflicted with ordinary usage, was empirically inseparable from rent in the common sense and thus should be abandoned.⁹²

From these same philosophic roots of ordinary usage and operational theories Whewell derived an attack on the Classics' talk of "tendencies." While he recognized that there was some sense in saying that a theory might yield predictions which were only "approximately true" he also realized that to say a theory asserted only tendencies was to say nothing concerning countervailing forces or the relative weights to be attached to the different casual agents.⁹³ The Classics' beliefs concerning the role of economic theory in the prediction of social events were thus tantamount to the belief that such theories were ultimately useless. The device that had been introduced to protect economic inquiry thus bred its own destruction.

Ricardo's Empirical Postulates

Finally, it should be noted that Whewell had many, less philosophic, objections to the Ricardian tradition in Political Economy. In large part he held that Ricardo had simply misstated the facts which were clearly evident around him and had then used the

conclusions from his fanciful analysis to proclaim on matters of policy.⁹⁴ Often Whewell was more concerned to set right the logical and epistemological errors of the Ricardians rather than engaging in a debate over facts, but he was not above an occasional guarded jab at "the absurd presumptions of the Ricardians." In a quote discovered by Wesley Mitchell he states, in reference to his own work in mathematical economics, that: I must, however, assert that if they (his formalizations of the Ricardian doctrines) are useless and inapplicable the fault resides in the postulates which I have borrowed from Mr. Ricardo ... and not in the (mathematical) mode of deducing the consequences of these postulates."⁹⁵ And in his Six Lectures On Political Economy he cites several instances relating to the conditions of land tenure and the mobility of labor and capital where "the nature of agricultural progress ... is not that which is supposed in the theory as stated by Ricardo ... " (emphasis in original).⁹⁶ We are now, however, treading dangerously close to matters which are properly treated under our next topic, Whewell's doctrines and achievements within the field of Political Economy proper, and having exhausted his major achievements in other related fields it is to that topic that we now turn.

Whewell's Political Economy

Apart from what Schumpeter has called Whewell's "sense of quality"⁹⁷ in editing the Literary Remains of Richard Jones the most mentioned of his economic writings have been his three papers on the application of mathematical methods to economic deduction. Whewell

explained at length the advantages of a mathematical formalization of economics in his first paper on the "Mathematical Exposition of Some Doctrines of Political Economy" (1829). This passage, which rivals and perhaps surpasses the similar apologia for mathematical economics in Jevon's Theory of Political Economy, proceeding by a comparison which is itself of some interest between past developments in the history of mechanics and Whewell's hopes for the future advancement of economics. He noted that "without the aid of consistent mathematical calculation ... There would have been three errors difficult to avoid" in the mechanical sciences. That of (1) "assuming ... principles wrongly ... or (2) (of) reasoning falsely from them in consequence of the complexity of the problem (or) (3) ... of neglecting the disturbing causes which interfere with the effects of the principle forces."⁹⁸ Yet with the wide acceptance and use of mathematical methods in mechanics "It made it necessary to state distinctly ... assumptions, and these were open to a thorough examination; it made the reasoning almost infallible; and it gave results which could be compared with practice so as to show whether the problem was approximately solved or not."⁹⁹ Although realizing the controversy which would inevitably arise from any attempt to undercut the autonomy of the moral sciences Whewell was willing to proclaim his faith that "the science of Mechanics and Political Economy are so far analogous, that something of the same advantages (realized in mechanics) may be looked for in the application of mathematics in the case of Political Economy."¹⁰⁰

As the years passed Whewell became increasingly bold in his

advocacy of a thorough-going translation of economics into mathematical form. Although in his first paper he had been careful to limit his efforts to select topics in the areas of rent theory and taxation, claiming only that "some parts of this science ... may be presented in a more systematic and connected form ... by the use of mathematical language,"¹⁰¹ by the time of his second paper (in 1831) he felt confident enough to state that "Mathematics is the logic of quantity, and will necessarily, sooner or later, become the instrument of all sciences where quantity is the subject treated, and deduction the process employed."¹⁰² Even at this date, however, his confidence in the new mode of treatment was none too firm, for reading on down that very page we find "I am, however, well aware that the pretensions of Political Economy to such a scientific character, are as yet entirely incapable of being supported. Any attempt to make this subject at present a branch of Mathematics could only lead to a neglect or perversion of facts, and to a course of trifling speculations, barren distinctions, and useless logomachies."¹⁰³ It took nearly twenty years before Whewell would again ignore his own admonitions, but in 1850 he penned his last and most sophisticated "mathematical memoir" dealing with J. S. Mill's exposition of international trade, and containing, in embryonic form, a discussion of the fundamentals of what would eventually be known as Neoclassical microtheory.

Despite his pioneering work in the field of mathematics as applied to economics Whewell was never completely sanguine about possible abuses of these techniques. As Hutchison so well states,

and as we have already indicated, he was "highly cautious and critical about its application"¹⁰⁴ and was quick to notify his readers that the results of mathematical inquiry could never "supply the place of moral reasoning" nor could one ever "obtain any results of a nature different from those which we can obtain otherwise."¹⁰⁵

The primary target of Whewell's caveats concerning the abuse of mathematical models was the concept of equilibrium. In commenting on the process by which the rate of profit would come to be equalized in all areas of production he added "this is a process which manifestly would require a considerable time, and would never be completely performed ... the changes ... which it supposes would require ... a considerable course of years, during which new causes and circumstances might come into action, so as entirely to modify the result, even if the tendency of the original causes had been rightly stated."¹⁰⁶ Although this was a problem which arose in all long term economic predictions, with those involving "normal price" as well as those involving wages and population, and although there seemed to be no other systematic tool which could be used to attack such problems: "In reality ... equilibrium is never attained: probably in most cases it is never approximated to."¹⁰⁷

The alert reader cannot help but recognize that Whewell's antipathy toward the concept of equilibrium is mirrored in our own day by economists such as Ludwig von Mises, Israel Kirzner, Murray Rothbard, and Robert Lachmann. This is, in fact, no accident for these "modern Austrians" are in large part identifiable as modern disciples of William Whewell. Not only did they adopt his attitude toward

equilibrium, but they also adopted his arguments, nearly word for word, to justify this opinion. Von Mises, in fact, would also adopt, and misinterpret, his Kantian formulation of the "epistemological foundations" of economic science and would build upon a suggestion in his correspondence for using "human action" as the "fundamental conception" of the moral sciences.

The Austrian's attempt to base a rejection of social prediction upon Whewell's comments concerning equilibrium is almost certainly a distortion of his view (although it is not therefore invalid within itself). But it is easy to see how such a view might be derived from an isolated consideration of comments such as the following:

There is a constant tendency towards the state of (equilibrium) ... but this is a tendency like that which the waters at the source of a river have to descend towards its mouth. We cannot from such a tendency infer that the whole course of a river is at the same level; and just as little may we flatter ourselves that we have solved the problem of the course and distribution of the current of wealth, when we have combined the laws according to which an exact balance might be produced.

We are to recollect therefore, that even if our principles were exact, deductions from them made according to the method (of equilibrium models) ... would give us only a faint and distant resemblance of the state of things produced by the perpetual struggle and conflict of such principles with variable circumstances."¹⁰⁸

As we have already seen, however, Whewell was considerably more optimistic than this quote alone might indicate for the eventual progression of economic methods to the point where they would serve as a guide to political policy. That he was not at all satisfied that it had yet reached that point and was frankly horrified by many

of the bromides bandied about by the Benthamite reformers is evident from his correspondence.

Returning to Whewell's own caveats concerning the abuses of mathematical methods, however, there was at least one of his objections that is as applicable and as "well-taken" in our day as it was in his. In his last "Memoir" (1850), after again summarizing the advantages to be expected from an increased use of mathematics in economic studies, he states:

It would, however, be to take a very erroneous view of the consequences of this application of mathematics to Political Economy, to suppose that it can add anything to the certainty of the fundamental principles. There is perhaps in some persons a propensity to believe that any subject, when clothed in a mathematical shape, acquires something of mathematical demonstrative character; and that by applying mathematics to assumed principles of knowledge, we in some measure create a science. I must beg leave very distinctly to repudiate all pretensions of this kind ... All that we pretend to say is, that if the conclusions be false, the fallacy must be in the principles, if the process of deduction be truly mathematical."¹⁰⁹

Finally, in evaluating the relative importance of the purely deductive enterprise of mathematical formalization against those "speculations of Political Economy ... which are employed not in reasoning from Principles, but to them: in extracting from a wide and patient survey of facts the laws according to which circumstances and conditions determine the progress of wealth and the fortunes of men" Whewell was not hesitant to declare his own inferiority (or that of his enterprise) and to extend to Jones a rather direct complement. "I am perfectly ready to admit," he stated, "that the discovery of such laws, and the investigation of their consequences, is an

employment of far higher philosophical dignity and importance than any office to which Mathematicians can aspire."¹¹⁰

Other Contributions to Political Economy

Although Whewell's skill in arranging and editing Jones' economic papers and his own early writings in the "mathematical translation" of economic doctrines have been the features of his works most widely recognized in economic circles many of his other contributions to the subject have been at least mentioned by past historians. Hutchison, for instance, has recognized that he accurately stated the theoretical determinants of both short run and long run equilibrium price, that he provided a "thorough mathematical analysis" of the elasticity of a demand curve and a classification of goods as Necessities or Luxuries on that basis, and that he traced the relationship between the elasticity of demand and changes in total revenue with a given change in price.¹¹¹

Cochrane has noted Whewell's skill in dealing with the question of durable inputs in investment and production.¹¹² And even Schumpeter had some kind words in defense of the originality of his demand analysis.¹¹³

Yet in some instances Whewell's commentators have been simply too generous in their appraisals of his originality and understanding, and in other instances they have overlooked anticipations of important theoretical points. Cochrane, for instance, states that "Whewell to some extent anticipated the advent of marginalism. It is not without interest that every proposition of his exposition of Ricardo's

Principles was based on incremental changes."¹¹⁴ The evidence for this claim is, however, non-existent and the actual case is probably quite the opposite. It is clear from the discussion in parts of Whewell's Six Lectures On Political Economy that a great deal of the ambiguity which he discovered in Ricardo was due to his own complete ignorance of the marginal principle.¹¹⁵ And it can hardly be claimed that working with units such as "one acre of land" or "one laborer laboring for one year" is working with "increments" in any sense of that term acceptable in the calculus.¹¹⁶

Further, Whewell's critical remarks on Say's Law, which so delight Hutchison, are totally without theoretical foundation. They, in fact, reflect his rather marked confusion between the rise in the price of one good and a rise in the general price level: a confusion which was to become popular among later historicists (i.e., T. E. C. Leslie).¹¹⁷

Finally, we must mention several of Whewell's contributions generally overlooked in past accounts of his writings. He demonstrates a fair appreciation for the structure of markets: commenting both on price dispersal and on the effects of changes in search time on the prices paid for goods.¹¹⁸

His demand and supply analysis is refined far beyond anything written at his time. In addition to the points already mentioned it included a formal mathematical exposition of the difference between changes in demand and changes in the quantity demanded, an explanation of changes in elasticity along a linear demand curve¹¹⁹ and a statement of the analytic necessity for determining the quantitative

dimensions of demand and supply as well as for considering their general properties.¹²⁰ Whewell must also be credited for a formal presentation of the principles of capital discounting when comparing the profitability of two invest projects of different length (time duration), and increasing cost industries.

Concluding Evaluation

Despite Whewell's significant contributions to what might have been the more rapid advancement of economic theory his works remained virtually unnoticed for nearly a century. It is significant that even Marshall, who is claimed to have read virtually every economist up to his own time, demonstrates a complete lack of awareness regarding Whewell's writings (excepting, of course, his edition of the Literary Remains of Richard Jones).

Despite the great promise which Whewell had originally foreseen for both the refinement and formal development of economic theory and for the empirical researches of Richard Jones, he was to end his economic speculations on a rather sour note. In one of his last communications with Jones, written just a few months before the latter's death, he observed that, "Such a scientific exposition (as you have given and may yet give anew to the world) may be of use when men become sane on such subjects; which, at present, they do not appear to me to be. It seems to be an absurd and humiliating result of so many years spent by Englishmen in the speculations of political economy, that the governing body should carry on the business of the nation on the basis of fallacies so very palpable (as those which

have recently become popular) ..."¹²¹ Thus, despite the strictly positive character of much of his research Whewell became discouraged with further efforts not because of his fellows' persistence in fallacious methodological practices, but, rather because of the popularity of economic policies which he found personally distasteful. It is unfortunate that this preoccupation with the normative dominated not only the Orthodox school of Nineteenth Century British economists but also many of the later writers in the British Historical School.

Footnotes to Appendix D

1. T. W. Hutchison, A Review of Economic Doctrines, 1870-1929 (Oxford: Clarendon Press, 1953), p. 65 fn.
2. Joseph Schumpeter, History of Economic Analysis (New York: Oxford University Press, 1954), pp. 448-449 fn.
3. James Cochrane, "The First Mathematical Ricardian Model," History of Political Economy, Vol. 2 (2) (Fall, 1970), pp. 419-431. For further comment on the purely technical consequences of Whewell's critique and reformulation of Ricardian rent theory see the appendix to Hans Brems', "Ricardo's Long-Run Equilibrium," History of Political Economy, Vol. 2 (2), (Fall, 1970), pp. 242-243.
4. For Todhunter's appraisal of Whewell's life and works see Issac Todhunter, William Whewell, An Account of His Life and Writings With Selections From His Literary and Scientific Correspondence (London: Macmillan & Co., 1876), Volume I (hereafter referred to as Todhunter I). The less academic and more personal account of Douglas is also instructive for the light which it throws on Whewell's personality and academic career, see Janet M. (Mrs. Stair) Douglas, The Life and Selections from the Correspondence of William Whewell (London: Kegan Paul & Co., 1881).
5. For Herivel's informative, but often rather muddled survey of Whewell's scientific achievements and philosophic views see William Whewell, The Philosophy of the Inductive Sciences, Two Volumes with an Introduction by John Hervivel (New York: Johnson Reprint Company, 1967, pp. ix-xxxviii (hereafter abbreviated as PIS). For a somewhat biased, but well-argued, evaluation of Whewell's philosophy of scientific method see Robert Butts (ed.) William Whewell's Theory of Scientific Method (Pittsburg: University of Pittsburg Press, 1968), pp. 3-29 (hereafter referred to as "Butts"). Also of interest for those interested in a more comprehensive discussion of Whewell's views concerning the philosophy of science (including issues in both the physical and social sciences) is the list of articles and other secondary sources appearing in Butts, op. cit., pp. 339-341.
6. Sir Leslie Stephens (ed.), Dictionary of National Biography, Vol. XX (London: Oxford University Press, 1899), pp. 1365-1374.
7. PIS, op. cit., p. xi.
8. Ibid., pp. xv-xix.
9. William Whewell, History of the Inductive Sciences, Vols. 1-3 (New York: D. Appleton & Co., 1865).
10. William Whewell, On the Philosophy of Discovery (New York:

Burt Franklin, 1971) (hereafter referred to as POD).

11. William Whewell, Six Lectures on Political Economy (1861) (New York: Augustus Kelley, 1967).

12. William Whewell, Mathematical Exposition of Some Doctrines of Political Economy (1829, 1831 & 1850) (Westmead, G. B.: Gregg International Pub., 1968) (hereafter referred to as MP, I, II, III).

13. See Todhunter I, op. cit., pp. 101-102, 129-130 for citations to contemporary criticisms of Whewell's views.

14. Butts, op. cit., pp. 340-341.

15. That Whewell's views were always developed within the context of a history of scientific thought and continually referred back to this history in illustration of his various views is explicitly mentioned by Whewell, himself, in Butts, op. cit., p. 279.

16. Todhunter II, op. cit., pp. 272-273.

17. Butts, op. cit., p. 47.

18. Schumpeter, History of Economic Analysis, op. cit., p. 450.

19. Todhunter II, p. 314.

20. Ibid., pp. 314-315.

21. Reprinted in Butts, op. cit., pp. 265-308.

22. Butts, op. cit., p. 268. The notion of "central propositions" is almost certainly Kantian in character, even though Whewell specifically denied any strict adherence to the epistemology or methodology of Kant. It is of interest, however, that Whewell recognized a class of ascientific characteristics of human action, and that this class is quite similar to Gilbert Ryle's "propensities to act" or "mental predicates" (see Gilbert Ryle, The Concept of Mind (New York: Hutchison's University Library, 1949). Whewell's distinction between the properties of action which are within the confines of science and those which are not is sketched in the following quote from his essay on Mill's Logic:

The elements of such ("Inductive" or empirical) sciences are principles which we know; truths which can be contemplated as being true. Practical habits, practical skills, instincts and the like, appear in action, and in action only. Such endowments or acquirements show themselves when the occasion for action arrives, and then, show themselves in the act; without being put, in the form of truths

contemplated by the intellect. But the elements and materials of Science are necessary truths contemplated by the intellect.

Butts, op. cit., p. 270

23. Ibid., p. 269.

24. Ibid., p. 270.

25. Ibid., p. 278.

26. Ibid., p. 280.

27. Ibid., p. 279.

28. Ibid., p. 280.

29. Ibid.

30. Ibid., p. 281. The discussion dealing with man as the "interpreter of nature" may itself be interpreted as no more than an expression of the function which hypotheses and systems of hypotheses play in organizing and predicting observation statements. On the other hand, in the light of the Kantian bias implicit in much of Whewell's work this phrase may, rather, refer to an implicit belief in the "theory laden" character of all observations (i.e., a belief that all of science is little more than a linguistic game concerned with "puzzle-solving" or spinning out what is already imbedded in our use of certain explanatory terms.)

31. William Whewell, "Comte and Positivism," Macmillan's Magazine, Vol. XII (77) (March, 1866), p. 358.

32. PIS, II, op. cit., p. 321.

33. Ibid., p. 324.

34. PIS, II, op. cit., p. 322.

35. Ibid., pp. 32-325, see also p. 356. For an extension of Nagel's views as applied to the questions of economic methodology see his "Assumptions in Economic Theory," reprinted in William Breit and Harold Hochman (eds.), Readings in Microeconomics (New York: Holt, Rinehart & Winston, Inc., 1968), pp. 60-61, especially pp. 61-62 which contain the material dealing with "theoretical terms."

36. PIS, II, p. 322.

37. Ibid., pp. 322-323.

38. "Comte and Positivism," op. cit., p. 353.

39. Ibid.
40. Ibid.
41. "Comte and Positivism," op. cit., p. 353.
42. "Comte and Positivism," op. cit., pp. 354-355.
43. Ibid., pp. 355-356.
44. "Comte and Positivism," op. cit., pp. 359-362.
45. Ibid., p. 356.
46. POD, op. cit., p. 28.
47. Ibid.
48. Todhunter, II, p. 432.

49. POD, op. cit., pp. 512-513. In these and subsequent pages Whewell quotes some rather ludicrous and quite devastating passages from Hegel's works on Physics, in illustration of the primitive character of his thoughts concerning that subject.

50. Todhunter, II, p. 353.

51. In a letter dated August 16, 1822 addressed to Richard Jones, Whewell explained his use and understanding of the term "metaphysics":

... touching metaphysics, I am almost surprised at the fierce burning of your indignation against the poor word. It is no doubt true that people apply it to the speculations of others when they want them to appear abstruse and unsubstantial, and to their own when they would have them seem profound and philosophical, but if there were no such word they would find some other to answer those laudable purposes.

... I think that if you will not allow "metaphysical" to have its application to the philosophy of mind you will find no precise use for it. In general it seems to mean either that which depends upon the examination of our intellectual powers and properties ...

Todhunter II, op. cit., pp. 48-49.

Translating this into more modern views concerning empirical and linguistic questions it would perhaps be not too distortive to equate Whewell's interpretation of "metaphysics" with inquiries aimed at clarifying linguistic questions.

52. Butts, op. cit., p. 6.

53. POD, op. cit., p. 335.
54. POD, op. cit., p. 333.
55. An attempt to reinterpret Whewell as a pure Kantian is cited in Butts, op. cit., p. 343 fn. as George C. Seward, Die Theoretische Philosophie William Whewells und der Kautische Einfluss (Tubingen, 1938).
56. POD, op. cit., p. 335. Whewell himself felt that his contemporaries had done Kant an "injustice" by confusing his (Whewell's) methodological discussions with Kant's epistemological writings. (Ibid., p. 334).
57. Butts, op. cit., p. 65.
58. The distinction between linguistic and empirical truths was clearly recognized in a passage from Whewell's writings appearing in Butts' own collection of his methodological papers. See Ibid., p. 281.
59. "Auguste Comte and Positivism," op. cit., p. 355.
60. Butts, op. cit., p. 10.
61. Ibid., p. 11.
62. Ibid., pp. 15-16.
63. Ibid., p. 16; Todhunter, II, op. cit., p. 140.
64. Ibid., pp. 16-17 contains an acknowledgement, by Butts, of the ultimately empirical character of Whewell's criteria for judging the "correctness" of an hypothesis. This is no more than an anomaly in his argument, however, for his central contention is quite the opposite (i.e., that Whewell was completely "essentialistic" and "metaphysical").
65. Ibid., pp. 17-18.
66. Ibid., p. 24.
67. Ibid., p. 22. Butts was so blinded by his own inductivist prejudices that he rejected Whewell's hypothetical-deductive views in favor of Mill's "logic of confirmation," Ibid., p. 28.
68. The passage quoted by Butts from Whewell's writings reads "science does not begin with necessary truths, but arrives at them." Ibid., p. 24.
69. Todhunter, II, op. cit., p. 418.

70. Butts, op. cit., p. 418.
71. Todhunter, II, pp. 416-417; Butts, op. cit., p. 20.
72. For Whewell's views concerning Bacon's scientific methodology, see Todhunter, II, p. 234.
73. MP I, op. cit., pp. 2-3, 5.
74. Ibid., p. 4; MP II, op. cit., p. 2.
75. Todhunter, II, op. cit., p. 353.
76. Six Lectures on Political Economy, op. cit., p. 84.
77. Richard Whewell (ed.), The Literary Remains of the Late Rev. Richard Jones (1859) (New York: Augustus Kelley, 1964), p. x.
78. Whewell's view of "induction" is markedly different than the rather unsophisticated and facile declarations of many other "historical" economists:

I do not believe the principles of induction can be either taught or learnt without many examples. Of subjects other than Natural Philosophy. I hardly know one fair example. Your book is one. A good deal of Malthus's population is a beginning of such a process, excluding of course his anticipatory thesis, the only thing usually talked of ... how can you expect to lay down rules and describe an extensive method with no examples to guide and substantiate your speculations? You may say a number of fine things and give rules that look wise and arguments that look pretty, but you will have no security that these devices are at all accurate or applicable ... If there be any practical inferences to be drawn from the nature of true philosophy, it is this, that general propositions can not otherwise be understood than by understanding the instances they include.

Todhunter II, op. cit., pp. 115-116.

79. Literary Remains of Richard Jones, op. cit., pp. x-xi.
80. Six Lectures on Political Economy, op. cit., p. 84.
81. POD, op. cit., pp. 295-296.
82. Ibid., p. 299.
83. Ibid.
84. Todhunter, II, p. 116.

- 85. MP II, op. cit., pp. 7-8, 14.
- 86. POD, op. cit., p. 293.
- 87. MP II, op. cit., p. 13.
- 88. Ibid., pp. 12-14.
- 89. Six Lectures on Political Economy, op. cit., p. 4.

90. It should be noted at this point that Whewell was somewhat more judicious in the way in which he stated his views in print than it might seem from the foregoing quotations. In his Philosophy of Discovery, for instance, he carefully considers an anti-naturalistic doctrine: that a prioristic methods are appropriate to the moral sciences, while Inductive or empirical methods are suited to the natural sciences. While admitting the validity of this point of view for the study of Ethics, Whewell denies it for those social sciences concerned with legal institutions and "the principle notions which operate upon men regarded in masses," POD, op. cit., p. 293. The actions of individuals, as such, he believes to be unpredictable, at least by any known methodology, Ibid.

91. Six Lectures on Political Economy, op. cit., p. 2; Todhunter, II, pp. 155-156.

92. Ibid., p. 6; Literary Remains of Richard Jones, op. cit., p. xii. Whewell also advised Faraday concerning technical terminology to be used to described his discoveries in the area of electricity. Although "conventional definitions were impossible, since the convention was to express technical names in either Latin or Greek, Whewell did express great concern for naming things in such a way that those knowledgeable in the languages would have ready and accurate associations for the term used, see Todhunter, II, pp. 178, 363-365 & 426.

93. The Literary Remains of Richard Jones, op. cit., pp. xiv-xvi; MP II, op. cit., pp. 12-13.

94. Six Lectures on Political Economy, op. cit., pp. 34-35; Literary Remains of Richard Jones, op. cit., p. xiii.

95. Wesley C. Mitchell, Types of Economic Theory, Vol. I, (New York: Augustus Kelley, 1967), p. 517.

- 96. Six Lectures on Political Economy, op. cit., pp. 67-68, 83.
- 97. Schumpeter, History of Economic Analysis, op. cit., p. 448.
- 98. MP, I, op. cit., p. 4.
- 99. Ibid., pp. 4-5.

100. Ibid., p. 5.
101. Ibid., p. 1.
102. MP, II, op. cit., p. 43.
103. Ibid., p. 43.
104. Hutchison, A Review of Economic Doctrines, 1870-1929, op. cit., pp. 65-66.
105. Ibid.
106. MP, II, op. cit., pp. 11-12.
107. Ibid., p. 12; MP, III, op. cit., p. 22.
108. MP, II, p. 12.
109. MP, III, op. cit., p. 1 pt. 2.
110. MP, II, op. cit., pp. 43-44.
111. Hutchison, A Review of Economic Doctrines, 1870-1929, op. cit., pp. 65-66 fn.
112. Cochrane, "The First Mathematical Ricardian Model," op. cit., p. 430.
113. Schumpeter, History of Economic Analysis, op. cit., p. 449 fn.
114. Cochrane, "The First Mathematical Ricardian Model," op. cit., p. 431.
115. Six Lectures on Political Economy, op. cit., pp. 71, 72.
116. MP, II, op. cit., pp. 5, 8.
117. Hutchison, A Review of Economic Doctrines, 1870-1929, op. cit., p. 66 fn. For Whewell's rather confused criticisms of Ricardo's "incremental" analysis, see, for instance, Six Lectures on Political Economy, op. cit., p. 56.
118. Six Lectures on Political Economy, op. cit., p. 67.
119. MP, III, op. cit., p. 6, pt. 18.
120. Ibid., pp. 1-2, pt. 3.
121. Todhunter II, op. cit., p. 370.

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