METHODOLOGICAL PROBLEMS IN THE ANALYSIS
OF MONOPOLOID MARKETS

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PREFACE

There is a legend in economics which says that economists should postpone until the twilight years of their active careers any efforts to deal comprehensively with the methodology of economics. The subject, so it is claimed, requires a long process of development with the discipline before the fruits of experience, of mature, reflective thought can be brought to bear on the demanding nature of the subject. Of course, some have started at early stages, witness Schumpeter's first major work, but in general the study of methodology seems to take a strong ego, an impatience and deep concern with the accomplishments of a chosen field, or ignorance and naivete upon which age and experience have as yet left little of their stamp. Although the force of each partially explains the selection of the topic, only the reader can judge which force should be weighted more heavily.

That this study has been brought to a conclusion is the consequence of the encouragement and assistance of many persons. Several must, however, be singled out for special acknowledgment. To Professor Clifford L. James, who encouraged me in this topic and closely followed its development, I offer my warmest gratitude and deepest appreciation. Professors Robert D. Patton and Alva M. Tuttle offered important assistance with concepts and form and made invaluable suggestions to improve the quality of the writing for which I am very grateful. Yet, I would not wish others to associate them with any errors that might be found in this study. This is the responsibility of the author, and, if the intellectual progeny should
have strayed too far, that, too, would be a singular responsibility.

Finally, to my wife who did far more than merely type copy, who experienced the distress of a dissertation in so many ways, I fondly dedicate this study.

May 20, 1959

Charles E. Helppie
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Introduction

Economists have expressed dissatisfaction with the quality of the scientific performance of their discipline when asked to evaluate the theoretical and empirical findings it produces. This has been especially true in the analysis of monopolistic markets where there is little room for sanguineness with the performance of economics when it is criticized for: (a) faulty theoretical formulations, (b) insufficient, conflicting, and inadequate evidence, and (c) absence of standards for evaluating this type of phenomena.¹

In light of such criticism several conventional improvements and corrections are often advised. Concentration on the theoretical deficiency with a careful application of logical analysis to eliminate redundant and misleading propositions and unrealistic assumptions is mentioned. Also, the suggestion is advanced that extensive and elaborate empirical investigations would eliminate the inadequacies resulting from the paucity, or imperfections, of the data. Finally, there is the expectation that group meetings and interdisciplinary teams will provide the insights and programs for the construction of an adequate system of standards for policy.

However, there may be an important reason for doubting the capacities of the conventional suggestions. Much of the effort

expended along the conventional lines will be dissipated if the basic
problems plaguing analysis are deeply imbedded in the corpus of
economic science, i.e., if the problems are fundamentally methodo-
logical. Under these circumstances a different orientation would be
necessary, one that would probe the foundations of analysis.

Recognition of Methodology

In the theory of monopoloid markets the influence of methodology
has been duly recognized. Support has been given to the argument that
a methodological reorientation is essential if such markets are to be
analyzed successfully. Professor Triffin has made the point
explicitly: "Something might be gained if we could approach the
problem from a new angle and escape from the particular tradition and
methodology within which the discussion has proceeded so far". Thus,
on methodological grounds certain of the orthodox theoretical concepts
are severely criticized.²

While explicit reference to methodology may be absent in theore-
etical works, the theorist alludes to issues of methodology. In his
Theory of Monopolistic Competition, Professor Chamberlin refers to a
problem of methodological importance.

This association of the theory of competition with facts
which it does not fit has not only led to false conclusions
about the facts; it has obscured the theory as well. This

²Robert Triffin, Monopolistic Competition and General Equilibrium

³"In the general theory of value, the group and industry are
useless concepts. The new wine of monopolistic competition should not
be poured into the old goatskins of particular equilibrium
methodology." Ibid., p. 89.
is the more serious because the mixture of the two is a chemical process and not merely a matter of addition. Slight elements of monopoly have a way of playing unexpected logical tricks, with results quite out of proportion to their seeming importance.  

Similarly, Mrs. Robinson's distinction between theory, practice, and evidence reflects a significant methodological position.  

The presence of methodology on the theoretical side of analysis

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4 Edward H. Chamberlin, The Theory of Monopolistic Competition, Fifth Edition, Cambridge: Harvard University Press, 1946, p. 3. Another point of methodological interest appears in a comparison of his analysis with that of Joan Robinson: "It has been unfortunate that two theories as divergent in their interpretation of economic phenomena as Mrs. Robinson's and my own should have become identified in the minds of so many, even to the point of regarding them as differing only in terminology...Gradually it dawned that the explanation lay in a difference, not merely of words, but of fundamental conception as to how the phenomena in question were to be explained." Ibid., pp. vii-viii.

5 The purpose of this book has been to provide a box of tools for the analytical economist. The area within which these tools can work is very narrowly bounded. A number of unsolved problems lie behind and before the problems with which they are adapted to deal. Behind lie the fundamental problems on whose solution depends the validity of the whole supply-and-demand-curve analysis. To these general questions the tools, in the nature of the case, can have no contribution to make. But even within their own sphere the tools can do no work unless they are given some material to work on. The imaginary examples of the shapes and movements of demand curves and cost curves, constructed in order to display the apparatus, serve to show the kind of results that the tools could produce if they were given some realistic material on which to exercise their ingenuity... The level of abstraction maintained in this book is distressingly high. The technique can only survive in an atmosphere rarified by the adoption of very severe simplifying assumptions. The reader who is interested in results immediately applicable to the real world has every right to complain that these tools are of little use to him. The knives are of bone and the hammers of wood, only capable of cutting paper and driving pins into cardboard. But the analytical economist who is prepared to work stage by stage towards the still far-distant ideal of constructing an analysis which will be capable of solving problems presented by the real world may perhaps find in this tool-box some implements which will serve his turn". Joan Robinson, The Economics of Imperfect Competition, London: MacMillan and Company, Ltd., 1933, p. 327.
has close counterparts on the empirical side. The relevancy of methodology to empirical work arises with the demand that a "major task for students of such problems is to devise programmes for getting the relevant facts on a scale sufficient to facilitate agreement". 6

It is granted that empirical research involves methodological problems comparable to those present in theory. The quest for a perfect method or empirical format requires methodological exploration and perspective.

The variety of types of empirical price study reviewed... suggests that there is as yet no settled agreement on specific objectives of research or on the method of analysis. At this stage, of course, there is no strong case for standardization of method; the field is new and much of the work involves exploration in method. 7

Moreover, it must be noted that methodological controversies have been common in economic analysis. Ideally perhaps, many of the disagreements among scientists should eventually be reconciled with the elimination of imperfections in research. Often, however, there is more obfuscation than clarification, consequently reducing the importance of gains made and perpetuating some of the schisms among economists. 8

Methodology shows its influence directly and indirectly at the


8. An example of the width of such a gap involving empirical research arose with the Lester-Machlup debate, initiated by the article by Richard A. Lester, "Shortcomings of Marginal Analysis for Wage-Employment Problems", The American Economic Review, Volume XXXVI, Number 1, March, 1946, pp. 63-82, and followed by the criticisms of
policy level. The demands for improvements in analysis, in order that policy can be justified on more objective grounds, reflect a concern with the foundation upon which the standards and policy rest. If the foundation is weak, where serious deficiencies exist in theory and factual evidence, there is a tendency for economists to exercise excessive caution and hesitation in policy recommendations. Or, the same type of weakness has different consequences, with analysis disregarded because of its inherent inadequacies and policy formulation thus evolving into a matter of whim and prejudice. In either case, the results are seriously questionable for the continued development of economic analysis and its application.

Thus, recognition of the influence of methodology at different levels of analysis— theoretical, empirical, and policy— suggests that a study cast in terms of methodology may afford benefits that the conventional approaches are unable to provide.

The Basic Issue of Methodology

The proposal to consider the performance of economics within a framework of methodology poses a difficult issue. In general terms, the issue relates to the contribution or potential success of any study. In this case, however, it refers to the possibility of even modest success or accomplishment from pursuing problems of analysis in economics from a methodological point of view. Frequently two strong, and stringently expressed, points of view are advanced when

the contribution of methodology to a science is judged. At one extreme is the view that methodology and methodological study is "sterile", that it can make no contribution to the advancement of scientific knowledge, and that it is merely an atavistic appendix in the body of genuine science. At the other extreme, methodology is hailed as a "panacea" that can correct all imperfections, eliminate confusion and contradictions, and transform any science into a perfect engine for acquiring knowledge.

Status of the Issue

Since the two positions are sharply contradictory, with one rejecting methodology completely, it is necessary to review some of the ways by which economists have dealt with this issue. An early advocate of the need for concentration on economic methodology defended the economists' interest in the subject. The discussion of the scope and method of economics "does not directly advance our knowledge of economic phenomena themselves. For this reason, a certain impatience is sometimes felt when any such discussion is proposed".9 Referring to the tendency of many economists to become impatient with methodological discussions, they were characterized as thinking that

What we want, it is said, is not any more talk about method, but rather useful application of the right method; let us increase our knowledge of economic truths, instead of indulging in barren disputes about the way in which

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economic truths are to be attained.\textsuperscript{10}

It was against this type of thinking, and the reaction it represented, that brought forth the argument defending methodological study in economics.

A moment's consideration will shew \textit{sic} that, from the point of view of political economy itself, it is of material importance that its scope and method should be rightly understood.\textsuperscript{11}

However, the statement of a categorical imperative, as the above might be considered, does not provide sufficient justification. A stronger argument that would convince, if not persuade, economists that their concern with methodology was worthwhile needed construction. Thus, there was included in the literature of economics a now "classic" statement of the purported benefits of methodology:

...it is said that instead of arguing about what method of investigation is the proper one, it is better to exemplify the right method by employing it in the actual attainment of new economic truths. But are we then to beg the question of its rightness? In the long run, time cannot but be saved by making a preliminary study of the instruments of investigation to be used, the proper way of using them, and the kind of results that they are capable of yielding. For in so far as the methods of reasoning are employed without due regard to the conditions of their validity, the results gained must likewise be of uncertain validity, and the progress of economic knowledge, instead of being advanced, will be retarded.\textsuperscript{12}

Other economists have followed this path in defending and justifying the significance of methodology for economics. Professor T. W. Hutchison recognized the possible disrepute of methodology among

\begin{footnotesize}
\begin{enumerate}
\item \textit{Ibid.} \footnote{10}
\item \textit{Ibid.} \footnote{11}
\item \textit{Ibid.}, p. 4. \footnote{12}
\end{enumerate}
\end{footnotesize}
economists and, imitating the earlier proclamation, argued in support of the contribution of methodology.

methodological writings and the discussion of the Grundprobleme from every conceivable philosophical standpoint—Idealist, Materialist, Phenomenological, Transcendentalist, Neo-Kantian, etc., etc.—have, not unjustifiably, won a bad reputation among economic scientists....In the less philosophically-minded Anglo-Saxon countries it is hardly surprising that many have turned their backs in impatience on 'this noisy conflict of half-truths angrily denying one another,' and have abandoned the interminable wranglings and controversies of the 'methodologists' and 'philosophers' for seemingly more constructive work. But this evasion can only be temporary. For it can fairly be insisted that no advance in the elegance and comprehensiveness of the theoretical superstructure can make up for the vague and uncritical formulation of the basic concepts, and sooner or later—and at the moment it seems to be sooner—attention will have to return to the foundations.13

Moreover, even in the case of fundamental disagreement between economists concerning the problem of how methodology should be used, close agreement has existed with respect to the need or justification of methodology. In a manner of speaking, economists may have the same faith concerning methodology but worship in different churches. Professor Robbins, for instance, is a methodological apostate from the Hutchisonian position, although a very close ally during the stage of justifying it.14 In brief, economists may disagree seriously over


14 At any earlier stage [in the evolution of economic science], any attempt to discover the ultimate nature of the science was necessarily doomed to disaster. It would have been waste of time to have attempted it. But once this stage of unification has been reached not only is it not waste of time to attempt precise delimitation; it is waste of time not to do so. Further elaboration can only take place if the objective is clearly indicated. The problems are no longer suggested by naive reflection. They are indicated by gaps in the unity of theory, by insufficiencies in its explanatory
the content of their respective methodological positions but continue to accept the thesis that additional work in methodology is essential.¹⁵

Yet, there are limits to the extent to which the argument in defense of methodology can be pushed. For one, the actual achievements of economic methodologists have not been so impressive as to command widespread acceptance of their work in economics. Then, too, there is considerable indifference to methodology as representing work far removed from the province of economic science strictly construed. Finally, the belief that methodology is sterile can be an active factor limiting the acceptability of such work.

¹⁵In a recent plea for more methodological work in economics, the author argued that the "polemical questions implicit and explicit in discussions of the nature, scope, and significance of economics are of such fundamental significance to all the social sciences that further inquiry is to be fostered". Campbell R. McConnell, "Advocacy Versus Analysis in Economics", The Southern Economic Journal, Volume XXII, Number 2, October, 1955, pp. 145-163. Partial support of this position is by way of reference to the statement by Milton Friedman, that "More than other scientists, social scientists need to be self-conscious about their methodology". Essays in Positive Economics, Chicago: The University of Chicago Press, 1953, p. 40.
Even the sympathetic appraisal of methodology leads to a hesitant evaluation of its contribution. Scientists may be very concerned with methodological problems in their science. As Professor Schumpeter has described the condition:

> It is only when a field has grown into an established science that its votaries will develop an interest, not untinged with anxiety, in problems of scope and method and in logical fundamentals generally.\(^{16}\)

This represents a "perfectly natural" interest on the basis that "we are all of us bad interpreters of ourselves and untrustworthy witnesses to the meaning of our practice". Thus, scientists cannot afford to neglect methodology.\(^{17}\) However, recognition of its significance at this point does not confer upon scientists an unrestricted license to practice methodological evaluation. Boundaries of reasonableness are easily exceeded, that "excessive activity of this type may be a pathological symptom—there is such a thing as methodological hypochondria".\(^{18}\)

It has been necessary, up to this point, to consider basic views and attitudes affecting the judgment of economists towards methodology.

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16. Joseph A. Schumpeter, *History of Economic Analysis*, New York: Oxford University Press, 1954, p. 534. The point has also been made for sociologists: "When in the course of our work we are uncertain, we sometimes become more concerned with our methods than with the content of our problems". C. Wright Mills, "Two Styles of Research in Current Social Studies", *Philosophy of Science*, Volume 20, Number 4, October, 1953, p. 266.

17. Schumpeter, op. cit.

18. Ibid. Others have warned against this tendency among social scientists, that they resort to a study of method, like disappointed patients to a new quack remedy, hoping to find a cure-all for the ailment". Stuart A. Rice, *Methods in Social Science: A Case Book*, Chicago: The University of Chicago Press, 1931, p. 3.
from a more or less passive vantage point. In this fashion it has been possible to direct attention to a critical issue for methodological work in economics, i.e., the determination of the benefits of methodological study. If economists suffer at the hands of methodologists, or if the stream of opprobrium runs in the opposite direction, part of the responsibility for the situation rests in misunderstanding and over-emphasized claims. The economist who claims too much for methodology is likely to fail, both in his efforts to achieve the ultimate clarification of problems in his discipline and in his attempts to convince economists that such an accomplishment has been realized. What can be considered the domain of methodology is immensely complicated, replete with internal contradictions and unanswerable questions. The failure to present this characteristic of methodology has been a glaring one. The methodologist who slights the complexity of methodology presents in many cases a distorted view of the process of methodological evaluation and mainly publicizes a single system of doctrine.

In this sense it is understandable that an economist should hesitate to borrow the ideas and conclusions of the methodologist. He can expect his methodological system to be on the same order as any other; and, in most cases, he will be correct. However, the economist can make a mistake in categorically rejecting all such work. The methodologist may be able to provide important insights into problems of analysis, problems that remain buried in orthodox scientific activities. One may be too close to theory, empirical work, or policy issues to appreciate the underlying set of problems
that are complicating the analysis. Here the methodologist can perform invaluable service by examining problems from a different point of view; isolating the basic difficulties; and suggesting the types of modifications which seem required.

Direction of Study

The following chapters illustrate the inherent characteristic of methodology: how methodological variety permeates science at different levels from the physical to the social sciences. A point of view, or perspective, based on the variety is elaborated that requires the economist to function as a methodologist within his own discipline by adapting methodological insights from sources that are non-economic in origin. Since it is after careful consideration of basic methodological ideas that the economist is in a position to evaluate problems within his field, the next three chapters are devoted to general and specific issues in methodology. Then, once the perspective has been developed, the remaining chapters include an investigation of problems of monopoloid market analysis.

Summary and Conclusions

The performance of economics as a scientific discipline has been a source of disenchantment for economists concerned with solutions to contemporary problems. In particular, the status of analysis in dealing with the phenomena of monopoloid markets has been seriously questioned, resulting in criticisms ranging from the charge of faulty theoretical formulations and insufficient, conflicting, and inadequate evidence to the accusation that there are no relevant standards for
policy recommendations. Unfortunately, the conventional procedures for correcting these analytical imperfections have not been notably successful. Theoretical refinements, improvements in logical rigor, extensive model-building, additional factual discoveries, new empirical techniques, and the use of interdisciplinary groups for the derivation of policy guides have failed to eliminate the analytical problems. However, it is doubtful if conventional approaches are capable of dealing with such problems. If the problems are not simply theoretical in nature, nor purely matters of fact, then orthodox efforts to solve them may not be able to afford any appreciable degree of success. Thus, a different orientation would be essential.

It has been recognized by theorists, empirical researchers, and policy students of monopolized markets that methodology is an influential force in shaping the structure of economic analysis. Thus, an attack on the problems of analyzing markets from the vantage point of methodology would not be unwarranted. Moreover, the prevalence of weaknesses at each level of theory, empirical study, and policy formulation suggests that a broad approach is necessary, that the deficiencies should be handled coterminalously. Methodology offers an approach and a framework for such a purpose.

However, while any study involves the issue of the justification of the procedures followed, a methodological study is more deeply involved than others in this issue. Two extreme views continue to affect the economist's judgment with reference to the value of methodology. On one hand methodology is condemned as a sterile subject, while opponents defend methodology as the only means for
overcoming the errors of a science. Complete acceptance of either view is untenable, for the former would eliminate all consideration of important issues of method, results, etc., and the latter would indefinitely postpone all contemporary work in science. Thus, it is necessary to recognize the impact of the views on the judgment of economists and to neutralize most of their influence in advance.

This study proceeds from a general consideration of fundamental problems of methodology to the application of findings to problems of analysis in monopoloid markets. A perspective towards methodology is built up from the ideas and insights of philosophers, physical and natural scientists, and social scientists. Influential ideas in economic methodology are presented to form a comparison with those originating outside of economics, to develop contrasts within economic methodology, and to deepen methodological perspective. Then, the perspective is applied in the analysis of key issues in monopoloid markets, as a means of suggesting solutions to the current problems of theory, empirical studies, and policy standards.
Chapter II

General Elements of Methodological Perspective

A basic requirement for methodological evaluation is an awareness and understanding of the variety inherent in the subject. Without such understanding methodologically-derived judgments are subject to serious error. In order to minimize conceptual errors of this type and others, basic dimensions of methodology need examination and their implications for evaluation have to be explored.

Dimensions of Methodology

Five dimensions of methodology have been developed as a means of illustrating and emphasizing the conceptual variety of the subject: (a) definition and meaning of methodology, (b) functions of methodology, (c) approaches or frameworks, (d) standards and tests, and (e) attitudes and preconceptions. However, it should be noted that in one sense these dimensions have a high degree of artificiality. They indicate an inflexible division of a subject that should be thought of in more elastic terms. Historically, philosophers have struggled with varied issues of methodology within the context of an ebb and flow of

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1 John Neville Keynes described a common and easily perpetrated error that he called the "fallacy of exclusiveness", in which a "single aspect or department of economic study is alone kept in view, and the method appropriate thereto aggrandized, while other methods, of equal importance in their proper place, are neglected or even explicitly rejected..." The Scope and Method of Political Economy, Fourth Edition, London: MacMillan and Company, Ltd., 1917, p. 9. A different kind of error also significant in this connection is mentioned by A. E. Wolfe, that of "dogmatic egotism, assuming that
changing ideas. And, to a large extent, a similar development has occurred in economics. Yet, while an artificial taxonomy may ill suit the nature of methodology, it provides a convenient form for developing insight and perspective.

Broad Issue of Definition

The definition or meaning of methodology involves two different classes of issues and problems. On one hand issues surrounding the relevancy of definition for any subject matter must be considered. On the other, the direct problem in methodology is that it can be defined in different ways. Although the former issue is not a critical one, a brief discussion of the influence of contradictory ideas concerning general definition is helpful in understanding the latter.

From one point of view the definition of a subject is not expected to have any meaningful consequences on its principles or practice. Thus, a careful definition of a subject can be safely disregarded. The logistics of defending this position vary, e.g., the reference to a practical test of significance, the use of a philosophical discussion as in some forms of pragmatism. The end product, however, remains unchanged—definition is ignored.

Yet, intense criticism of this view exists, typified by a definition controversy in economics. Attempts to ignore the defi-

one's ideas are more valid than the next man's simply because they are one's own..." "Functional Economics", in The Trend of Economics, edited by Rexford Guy Tugwell, New York: Alfred A. Knopf, 1924, p. 451.
nition of economics met with charges such as: the subject cannot be rightly understood unless it is properly defined; without definition analysis is impossible; and, logical relationships will be confused unless the terms are distinctly defined.

Each view is an extreme interpretation of the value and necessity of definition. Each has its own merits, i.e., an underlying reasonableness. However, the views collide, and the consequent contradiction must be recognized, and, if possible, reconciled. This need not be done, however, independent of a series of contradictions that are inherent to methodology. In fact, for purposes of understanding the issues involved in formulating a system of methodological evaluation it is better that the economist postpone a decision at this point. He would do well to consider the contradiction as part of a broad, complex set of ideas and issues.

Definition and Meaning of Methodology

We shall assume that methodology is definable, but not that it must be defined (which would reintroduce the preceding issue). It then becomes possible to illustrate the various meanings of the term and to indicate some of the implications for methodological evaluation arising from different treatments of the subject. Methodology is defined as a science, logic, study, and description of methods for acquiring knowledge. In addition, it has been further limited to restrict it to methods for acquiring only scientific knowledge. It is discussed as the "logical analysis of scientific procedure" that "must be clearly distinguished from deductive logic and recognized as
an autonomous rational discipline. It is termed the science dealing with the theory of method and a branch of logic dealing with principles of procedures. For some philosophers it is a division of analytical philosophy, the part that deals with science. Here it is the task of philosophers "to analyze the way in which it [science] achieves its results, as well as to compare it with other possible intellectual methods." Furthermore, there is the idea that methodology is simply the description of scientific activity, the recording of the actual procedures of scientists.

Basically, the definitions of methodology refer to a subject matter that is rational and analytical or descriptive and empirical. When methodology is defined as a rational-analytical discipline, emphasis is placed on fundamental axioms, theoretically-deduced propositions, and purely logical form. The crucial test associated with this formal methodology is consistency, logical conformity to the principles of the system. The empirical conception of methodology stresses the observation of the procedures and techniques of working scientists and minimizes the importance of logical form and pure analysis.

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4 Abraham Wolf, "Scientific Method", The Encyclopedia Britannica, 14th Edition, Volume 20, 1956, pp. 127-136. It is interesting to note that this voluminous collection of information does not include a direct treatment of methodology. Instead, the reader is referred to the discussion of scientific method as an equivalent of methodology.
There are several important implications evolving from the differences in conception of methodology as illustrated by the definitions. The formal, rational interpretation of methodology leads to the development of an external discipline, one that exists outside of scientific procedure and passes judgment from a definite system of standards. The empirical, descriptive conception closely follows the practices of scientists and indulges in much less critical appraisal. Yet, to determine which conception is best suited for methodological evaluation is impossible. The subject differs so greatly depending on definition that one meaning automatically eliminates the other. Thus, instead of attempting to select the best meaning, the scientist should recognize the inherent differences and delay a decision until that time when he sees the issue as part of many similar issues.

Functions of Methodology

The idea that methodology is inherently complex, when properly understood, is further substantiated through an examination of the various functions of the subject. According to a pioneer in the formalization of social science methodology, there are six functions of methodology, or six tasks that methodologists can perform for science: (a) the settlement of methodological controversies, (b) the analysis of the foundations of prediction, (c) the specification of implicit theoretical laws in a science and the development of their interrelations, (d) the coordination of different kinds of scientific activity through the attainment of methodological insight, and (f) the promotion of mutual understanding among scientists.
Settlement of methodological controversies as a function of methodological work has received attention in many quarters. For the economist, this is sometimes thought of as the sole function of methodology. Yet, the modus operandi for disposing of controversies within a discipline has been open to as much debate as any particular controversy. To attempt to settle disagreements among scientists with the use of a narrow system of methodological principles is no solution to the issue, for in many cases this involves the mere substitution of one problem for another. Later analysis will show the ease with which issues may be replaced and hence the necessity for developing a correct appreciation of the complexity of the issues involved. However, it might be noted that, in recognition of the problem, the idea has been advanced that controversy might be eliminated or modified with the application of general principles broader than a single social science:

It will become apparent that the major methodological controversies in the social sciences cannot be settled if we regard them as peculiar to particular fields of social inquiry or even to the entire domain of social research. One has to discriminate between different levels of generality in the arguments involved and to determine the range of generality of each. It will then be seen that issues of general methodology play an essential part in these controversies and that almost all of the allegedly irreconcilable differences between conflicting views are on this level.5

Given the inflated importance of prediction as the ultimate test of science, it is not unusual to find the methodologist claiming that the proper task of methodology is to examine the foundations of pre-

5Kaufmann, op. cit., p. 2.
diction. That is, while the scientist may unquestioning accept prediction as the sole criterion of a scientific hypothesis, the methodologist is more cautious.

We are not concerned with the success of predictions but with their foundations. The social scientist who declares a certain prediction to be warranted must be able to substantiate this claim by referring to the laws in terms of which it is warranted.6

Another task or function of methodology that indicates its wide range is the attempt to improve coordination among the social sciences. For this function the methodologist emphasizes the need for acquiring methodological insight.

Successful co-operation in science involves planned co-ordination of different kinds of accomplishments in science. Such co-ordination requires insight into the extremely complicated structure of their relationship. It is one of the objectives of methodological analysis to attain this insight.7

Another important function of methodology, one that is familiar to scientists in every field, is methodological criticism. This function has obvious potency, since it provides a means for bringing about improvements in a discipline. However, the methodological critic may grossly extend the limits of valid criticism, attacking procedures and findings in heavy-handed fashion. The dangers of excessive critical zeal in methodology have been duly noted.

Criticism is one of the most important forms of scientific co-operation. To label the activity of the critic as 'destructive' is misleading. By showing a person that he has departed from the right path, we help him and all who

6Ibid., p. 241.
7Ibid., p. 243.
follow him; we contribute to their progress in the direction of their goal. Invalidation no less than verification of propositions is progress in inquiry.

Yet criticism may be destructive, i.e., detrimental to scientific progress, when the scope of an objection is over-rated. The critic may then be compared to a surgeon who, in operating, removes more than is necessary. A surgeon with a thorough knowledge of the anatomy and the physiology of the human body will seldom commit such an error, nor will a critic who has a clear idea of the structure of scientific procedure be likely to err in this way. He will be able to determine precisely the rules violated and the type of violation. It may be assumed that methodology will substantially contribute to the progress of social sciences by making explicit the implicitly presupposed standards of scientific criticism.\footnote{Ibid., p. 244.}

Evidence of the destructive nature of exaggerated criticism in methodological terms is obviously apparent in the resentment arising over investigations of theoretical assumptions, the accuracy of empirical research, and related matters. The attack of the logical positivists on the allegedly fallacious concepts and preconceptions of scientists illustrates the degree to which methodological criticism may be "pushed." However, the criticisms of the logical positivists have recently been tempered, with the admission that the original censuring was too severe.\footnote{In psychology, still more than in physics, the warnings by empiricists and operationists against certain concepts, for which no sufficiently clear rules of use were given, were necessary and useful. On the other hand, perhaps due to the narrow limitations of the earlier principles of empiricism and operationism, some psychologists became overcautious in the formation of new concepts. Others, whose methodological superego was fortunately not strong enough to restrain them, dared to transgress the accepted limits but felt uneasy about}

The final function or task of methodology is to promote mutual understanding among scientists. Although it is easy for the method-
The presentation of the various functions or tasks of methodology shows the generous scope possible in methodological work and illustrates the various activities performed under its aegis. True, such a conception connotes a super-discipline that operates on every science in diverse ways, which may be a mistake when its actual accomplishments are more modest. Yet, to narrow the conception of methodology, to restrict it to a single function, would result in the elimination of several valuable guides and directives for work along methodological lines.

Approach or Framework of Methodology

Any examination of the conceptual diversity of methodology that

it. Some of my psychologist friends think that we empiricists are responsible for the too narrow restrictions applied by psychologists. Perhaps they over-estimate the influence that philosophers have on scientists in general; but maybe we should plead guilty to some extent. All the more should we now emphasize the changed conception which gives more freedom to the working scientist in the choice of his conceptual tools”. Rudolf Carnap, "The Methodological Character of Theoretical Concepts", in Minnesota Studies in the Philosophy of Science, Volume 1, The Foundations of Science and the Concepts of Psychology and Psychoanalysis, edited by Herbert Feigl and Michael Scriven, Minneapolis: University of Minnesota Press, 1956, p. 70.

But methodology should not be evaluated exclusively in terms of its direct contribution to scientific progress. It fulfills a most important social function in promoting mutual understanding among scientists, particularly social scientists. The power of argument rests on this understanding; and when the foundation is too weak, it
neglects treatment of variations in approach or framework automati-
cally eliminates a strategic factor. Although there are some
benefits from a restrictive treatment of methodological issues and
questions from within a carefully delineated system, such a procedure
too often slights the fact that differences exist among systems.
And, these differences may represent the source of contradictions in
methodological evaluation. Thus, it is necessary to illustrate the
variations in approach, framework, or system that apparently shape
the type of evaluation forthcoming from them.

It is not an elementary task to describe and to identify the
major kinds of frameworks utilized in methodological work. One
encounters the difficulty that classification of methodological frame-
works involves methodological issues of principles of classification.
Moreover, it is not as if the subject is so conventional or orthodox
in conception that frameworks are mere or less a matter of custom.
Thus, we must expect the following to be somewhat artificial, with
"shadings" of differences among additional frameworks if they are to
be included, and somewhat less precision among the frameworks that
are discussed. Granting these circumstances, methodological approaches
or frameworks are generally adaptations of the following types: his-
torical, descriptive, comparative, and analytical. The scientist
ought to recognize, therefore, that the development of methodological
criticism and the more general construction of criteria represent the
fruits of a particular framework; that there are important differences

is the argument of power that triumphs". Kaufmann, op. cit., p. 244.
among these frameworks; and that they give conflicting advice.

The Issue of the "Best" Approach or Framework

A question that arises inevitably after consideration of the possibility of different frameworks or approaches is which is the "best" one. If it were possible to answer the question conclusively, then the examination of various ones would be redundant or merely a matter of providing information. Unfortunately, the criteria for determining the "best" approach or framework involve either the application of higher-ordered frameworks and their corresponding principles or the administration of tests from within the frameworks under review. The latter alternative obviously slants an answer in favor of the particular framework, while the former represents the same general class of issues under review. More specifically, the issue of the best way to develop methodology has been a matter of concern in many quarters, and the difficulties inherent in it have been noted.

The Social Science Research Council made an attempt to study methodology in the social sciences. Facing the question of the way in which methodology might best be studied, the Council had doubts over the approach to adopt. Alternative ones were considered, and

11 The Council considered: (1) A general treatise upon the nature of scientific method in social science, by some prominent specialist with a broad philosophic outlook. (2) Quotations from various contributions to show social science in the making. (3) A symposium by specialists upon the state of the respective disciplines. (4) Autobiographical versions of the growth of their scientific interests from distinguished scholars who have contributed to the development of social science. (5) Republication of past contributions to methodological theory. (6) An extensive bibliography of works on
consequently rejected for reasons that were not methodological.\textsuperscript{12} Eventually, however, it was agreed that of the possible alternatives, a "case book" would be most desirable, on the basis that this "seemed to have the special advantage, in comparison with these alternatives, that it would keep theoretical considerations closely related to the particular".\textsuperscript{13}

An unsettled question remained. Which scientific contributions were to be included and what criteria could be used for the selection and classification of the contributions. The answer to this supported the idea that methodological insights, understandings, and principles are not confined to a single framework.\textsuperscript{14}

\textsuperscript{12} In the above, the first was discarded because it "was objected that this proposal would be too dependent upon the capacities and viewpoints of a single individual. Such a treatise would necessarily be biased, and largely speculative". With the second it was felt that had "this procedure been followed, the reader himself would have been left to discover the methodological issues and principles involved." The third "was ruled out because of its duplication of other studies now available". The fourth was defeated for the reason that while "the committee saw advantages in such a venture, it was regarded as impracticable because of the probable length to which such a series, if sufficiently comprehensive, would run". Although the committee believed that the fifth proposal merited attention, because "many valuable discussions are imbedded in the voluminous works of half-forgotten scholars, or otherwise inaccessible to the present student", they eliminated it for the reason that it "would not have served to the best advantage the Committee's desire to portray the present state of methodology in the social sciences". The sixth also "was open to the same objection as the preceding proposal". Ibid.

\textsuperscript{13} Ibid., pp. 733-734.

\textsuperscript{14} The criteria previously employed or suggested seemed inadequate. Thus, for example, some of the writers whose influence has been most far reaching in the development of social sciences have
Experience with the issue of the best approach or framework in methodology has apparently affected the position of non-social scientists. It has long been pointed out that mathematicians deal not with a geometry, but with geometries. Thus, the contention that the perspective of the scientist must embrace, or at least tolerate, the existence of multiple forms of frameworks is not an unusual one.

Differences in Approaches or Frameworks

To illustrate the differences in methodological frameworks that are possible in developing methodology, several studies are examined in the following. Their significance for the present, however, is not in principles and in major findings, but in conception of the approach for methodological study.

An historical-type study into the foundations of physical science, for instance, attempted to determine the "precise nature and assumptions of modern scientific thinking" from the reference position made no particular contribution to method in the logical or technical senses. The significance of their work is to be found elsewhere—perhaps in the breadth of their generalizations or in the new and vitalizing ways in which problems have been conceived. Logical or technical criteria for the selection of contributions for analysis, in consequence, would remove from view some of the most important work as judged by its cumulative effects. Again, it was felt that a classification based upon traditional divisions among the social sciences would help to perpetuate distinctions which are frequently the result of historical accident rather than contemporary utility."

Ibid., p. 737.

*What are we to think of that question: Is the Euclidean geometry true? It has no meaning. As well ask whether the metric system is true and the old measure false; whether Cartesian coordinates are true and polar coordinates false. One geometry cannot be more true than another; it can only be more convenient." Henri Poincaré, The Foundations of Science, translated by George Bruce Halsted, New York: The Science Press, 1913, p. 65.
of centuries of development and evolution. According to its author, what we are proposing is a rather neglected type of historical inquiry, that is, an analysis of the philosophy of early modern science, and in particular of the metaphysics of Sir Isaac Newton. Not that much of this has not been written... But a much more radical historical analysis needs to be made. We must grasp the essential contrast between the whole modern world-view and that of previous thought, and use that clearly conceived contrast as a guiding clue to pick out for criticism and evaluation every one of our significant modern presuppositions.

From such historically-fashioned studies it was expected that the "historical approach might hope to contribute to the clarification of the issues involved". And, the historical approach would serve as a device for understanding the foundations of science.

The comparative approach or framework involves the establishment of "ideals" of science, a comparison of actual scientific practice with the ideals, and, in some cases, a comparison of the status of the social sciences with the physical and natural sciences. In one case of the comparative approach, its creator developed four ideals of science: certainty, exactness, universality, and system. Then, scientists were urged to pursue diligently these ideals as the only proper path to scientifically reliable results. Moreover, a com-

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17 Ibid., pp. 29-30.
18 Ibid., pp. 304-325.
parison of the social and natural sciences in terms of subject matter, ideal goals of science, form of research, and intellectual skills resulted in the evaluation that the social sciences are "inferior" to the natural sciences as precise disciplines. 20

In the physical sciences, various frameworks have been used to deal with methodological issues. The "analysis and definition of our fundamental concepts, and the clear statement and resolute criticism of our fundamental beliefs" 21 became the objectives of an approach described as "Critical Philosophy". Yet, while other philosopher-scientists in the physical sciences subscribe to the objectives, 22 different frameworks are followed. The reliance upon a rational, analytical framework in which logic and language clarification are the chief origins of methodological findings is a markedly different dependence from the framework of observation of the activities of scientists. 23

A substantially descriptive-taxonomic approach also has been adapted to methodological exposition. According to this approach, there are three levels of methods by which science provides results that are "impartial and systematic". At one level, and common to

exactness, universality, and system". p. 83.
20 Cf. Ibid., pp. 334-368.
23 The difference between Broad's Critical Philosophy and Bridgman's Operationalism.
all of the sciences, "are certain mental activities which are so absolutely indispensable to science that they are practically always employed in scientific investigations". At the other two levels of logical and technical methods individual variations occur among the sciences.

Close-knit integrated systems of analysis govern at times the development of methodology. Within such a framework, general methodological principles are constructed, principles that transcend the domain of particular sciences. In turn, the principles of the general analytical machine are brought to bear upon the issues of science that are of broad interest and also those that are of special interest.

Finally, there are frameworks that are open-ended. Recognizing that variety and innovation, changing concepts and techniques, are inevitable products of an expanding science, the authors of open-ended frameworks develop methodology with the same features. Closed methodological systems are expressly denied and permanent and universal classifications rejected.

24 Abraham Wolf, op. cit., p. 128.

25 For a prototype, see Methodology of the Social Sciences, op. cit., especially pp. 1-3.

26 "The stream of immeasurable events flows unendingly towards eternity. The cultural problems which move men form themselves ever anew and in different colors, and the boundaries of that area in the infinite stream of concrete events which acquires meaning and significance for us, i.e., which becomes an 'historical individual', are constantly subject to change. The intellectual contexts from which it is viewed and scientifically analyzed shift. The points of departure of the cultural sciences remain changeable throughout the limitless future as long as a Chinese ossification of intellectual life does
In sum, to neglect a consideration of variations in approaches or frameworks will constrict the methodological perspective of the scientist and tend to make judgment a matter of conformity to particularized standards. Methodological evaluation will suffer as a consequence, since the errors resulting from restricted perspective are not always evident. If the scientist has an intellectual commitment to a specific framework and fails to recognize the existence of competing ones, he is apt to pass judgment on narrow grounds. Yet, the value of methodological evaluation cannot be interpreted in terms of internal consistency alone. At a more advanced level it should be seen that evaluation and judgment involve the frameworks themselves.

Methodological Standards and Tests

The preceding recognition of the variation developing in the concept of methodology in terms of its meaning, functions, and frameworks makes it clear that similar differences will exist in the standards, tests, rules, grounds, or criteria of methodology. In this dimension of methodology two distinct sets of issues must again be noted. The first involves the ability of the methodologist to arrive not render mankind incapable of setting new questions to the eternally inexhaustible flow of life. A systematic science of culture, even only in the sense of a definitive, objectively valid, systematic fixation of the problems which it should treat, would be senseless in itself. Such an attempt could only produce a collection of numerous, specifically particularized, heterogeneous and disparate viewpoints in the light of which reality becomes 'culture' through being significant in its unique character". Max Weber, Max Weber on the Methodology of the Social Sciences, translated and edited by Edward A. Shils and Henry A. Finch, Glencoe, Illinois: The Free Press, 1949, pp. 84-85.
at reliable standards or tests, while the second refers to the issue of determining the best set of standards under conditions of conflicting and contradictory criteria. However, since the analysis of the former ultimately depends on the entire development of methodological perspective, i.e., understanding of the inner complexity of methodology, the emphasis at this juncture will be on the second one.

That "rules", standards, tests, and criteria are the most important part of methodology is a claim common to the analytical conception of the subject. Yet, it is apparent that differences will exist in the types of standards and tests advanced, due to variations in conceptions. Thus, we must be prepared to entertain different viewpoints. A methodologist may argue that his rules or standards are the only valid ones, demand their application, and condemn all other contradictory ones. Or, the methodologist may adopt the position that he

is concerned to seek solutions of certain basic problems of economic science in accordance with the criteria outlined. His work ...is not concerned to urge or to appear to urge any ultimate 'necessity' or 'absoluteness' about these criteria, although, not attempting to hide our personal intellectual tastes, we have tried to indicate

27 "As we have already pointed out, one fundamental difference between deductive logic in the strict sense and the logic of empirical procedure is that the rules of deductive inference are derivable from the propositional meanings and are thus 'ultimately rational', whereas the rules of empirical procedure are not. On the one hand, it should not be overlooked that, the rules of empirical procedure and a scientific decision being given, the correctness of scientific decision is provable by pure reason. In this respect the logic of empirical procedure is not 'less rational' than deductive logic in the strict sense. Otherwise, it would be inappropriate to speak of a logic of empirical procedure". Kaufmann, op. cit., p. 65.
hero some consequences of their adoption on the one hand and their rejection on the other.\textsuperscript{28}

Yet, although the opportunity to construct different methodological rules and standards is allowed, there may be a requirement that the unique rules of a given methodologist be applied exactly.\textsuperscript{29} Another complicating factor to consider is the possibility of a dichotomy between the standards and rules in "theory" and their use in "practice". Not only are there questions to answer in connection with the variations of standards but also there are issues involved in the relationship between the rules and their application.\textsuperscript{30}

A later section will explore the content of several different systems of methodological standards and tests. Thus, suffice it to say at this point that methodological perspective and its corresponding evaluation involves conflicts among systems of standards and conflicts over the way in which they are to be appraised.

\textbf{Attitudes and Preconceptions}

Differences in attitudes and preconceptions among scientists create an environment for methodology that fosters controversy. No matter how dispassionately and objectively one may try to handle


\textsuperscript{29} \textit{Ibid.}, p. 13.

\textsuperscript{30} "The frequency with which methodologists fall short of the standards set for themselves is impressive and disquieting... The authors of this volume are thoroughly aware that they may find themselves placed in the same class..." W. E. Spahr and R. J. Swenson, \textit{Methods and Status of Scientific Research}, New York: Harper and Brothers Publishers, 1929, p. 49.
methodology, it is almost inevitable that preconceptions concerning the subject will intrude into its understanding. Thus, a brief exploration into the difficulties originating from contrasting attitudes and preconceptions is essential.

A consideration of two questions facilitates the exposure of the implications of conflicting preconceptions. First, is methodology an independent subject, disassociated from any science, or must it be pursued within the confines of a particular science? Secondly, does methodology provide a contribution to science, or is it inherently sterile? Strongly held beliefs among methodologists and scientists in every field are associated with such questions. Unless the beliefs are brought to the surface, they are likely to continue to affect the evaluation of methodological work indirectly and to magnify the problems this work involves.

One way to handle the issues of attitudes and preconceptions in terms of the above questions is to develop the concept of a norm (or value or directive) that would express a belief in terms of what methodology ought to be able to accomplish. By placing the implicit attitudes and beliefs on the plane of value judgments, critical distinctions can be made. For this purpose, then, two normative positions are developed.

Pure or Discipline Methodology

What will be termed Pure Methodology and Discipline Methodology involve unique, and contradictory, norms. The Pure Methodologist would be expected to understand the entire ramifications of methodology
in any scientific field. For example, the controversy among physicists concerning the relational theory of time versus the absolute theory of time would not be examined within the dimensions of physics. In that way the Pure Methodologist would not be influenced by physical preconceptions, pet physical theories, or venerated ideas in physics. However, he would be expected to (meaning that he ought to be able to) evaluate the claims of one theory over the other from the methodological analysis of the standards and principles utilized by the physicists in their efforts to determine the better theory. In economics, the Pure Methodologist would not be expected to follow the cardinal-ordinal controversy in demand theory throughout its many subtleties, but it would be expected that he ought to be able to evaluate the implicit methodological standards that the economists apply.

On the other hand, a Discipline Methodologist would be expected to function as an expert, i.e., he ought to, in at least two fields. Using the above examples, there would have to be two kinds of methodologists, one a physics methodologist and the other an economic methodologist. The first would be denied the opportunity of engaging in the methodological evaluation of the latter, with a similar proscription operating on the activities of the latter. Thus, the belief or norm holds that only through an understanding of the specific subject matter of a science can the methodologist be a properly responsible one.31

31 The condition of opposing norms helps to explain the gap which appears among methodologists and scientists, and, perhaps, the gap
Within the literature of science, methodology, and philosophy the two beliefs appear explicitly and implicitly. Kaufmann's methodological approach must be viewed as one based on a conception of pure methodology, in which methodology is broader than a single science. For Broad, however, discipline methodology would be followed with a composite of philosophy and science, with his study "written primarily for scientists who are interested in philosophy, and secondarily for philosophers who are interested in science". Others would support discipline methodology, methodology carried on within the scope of a specific science.

arising between different schools of thought within a given science. Two journals have at least intimated that fields may grow apart, and that, consequently, it is necessary to bring them closer together. "Philosophy of science is the organized expression of a growing intent among philosophers and scientists to clarify, perhaps unify, the programs, methods and results of the disciplines of philosophy and of science". W. M. Malisoff, "What Is Philosophy of Science", Philosophy of Science, Volume I, Number 1, January, 1934, p. 1. Also, "Today... the philosophical aspect of science has become obscured; science is no longer primarily a philosophy but an investment...In brief, science is in danger of becoming transformed from a vision to a blind automatism, its pursuit from a pilgrimage to a chase. Now while this can be seen to have been to some extent inevitable, and not necessarily wholly a disaster, it is nevertheless in the highest degree undesirable that the extended application of science to practical affairs should be accomplished by a loss of apprehension of its philosophical significance". The Editorial Board, "Foreward", The British Journal for the Philosophy of Science, Volume I, Number 1, May, 1950, pp. 1-2.

33 Scientific Thought, op. cit., p. 86.
34 "To be of use to the scientist the 'methodological' problems must be worked out in closest co-operation with the scientific worker....The discussion of 'methodological' questions—for the scientist at any rate—only has sense in connection with the practical problems of science...the critical analysis of the basic concepts of economic science must be carried through to the end within the science
The conflicting attitudes and preconceptions, as introduced in the form of contrasting norms, have an impact on methodological evaluation. Perhaps for this reason we should conceive of the possibility that serious disagreements over methodological issues result as much from a clash in values or attitudes as from specific content. Moreover, if the methodologist must engage in persuasion to convince the scientist of the validity of findings, the scientist should understand the tactics. Recognition of the influence of attitudes and preconceptions and the appeals to norms would be, therefore, a valuable part of the perspective of the scientist.

Itself and according to the scientific criteria, not according to the criteria of outside philosophers. T. W. Hutchison, op. cit., pp. 17-18. Also, "When one of our most eminent historians feels impelled to give an account to himself and his colleagues of the aims and methods of his scholarly work, this must necessarily arouse an interest far beyond the limits of his special discipline because in so doing he passes beyond the boundaries of his special discipline and enter into the area of methodological analysis. This has to begin with certain unfavorable consequences. The categories of logic, which in its present state of development is a specialized discipline like any other, require, if they are to be utilized with assurance, the same daily familiarity as those of any other discipline. Obviously, Eduard Meyer, whose Zur Theorie und Methodik der Geschichte (nadle, 1900) we are discussing here does not and cannot claim such constant contact with logic...The methodological details of his work are, so to speak, a diagnosis not by the physician but by the patient himself, and they are intended to be evaluated and understood as such. The professional methodologist will take umbrage at many of Meyer's formulations...But this does not diminish its significance for the neighboring special disciplines...methodological discussions rooted within their own subject matter may be more useful for the self-clarification of special disciplines in spite of, and in a sense even because of, their methodologically imperfect formulation." Max Weber, op. cit., pp. 113-114.

35 The "question of the proper subject-matter and method of the social sciences is a normative question" that "most of the current philosophy is still unable to deal with...in a satisfactory way". Alan Gewirth, "Subjectivism and Objectivism in the Social Sciences", Philosophy of Science, Volume 21, Number 2, April, 1954, pp. 158-
Sterile Versus Constructive Methodology

Scientists who are deeply immersed in the work of their particular disciplines may fail to realize that in some ways they face issues common to other sciences. This is true for attitudes concerning the value of methodology. A previous section has noted the situation in economics where methodology has been judged as sterile and constructive. Yet, the economist's plight with methodology as a sterile or constructive subject has important parallels in the physical sciences. There, attitudes towards methodology are viewed as being highly influential. And, while the belief that methodological work is sterile might have had validity in the past, the physicist argues that such an attitude no longer has the same merit.

Attitudes and preconceptions, the beliefs of scientists, have effects on methodological evaluation. As a social science, economics may have more difficulty with this factor than other sciences.


36 "It is no new thing to attempt a more critical understanding of the nature of physics, but until recently all such attempts have been regarded with certain suspicion or even sometimes contempt. The average physicist is likely to deprecate his own concern with such questions, and is inclined to dismiss the speculations of fellow physicists with the epithet 'metaphysical.'" P. W. Bridgman, The Logic of Modern Physics, New York: The MacMillan Company, 1938, p. vii.

37 "This attitude has no doubt had a certain justification in the utter unintelligibility to the physicist of many metaphysical speculations and the sterility of such speculations in yielding physical results. However, the growing reaction favoring a better understanding of the interpretative fundamental of physics...is a reaction forced upon us...." Ibid., pp. vii-viii.

38 "As things are now, one faction is delighted at the
yet, to a degree the same thing influences other sciences. Thus, if methodology were examined in terms of attitudes, beliefs, norms, and values, the findings might show that what has been considered an extraneous element in evaluation actually influences methodological evaluation.\textsuperscript{39}

A Concept for Methodology: Methodological Matrices

The preceding discussion has attempted to illustrate some of the inherent variety and complexity of methodology. The existence of this condition and the ease with which differences in conception lead to conflict represent a critical problem for a scientist. He must deal with three basic features of the methodological environment, and the assistance that any given methodologist is able to afford is subject to question. The three basic features are:

(1) There are direct contradictions of methodological principles, findings, and positions.

(2) There is direct agreement.

(3) Certain principles, findings, and positions are excluded from methodology.

discomfiture of another school when a forecast fails to materialize. The anti-Keynesians were overjoyed when the prophecy of the Bureau of Labor Statistics about unemployment during the reconversion period proved wide of the mark, as they had anticipated. But the man on the street took it as one more sign that all economists are unreliable. Real scientists take pride in their discipline and are concerned when careful forecasts go wrong. Can one imagine, for instance, a group of astronomers rubbing their hands and saying: 'Splendid, Halley's comet never came around the way he said it would'. Stuart Chase, The Proper Study of Mankind: An Inquiry into the Science of Human Relations, New York: Harper & Brothers, 1948, p. 187.

\textsuperscript{39}Perhaps something on the order of the Hudson-Rosen structured questionnaire would be an appropriate device for such purposes. Cf. "On the Definition of Attitudes: Norms, Perceptions, and Evaluations","
What this structuring signifies is that a single methodological idea can appear in one of three ways: claimed to be "true"; claimed to be "false"; and, "excluded". Formalizing the three cases, we let P, Q, R, S, ...represent statements that are offered as "true" ones. Then, *P, *Q, *R, *S, ...will be the contradictions of the respective statements, i.e., claims that the respective statements are "false". Finally, (P), (Q), (R), (S), ...will stand for the exclusion of the corresponding statements. On the basis of the structuring of methodological statements it is possible to develop methodological patterns of ideas, or what might be termed methodological matrices. With a single statement about a methodological principle, say, for example, "metaphysical aspects of science are important", there are three possibilities and three methodological matrices: P, *P, (P).

If the scientist were to choose among them, he would subscribe to one of the three. If a second methodological statement were offered, e.g., "every scientific proposition must be empirically verifiable", then we would have an additional three possibilities: Q, *Q, and (Q). However, the number of matrices would have increased to nine. The scientist would be expected to choose from: P and Q; P and *Q; P and (Q); *P and Q; *P and *Q; *P and (Q); (P) and Q; (P) and *Q; (P) and (Q). If there were three statements about methodology, the number of matrices involved would number twenty-seven. And, if the structuring were expanded to incorporate ten statements, there

University of Illinois Institute of Labor and Industrial Relations, Reprint Series, Number 20, July, 1953.
would be the theoretical possibility of well over fifty thousand
different methodological matrices. Thus, the difficulty of choice
among the matrices is obviously formidable.

Yet, there is an added complexity to this picture of the
structure of methodology. Suppose that methodological principles,
findings, and positions vary through time. If it is granted that
scientific progress by its very nature continually supplies new
techniques and new problems to be solved, then it would not be unusual
to expect a similar development in methodology. Thus, new and
changing methodological statements will continually arise. The
implication of this would be that the structure of methodology, the
content and number of matrices, is open-ended, indefinite, and
indeterminate. Therefore, the introduction of the time element would
make the choice problem as suggested above an insoluble one for the
scientist.

The assumption of a timeless, static system of matrices will now
be made in order to clarify the conditions facing the scientist and
to delineate more precisely the problem of methodological choice.
Initially, each of the matrices is on an equal basis, with the
absence of any criteria or rules for the selection of a single matrix.
Thus, in a sense the scientist confronts a random distribution of
equi-valued matrices. Then, if random selection is ruled out, we must
search for some other method of selection, or some other decision
criteria.

Several procedures or decision criteria are likely, and should be
given attention.
(a) The complete rejection of methodology  
(b) The attempt to discover a reference standard or criteria for selection  
(c) To develop agreement among scientists concerning a single matrix  
(d) To attempt to reduce the number of matrices  

However, each of the likely procedures possesses weaknesses. Methodological rejection is the easiest procedure to adopt, particularly if the scientist has an implicit belief or attitude that methodology is inherently "sterile". The quest for a reference standard or criteria for selection not only would involve us immediately in the realm of technical philosophy but also would be subject to the same dilemma of matrices once confronted with the mention of the existence of "different" standards. Agreement would be possible, but only in a frequency distribution sense, with choice made solely on the basis of what the majority considered to be the "right" matrix. Finally, the preliminary reasonableness of the attempt to reduce the number of matrices would eventually force the scientist to cope with the limitations of the second and third approaches.

It would seem that the choice problem for methodology, as outlined in the preceding, has no definite solution. The scientist confronts a multi-dimensional indifference map of matrices from which he is expected to make a "best" choice or decision. However, no provision for determining the best choice is available, that is, without destroying the content of methodology. Thus, the only practical alternative is to admit the basic difficulty and to attempt to develop an individual perspective that would avoid the restrictions of a
narrow methodological conception.

Initially, such perspectives would vary tremendously among scientists, for there are many ways by which insight and understanding can evolve in methodological work. Ultimately, however, it might be hoped that the practice of methodological evaluation, carried on largely in terms of a sympathetic understanding of the issues inherent to this activity, will result in improved methodological criticism and lessened controversy. The actual content of methodological perspective, of course, cannot be sharply defined, only general attributes can be suggested. They would include: an awareness of the inherent complexity of methodology; knowledge of contradictory frameworks and approaches, standards, and attitudes; understanding of the way in which issues are generated by contradictory positions; familiarity with the work of methodologists outside a given science; acquaintance with the ideas of scientists dealing with methodology within the scientist's own discipline; and, a background of training in the problems, theories, and techniques of a particular science.

In brief, the evaluation of problems of a discipline in methodological terms requires a perspective in methodology. It is not something that can rightly be pursued with the application of a set of preconceived notions about the "proper" way to conduct a science. Nor is it something to use on a science, even after the development of specified methodological criteria, unless the criteria themselves are subject to the same type of scrutiny as the details of the science. Thus, the scientist must seek out the insights and understanding, weigh them carefully in the light of the previous obser-
vations, prior to methodological evaluation. For this reason, the next two chapters are devoted to a consideration of methodological issues in fields outside of economics (Chapter III) and to methodological ideas within economics (Chapter IV) to indicate the foundation upon which methodological evaluation of problems of economic analysis must rest.

Summary and Conclusions

The failure to understand the complex nature of methodology has been an important reason for the failure of methodological work to command authority and respect in science. To overcome this inadequacy of comprehension, methodology is presented in terms of five dimensions: (a) definition and meaning, (b) functions of methodology, (c) approach or framework, (d) standards, tests, and criteria, and (e) attitudes and preconceptions.

Definition in methodology embraces two distinct issues, one related to the general problem of definition and the other to differences in meanings of methodology. While the first issue has some relevance for methodological understanding, its most significant aspect is that two extreme points of view can affect attempts to provide meaning for methodology. The scientist should be prepared to tolerate the views that: definition of a subject matter has no meaningful consequences on its principles; and, methodology cannot be understood unless it is properly defined. For the issue of differences in defining methodology, it should be recognized that the word has been used to refer to a separate science, a logic, a study.
and description. In skeletal form, then, methodology is thought of as rational-analytical or empirical-descriptive. Yet, while either meaning has some merit, it is not necessary to determine which best fits methodology at this early stage in exposition.

In terms of functions of methodology a wide, varied field unfolds. The functions of methodology that might be performed for a science include: (a) settlement of methodological controversies, (b) analysis of the foundations of prediction, (c) specification of implicit theoretical laws in a science and the development of their interrelations, (d) coordination of different kinds of scientific activity through the attainment of methodological insight, (e) activity as a methodological critic, and (f) the promotion of mutual understanding among scientists. These various tasks or functions of methodology show the generous scope possible in methodological work, illustrate the many activities performed under its aegis, and free the subject from narrowness in conception.

Methodology is handled from different frameworks or approaches, including historical, descriptive, comparative, and analytical ones. Differences among methodologists thus have an origin in contrasting frameworks or approaches. It is not possible, however, to determine the best framework, although such attempts have been made. Instead, the importance of the diversity at this level is the realization that insights, inferences, and evaluations arise out of differences in frameworks for handling methodological problems. To neglect the variety existent unduly restricts the perspective of a scientist and tends to make methodological judgment and evaluation a matter of
conformity to particularized standards. Errors resulting from narrow conceptions are difficult to eliminate when they are considered only from the standpoint of internal consistency with a specific framework. Thus, it is necessary to view conflicting methodological ideas as representing disagreement, not so much in observation, but in conception.

There are also significant differences occurring in the standards, tests, rules, or criteria of methodology. Evaluation thus involves a conflict among systems of rules and a conflict over the way in which the rules themselves are to be appraised. Much of the heated controversy in methodology originate at this point.

Differences in attitudes and preconceptions create an environment for methodological controversy that makes agreement among disputants difficult. From consideration of two questions: Is methodology an independent subject or must it be pursued within the confines of a special science?; and, Does methodology provide a contribution to science or is it sterile?; it is indicated that attitude plays an important role in methodology.

The discussion of the inherent complexity of methodology, the differences present in the "dimensions" of the subject, leads to a broad conception. The scientist must be prepared to cope with three conditions of the methodological environment: (1) direct contradictions of methodological principles, findings, and positions, (2) direct agreements, and (3) exclusion of principles, findings, and positions. Methodological ideas, in terms of this structuring, appear as claimed to be "true", "false", or "excluded". Patterns of
methodological matrices evolving from this construction are almost limitless, posing a difficult choice problem for the scientist. The reconciliation of the choice problem, i.e., determining the correct methodological matrix, requires a perspective in methodology.

The development of methodological perspective cannot be limited to any one framework, systems of standards and tests, or concept. The scientist, in order to carry on methodological evaluation within his discipline, must accumulate insights from various sources. He must seek them out in other sciences, in the methodological work of his discipline, and weigh them in light of the foregoing discussion of the complex issues of methodology. Then, and perhaps only then, is he in a position in which his evaluation of the technical and theoretical problems of his science will have meaning and significance.
Chapter III

Methodological Perspective: Non-Economic

In the broadest sense of the word, the methodological perspective of a scientist is fashioned by a variety of forces: intellectual equipment, training, age, experience with his discipline, contact with methodology in other fields, etc. For this reason, perspective is best viewed as the result of accumulated insights. However, while such accretion in understanding is invaluable, much will be wasted if the process of acquiring the insights of methodology is limited to a particular methodology or to issues of a given science. Thus, it is almost mandatory that the scientist have acquaintance with methodology and related problems existing in areas removed from his special discipline. In order to accomplish this purpose this chapter is devoted to an examination of some methodological ideas external to the scope of economics and its corresponding "methodology", to several methodological issues arising in the physical and natural sciences, and to the implications of the findings on the status of methodological evaluation. Of course, it is not possible to develop methodological perspective in its fullest on the basis of the selections discussed, for there are many conceivable topics and issues to be examined and the few considered at this point can only represent a small fraction of the total picture. Nevertheless, the expectation that guides the selection and presentation is an optimistic one, to hope that the following sheds light on the subject of methodology from a non-economic
point of view and that it offers suggestions for future work along these lines.

Scientific Weltanschauung and Metaphysics

It would appear to one with modest understanding of the nature of science that its chief characteristics would be a precision of form, content, and results greatly transcending the fuzzy nature of common sense and the crude complacency of conventional wisdom, along with the possession of a power of generalization to remove science from the pedestrian accomplishment of collecting and storing bits and pieces of unrelated data. Yet, a deficiency in understanding may arise when the imagined characteristics are attributed to all scientists, or even to the physical scientists alone. There is less than unanimity among scientists concerning their understanding of the manner in which what is called scientific activity acquires, organizes, and generalizes phenomena.

In some quarters of the physical sciences the view is held that science, the "genuine, valid" kind and not pseudo-science, must press for precision and purity in the form of scientific propositions. Those propositions that have an inherent indeterminate construction, the Heisenberg principle notwithstanding, are suspect and receive the cold enthusiasm that a Supreme Court justice finds at a meeting of a White Citizen's Council in South Carolina. Above all, the weltanschauung of the members of the group requires that science develop "the" general theory for science, a theory broad enough to embrace every facet of experience and sufficiently comprehensive to explain
conclusively the interrelationships of all data.

Of course, many scientists would hesitate to admit that this exaggerated viewpoint influences the structure of their work. It suggests an impossible objective for science; and, the present accomplishments of science, measured in terms of the yardstick of this type of performance, are openly meagre. Nevertheless, it must be recognized that degrees of this view persist in science and that they have much to do with the evaluation of science.

Contrasting with the idea that science must be conclusive and completely general is the view, or scientific Weltanschauung, that the proper direction for science involves greater reliance upon the philosophy of empiricism, the minimization of fixed a priori principles, and reduced emphasis on the development of the general theory or formula for nature. From this point of view, uncertainty and indeterminate interrelationships among variables are not treated as orphan concepts lying outside the formal structure of integrated general theory but as fundamental parts of an activity continually unfolding into new dimensions. ¹

¹"The attitude of the physicist must therefore be one of pure empiricism. He recognizes no a priori principles which determine or limit the possibilities of new experience...we must give up the demand that all nature be embraced in any formula, either simple or complicated. It may perhaps turn out eventually as a matter of fact nature can be embraced in a formula, but we must so organize our thinking as not to demand it as a necessity". P. W. Bridgman, The Logic of Modern Physics, New York: The MacMillan Company, 1938, p. 3. Also, see his Reflections of a Physicist, New York: Philosophical Library, 1950, pp. 7, 83. Cf., Henri Poincare, The Foundations of Science, New York: The Science Press, 1913, pp. 129-130; and, Abraham Wolf, "Scientific Method", The Encyclopedia Britannica, Fourteenth Edition, Volume 20, 1936, p. 131."
The dichotomous nature of the opposing scientific viewpoints carries several important implications for methodology and methodological evaluation. If it is thought that science seeks universals, certainty, and a general and comprehensive theory,\(^2\) then findings are likely to be evaluated and judged in terms of such characteristics. The presence of uncertainty and indeterminacy would be evidence of weakness in science. Moreover, the development of a science would be shaped more in the form of a logical, deductive system, with basic assumptions and deduced propositions given more attention by scientists than the refinement of investigative techniques and the empirical study of apparently disassociated phenomena.

If, however, scientists are resigned to the impossibility of a fully integrated general theory to explain nature, then evaluation is likely to be conducted on different terms. Scientific findings would be expected to include some element of uncertainty. Grandiose theoretical presentations allegedly explaining with absolute finality all of experience would be rejected. And, individual, scattered empirical studies would make up the major part of science from this point of view.

The presence of conflict at this level of science helps to explain why scientists are drawn into metaphysical discussions and debates. Matters of deep concern to professional philosophers, which is the case with metaphysics, may not be of direct concern to

scientists. Yet, the differences between scientific outlooks are so pronounced and have such implications for methodology that it is necessary to examine the status of metaphysics in science.

There are contradictory views concerning the role of metaphysical ideas in science. In some quarters the argument is advanced that in metaphysical notions is found "the final controlling factor in all thinking whatever". On the other hand, the efforts made to eliminate metaphysics from science are highly commended. Moreover, some would admit that metaphysics permeates science, yet make the issue of the fundamentals of science a matter of "what do we mean" and "what are we really talking about" with metaphysics.

The conflicting views have to be recognized by scientists as influential factors in the appraisal of the structure of a science.

3 Edwin A. Burtt, The Metaphysical Foundations of Modern Physical Science, Garden City, New York: Doubleday Anchor Books, Revised Edition, 1955, p. 17, and esp. pp. 227-230. Another claim is that science cannot avoid metaphysics; that science is dominated by metaphysics; and that the attempts by scientists to deny metaphysics with semantics, hypothesis, and the like, are a waste of motion. The conclusions of science "are always subject to revision, yet always assume the form of finality. This shows the metaphysical matrix in which science is cast." Peter Carmichael, "The Metaphysical Matrix of Science", Philosophy of Science, Volume 20, Number 3, July, 1953, p. 211.

4 For the praise showered upon the work of Ernst Mach in eliminating metaphysics from science, see Philipp Frank, Between Physics and Philosophy, Cambridge: Harvard University Press, 1941, p. 226.

5 "I do not at all wish to investigate here the foundations of the principle of induction; I know every well that I should not succeed; it is as difficult to justify this principle as to get on without it". Henri Poincare, op. cit., p. 345.

A discipline may be placed under attack on methodological grounds that are strongly tinged by metaphysical ideas. The physical science that devotes attention to the search for ultimate truths can be criticized on a metaphysical basis that claims the foundations of the search are illegitimate, i.e., ultimate truth is not possible for a world of uncertainty and change. Conversely, similar criticisms can be directed at the metaphysical foundations of the science that denies the permanence and certainty of scientific findings. Finally, it is clear that in economics such contradictory ideas are seen as the reason for different types of development in the nature, scope, and problems investigated in the discipline.  

Thus, it is important that the scientist recognize the role that diverse scientific outlooks, conflicts in scientific Weltanschauung, and contradictory metaphysical ideas have for his science. Scientific theories and empirical findings may appear very reasonable and safely protected from adverse methodological evaluation due solely to the fact that the entire process is conducted within a given scientific outlook and system of metaphysical notions.

Methodological Standards and Rules

According to proponents of a metaphysical position mentioned in

7 Whether the economist takes the essence of economic change to be mere mechanical motion or, instead, significant evolutionary development has much to do with his views on the nature and scope of economic science. Where the economist takes the former approach to economic change, economics becomes a narrow discipline which is mainly concerned with the study of economic equilibrium...Where the economist considers change to be a matter of growth, development, and evolutionary expansion, economics turns out to be a broad social science which is more capable of coming to grips with the major
the preceding section, the practices of a science cannot be separated from the foundations of the science. Nor, for that matter, is it possible to isolate the standards and rules used to evaluate what is, or is not, science from the same foundations. However, in order to present another source of variety in methodological evaluation, that which arises in the standards and rules applied to science, that particular position will need to be rejected. In brief, widening the methodological perspective of the scientist requires a review of conflicting ideas existing in the standards and rules of methodologists.

In the physical sciences confusion and contradiction among advanced hypotheses have been evaluated according to different standards. In one case the standards emphasize the need for clarity in language use and the reliance upon facts to settle inconsistencies arising among hypotheses. It is argued that there are many instances in the physical sciences in which plausibility of findings has depended on obscurity in their presentation. The removal of the innate obscurity requires clarification, which many times depends "on mere matters of definition". However, there are other situations in which the definitions themselves differ, where the conflict is not amenable to language clarification. Thus, a different principle must be used: the appeal to fact. The scientist who disapproves of a given definition "will still have to recognize and deal as best he

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8 Bread, op. cit., pp. 112-113, p. 356.

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can with all the facts which have been passing under review."^9

In theoretical constructions of scientific hypotheses, the assumptions are subjected to a factual test, along with submission to the test of clarity in language use. Moreover, the assumptions must meet another type of test or standard relative to traditional theory. Besides fulfilling the test of fact, then, the assumptions must agree with traditional theory and be able to account for things not accounted for by traditional or established theory. When several theories fit the same set of facts another test is applied. The more complicated and artificial theories are to be rejected in favor of the more simple. Structurally, the standards offered for theories in the physical sciences would resemble the following:

(1) Two theories based on different assumptions give roughly the same result.

(2) But, one is to be preferred over the other because it avoids:

(a) assumptions conflicting with laws established in other cases, and,

^9^Tibid., p. 358.

^10^Tibid., p. 202. The core of the idea might also be considered in a different sense: "I can hardly hope that what I have been saying about Time and Change will satisfy most of my readers, or indeed, that it is more than a shadow of the truth, if that. It is admitted that this is the hardest knot in the whole of philosophy. The Dean of Carlisle judiciously remarks that 'we cannot understand Time, but we shall not understand it better by talking nonsense about it". (p. 84) And, "As Critical Philosophers, it is our business to try to deal with all the facts, and not to hush up the existence of abnormal sense, as though they were the pecadillos of a Cabinet Minister". (p. 324).

^11^Tibid., p. 203.

^12^"Let us now consider what these arguments have to teach us.... We see that three extremely different lines of argument tend to the
(b) disconnected assumptions, i.e., each assumption would be independent of others;

(3) while the preferred one:

(a) is based on a single assumption explaining all the different facts, and,
(b) conforms to established laws.

Yet, the automatic or mechanical appearance of the standards is apt to be misleading for application. It is well recognized that many times assumptions and theories can only be checked or proved in an indirect fashion.\[13\] A further complication for application of standards is that even well-grounded theories of a science, almost fully accepted on the basis of theoretical tests, may lose much of their effectiveness through their communication or "popularization."\[14\]

Social scientists are provided with an explanation of one of the difficulties they experience with the lay understanding of the "law" of supply and demand. This time, however, it is the physicist who speaks: "I have now sketched to the best of my ability the gradual modifications which experimental facts and reflection upon them have forced upon physicists. There are two dangers to be avoided by plain men. One is to think that the Theory of Relativity is essentially unintelligible to all but profound mathematicians, and that therefore it is useless to try to understand it. The other, and
The significance of the foregoing standards for scientists in other fields, particularly economics, depends largely upon the methodological perspective of the individual. For many, there is nothing strikingly new or unusual in their content. The standards represent the inner profile of tests underlying procedures of science commonly followed; they are part of the orthodox and traditional ideas guiding and directing science. However, since they are part of the conventions and customs of science, they are not likely to be examined critically nor to be questioned from the perspective of different standards. This can be a dangerous consequence for the advancement of science. Traditional standards may be ill-equipped to deal with the development of new hypotheses or new understandings of changed phenomena. They may grant disproportionate validity to theoretical constructions that have served in the past as fruitful explanations of the data of experience but which have lost their power for contemporary analysis.

Thus, we should recognize that standards of this type have important implications for a science. It may be helpful for a scientist to evaluate his work, and the work of others, in terms of such standards that give preference to precision and clarity of language use over ambiguity; to the simple formulation of theory over much more serious danger, is to suppose that it can be made intelligible in popular expositions of a few pages to men who never had occasion to consider the subjects with which it deals. Like every other conceptual scheme it grew up, by a kind of inner necessity, against a whole background, of interconnected concepts, principles, and experimental facts. Presented in the absence of this background it is and must be as unintelligible as the orthodox doctrine of the Trinity is to persons who know nothing of the theological controversies which preceded the formulation of the Athanasian Creed". Ibid., pp. 214-215.
complicated statements; to the appeal to facts over raw speculation; and to the integrated explanation over loosely framed ones. However, complications arise in the administration of the tests and standards in the form of conflicting definitions, the "popularization" of elementary theoretical constructions, the impossibility of direct testing of the assumptions of theory, and the likelihood that integrated explanations may not be suitable. Limitations of this order suggest that the orthodox reliance upon these standards may be somewhat misplaced.

However, the position of standards in the physical sciences would be distorted if it were assumed that the foregoing summarized the ideas of all physical scientists. To the contrary, there are other systems of standards advanced for their science, including one that is called "operationalism." Proponents of operationalism view it as a powerful system for determining the accuracy of scientific hypotheses. And, for several reasons, operationalism has invaded other sciences.  

Difficulties in the formative stage of the idea of operational tests partially explain the confusion that often surrounds the standards involved. The origin of the standards was not sharply distinguished from the application of the standards to scientific

15 Economics, for example, has been criticized by some economists for its failure to adopt operational techniques and for its operationally inadequate procedures. John Perry Miller criticized empirical indexes of monopoly and competition, that with the exception of the Lerner index, "the operational usefulness of these indexes is not great". National Bureau of Economic Research, Business Concentration and Price Policy, Princeton: Princeton University Press, 1955, p. 123. Also, see the remarks by A. G. Papandreou, that economic concepts "have not been formulated in any operationally meaningful fashion". Ibid., pp. 439-440, and, p. 396.
hypotheses. Historically, operationalism grew out of personal observations of the way in which science was actually conducted: "For of course the true meaning of a term is to be found by observing what a man does with it, not what he says about it." The origin of the system would be found in the observation and description of methods which some physicists had already adopted and had used successfully.

The familiar understanding of the content of operationalism rests in the idea that scientific concepts have meaning, not in terms of Newtonian properties, but in terms of the operations involved in the concepts. However, the core of the system of standards used in operational testing also contain reference to checking and testing theories. In brief, a theory that is operationally sound must be one that can be checked step-by-step, if necessary, by some other scientist. The terms or concepts of theory, if they are to have operational meaning, must be stated in such a way that they can be "checked" by others.

Terms used in a scientific context must be subject to the presuppositions of scientific enterprise. One of the most important of these is the possibility of checking or verifying the correctness of any statement; in fact, this may be taken as characteristic of any serious enterprise. Checking that the conditions are satisfied is done by performing certain operations, so that for all essential


purposes the definition may be specified in terms of the checking operations. In order to be of practical value, the operations must, at the very minimum, be such that they are repeatable and performable on demand; in fact, this would seem implied in the idea of checking. Whether there are such operations or not can be found only by experiment, and, as in every experimental situation, absolute precision is not attainable. No operation can be specified with absolute precision, and no attempt to repeat an operation can be proved to have been completely successful. But in practice there are operations which can be repeated by the same person or different persons under the same or different conditions without hesitation and with the accompaniment of no phenomena which demand the assertion that there has been failure to repeat. Definitions should be framed in terms of operations of this sort. It is not necessary that such operations be especially simple, or that they be analyzed into all discernable components—merely that they be repeatable with assurance.\textsuperscript{19}

The requirement of personal checking by a scientist is an important cornerstone of operationalism. According to this standard, the "examination of what I do shows me that I accept the verdict of my neighbors only as an easier method of getting the result to which my own activity would have led me..."\textsuperscript{20} While the standard is basically an \textit{ex post facto} one to be applied to those scientific hypotheses that have already been advanced, it also has influence at the stage of developing new hypotheses. In this case, the operationalist claims that testing necessitates the willingness on the part of the scientist to try out the new theory.\textsuperscript{21}

The extent to which a scientist relies upon, or puts faith in, the orthodox standards of scientific procedure or in the application of

\textsuperscript{19} Bridgman, \textit{Reflections of a Physicist}, op. cit., pp. 27-29.

\textsuperscript{20} Ibid., p. 47. Also, see pp. 72-73.

some different sets of standards and criteria depends on various factors. However, the important aspect of such activity should be the clear recognition that criteria are being applied in evaluation and that different criteria are feasible. While it may be difficult to determine which criteria is the "best", due to the basic complexity of methodology, the scientist would be in a better position to come to grips with the issue of standards if he has noted that there are differences.

Added Insights into Methodology

To trace out in comprehensive style the full implications of the methodological ideas of scientists in the non-economic disciplines would be a worthwhile goal for the elaboration of perspective. Upon completion, the scientist would have sufficient grasp of the materials upon which methodological evaluation is based, would recognize critical issues and restrictions, and thus be in a position to evaluate with considered judgment the problems, controversies, and dilemmas of his field. Unfortunately, within the brief span of this paper it is not possible to advance a great distance along such a path to the ideal. However, it is possible to suggest several additional areas of difficulty for methodology and to utilize them as prototypes of the work that remains to be done.

General Theory or Dualism

One of the issues that has confronted the physical scientists in their questioning of the fundamentals of scientific procedure is the extent to which a single, general theory of phenomena is possible. In
some quarters it is held that physical science is inevitably driven
towards the status of a completely integrated general theory, that the
ultimate goal of science is the creation of a general theory to explain
all of the relevant physical data.\textsuperscript{22} On the other hand, we need to
note the equally prominent view that denies the possibility of achieving
that stage in physical science and instead claims that "dualism", i.e.,
the existence of several theories, is more valid.\textsuperscript{23}

The interest of physical scientists in this issue has real meaning
for the economist. If dualism is, in fact, unavoidable, then the
efforts of economists to join two distinctive theories into one will be
misdirected.\textsuperscript{24} Or, to state the consequence of the possibility of

\textsuperscript{22} Broad, \textit{op. cit.}, pp. 547-548.

\textsuperscript{23} "It seems to me that the discovery of inherent limitations to
the accuracy of physical measurements disclosed by wave mechanics opens
the door to a flood of 'possible' theories....We are thus faced with a
radically new situation which may well alter the entire future of theory
building. Doubtless a great many alternative theories will be possible,
and we shall have to choose between them on grounds of simplicity or
convenience of calculation or perhaps on purely esthetic considerations.
It may be that one type of theory will prove to be simple and convenient
when dealing with certain aspects of phenomena, while a radically
different theory may prove better for other aspects. We have a
suggestion of this sort of thing in relativity theory, where one
procedure is mathematically simplest for measuring the length of a
moving object, whereas an entirely different procedure is simplest
hypothetically. Under such circumstances we would be foolish to stick
to a single type of theory for all uses, in the idea that only one theory
could be the 'right' theory. I suspect that Bohr, with his dual aspects
of reality, is one of the first to exploit this possibility. The
celebrated remark of Sir William Bragg that we seem forced to use
classical mechanics on Monday, Wednesday, and Friday, and wave mechanics
on Tuesday, Thursday, and Saturday may prove not to be a reductio ad
absurdum, as it is usually taken to be, but an ultimate and necessary

\textsuperscript{24} Some economists view with alarm a "gap" between micro- and
macro-analysis, calling for an integration of theory to bridge the gap.
"In conclusion, it should be repeated that the process of including
dualism in slightly different fashion, the economist may be better off than is sometimes imagined when attention is focused on the development of an independent theory even though the theory departs from what is considered as the general core of the theory of the subject matter. Moreover, if physicists are unable to accomplish the merging of theories with what they admit is relatively less complicated data than that of economics, perhaps it is unreasonable to require this of economists. And, the same consequence might be directly applicable to the problem of aggregation.

Mechanical Models

The notion that "mechanical" models are the exclusive property of problems of the cost-price structure in the theory of employment is still in its early stages. The theory of employment is frequently made to proceed on the assumption of a given cost-price structure, and it is then concerned almost exclusively with aggregate money flows, without investigating the relationship between these, on the one hand, and cost-price problems, on the other. At the same time, value theory typically proceeds on the assumption of a given aggregate output and employment, and it is concerned merely with the relative allocation of resources. It is to be expected that the links between the two will grow tighter and that significant interactions will be explored more fully. William Fellner, "Employment Theory and Business Cycles", in A Survey of Contemporary Economies, Volume I, edited by Howard S. Ellis, Philadelphia: The Blakiston Company, 1949, p. 86. Of course, economists implicitly accept the existence of dualism in theory with the distinctions between the consequences of theory under the contradictory assumptions of a full-employment versus a less-than-full employment economy.


26Bridgman, The Logic of Modern Physics, op. cit., p. 51. Sociologists have mixed reactions to the problem of aggregating atomistic parts into a macro theory. According to C. Wright Mills, both "macroscopic and molecular" models of inquiry are currently used in social studies, posing serious questions of the process of building up one from the other. "Two Styles of Research in Current Social Studies", Philosophy of Science, Volume 20, Number 4, October, 1953, pp. 266-274.
the physical sciences is sometimes held by social scientists, with the disturbing consequence of "coloring" methodological evaluation. Frequently the idea is associated with an organic or evolutionary view of the substance of social phenomena, although this need not be the case. What is significant is the recourse to the argument that mechanical models are inappropriate for organic social phenomena while fitting and proper for mechanistic physical phenomena. If it were true that the nature of the data of the physical versus the social sciences was of such an order that it predetermined the type of models used in explanation, the argument might have greater validity. However, the physical scientists, who are expected to use mechanical models exclusively, are skeptical of its wholesale application. Thus, this might serve as a precautionary reminder to social scientists in considering methodological evaluation to hesitate in too rapid reference to the mechanical models of the physicists.

The point at issue for the economist's development of perspective does not involve so much the issue of which model is correct, at least at this stage, but the recognition that physical scientists are not convinced that their phenomena has to be explained with the use of mechanical models.

"I believe many will discover in themselves a longing for mechanical explanation which has all the tenacity of original sin. The discovery of such a desire need not occasion any particular alarm, because it is easy to see how the demand for this sort of explanation has had its origin in the enormous preponderance of the mechanical in our physical experience. But nevertheless, just as the old monk struggled to subdue the flesh, so must the physicist struggle to subdue this sometimes irresistible but perfectly justifiable desire." Bridgman, The Nature of Physical Theory, op. cit., pp. 46-47.
Continuant Versus Occurrent Conditions

A problem that faces the physical sciences, and one having significance for other disciplines also, is the determination of the conditions bringing forth an observed result.

Every event depends on two kinds of conditions, which we may call occurrent and continuant, borrowing two useful names from Mr. W. E. Johnson. We are always very liable to notice the concurrent and to ignore the constituent conditions, and then to think that the former are sufficient to produce the event. It would commonly be said that the stroke of a bell is a necessary and sufficient condition of the occurrence of certain vibrations in a surrounding medium. So it is, provided that there is a material medium in contact with the bell, and that it is capable of being set in vibration by a disturbance of this particular period. It is evident that the latter condition is as necessary for the setting up of vibrations as the former. But the striking of the bell is a short outstanding event in that long and fairly uniform strand of history which is the bell; whilst the medium and its structure existed before the bell was struck, and will exist with very little change for long afterwards. Moreover, bells are much more often than not surrounded with such a medium. The medium is thus such an unexciting and such a usual piece of physical history that we hardly think it worth mentioning.28

The implications of this problem for methodological evaluation deserve attention by the social scientists. The physical scientists here suggest that it is possible to be deeply impressed by the outstanding event, consequently ignoring the latent conditions also critical for the occurrence of the event. This implies greater caution in the identification of the conditions surrounding an event, more concern with all of the factors bearing upon an event. Moreover, there is the express warning that the scientists may take conditions for granted, when the conditions themselves are an essential part of the determi-

nation of the happening.

Of course, for those social scientists to whom the entire socio-economic, cultural fabric of an environment, and the evolution of such environment, is the proper stuff of scientific research, this presents no relevant warning. They are conceptually prepared to identify and to analyze the constituent conditions of unique events. Nevertheless, it seems useful to advance this type of problem as conceived by physical scientists as a constant reminder to social scientists to exercise due caution in analysis.

Meaningless Questions

It is possible to ask a far greater number of questions in scientific enterprise than can conceivably be answered. Thus, there arises a situation concerning questions and their answers that is analogous to the scarcity problem in economics. In economics, from one point of view, the central problem of a society is that wants exceed the means or resources for satisfying those wants. Similarly, in science the number of questions that can conceivably be asked exceeds the resources available for their answers. For this reason, a choice problem, or decision issue, develops for the scientist. What, he asks, is the basis for selecting the significant and meaningful questions from the much larger group of conceivable questions.

Since the operationalists in the physical sciences have claimed that one of the chief benefits of operational tests is the ability to dispose of meaningless questions, the basis is one rooted in the

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29Bridgman, The Logic of Modern Physics, op. cit., pp. 28–30, and
standards of operationalism. Others, however, would review the content of basic questions that have plagued philosophers and scientists for the course of many centuries against the yardstick of the contemporary equipment of science. In this case, those questions that deal in ultimates are judged to be meaningless, on the basis that scientific procedure can only provide probable results when dealing with what is inherently uncertain data, i.e., data subject to persistent change.

Thus, the problem of the significance, or conversely the meaningfulness, of questions is of direct methodological concern to any science. Given the existence of many conceivable questions, the scientist must decide which questions need answering. However, the decision involves grounds or rules for the decision, and this is a methodological matter. Even if the grounds, rules, or bases are fully developed, that is, have been explicitly formulated, they will not be free from methodological criticism. The scientist would still have to contend with the methodological issue of the primacy of his grounds over other possible grounds or rules.

Yet, while it is beneficial to call attention to the problem of meaningless questions as a device for sharpening the awareness of scientists in what they are doing and for emphasizing the critical nature of their selection of questions, there is a possible danger in excessive concentration on the grounds or bases of selection. If it should develop among scientists the belief that contemporary scientific

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concepts and techniques are the real determinants of what is, or is not, a meaningless question, then this might lead to the elimination of many of the provocative questions of public policy in the social sciences and the stultification of social research.\(^{31}\)

Comparison of Sciences

The comparison of the physical and natural sciences with the social sciences may be the source of ego-satisfaction for the former:

Professional pride rather than the desire for scientific accuracy makes one deny the inferiority of the social to the natural sciences with regard to established and universally verifiable laws.\(^{32}\)

Although the points of comparison vary, usually they include reference to the following types: (a) vagueness in concepts and general rules of evidence, (b) absence of precise instruments for testing and measurement, (c) inability to apply the probability curve to the few isolated cases and observations collected, (d) lack of experimentation and the opportunity of performing the critical experiment, (e) failure to predict with certainty, and (f) excessive concern with practical problems and their solutions.\(^{33}\)

However, there are impressive difficulties in the attempt to compare sciences. A basic problem that arises in such attempts is a methodological one of determining the points which are to be compared.

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\(^{33}\) Ibid., pp. 354-357.
Differences may easily arise at this level. Moreover, once it is admitted that a variety of comparisons is possible, depending on one's selection of yardsticks of performance, then there is the additional methodological issue of determining which of the yardsticks should serve as the valid standard of comparison. Perhaps the easiest "trap" into which the scientist falls is to identify certain key aspects of the procedures of his particular discipline as the only proper ones for science. Sciences in which measurement to very exact tolerances is possible are likely to consider the precision of measurement as a cardinal virtue of all science. Similarly, where experimentation on controlled variables is more or less simple to conduct, this may result in the elevation of controlled experimentation as a basic feature of science. Thus, it is not unreasonable to expect other sciences to develop the virtues of their procedures and to defend them against outside attack. Hence, comparison of sciences affords a potent source of controversy.

If social sciences were to evaluate the conduct of their disciplines against the yardsticks of the physical and natural sciences, they would almost certainly come off second best. Yet, some of the comparative points of the latter sciences are questionable by the physical scientists themselves. To the social scientist, the physical sciences give an impression of carefully-controlled and administered experiments, conducted within the confines of an immaculate laboratory. In many cases, this may be true; nevertheless, it should be recognized that the ability to conduct controlled experiments and to multiply such experiments almost endlessly does not automatically provide
positive accomplishments. In much the same fashion, the social scientist might do well to re-examine the promises that are often made for increased emphasis upon logic and mathematical formulation in science.

In brief, the efforts to compare sciences require the exercise of considerable caution. Any criteria useful for such an activity may be slanted to the internal structure of a particular science, consequently increasing the probability that for simply this reason the particular science will rank at a superior level to other sciences. Furthermore, comparisons of sciences should involve less of the spirit of a "contest", in which a winner must be determined, and more of the quest for developing striking similarities and identities for enhancing the understanding of all sciences.

Social Science Methodology and a Formal Analysis

If the methodological perspective of the economist is shaped by

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34 "Yes, you reply, 'a single experiment is insufficient, because it gives me only a single equation with several unknowns; but when I shall have made enough experiments I shall have equations enough to calculate all my unknowns.' To know the height of the mainmast does not suffice for calculating the age of the captain. When you have measured every bit of wood in the ship you will have many equations, but you will know his age no better. All your measurements bearing only on your bits of wood can reveal nothing to you except concerning these bits of wood." Poincare, The Foundations of Science, op. cit., p. 86.

35 "I see in logistics only shackles for the inventor. It is no aid to conciseness—far from it, and if twenty-seven equations were necessary to establish that 1 is a number, how many would be necessary to prove a real theorem." Ibid., p. 472.

36 "Every age has ridiculed the one before it, and accused it of having generalized too quickly and too naively. Descartes pitied the Ionians; Descartes, in his turn, makes us smile. No doubt our
ideas and arguments of other scientists outside the scope of economics, then it appears necessary to direct attention to the external origin of such perspective. Part of the understanding of the economist thus would originate in the work of physical and natural scientists, an area of topics and issues discussed in the above. Moreover, some of the perspective might also be fashioned by social scientists whose fields of specialization would be other than economic. Thus, a limited examination of social science methodology would be helpful in illustrating some of the methodological variety present in these sciences.

Positions concerning the accomplishments of methodology in the social sciences, the manner in which controversial issues should best be handled, and the difficult methodological problems of the social disciplines are mixed. Some believe it impossible to construct a system of definitive methodological principles and standards for the social sciences, that only improved methodological understanding can be reasonably expected. While science may benefit from concentration upon the epistemological and methodological reflections of scientists, this cannot provide a sure and certain path to improvements in the conduct of scientific procedures. It may even be the case that rigid conformity to a methodological system would slow down the advance of

children will some day laugh at us.\textsuperscript{37} \textit{Ibid.}, p. 127.

For methodological reasons, social scientists are urged to improve prediction in their fields and to give increased attention to eliminating problems of communication arising from language use. They are advised to allow for methodological differences of "degree" and to be willing to accept as proper standards the criteria of "reasonableness". Comparisons of sciences are judged to have been faulty in execution, that the complexity of the phenomena in the social sciences "has been too greatly exaggerated", and that the differences existing in the quality of predictions among the sciences is a matter of degree.

Social science methodologists sometimes expect that contributions by different specialists crossing traditional boundaries of specialization will "present an integral picture of the present interrelations of the social sciences...as well as to lay bare the potentialities for future developments". Some would deliberately bypass the attempt to create a "treatise on sound principles of methodology", preferring


41 Ibid.

to rely upon detailed descriptions of techniques and research facilities for social scientists as a means for conveying insight into methodological problems and issues.  

Workers in the field of social science methodology also claim that science in general consists of the "duality" of theory and observation, further arguing that over-emphasis of either theory or observation must be avoided. From such a vantage point, allegations concerning the alleged inferiority of the social sciences are not justified. Such criticisms illustrate not the "impossibility of doing scientific work in the social sciences", but show instead the "relative complexity of the subject matter to be dealt with and the immature condition in which such studies find themselves."  

The demand for a change in the orientation of social research is made by some students of social science methodology. In place of the orthodox or conventional attack on contemporary problems would be substituted a framework for analysis that is policy oriented. This would require the cultivation of a technique or device for gaining cooperation among specialists from different social disciplines. It would generally consist of emphasis upon clarification of goals of a society, careful investigation of the problem currently faced by a


society, selection of a "most favorable" course of action in terms of
the criteria of probable outcomes, and the determination of optimum
means for carrying out the action. 46

While the excursions into social science methodology of the
preceding types serve as potential sources of insight into methodological
topics and issues, they often fail to emphasize the significance of the
standards and rules underlying social science procedures. Or, to state
the basic methodological issue somewhat differently, such studies often
neglect the formalization of social science methodology. For that
reason, some acquaintance with more formal, or theoretical, treatments
of social science methodology would be desirable for extending the
methodological perspective of the social scientists.

In one important attempt along these lines, the author states
that the major methodological problems in the social sciences are the
result of a failure to see the broad and general character of the
problems, to comprehend the different levels of arguments, and to
distinguish between analytic and synthetic propositions in science. 47
According to this treatment, science consists of rules of procedure
governing the conduct of the scientist in a way similar to that by
which the rules of logic govern the actions of the logician. Just as
the logician must obey logical rules in making decisions, so must the

46 Daniel Lerner and Harold D. Lasswell (Editors), The Policy
Sciences: Recent Developments in Scope and Method, Stanford,
47 Felix Kaufmann, Methodology of the Social Sciences, New York:
Oxford University Press, 1944, pp. 1-3.
scientist obey the rules of science in making scientific decisions.  

It is important for the scientist to be conscious of the rules of scientific procedure at the time propositions are accepted or rejected. And, the rules by which the scientist makes his evaluation should be explicit.

A scientist who rejects a certain approach to the solution of a given problem as inappropriate is bound to give his reasons, and, in so doing, he has to make explicit the implicitly presupposed preference rules... all objective criticism, whether it judges the acceptance of an assertion as unwarranted or an attempt toward the solution of a given problem as presumably irrelevant, refers to given rules that are declared to have been violated in the case under consideration. In other words, the criticism is in terms of these rules, just as the criticism of a mode of speech as ungrammatical is in terms of presupposed rules of grammar. One cannot substantiate either type of criticism without making explicit the underlying rules.

If it were possible to formalize social science methodology along the lines suggested, methodological controversy would cease to exist in its present form. A logic of scientific procedure would be applied to every procedural matter of science, and when a procedure violated the rules, it would be eliminated. However, viewing this consequence from the vantage point of the issue of conflicting logical systems, there would always be an issue of determining which of

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48"From the point of view of the logician, the procedure of an empirical science consists in the acceptance or elimination of propositions in accordance with given rules.... The scientist must not make a decision arbitrarily.... He must give grounds for each decision, i.e., he must show that it is possible (correct) in terms of the presupposed rules of scientific procedure. There are three different types of correct scientific decision in a science S concerning a given proposition p...." Ibid., pp. 48-54.

49Ibid., p. 71.
several conceivable systems should be adopted. Here, the formal system would need to be evaluated with a higher system of criteria. However, an important benefit of an attempt to formalize social science methodology should be recognized. If in the effort to construct a formal system the author identified the complexity of methodology and the problem of identifying the underlying assumptions of evaluation, this would tend to enlarge the subject conceptually.

Summary and Conclusions

The methodological perspective of a scientist is fashioned by a variety of forces and should thus be viewed as the product of accumulated insights. Much of the accretion in methodological understanding on the part of the scientist will, however, be of smaller value if the process of acquiring insight is limited to a particular form of methodology or to issues of a highly specialized discipline. For this reason, the scientist should be familiar, or attempt to become better acquainted, with methodological ideas that are "external" to his discipline.

Physical scientists vary in outlook or ideas concerning the proper structure and goals of scientific enterprise. To the methodologist, differences at this level influence the type of evaluation performed on science. One view of the structure and goals of science is the idea that science must have a precision of form, contents, and results transcending all other forms of acquiring knowledge. Theories developed by science must be precise and free from ambiguity and uncertainty. A contrary view places greater reliance upon the
philosophy of empiricism, the minimization of purely logical form and 
a priori principles, and the reduced emphasis on one "general" theory. 
Such views, or scientific Weltanschauung, carry implications for 
methodological evaluation, suggesting that it may be a product of a 
particular outlook towards science.

Related to differences in outlook toward science is the issue of 
the role of metaphysics in science. Controversy surrounds the question 
of the extent to which metaphysical ideas affect the course of science, 
with some argument that: (a) it has no effect, (b) it is the 
profoundly disturbing element in science, and (c) that metaphysics is 
basically a problem in semantics. Scientists, therefore, need to 
recognize the diverse positions concerning the status of metaphysics in 
science.

Conflicting ideas exist among physical and natural scientists with 
reference to the type of standards, rules, or criteria applicable to the 
evaluation of scientific propositions. In one system of standards the 
emphasis is on the need for clarity in language use and the reliance 
upon facts to settle inconsistencies. The assumptions of newly developed 
theories must agree with the assumptions of traditional theory and the 
ew theory must explain more than the older theories. Physical 
scientists, however, also lend support to the standards of 
"operationalism," in which scientific concepts are considered to have 
meaning, not in terms of properties, but in terms of the operations 
involved in the concepts. Moreover, operational standards or tests of 
scientific propositions require the ability on the part of scientists 
to be able to follow, step-by-step, the operations of a scientist who
offers a scientific hypothesis. Thus, methodological perspective demands familiarity with the problems of the underlying standards and criteria of science, recognition of conflicting systems of standards, and understanding of the possible consequences of differences on the character of methodological evaluation.

In a variety of ways, methodological problems and issues in the physical and natural sciences have bearing on social science methodology, particularly economic methodology. The possibility of the "general" theory of physical phenomena is debated among scientists, with some holding that the quest for such theory is essential, while others prefer to accept "dualism" in science, i.e., the existence of several theories. Mechanical rather than organic models are not, we discover, accepted wholeheartedly in the physical sciences. The failure to distinguish between occurrent and continuant conditions in explaining events poses difficulties for all sciences. Which questions are meaningful for science, and which ones are meaningless, involve important considerations for the scientist who is limited in the number of questions that can be answered. The comparison of the physical and natural sciences with the social sciences also emphasizes the difficulties inherent in the process of developing a singular, well-defined methodology for any discipline. In sum, the ideas of scientists in fields external to economics carry importance for methodological perspective. The questions and issues of methodology facing physical scientists may be identifiable in the methodological problems of economics. Thus, the manner in which issues are handled provide insights into the practices of economic methodologists.
For the economist, as well as other scientists, social science methodology contains ideas that influence the development of methodological perspective. And, the ideas of social scientists vary widely, with striking instances of contradictory positions on methodological questions and issues. The formal, highly theoretical and general attempts to construct a methodological system for social science are not impervious to criticism.

Thus, the major importance of a study of methodology external to economics is to free the economist, at least partially, from the confines of the concepts and terminology of his discipline. Ultimately, the economist must judge the scientific practices of his field in terms of its own unique characteristics, but this should occur only after he has developed an understanding of the innate complexity of methodology. The examination, then, of questions and issues encountered by other scientists in evaluating their work is essential for this understanding. Such examination illustrates diversity in fundamental points of view concerning the proper structure and objectives of science; the questioning conducted by other scientists into the standards and tests of science and the absence of unanimity; and the existence of contradictory ideas concerning the manner in which methodological issues are to be decided.

This places methodological evaluation for economics on a different basis. It cannot be assumed that physical scientists have been successful in constructing a definitive system of methodological criteria, which the economist need only to borrow when needed. Economists cannot refer to a position in methodology in the physical
sciences as sole justification of a similar position in economics. The use of physical science analogies for economics must be applied with caution. On the positive side, the economist should attempt to follow criteria fashioned by the study and analysis of the diversity of methodology. The acceptance of an open-ended methodological system has drawbacks, since evaluation will be on an impermanent, flexible basis. However, if it is not possible to close methodology completely, to establish permanent principles for the subject matter, the search for the immutable criteria will be less significant than the quest for greater understanding and appreciation of special criteria for specific problems of analysis.
Chapter IV

Methodological Perspective: Economics

The questioning of the reliability of the results of economic analysis may be traced back to the structure and procedures of economics as a scientific discipline, which would mean that problems of analysis are methodological. However, the attempt to eliminate, or to solve, such methodological problems is exceedingly difficult. Methodology, viewed as a subject matter furnishing guides and criteria for the procedures of science, is inherently complex. This implies, therefore, that the economist cannot expect to be able to borrow directly from methodologists a fixed set of standards for an evaluation of his theories, empirical investigations, and policy recommendations. The standards to cope with problems of analysis must be constructed or created.

The bases for developing standards and criteria of methodological evaluation will vary widely. Of course, economists may prefer to ignore the issue completely by claiming that the ultimate test of the adequacy of a scientific discipline is its ability to provide results that are publically accepted within the discipline, or publically accepted outside the discipline. From this point of view, the only basis for the evaluation of a science is consensus and acceptability, with the further implication that the economist need not become concerned with the foundations of analysis. So long as the results are accepted, the way in which the results are provided is unimportant.
However, the admission that procedures leading to results are important, that results are not the sole criterion, leads to examination of the inner structure of a science. Economists view the procedures of their discipline in different ways, from within different frameworks; thus determining the adequacy or inadequacy of analysis with the use of different yardsticks. Therefore, the basis of methodological evaluation will be subject to variations among economists.

If we consider the basis of evaluation as a reflection of the methodological perspective of the economist, we should be prepared to understand the nature of this perspective and its influence upon evaluation. The preceding chapters have indicated that scientists in other fields face conflicting methodological ideas, that there are important differences in methodological positions. A similar condition exists in economics. Economists engaged in the examination and evaluation of the procedures and practices of their discipline differ in outlook, principles, and ideas of methodology. It becomes necessary, therefore, to examine the positions held by economists with reference to methodology in economics.

A study of the works of John Neville Keynes, Lionel Robbins, and T. W. Hutchison will serve as a convenient device for highlighting conflicting methodological perspectives in economics. The three afford a classic instance of the origin of differing standards of evaluation. Each would defend the significance of methodology for economics, but each sees economic methodology in a different light. One would try to unify economists by allowing for the existence of different viewpoints on debatable issues; one would stress the necessity for a logical
system of economics; one would deny purely logical structuring and support the empirical development of economics.

The Attempt at Compromise and Synthesis

The work of John Neville Keynes represents an important contribution to economic methodology as a deliberate effort to reduce the divisive effects of methodological controversy. Directed at the factionalism between the historical and classical schools, the work embodies the idea of "compromise" and "synthesis" in economic methodology. The conceptual foundation of this approach is based on the idea that variety is unavoidable in the practices and procedures of economists, that "it is impossible to establish the right of any one method to hold the field to the exclusion of others."1

From such a framework, many of the methodological issues of contention among economists are seen to involve misdirected emphasis and understanding. The issues debated in methodological controversies are not really significant in the actual practice of economists. Thus, there are two divisions of economic methodology, what the economists have said about method—here differences are generally exaggerated—and the manner in which methods have been applied or practiced.2

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1John Neville Keynes. The Scope and Method of Political Economy. London: Macmillan and Company, Ltd., Fourth Edition, 1917, p. 6. Also, "The method of political economy cannot adequately be described by any single phrase; and accordingly no one method will be advocated to the entire exclusion of other methods. It will, on the contrary, be shown [sic.] 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. p. 30.

2An examination of the writings of Smith, Malthus, Ricarde, Mill,
Even if the so-called "characteristic doctrines" of schools of thought concerning method were singled out, it would be discovered that in actual practice "the difference is strictly speaking one of degree only". This would be the case, although the doctrines removed from the actual writings would appear to indicate a wide breach.

A prominent methodological issue among economists concerns the "scope" of their science, an issue that has been handled in different ways. In this approach, however, the issue of economics as a

Senior, and others would show "that their practice does not precisely correspond with their theory, and we are led to the conclusion that, judged by their own writings, they state their doctrines on method in too absolute a manner, in particular exaggerating the abstractness of political economy taken as a whole." Ibid., pp. 17-18.

\(^3\) Ibid., p. 29.

\(^4\) Keynes analyzed the central doctrines of method that were debated by economists to include: A. Economists who have a conception of economics as a positive science usually claim that: (1) A sharp distinction must be maintained between economic laws and their application. (2) Economics is an independent discipline divorced from social philosophy in general. (3) Economics is an abstract science, using a deductive method, and reasoning from a few ultimate premises selected from the many possible ones in nature. (4) Verification of laws is obtained by comparing the observed facts with the conclusions deductively obtained, and this enables the economist to determine the limits of application of his generalizations. B. Members of the opposing school, who conceive of economics as an ethical, realistic, inductive discipline usually hold to the ideas that: (1) There can be no purely positive science of political economy as typified by Cairnes' conception. (2) Economics must deal with ethical questions. (3) Insistently it is claimed that economic and other social phenomena are interdependent, and that it is impossible to have an independent discipline such as economics. (4) In method, emphasis is given to facts, induction, statistics, and evolution. Ibid., pp. 12-17, and 23ff.

\(^5\) Keynes asked if economics treats "of the actual or the ideal? Is it positive science concerned exclusively with the investigation of uniformities, or is it an art having for its object the determination
positive science, a normative science, or an art, becomes "to a certain extent a question merely of nomenclature and classification," although it is still important "to make clear their mutual relations. Confusion between them is common and has been the source of many mischievous errors." Although it is preferable to stress economics as a positive science, when action must be taken, when guides are established for the statesmen and legislators, then ethical and political considerations must be given due consideration. Policy and action constitute the end or goal of economic analysis, and specialization of this type will, by keeping the boundaries between the different divisions well-defined, facilitate rather than hinder the efforts of economists to achieve the policy level. However, the reliance upon specialization and concentration in positive economics should not induce the economist to forget that ethical considerations influence policy suggestions.

of practical rules of action?" Ibid., p. 31. For additional distinctions, see pp. 34-35.

6 Ibid., p. 35.

7 "No one desires to stop short at the purely theoretical enquiry. It is universally agreed that in economics the positive investigation of facts is not an end in itself, but is to be used as the basis of practical enquiry, in which ethical considerations are allowed their due weight. The question is not whether the positive enquiry shall complete as well as form the foundation of all economic discussion, but whether it shall be systematically combined with ethical and practical enquiries, or pursued in the first instance independently.... The latter of these alternatives is to be preferred on grounds of scientific expediency. Our work will be done more thoroughly, and both our theoretical and our practical conclusions will be the more trustworthy, if we are content to do one thing at a time." Ibid., p. 47.

8 "No solution of a practical problem, relating to human conduct,
The methodological issue of language and definition is another point of application of the "compromising" framework. Language introduces complex problems for a science, and correspondingly requires the exercise of greater care in the formulation of definitions, but it should not be thought of as the single source of all methodological difficulties. Wealth, for example, "is one of those words that may without disadvantage be defined somewhat differently from different points of view." Similarly, the attempts to define economics may be handled in different ways.

The question of the necessity for developing a single, comprehensive science of man and society, "the general theory" of human behavior in society, can also be disposed of with a compromise.

... can be regarded as complete, until its ethical aspects have been considered. It is clear, accordingly, that practical discussions of an economic character cannot be isolated from ethics. Ibid., p. 60.

9 Ibid., p. 94.

10 Ibid., p. 101. The basic philosophy concerning language and definition seems apparent in the following: "Here definition carries us a very little way; and to bind ourselves by rigid definitions, or even to attempt perfect consistency in the use of terms, may sometimes... hinder rather than advance scientific knowledge. Although much controversy in economics might be avoided by a clear understanding of the different senses in which terms are used, and the relation of different meanings one to another....

The truth is that in drawing hard and fast lines—as definitions compel us to do—there is necessarily something artificial; for such lines are not drawn by nature. Here, as elsewhere in economic matters, a principle of continuity is in operation, and different classes imperceptibly merge into one another. Hence arises the necessity of being content, in some cases, with definitions that are not absolutely unequivocal and determinate. Where this is so, the characteristics of the limiting cases that may arise will form a valuable subject of consideration, and attention should be called to them. But they may then be neglected, except where, in special connexions, they rise into exceptional importance. Ibid., pp. 154-155, 171.
according to this framework. Those who would press for the merging of
economics with other social disciplines are credited for their
recognition of the complexity of phenomena. The economists who demand
increased specialization of economics are praised for the benefits
derived from division of scientific labor and specialization. Both
types of pressures, one for the general science and one for
specialization, serve different purposes and thus are essential.
Neither pressure should eliminate the opposing one.\textsuperscript{11} In the same
fashion, the issue of the role of abstractions and concepts in economic
science, including motives and the "economic man", are handled by a
compromise.\textsuperscript{12}

The use of the experimental method in economics, credited with a
high degree of success in the physical sciences, presents obstacles
for the economist.\textsuperscript{13} Experimentation in economics cannot be regarded
as equivalent to experiment in physical science. Yet, it is possible
to develop quasi-experiments, means by which may be adopted "avowedly
tentative measures, with the express object of gaining insight into

\textsuperscript{11}See pp. 114-115.

\textsuperscript{12}"Neither the conception of the economic man nor any other
abstraction can suffice as an adequate basis upon which to construct
the whole science of economics. In completing our investigations we
have generally to deal with something far more complex. As Roscher
puts it, we must in our finished theory 'turn to the infinite variety
of real life'.\textit{Ibid.}, p. 128.

\textsuperscript{13}"The phenomena are for the most part not such as can be
manipulated at will; and even when some kind of experiment is possible,
our power of controlling and varying the concomitant circumstances is
very limited; nor can the experiment be freely repeated." \textit{Ibid.},
p. 182.
Thus, there would be similarities between the conception of experimentation in the physical sciences, and experimentation as practiced in the social sciences, perhaps to be called "social experiment".¹⁵

Methodological advice concerning generalization reflects the desire of the author to have generalization without having excessive degrees of generality. Whenever the economist proceeds with the use of the inductive method, accumulating facts and formulating generalizations, he is advised that "taking empirical generalizations at their best, great caution is necessary in extending them beyond the limits of actual experiment".¹⁶ Economists who conceive of economic generalizations as conclusive when based upon a few restricted premisses developed within the framework of an abstract science would also be cautioned. The requirement of "generality and simplicity" in the premisses of abstract reasoning is no sure guarantee of valid generalization.¹⁷ The compromise of theory is apparent: "What is here

¹⁴Ibid., p. 183.
¹⁵Ibid., pp. 184-186.
¹⁶Ibid., p. 207. Almost regardless of the origin or the way in which the generalizations have been fashioned, Keynes warns that there is need "to guard against the danger of unduly extending the range of our generalizations...." Ibid., p. 283.
¹⁷"The validity...of economic postulates varies not only from time to time, and place to place, but also in different connexions at the same time and place. Hence even if a preliminary enumeration of premisses, supposed to underlie the whole science of economics, were really feasible, it would not be possible to examine once for all the validity of such premisses; and on the whole it seems best to regard any preliminary enumeration and examination of economic postulates, not as definitive or exhaustive, but simply as illustrative of the general character of economic theory." Ibid., p. 243.
maintained is that the abstract theory is invaluable as a preliminary study. A similar argument covers the status of history as a method of economics. Economic history affords benefits in analysis, and it is considered essential whenever the economist must deal with problems of economic progress and growth.

Thus, from Keynes' work in economic methodology arises a methodological position characterized by a willingness to compromise the sharply disputed issues of some of his fellow economists. His emphasis upon the development and application of alternative methods, each of which promises benefits yet contains limitations, stands in contrast to other methodological viewpoints in which the methods, rules and standards are rigidly constructed and alternatives are expressly denied.

Emphasis on a Logical System of Economics

The methodological framework of Lionel Robbins contrasts sharply with that of Keynes for two reasons. First of all, Keynes attempted

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18 *Ibid.*, p. 313. With problems of "considerable intricacy", abstract theory may be an essential device: "where a number of alternative conditions are taken, which between them cover all cases that are practically possible, and an enquiry is instituted as to what will happen under each in turn. In this way the limits within which the truth will lie may be determined." *Ibid.*, p. 248.

19 It "teaches the limits of the actual applicability of economic doctrines. It calls attention to the shifting character of economic conditions, and shews how, as these conditions vary, some at least of the principles by which economic phenomena are regulated vary also." *Ibid.*, p. 278.

20 "There are in fact few departments of political or social science in which the a priori method avails less than in the study of economic development." *Ibid.*, p. 284.
to reconcile the conflicting methodological positions of two schools of thought in economics. Robbins, on the other hand, developed a framework for evaluating the practices and procedures of economics that would eliminate almost all but theoretical propositions, making economics a close-knit, deductive system. And, in this process, alternatives in methods and concepts, the possibility of varying rules and criteria of evaluation are rejected. Secondly, while Keynes tried to be gentle with differing viewpoints, to minimize the possibility that his language use would bring emotional reactions, Robbins seems to have made a deliberate effort to inject the power of words in order to generate emotional responses. It is evident that he was successful.

21 "The old treatment of the subject was very unsatisfactory: A few trite generalisations about the advantages of the division of labour copied from Adam Smith, and illustrated by a few examples from Babbage; then extensive discussions on industrial 'forms' and the 'entrepreneur' with a series of thoroughly unscientific and question-begging remarks on national characteristics—the whole wound up, perhaps, with a chapter on localisation. There is no need to dwell on the dreariness and mediocrity of all this." Lionel Robbins, An Essay on the Nature and Significance of Economic Science, London: MacMillan and Company, Ltd., Second Edition, 1935, p. 70.

"The borderlands of Economics are the happy hunting-ground of minds adverse to the effort of exact thought, and, in these ambiguous regions, in recent years, endless time has been devoted to attacks on the alleged psychological assumptions of Economic Science." p. 83.

22 Robbins was answered in kind by R. W. Souter, Prolegomena to Relativity Economics: An Elementary Study in the Mechanics and Organic of an Expanding Economic Universe, (New York: Columbia University Press, 1933), Cf. pp. 139n, 143 and 143n, and 144. This exchange of less than complimentary remarks usually does not pass by unnoticed. Referring to the Robbins-Souter exchanges, Professor Knight commented: "Professor Souter's essay... may be regarded as an expansion of his article of last May replying to Professor Lionel Robbins. In the book version, it becomes explicitly a defense of Marshall as a kind of hero-saint, with Robbins as the chief enemy...."
Within this methodological framework the express purpose is the establishment of the boundaries of economics, which involves problems of the scope and method of economics. Definition is crucial in this connection, for the economist must be able to delimit the subject matter of economics, "to provide a working definition of what Economics is about." This is a necessity because of the widely divergent views of economists and the lack of "unanimity concerning the ultimate nature of the common subject matter."23

In contrast with the preceding framework, Robbins would deny the relevance of ethical questions to the science of economics. Economics "is entirely neutral between ends."24 In this approach "the fundamental propositions of economic analysis" are "the propositions of the general theory of value"25 which make up a logical system of economics. The "validity" of the fundamental propositions, however, is not determined by experimentation, nor by history, but by a prioristic reasoning and common sense.26 With this assumption and

The book is not very pleasant to read, but if one can get behind the turgid, swash-buckling style and opinionated attitude, it will be found to deal with fundamental issues." "The Nature of Economic Science in Some Recent Discussion", The American Economic Review, Volume XXIV, Number 2, June, 1934, p. 225.

24 Ibid., p. 24.
25 Ibid., p. 73.
26 "It does not require much knowledge of modern economic analysis to realise that the foundation of the theory of value is the assumption that the different things that the individual wants to do have a different importance to him, and can be arranged therefore in a certain order." Ibid., p. 75.
the view that the generalizations of economics are components of a tight-knit logical system, the truth of deduced propositions will depend on logical consistency, and the "applicability" of the propositions will depend upon the existence in the situation of the elements postulated. 27

In brief, this framework results in the development of a subject matter modeled after *Principia Mathematica*, and, historically, would rival the attempts by Cairnes and Senior to construct an abstract, deductive science of economics. If economics were to be shaped by such a methodological perspective, economists would need only to state their basic axioms and postulates, then to grind out one after another series of propositions in the same fashion as logicians.

27"The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality. The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of a theory of production is the fact that there are more than one factor of production. The main postulates of the theory of dynamics is the fact that we are not certain regarding future scarcities. These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious. Indeed, the danger is that they may be thought to be so obvious that nothing significant can be derived from their further examination. Yet in fact it is on postulates of this sort that the complicated theorems of advanced analysis ultimately depend. And it is from the existence of the conditions they assume that the general applicability of the broader propositions of economic science is derived....The truth of the deductions from this structure depends, as always, on their logical consistency. Their applicability to the interpretation of any particular situation depends upon the existence in that situation of the elements postulated." *Ibid.*, pp. 78-79.
with a logic. The human mentality may require this type of structuring in science, i.e., a well-defined system of propositions that will remain unaltered during the course of study, and that can be chained inexorably to others. Psychologically, such a system would have some virtues: it would be simple, have no loose ends, and represent an architectural masterpiece of the intellect.

However, one may doubt the conceptual adequacy and practical meaningfulness of the structure. Logical consistency is an appropriate test for the logician, but the logician does not deal directly with perverse data such as with human behavior. Moreover, we would recognize the feasibility of different logical systems. Thus, a test of internal consistency will depend upon the logical rules used. Under one logical system the consistency test will provide for a result quite different from that obtained under a different system. The truth of a deduction would be only logical truth—and a truth developed in accordance with certain presupposed or given rules of the logic. It may be the case that the propositions developed with the use of some form of logical system will be closer to the "ideal" truth. In terms of a very tentative guide or criterion, one might prefer to grant higher status to a proposition developing out of a tight, logical system than one originating in the intuition or whim of an individual, but perhaps only because the former has definite rules that can be followed by all others who might wish to duplicate the process by which the proposition was obtained.

However, criticisms or questions of this order with reference to the logical structure of economics are not answered methodologically.
Attacks on the logical conception of economics are judged as political in nature. They have come from men with an axe to grind—from men who wished to pursue courses which the acknowledgement of law in the economic sphere would have suggested to be unwise.28

The Stress of Empiricism

Reference to the criteria of science sets Hutchison's methodological ideas in a framework different from Keynes's or Robbins's. Keynes allowed for the existence of variety in method; Robbins rigidly held to abstraction and deduction, denying variety; while in Hutchison arises the appeal to scientific criteria. In the latter is a conception of scientific activity, a Bridgman-like intersubjective test, a philosophical-pragmatic requirement, and an emphasis on the appeal to fact.29

The emphasis, then, in science is on the irrevocable appeal to

28 Ibid., p. 94.

29 According to Hutchison, scientific activity consists of "two inextricably inter-connected activities of empirical investigation and logical analysis." T. W. Hutchison, The Significance and Basic Postulates of Economic Theory, London: MacMillan and Company, Ltd., 1938, p. 9. In order to have valid scientific propositions "it must be possible to indicate intersubjectively what is the case if they are true or false; their truth or falsity, that is, must make some conceivable empirically noticeable difference, or some such difference must be directly deducible therefrom." (pp. 9-10) The latter requirement of the necessity for either direct or indirect empirical confirmation of a scientific proposition, an issue inspired by the pragmatists, was an object of debate between Professors Machlup and Hutchison. See "Professor Machlup on Verification in Economics" and "Rejoinder to a Reluctant Ultra-Empiricist", The Southern Economic Journal, Volume XXX, Number 4, April, 1956, pp. 476-493. Misunderstanding of the meaning of terms may have been the source of controversy, for Machlup asked: "But do we really mean the same thing when we speak of 'indirect testing'? Perhaps the crucial misunderstanding lies right here." (p. 484)
However, such emphasis is not meant to "exalt" science as the only form of knowledge for economics. Instead, a line must be drawn between science and non-science with reference to facts (direct or indirect) in order that the consequences of established criteria might be traced for economic science. Other criteria than science and facts might well be used. In this approach, however, methodology in economics would be conducted: (a) with the criteria of empirical testability and logical consistency, the same as for scientists working on the superstructure of a science; (b) with metaphysical discussion kept strictly separate from scientific discussion; and (c) with methodological problems analyzed in conjunction with the practical problems of a science.

In their application, the scientific framework and the established rules of this methodological approach become a system for the classification of the propositions of economics. Unless such a two-

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30 The essence of science is the appeal to fact. This is an appeal which the scientist must always be ready and eager to see made, and where this appeal cannot conceivably be made there is no place for the scientist as such." Hutchison, op. cit., p. 11.

31 Ibid., p. 18. In one respect, the willingness to allow for variety resembles Keynes' position in Scope and Method. Yet, the attitude of Hutchison towards possible multiple criteria is on a different level than Keynes'. Hutchison allows for variety in the approach to methodological problems, not merely variety in economic methods. The application of the attitude to methodological works of other writers would, apparently, give each economic methodologist equal opportunity to be heard. Thus, there is some irony in the situation that finds Hutchison being used as a whipping-boy for the proponents of "free enterprise economics".

32 Ibid., pp. 16-18.

33 According to our proposed definition all propositions with scientific sense, then, are either conceivably falsifiable by
way classification is both created and used, it is feared that the progress of economic science will be obstructed and no protection against the fallacies and errors of pseudo-science provided. There must be relentless effort on the part of economists, as scientists, to isolate propositions and to determine their factual basis. Thus, the framework incorporates the mandamus that economists who offer propositions with empirical content should always be ready, ultimately, to state precisely how they would test these propositions in such terms, and they certainly should not indulge in controversy over them without being able to indicate these tests unambiguously.

Unfortunately for the economist, the mere statement of the criteria does not suffice for improving his practice. While Hutchison develops rules for good scientific procedure, they remain rules, with no reference to the way in which the propositions can actually be tested. Analogous to this dilemma for economic methodology is the situation where a rule of personal health would require every adult to sleep eight hours each day, a proper rule perhaps, but one that the insomniac finds impossible to fulfill. The problem here does not involve for economics any debate of the necessity or desirability of empirical testing of propositions, but rather a serious question

\begin{quote}
empirical observation or not, and none can be both." \cite{Ibid.}, p. 27.
\end{quote}

\begin{quote}
"If the door is now opened to propositions that can never conceivably be brought to any kind of intersubjective test, but at the same time are supposed to have some kind of scientific validity, the progress of economic science will constantly be obstructed by all sorts of controversies, interminable in their very nature, and there will be no effective barrier against pseudo-science." \cite{Ibid.}, p. 141.
\end{quote}

\begin{quote}
\textsuperscript{35}\cite{Ibid.}, p. 150.
\end{quote}
as to the procedures that can actually be followed. Obviously, such
criticism of Hutchison's efforts should not be construed as a complete
repudiation of his work. A methodologist may have considerable
sympathy for another's attempt to eliminate critical issues, to
provide the recipes for science, since this is a most difficult task.
Yet, somewhere in the process, ingredients must be placed in the pot;
and it is the latter that has been omitted.

For the propositions without empirical content, the propositions
of "pure theory" arrived at "by pure deduction," the factual test can
be ignored, since the relationships between symbols of pure theory
say "nothing about real experience". Pure theory may provide
benefits for economics but it should be used with caution. For
example, the so-called ceteris paribus clause, a clause that at one
time had empirical content, is commonly used as an empty analytical-
tautological proposition.

Criticism is directed at the methodological attempt to define

36 Ibid., pp. 38-40.
37 Ibid., see pp. 34-35.
38 Thus interpreted the ceteris paribus clause is an accessory
assumption of pure theory, and ceteris paribus propositions may be
analysed in the same way as the propositions of pure theory have been.
The ceteris paribus assumption makes out of an empirical proposition
that is concerned with facts, and therefore conceivably can be false, a
necessary analytical-tautological proposition. The ceteris paribus
assumption sweeps all the unknowns together under one portmanteau
assumption for a logical "solution." Ibid., p. 42. In place of the
misuse of the ceteris paribus assumption, Hutchison understandably
recommends that it "can only be safely and significantly used in
conjunction with an empirical generalisation verified as true in a
large percentage of cases but occasionally liable to exceptions of a
clearly describable type." Ibid., p. 46.
economics, a valueless effort that "does not appear to solve, or even help in the solution of, any useful scientific problem whatsoever". 39 Not only is this criticism a rebuttal of the Robbinsian position, but it becomes further strengthened by the rejection of the attempt to establish definite boundaries to the subject matter of economics, i.e., to delimit the subject. 40 And, in similar fashion, there is criticism of the conception of economics as an abstract, deductive system of propositions built up on the basis of a few fundamental assumptions. 41 There can be little doubt that the approach involves a methodological camp different from that of Robbins on the matter of the nature of economics.

The whole conception of Economics, as held for example by Senior, as a science resting on a very few general propositions...is shown to be entirely inadequate. 42

39 Ibid., p. 53.

40 "The laying-down of rigid frontier lines between the particular sciences seems an unprofitable undertaking leading to even more interminable disputes than those over national frontiers in Eastern Europe...The suggested unprofitability of drawing hard-and-fast lines between the individual sciences is markedly in contrast with the desirability of drawing a simple and unambiguous line between 'scientific' and 'non-scientific' propositions...." Ibid., pp. 53-54.

41 "Throughout its history, the idea of some 'Fundamental Assumption', some basic 'Economic Principle' about human conduct from which much or most of Economics can ultimately be deduced, has been deeply rooted in the procedure of economic theory. But at the present day, far from there appearing to be any definite agreement as to the precise formulation of this 'Fundamental Assumption', there appears not even to be complete agreement as whether it is necessary or in fact used at all." Ibid., pp. 83-84. At least on this point, Hutchison has been considered a radical or ultra-empiricist. Although, he admits that the fundamental assumptions "may yield results which turn out fairly accurate when tested, ultimately all such questions as these can only be decided satisfactorily by extensive empirical investigation...." Ibid., p. 114.

42 Ibid., p. 118. This holds also for the attempt to distinguish
Methodological Issues in Policy Orientation

Economists cannot avoid issues surrounding the application of their discipline to issues of public policy. Even if they are completely convinced that economics is a "pure" science, concerned only with the development of knowledge and not its application, they are certain to be trapped by policy debates. Methodology will be part of such debates, for issues of policy developing at the end of analysis will have their roots in the fundamental problems of the procedures and techniques leading to policy. An examination of the work of Oskar Morgenstern and E. Ronald Walker illustrates the extent to which methodological issues tend to be of influence on policy matters.

The Role of Theory and Facts

The concern with application and policy requires an extensive interest in theory and facts, since "it is impossible to grasp reality without the construction of theoretical formulae, and... every neglect of theory is attended by grave consequences". However, economics between positive and normative science, Hutchison writes that: "As Schlick made clear, 'the whole conception of a contrast between normative and positive sciences is fundamentally false', and for scientists at any rate the controversy must be very nearly played out. For any empirical 'science' we propose only the criterion of conceiv­able empirical testability." Ibid., pp. 153-154. The latter issue has had a long career in methodological controversy in economics and in other sciences, and while it is possible to use the empiricist's criteria to eliminate it, apparently the issue will remain in economics, particularly when economics is policy-oriented.

43 If one is to maintain a pure science of economics, he must be prepared to defend against its use for practical policy purposes.

should not become "purely theoretical". In the application of economic theory to policy, both the excessive reliance upon "pure theory" and over-confidence in the facts of reality are dangers to avoid. The realization of such dangers in policy formulation, however, does not mean that theory and facts can be safely ignored.

Economics "is subject to a continuous process of change". Policy, then, will be subject to change, as a resultant of changes in the economic system and changes in theory. In this connection, several policy ideas are offered to guide, direct, or modify the efforts by economists to strengthen theory and policy. For one, there is criticism of "rigid" economic systems, where everything relating to policy is fixed. Once an economic system becomes increasingly rigid, "as a result of continuous protective interference" from different organized groups,

45"A priori theory would be very easy if it were possible to dispense with the necessity of dealing with reality and with the flux of economic events and if it were sufficient to lock oneself in a room and invent the world of facts, adopting the attitude that if theory and reality did not then agree, so much the worse for reality. "Theory" of that kind can neither be confirmed nor refuted: nothing easier could be wished for. But, unfortunately, it has nothing to do with the real world. Ibid., p. 10.

46 Ibid., p. 11.


48 Ibid., p. 12. Although economics changes, "it is a strange paradox that the majority of economists seek to emphasize the finality of economic doctrines— their own doctrines it should be noted... It is, however, strange that the general public, and among them those who are most sharply contemptuous of theory, namely, the majority of business men, demand that theory should be of permanent validity." Ibid.
each group lets itself be bribed by all the others to vote dishonestly for measures not in conformity with its own interests. This is the true interpretation, however much the proceedings may be clothed in beautiful generalities. This observation refers particularly to the domain of commercial policy, but it applies, as experience shows, to all other spheres also.

Every attempt should be made to "keep economic policy free from inconsistencies." Thousands of cases in economic policy present issues where there exists an "either-or: you cannot have your cake and eat it". The economist should try then to apply the principle of "freedom from inconsistency" to show where the various measures clash with each other. This, however, is a task of major difficulty to perform. The following passage highlights the influences that affect the development of policy.

Applying an analogy from mathematics, the task of economic policy might be described thus: The problem is one of reaching a definite goal, as, for instance, maximizing the volume of trade with another country. The ideal situation would be the introduction of complete free trade, even though it were one-sided. From the practical standpoint, however, we will assume that this course is impossible, because, for example, one of its accompanying effects would be the ruin of an important section of the population, which for political reasons must at all costs be kept in its present position. This exigency must therefore be inserted in the data and modifies the problem of economic policy substantially, along the lines that it is necessary to find that method which conforms as closely as possible to the ideal, theoretically best solution, but still guarantees the existence of these particular groups of the population under the required minimum conditions. Instead of the shortest line, the 'geodetic line' must therefore be found,

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49 Ibid., p. 45. Also, see his Chapter III, "Rigid Systems of Economic Policy".

50 Ibid., p. 52.

51 Ibid., p. 53.
'geodetic' being the name given in mathematics to those lines which form the shortest distance between two points when the space in which they occur is no longer euclidean. These geodetic lines comply with the conditions of non-euclidean spaces: they belong to the systems of 'elliptical' or 'hyperbolic' geometry. It is just the same in economics, where the general propositions must, so to speak, be transformed into 'elliptical' or 'hyperbolic' ones if they are to be applied in concrete practical cases. The error made by many people has been that they have tried in doctrinaire manner to discover the comparatively simple relations of theory exactly repeated in practice.  

Thus, there are inherent difficulties in economic analysis and policy formulation of which economists should be aware. The economist needs a "perspective" towards his field and its policy. In this process are direct dangers of theory and evidence. "One of the foremost dangers to which a science is subject arises out of its lack of perfection and completeness," Morgenstern claims, and this may mean the "possibility that single statements and axioms may be mutually inconsistent." Furthermore, "the large number of possible combinations...makes it very difficult to formulate general laws and rules which are simple and yet more than truisms."  

Unfortunately, the dangers and difficulties of policy formulation

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52 Ibid., pp. 58-59. Mention is made of the "distribution" effects of policy: "The effects of the adoption of a measure of economic policy have a space-incidence and a time-incidence. By the former is meant that the effects are felt by various people, and by the latter that the effects felt by a given group of people are spaced out over time or that similar effects are felt by different people at different points of time. Now it depends on the manner of diffusion what the reaction will be, and—what is of primary importance—how visible will be the results of the measures of economic policy." Ibid., p. 30. Thus, a "technically" possible policy may be rejected if the effects are localized and concentrated in a short period of time.


54 Ibid., p. 118.
are not readily compensated for by developments in the data of economics. Many of the issues could be reduced if this were to occur, but the path to improving the data of economics remains a disappointing one. Serious problems and gaps arise in economic data, including:

1. Errors in observation and the failure to indicate reference to the errors.
2. Lack of designed experiments.
3. Hiding and false representation of information.
4. Inadequately trained observers.
5. Accumulation of errors in large quantities of data.
6. Lack of definition or classification.
7. Errors in instruments, machines and printing.
8. Time error.
9. Errors from questionnaires.
10. Specious accuracy.
11. Observations that are not reproducible.

Oskar Morgenstern, *On the Accuracy of Economic Observations*, Princeton: Princeton University Press, 1950, pp. 9-22. Perhaps the most significant aspect of errors and problems refers to the representation of information. There are three principal sources of false representation. First, the observer, by making a selection as to what to observe, introduces a bias which it is impossible to avoid because the entirety of a complex phenomena can never be exhaustively described. This bias is of no concern here. Second, the observer may deliberately hide information or falsify his findings to suit his hypotheses or his political purposes... Third, the observer may deliberately lie to the investigator. This is the crucial distinction between social and physical observations; in the latter, this factor is absent no matter how difficult it may be to observe the facts. Ibid., p. 14. Many economists may be drawn to the second source that calls attention to the possibility that private sources may deliberately lie in presenting information. In monopoly, for instance, information may be deliberately hidden as a form of strategy. Government may also hide information, particularly when inter-country comparisons are to be made. Hence, "lie-coefficients"
While it is one thing to recognize such errors, it is much more difficult to attempt to modify or to eliminate them. The recognition of possible sources of error must be supplemented by improvements in statistical theory with the strong possibility that the errors are "interdependent in various complicated forms". If this is the case, "apparently, economics will have to go through a stage of such qualitative description in order to arrive at more precisely expressed quantitative measurement of the errors." Moreover, the attitude of the investigators might have to be changed.

To facilitate a change in the attitude towards data on the basis of errors and consequences of fallacious information, are two conceptions involving successive increases in error and the impossibility of certain combinations of measurements. Conceptually, there is "a single hard core or kernal of accurate figures to which the ordinary ideas or errors apply, surrounded by successive layers of figures, gradually farther and farther in character from the core because of the manner in which they are conceived", and, to compound

might be needed. Finally, there may be an important "game element" in the collection of data, with the respondents trying to hide or exaggerate their experience.

"It is, however, one thing to explain that there is inaccuracy in economic and social statistics and to show where it arises. But it is another thing to arrive at quantitative estimation of the error which is needed for vigorous scientific work. Every effort must, therefore, be made to obtain strictly quantitative estimates...."

Ibid., p. 22.

57 Ibid., p. 23.

58 Ibid., p. 24.

59 "It is also necessary that worthless statistics be completely
the difficulty, the surrounding layers may have the appearance of the
core, though a false one. Also, as in physics with the Heisenberg
principle, there may be "certain types of measurements, or rather
combinations of measurements" in economics that are "in principle
impossible". In model-building with the use of theory and facts the
possibility of developing fine and coarse structures would be
recognized. A fine structure would be one in which a few or a single
fact could overthrow the theory, while a coarse one would last longer
in the face of contradictory facts. Since the economist must return
to facts, the study of errors of observation needs extension with the
development of "methods for the determination of numerical estimates"
of the errors.

Another study of the gap between analysis and policy in economics
shows that it results from the complexity of the issues and barriers
in applying the methods of science. Part of this gap has resulted
from "self-imposed limitations" by economists. The efforts by
economists to eliminate ethics, technology, and sociology from
economics has been an aggravating factor. In addition to self-imposed
limitations there are external limitations upon economic
investigations, including: gaps in data, high expense of factual

and mercilessly rejected on the ground that it is better to say
nothing than to give wrong information which—quite apart from its
practical, political abuse—in turn misleads hosts of later

60 Ibid., p. 31.
61 Ibid., p. 36.
62 Ibid., p. 94.
investigations, denial of access to relevant facts, and bias from the economists' background. Also, there is an obstacle that "the available descriptive material, while overwhelming to any single brain, is at the same time inadequate"; and, even if the data are adequate, "the tendency of the material to be out of date as soon as it has been recorded" adds to the difficulties. 63

Faced with limitations of this order, will it be possible for the economist to bridge the gulf between theory and policy? Evaluating several possible alternatives means of achieving the objective, it is claimed that "a mere multiplication of statistical and descriptive studies is no solution of the problem"; nor is the use of the contributions of specialists from other fields—a "team" approach—a promising alternative, since there "still remains the problem of synthesis". 64 Instead, the economist is encouraged to continue to acquire facts, to look for casual relationships, and not to be too fearful of stepping out of the conventional boundaries of economic theory. Thus, what is really needed, according to Walker, is a "wider theoretical scheme". 65

In the development of theory, and the criticism of existing theory, are problems of deep interest. How the problems are handled then becomes of important concern, particularly when the statements suggest criteria for methodological evaluation. Discussing what is

64 Ibid., pp. 9-10.
65 Ibid., pp. 10-13.
termed "toward realistic economic theory", Walker writes that the "distinction between factual generalization and logical analysis is not carefully drawn in some of the best-known economic treatises", and this contributes to the difficulties in working with theory. Economic theory, Walker argues, "guides" empirical investigations by means of constructing theoretical "models"—"logical constructions"—but the theory "is not intended to be a complete replica of the real world with all its complexities, but a simplified model". The tests of the model are two-fold: checking assumptions and checking conclusions to see if the model works. And, if the model does not test well, it is then modified. 66

Theory is essential: "With a well-tested theory to guide us, we can take up new problems." Theory provides "a map of an extensive territory", realizing that "no map can be complete". 67 Another view of theory is that in the "descent from generalities to particulars, which constitutes the application of theory to practice, the role of theory inevitably appears as the provision of a significant list of things to look for." In effect, he sums up the requirements of "realistic economics" as

The test that we must apply to economic theory, therefore, is not whether its results are contained in its premises; 66


67 Ibid., pp. 42-43. Of course, there are different types of maps, or in this case, different conceptual devices. A lawyer's theoretical "map" is different from that of the economist working with the monopoly problem. Walker's explanation is that "they have conceived the issues differently and have applied different conceptual schemes". Ibid., p. 44.
but whether it is a serviceable instrument in the study of concrete problems....

Then, the pursuit of theory for its own sake is criticized by Walker as evidence of "theoretic blight".

Thus, theoretical and empirical elements impinge upon the application and policy of economists. Even if theory could be finally revised in conformity to the several suggestions, policy itself has inherent limitations not solely confined to the analytical

68 Ibid., pp. 46-47. However, although emphasizing the practical aspect of theory, Walker also reconsiders the assumptions test: "We have seen that economic theory... works by the construction of logical systems, based on assumptions which are held to be significant for the problem on hand". The question arises "Is there any method which will guide us in our choice of assumptions?" Walker applies a test, one that "the practical man applies to all economic theory... namely, that it must not leave familiar features of the concrete world out of account altogether or definitely misrepresent them." Finally, he writes that "it is difficult to believe that the economist has put his finger on the clue to a problem if his analysis does not admit the possibility of events being influenced by the concrete conditions which stare him in the face." Ibid., pp. 54-55.

69 This development of economics under the impulse of theoretic construction, for its own sake, along paths which become even more remote from the real world, is best described by the term 'theoretic blight'. From the point of view of one who desires to use economics for the discovery of concrete truth, theories that build on incredible postulates seem a parasitic growth. All this has been said before by 'practical' critics. But many economists do not realize how much of economic theory is open to such criticism." Ibid., p. 57. In part, Walker offers an explanation for the theoretic blight involving technique and temperament. The technique reason is that economists can only use theory, so they stay with pure theory. Temperamentally, economists work with pure theory because they are enamoured by its elegant symmetry and contemplative orderliness and its lack of uncertainty and loose ends. Cf. p. 59.

70 "Realistic theory must take account of... personal ties and institutional forms of control," including family relationships, pressures from norms of society, and a legal system backed by the state." Ibid., p. 97. The chapter "Beyond the Market" (pp. 100-141) goes into great detail explaining examples of actual behavior outside of the traditional theoretical adjustments of price and cost, with
side of economics. The difficulties with normative versus positive economics, objectives of policy, and the interpretation of economic welfare are obstacles to the final, conclusive programs of economists. In addition, the actual practice of economists in policy-formulating positions in government contains inherent limitations, with the general lack of prestige and influence, policy made "on the run" in light of continually changing conditions, adaptations having to be made to the idiosyncracies of officials, and the problem of diligent technical study in an atmosphere hardly conducive to scholarly work.

Prediction in Economics

The attempt to evaluate the performance of a scientific discipline using the criterion of its ability to predict successfully involves methodological issues of considerable scope and complexity. From the perspective of this and preceding chapters it is apparent that the criterion of predictability is only one of many possible criteria. Its elevation to the status of the sole standard for evaluating procedures of a discipline should not be attempted without careful consideration of the methodological bases of such priority. An examination of two recent works serves to illustrate differences arising in the interpretation of the significance of prediction. Both studies emphasis on psychological patterns of response much broader than profit-maximization.

See pp. 220-263.

See pp. 18-33 for an interesting description of the role played by the economic adviser in government.
emphasize the importance of prediction. One, however, would modify its rigid application to economics while the other would use prediction as the sole criterion of the scientific character of economics. In the latter, the failure to predict is the vital defect of the discipline.

Prediction is the ultimate goal of a science and the major task of positive economics. However, the theory or hypothesis that leads to prediction is a complicated mechanism. It is "in general, a complex intermixture of two elements", one part of which is "language" and the other part consisting of a body of substantive hypotheses. From the point of view of language, theory is tautological, serving as a "filing system" for the organization and understanding of empirical data, for which the criteria of "right" or "wrong" propositions are the "canons of formal logic alone". If the categories of the analytical filing system are to have meaningful empirical associations, there has to be factual evidence showing that the propositions can be "useful in handling a particular class of concrete problems". The substantive character of theory requires fulfillment of the test of prediction: "the only relevant test of the validity of a hypothesis is comparison of its predictions with experience".

The task of positive economics "is to provide a system of generalizations that can be used to make correct predictions about the consequences of any changes in circumstances." Milton Friedman, "The Methodology of Positive Economics", in Essays in Positive Economics, Chicago, Illinois: The University of Chicago Press, 1953, p. 4. Also, 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." Ibid., p. 7.

Ibid., p. 7. This is an asymmetrical test; that is, factual evidence does not both prove and disprove a hypothesis: "Factual
However, this test is not the sole criterion of the work of economists. Economists still need criteria for selecting hypotheses. Any test of a single hypothesis is built upon the selection of the hypothesis from alternatives. Thus, there are criteria for hypotheses broader than predictability.

The criterion of prediction in economics is also handled from a perspective different from the preceding. In this case prediction as a methodological criterion is elevated to an exclusive position. Economics, so it is charged, has failed in its attempts to become scientific because of its failure to predict accurately. It has used obsolete models, techniques, and concepts unsuitable for prediction. Evidence can never prove a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexact, that the hypothesis has been confirmed by experience. 

Observe facts are necessarily finite in number; possible hypotheses, infinite. The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria 'simplicity' and 'fruitfulness', themselves notions that defy completely objective specification. A theory is 'simpler' the less the initial knowledge needed to make a prediction within a given field of phenomena; it is more 'fruitful' the more precise the resulting prediction, the wider the area in which the theory yields predictions, and the more additional lines for further research it suggests.

While there have been some fairly sizable alterations in the theoretical terminology employed in economics during the last 100 or 200 years, the power of accomplishment of economics—measured in the pay-off terms of prediction and control—has changed but little. Economists could not predict the future course of economic events in the early days of the discipline, and they cannot predict the future course of events today. Sidney Schoeffler, The Failures of Economics: A Diagnostic Study, Cambridge, Massachusetts: Harvard University Press, 1955, pp. 2-3.

The progress of economics has been and is so painfully slow
Economists persistently have used static, mechanical, and artificial concepts and tools that do not fit reality and are worthless for prediction. Thus, according to this view, it will be necessary to revolutionize the structure of economic science, to develop a science of economic reality that can be used for successful prediction.\(^79\)

However, the use of language must be noted particularly in dealing with this kind of methodological diagnosis, since its author selects words purposefully, with adjectives designed to arouse emotions.\(^80\) Without an appreciation of the polemic character of the criticisms, the economist will find the methodological critique difficult to understand.

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\(^79\) See especially Chapter 8, "Prediction in Economics," pp. 170-188.

\(^80\) It seems we must accept this interpretation, for Schoeffler admits that he has "conspired" with words to excite the passions and emotions of economists. Several examples illustrate his tactics. He initially writes that "The trouble with the early methodology was that it was premature. A discipline cannot start with methodology, or even be burdened with it in its developmental stages....But today the situation is quite different. We now have a substantial body of academic history behind us, and we have accumulated a great store of facts, theories, concepts, orientations, and procedures. The early stage is over...." (pp. vii-viii). However, later he reverses his position: "How is it, then, that in all these years, and with all the undoubted talent that has been lavished upon it, the subject of economics has advanced so little?" (p. 2) What he wants to accomplish, apparently, is to exaggerate for effect: "Further, there has been a certain amount of exaggeration in the preceding discussion...." (p. 39); and, "Having now acknowledged the one-sidedness of our approach...." (p.40); also, "But we shall deliberately strike the attitude that the ineffectiveness of modern economics is the fault of the economists...." (p. 16); and, "In a sense, the critique is unfair to the authors discussed...." (p. 56)
Viewed in this light, his emphasis on prediction involves some distortion of methodological evaluation, destined to leave emotional imprint rather than intellectual insight. Predictability is only one of many conceivable methodological criteria. It is seriously questionable and validly debatable if prediction deserves this elevation to the throne of methodology. This Schoeffler realizes, but he is unwilling to change his emphasis. Thus, he is forced to ride almost fanatically the bête noire of predictability, even when it means being trapped by his position.

In an early chapter (Chapter 2), Schoeffler sets out to reject the reasons economists have advanced to explain why they cannot predict accurately. Later, however, he must go back to the reasons previously rejected to use them as criticisms of models and concepts of economists. He, so to speak, throws his chestnuts into the fire in one chapter and attempts in later chapters (Chapters 3, 5, and 6) to retrieve them unnoticed by the reader. For example, if economist A should defend his inability to predict by claiming that the world is too complex, that "free will" of individuals is a disturbing element, that it is impossible to conduct experiments, etc., Schoeffler replies: "absurd" reasons, "apologies." But, when he examines the models and

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81 This study has made repeated reference to the importance of attitudes and language as possible barriers to methodological understanding. See the discussion of Keynes, especially pp. 82-84, and Robbins, especially p. 90, and n. 21.

82 See his pp. 15-16.
concepts of economist A, the reasons become serious and meaningful, i.e., he would condemn the theories of economist A for failing to deal with the complex real world, for neglecting "free will".

In sum, this methodological diagnosis has several basic attributes which, in terms of this study, are questionable. First, the slashing attack, paralleling Robbins and seemingly in the stream of emotional debate that J. M. Keynes earlier recognized and attempted to overcome, brings into methodology a geist of controversy. Second, the emphasis on prediction is exaggerated. Third, he argues from the position that science is nomothetic and empirical in all cases, which is a belief of restricted breadth concerning the processes and nature of science. Moreover, in an effort to analyze economic science against two conflicting methodological yardsticks, the orthodox conception of scientific laws and the contemporary emphasis on probability and uncertainty, he may be subject philosophically to metaphysical schizophrenia.

Summary and Conclusions

The complexity of methodology, i.e., the inherent variety in conception, frameworks, rules and criteria, sets limits to the ability of scientists to translate methodological principles into standards for evaluation. For economics, this means that the economic scientist cannot turn directly to the "methodologist" for the use of a fixed set of standards for evaluating economic theory, empirical investigations, and policy. When such standards become necessary, they have to be

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83 See pp. 154-156.
constructed or created from the insight and understanding that methodological perspective furnishes.

This development is a difficult task. Within economics, discussions and analyses of methodological issues and problems are conducted in terms of different frameworks and from different points of view toward science. As a consequence of differences at this level, the methodological conclusions and recommendations of economists vary widely, making the absence of unanimity a constant problem.

Illustrative of the differences existing in economic methodology are the works of John Neville Keynes, Lionel Robbins, and T. W. Hutchison. Each views economic methodology in a different light, reaching conclusions that the others find unacceptable. Keynes would attempt to unify economics by allowing for the existence of different viewpoints on many debatable methodological issues. Robbins would stress the necessity for a logical system of economics. Hutchison would offer support to the empirical development of economic science. Thus, the common dominator becomes one of conflict. Such issues as: the proper limits of economics, the extent to which ethics and value judgments influence economics, the need for clarification of language and terminology, the necessity for the development of a "general" theory, the importance of assumptions and abstract conceptions, the role of theory and experimentation, the place of the appeal to facts are handled in different ways with conflicting conclusions obtained.

Similarly, methodological issues form the basis for policy difficulties in economics. Problems of policy, occurring at the end of analysis, have their roots in the procedures, techniques, and
concepts of analysis. Specifically, the role of theory and facts for policy formulation involves issues of deep methodological concern. The examination of ideas presented by Oskar Morgenstern and E. Ronald Walker involves claims that theoretical constructions are essential, but subject to limitations at the policy level and that empirical, factual, data are necessary in policy determination, although such data are not easy to acquire nor their adequacy a simple matter to determine. Policy, however, is affected by more than methodological problems. Even if it were possible to improve greatly the validity of theory and the reliability of the facts involved, policy would not cease to be complicated. The interference of special interest groups, the inflexibility of an economic system, conflicting normative positions, among other forces, would necessitate modifications in theoretical-empirical analysis.

Prediction in economics, as the test of hypotheses and the justification for policy controls, involves methodological issues of considerable intricacy. Economists vary in their understanding of the significance of prediction as the criterion for their discipline. Friedman, for one emphasizes the importance of prediction in economics, then interpreting difficulties in formulating hypotheses for predictive purposes as a matter of the complicated mechanism of an hypothesis. Schoeffler, similarly emphasizing prediction, makes the failure to predict in economics the reason for its failure as a science.

Thus, examination of the ideas of several economists illustrates major characteristics of economic methodology. First of all, economists differ in viewpoint and framework with reference to the
inner structure of their discipline. They disagree over the manner in which the economist should conduct his analysis. They give conflicting answers to methodological questions. They urge the application of conflicting methodological criteria. These features of economic methodology are not, however, unusual nor unique. In the preceding chapter it was noted that the physical and natural sciences share in this inability to decide methodological issues in any conclusive fashion. Moreover, Chapter II indicated the degree to which conflict may be more the norm than the exception for any discipline. Thus, the methodological evaluation of economic analysis will carry the mark of this uncertain, tentative, and controversial basis.

The advantages of this position in economic methodology may appear very modest, at least in comparison to the claims made for a particular system of methodological ideas. The view suggests that methodological evaluation does not determine nor shape the procedures of economics directly. Instead, methodological examination and evaluation would be only an indirect influence on the discipline. However, it is possible for methodological study to afford benefits for economics, even if the benefits are obtained indirectly. The examination of economics from this methodological viewpoint should uncover and expose the hidden structure of procedures. It should develop in broad terms the existence of basic concepts of theory, empirical work, and policy. By uncovering the unnoticed fundamentals of analysis, methodological study may then be in a position to develop the implications of concepts, structures, and procedures on the course of analysis and policy.
Chapter V

Theory in Monopoloid Market Analysis

Theories concerning monopoloid markets provide appropriate materials for methodological investigation. Fundamental issues underlying the theoretical level have effects on controversy among theorists, changes in theoretical developments, alterations in styles of empirical research, and modifications of policy recommendations. It is the purpose of the chapter to direct attention to such major issues of theory from a methodological vantage point. For this purpose, four features of theory will be examined: scope or magnitude of theory, theoretical concepts or models, theoretical topics or problems, and theoretical hypotheses or conclusions.

Abstraction in Theory

Since the scope or magnitude of theory is closely allied to issues of abstraction in science, it is helpful to give abstraction some special attention. Contradictory viewpoints are involved in the importance of abstraction to science, and they have influence on the methodological evaluation of various types of theoretical scope. According to one view, the inherent complexity of phenomena necessitates abstraction. Abstraction must be carried out as a practical matter, for without some form of abstraction, analysis is impossible. The opposing view claims that abstraction is, in principle impossible, that the scientist cannot hope to withdraw from inherently
complex phenomena relationships that permit meaningful analysis. In addition, the latter has an implication that the unity of data is destroyed if attempts are made to withdraw only segments for analysis.

It is obvious that the existence of the contradictory views represents a potent source of controversy over theory and disagreement over methodological evaluation of theory. However, it may be possible to change the nature of the difference by considering a third viewpoint. Suppose that in dealing with highly complex phenomena we grant the possibility of an infinite number of conceivable abstractions, including what is a comprehensive abstraction in the second view above. The selection of a single abstraction then would restrict the range of conceivable abstractions to be utilized. An abstraction would become, in this sense, a restriction on potential abstractions and in turn a restriction on the data.

Thus, if a single abstraction restricts the range of conceivable abstractions, and also acts as a restraint on the complexities of data, the question of determining the better or best abstraction arises. This is the critical issue for the economist and for the methodologist. The development of alternatives leads to a problem of choice or selection, but the alternatives do not include the criteria to be used in the selection or choice. This would be a methodological issue, one that is apparently in the hands and power of the methodologist to decide. However, if we grant the existence of multiple methodological criteria or rules, an impasse exists, that methodology is unable to provide the criteria necessary for determining the appropriate abstraction or restriction. Thus, part of the burden of determining
the type of restriction (abstraction) useful to economics resides in the domain of the economist.

Scope or Magnitude of Theory

The extent or range of scientific analysis may be approached in several ways, e.g., the actual or the ideal, the magnitude of the subject matter, and interrelationships with other subject matter. For our purposes, however, the discussion of scope will be confined to theoretical scope, meaning the range or extent of theories or theory. And, the following is designed for several broad directions to illustrate the role of scope in a specific type of theory, monopoloid market theory.

In monopoloid market theory two general directions of scope arise in what might well be termed a lateral dimension. In one direction, theory broadens in scope as the theorist includes additional variables. In the opposite direction, the theorist narrows scope by intensive cultivation of a few, selected variables. It is broadened in monopoloid market analysis when the type of product is changed, theoretically, from a homogeneous to a differentiated product, or when the emphasis on results changes from price-cost-profit adjustments to include research, innovation, and technological change. Alternatively, theoretical scope is narrowed by eliminating variables with concentration on a few key ones and their conceivable relationships.

The theory of price adjustments under conditions of two sellers of a homogeneous product is a case in point. Thus, in a lateral dimension, it tends toward broadened and extensive scope or toward narrowed and
Theoretical scope is not, however, confined to lateral dimension. The extent to which time is incorporated or excluded from theory introduces another dimension. Thus, scope may be confined to instantaneous, timeless adjustments, or, in the opposite direction, include dynamic, time-influenced adjustments. The combination of the lateral dimension of theory with the time dimension then provides theory with significantly different alternatives. Theory may develop as fine and timeless; fine and dynamic; broad and timeless; broad and dynamic. Differences in this feature of theory carry important consequences for both theoretical and empirical studies.

Broadening the scope of theory enables it to handle additional complexities in the relations of variables. Yet, along with the gain from broadening is a possible loss, that of over-rapid multiplication of the range and number of the possible theoretical results. The fine-scope theory affords more readily determinate theoretical results, such as in the theory of perfect monopoly or perfect competition. But, once the theory of perfect competition or perfect monopoly is broadened to include other variables on the order of product differentiation, fewness and non-rational behavior, the possible theoretical conclusions become exceedingly numerous and complex. The addition of variables in theory results in a rapid expansion of possible combinations of results of apparently geometric degree. The inclusion of time also adds to the number and range involved and magnifies the possibilities.

For empirical work, changes in the scope of theory have several consequences. If the task of empirical investigators is chiefly the

intensive scope.
testing of hypotheses provided by theory (ignoring the issue of the
discovery and formation of hypotheses by empirical work), a broadened
range of theory creates problems. Decisions have to be made with
reference to the particular theoretical hypothesis or conclusion, or
sets of conclusions, to be investigated. Moreover, empirical findings
have to contend with the existence of a variety of contradictory
theoretical conclusions. Thus, it is not strange that empirical work
in monopoloid markets is patterned along lines of special studies of
firms, industries, etc. It might be considered somewhat strange,
however, to discover theorists denouncing the proliferation of special
"case studies", a development that the broadened scope of theory
seemingly necessitates.

Illustrative Variations in Scope

The literature of monopoloid market analysis contains illustra-
tions of the variety possible in scope. Professor Chamberlin, for
example, offers a study of such markets to deal "not with a special
and narrow problem, but with the whole of value theory".¹ The analysis
of Professor Triffin is an attempt "to blend into a unified whole the
general equilibrium theory of value and the more modern expression of
the Anglo-Saxon tradition: the theories of monopolistic competition".²

According to Professor Fellner, in his book

¹Edward H. Chamberlin, The Theory of Monopolistic Competition,
Cambridge, Massachusetts: Harvard University Press, Fifth Edition,
1947, p. xi.

²Robert Triffin, Monopolistic Competition and General Equilibrium
Theory, Cambridge, Massachusetts: Harvard University Press, 1940,
p. 13.
considerable emphasis will be placed on the fact that a significant proportion of the markets with which value theory must be concerned cannot be analyzed in terms of demand functions or supply functions derived from technological data and utility functions alone.\(^3\)

Joan Robinson writes that the "chief aim" of her book is to attempt to carry out "the whole theory of value...in terms of monopoly analysis".\(^4\)

For Professor Abbott, his volume represents a modest attempt to work toward the formulation of a more comprehensive theory. Its main concern is the neglected half of the study of competitive markets: analysis of the quality variable.\(^5\)

The theoretical work of Professor Brems deals with the questions involved in product equilibrium of the firm under single-period, single-product planning as well as under multi-period and multi-product planning. They also involve the theory of duopoly, collusive as well as non-collusive.\(^6\)

In the case of Professor Bain the volume concerns the nature and the effects of one potentially strategic dimension of market-structure—the "condition of entry", or the relative ease or difficulty of entry of new competitors to an industry. In other terms, it seeks to measure the varying force among industries of 'potential' competition, or threatened new entry, and to inquire whether and in what way variations in this force influence the market performance of established firms.\(^7\)


\(^7\)Joe S. Bain, *Barriers to New Competition (Their Character and
Another volume, by Professor Machlup, "is confined to the economic
type of competition in selling, though it cannot cover all of it:
for example, the analysis of discriminatory pricing is deferred to a
separate book..."

A review of the scope of theoretical analysis could be extended
to include other works, the many specialty articles on refinements and
and special topics of theory, the interest in dynamic theoretical
analysis, and the relation between employment theory and the theory

8 Fritz Machlup, The Economics of Sellers' Competition (Model
Analysis of Sellers' Conduct), Baltimore: The Johns Hopkins Press,
1952, p. vi.

9 Including E. A. G. Robinson's, The Structure of Competitive
Industry, Cambridge: At the University Press, 1935, and his Monopoly,
Cambridge: At the University Press, 1941; Heinrich von Stackelberg,
The Theory of the Market Economy, New York: Oxford University Press,
1952; L. J. Zimmerman, The Propensity to Monopolize, Amsterdam: North
Holland Publishing Company, 1952; and, F. Zeuthen, Problems of Monopoly

10 For which Edward H. Chamberlin has developed an extensive
bibliography in his Theory of Monopolistic Competition.

11 "On the order of the demand by John Perry Miller, that "we must
develop a verified theory of market structure and behavior relevant to
an economy in constant change", "Measures of Monopoly Power and Concent-
tration: Their Economic Significance", appearing in the volume by the
National Bureau of Economic Research, Business Concentration and Price
Policy, Princeton: Princeton University Press, 1955, p. 135. Also,
while credit for the encouragement of more dynamic theoretical analysis
can be bestowed on many economists, Joseph A. Schumpeter has been an
open advocate of the necessity for a time-extended scope of analysis;
for example, "The essential point to grasp is that in dealing with
capitalism we are dealing with an evolutionary process... that not
only never is but never can be stationary". Capitalism, Socialism, and
1950, p. 82 and ff.
of monopoloid markets.\footnote{A problem of analysis referring to the "process of including problems of the cost-price structure in the theory of employment..." It is to be expected that the links between the two will grow tighter and that significant interactions will be explored more fully. William Fellner, "Employment Theory and Business Cycles", in A Survey of Contemporary Economics, Volume I, edited by Howard S. Ellis, Philadelphia: The Blakiston Company, 1949, p. 86. Also, see Alvin H. Hansen, Business Cycles and National Income, New York: W. W. Norton & Company, Inc., 1951, pp. 575-576.} The advantage of such extension is obvious. It would offer further support to the thesis that a fundamental feature of theoretical analyses is the presence of alternatives in scope or magnitude of theory. However, even without the additions, the presence of differences in scope will be felt by theorists and empirical investigators. They must, it would seem, face the implications of alternatives in any methodological evaluation of theory. Yet, issues of theory are not solely confined to scope. Attention also needs to be directed to the development and use of concepts or models in theory.

Theoretical Concepts or Models

The role occupied by concepts or models in theoretical analysis is debatable at different levels and from different points of view. For the philosopher it may be examined in terms of metaphysics, ontology, and epistemology, perhaps engendering controversy from opposing philosophical points of view. The methodologist or philosopher of science may analyze the issues in terms of types of models and concepts, criteria, and implications for science in general. The scientist in a special discipline may evaluate the merits of a particular kind of model or concept with reference to special problems of his discipline. For the latter, controversy may arise over the
significance of orthodox or unusual models for an understanding of actual conditions. However, the area of discussion will be restricted here mainly to the more common interpretation of concepts or models in science.

If complex phenomena are operated upon by science, some form of organization generally will be applied. This may be called conceptualization or model-building. In this connection, it appears that every science utilizes models or concepts and that a wide range of conceivable models or concepts is available. A preceding chapter has noted that sciences other than economics have experienced a growth of different models with a corresponding interest in determining the best or better model to be used. A methodologist, then, might look for general model types common to all science, attempting to develop principles of concept or model construction and for the criteria determining their use. But, methodology would experience difficulty in its efforts to determine the answer to the question of selecting the appropriate model, again from the position of its multidimensional character. Moreover, outside the province of economics the possibility of the co-existence of different models in a single science has been recognized. Thus, the examination of models or concepts used in the theory of monopoloid markets should not be considered as a final determination of the best model to be used, but rather represents an attempt to emphasize several strategic model or concept forms used in monopoloid market analysis and their implications for additional work.

Four types of models seem to exert influence in theoretical analysis. They are identified as: equilibrium models, power and
bargaining models, organic models, and open-ended models. While the term "equilibrium" may be qualified to provide a variety of types of equilibrium, it generally means a balancing of opposing forces, a coming to rest, or a resistance to movement away from a point or level. And, as equilibrium models or concepts, there is generally present some reference to a determinate solution.

Equilibrium Models

The Economics of Imperfect Competition affords an interesting case of an equilibrium model, fashioned in strictly theoretical terms. References to subjecting the model to the test of facts of market adjustments are meager and generally minimized. To the author, the book is a deliberately theoretical study. Theory in this case develops tools of analysis that enable the economist to fashion a series of deductions concerning the market adjustment of firms.

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13 According to George J. Stigler, equilibrium may be: stable, neutral, and unstable; unique or multiple; partial or general; long or short run; and, dependent or independent of the path by which equilibrium is reached. The Theory of Price, New York: The MacMillan Company, 1947, pp. 26-31.

14 Joan Robinson, The Economics of Imperfect Competition, op. cit., pp. 7, 15, and 118.

15 The volume is "presented to the analytical economist as a box of tools. It is an essay in the technique of economic analysis, and can make only an indirect contribution to our knowledge of the actual world." Ibid., p. 1. Her "conclusion", p. 327, contains a strong statement of the theoretical position in economic analysis.

16 "I hope to have demonstrated in this book that theoretical analysis can be carried to a considerable distance by purely economic reasoning, combined with a knowledge of the conception of 'elasticity' and of one or two theorems from the book on triangles in a school geometry." Ibid., p. 12. "When the fundamental assumption is made every economic tendency can be analysed by a series of questions...."
Often the tools themselves represent a departure from preceding theory, such as the widespread application of marginal curves.\textsuperscript{17} Pursuing the course of deductive reasoning requires simplification and isolation of the material and the development of fundamental or basic assumptions.\textsuperscript{18} Many assumptions can be made in theoretical analysis, resulting in widely divergent and numerous theoretical results,\textsuperscript{19} but this work concentrates on the results possible using...
certain behavior and time assumptions. The behavioral assumption contains the ideas of maximizing, the balancing of gain and loss, and sensible conduct on the part of the firm and the individual. 20 The implications of different behavior assumptions on the course of economic analysis, while recognized, have to be ignored theoretically. 21

The time assumption in the theoretical scheme is one involving the absence of the influence of time. Though time is a crucial element it is omitted in this design of theory. 22 Thus, we have the

20 "The single assumption which it is necessary to make in order to set that piece of apparatus at work is the assumption that the individual firm will always arrange its affairs in such a way as to make the largest profits that can be made in the particular situation in which it finds itself. . . . It is the assumption that any individual, in his economic life, will never undertake an action that adds more to his losses than to his gains, and will always undertake an action which adds more to his gains than to his losses, which makes the analysis of value possible." Ibid., p. 6. "The fundamental assumption is that each individual acts in a sensible manner, in the circumstances in which he finds himself, from the point of view of his own economic interests," Ibid., p. 15. "The fundamental assumption of economic analysis is that every individual acts in a sensible manner, and it is sensible for the individual to balance marginal cost against marginal gain." Ibid., p. 211.

21 "If individuals act in an erratic way only statistical methods will serve to discover the laws of economics, and if individuals act in a predictable way, but from a large number of complicated motives, the economist must resign his task to the psychologist." Ibid., p. 7. The impact of different behavior assumptions may be less, however, in the sense that a "technique which would study the economic effects of neuroses and confused thinking would be considerably more complicated than the technique here set out." (p. 15).

22 "Many of the most formidable difficulties of economic analysis are connected with time. These will be glanced at as occasion arises in the course of the argument, but for the most part we shall be obliged to leave them on one side." Ibid., p. 22. "The problems connected with the influence of time remain to be solved, but no attempt is here made to solve them." (p. 129)
outlines of the construction of a strictly theoretical equilibrium model of analysis.

The technique set out in this book is a technique for studying equilibrium positions. No reference is made to the effects of the passage of time. Short-period and long-period equilibria are introduced into the argument to illustrate various technical devices, but no study is made of the process of moving from one position of equilibrium to another, and it is with long-period equilibrium that we shall be mainly concerned.

The equilibrium model of Chamberlin in his Theory of Monopolistic Competition might also be analyzed in terms of reference to the world of fact, the development of tools and techniques of theoretical analysis, the use of assumptions, deductive reasoning, isolation and synthesis, etc. In this fashion, the similarity between Chamberlin and Robinson as theoretical technicians would be readily apparent. However, since the rudiments of the mechanics of theory construction have been presented in connection with Mrs. Robinson's analysis, similar mechanics in Chamberlin will be ignored. What is of interest is the Chamberlinian version of an equilibrium model or concept. Chamberlin refers to many equilibrium results, but his equilibrium concept differs from the preceding. In both models adjustments occur

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23 Ibid., p. 16. Specific chapters are devoted to monopoly and competitive equilibrium (Chapters 3 and 7), with the possibility of multiple equilibrium mentioned (p. 57), and with continual reference to equilibrium results, including: "At any one moment all firms in an industry may not be in equilibrium...and yet the industry as a whole may be in equilibrium" (p. 91); "Clearly the answer depends upon the manner in which the demand curves move as they fall back towards an equilibrium position" (p. 99); "The net effect of these two contrary influences may establish an equilibrium position..." (p. 128); and others (pp. 97-98, 101, 123, 127, 250-251, etc.).

24 The Theory of Monopolistic Competition, op. cit., pp. 12, 15, 20, 25, 69, 73-75, 81, etc.
in the market behavior of firms in such a way as to result in some kind of equilibrium of zero profits, price equalling average total cost, and with numbers tending to remain the same. Yet, while the general equilibrium results (that is, the main results of the theory and not "general" in the sense of the entire system) provide price-cost-output-profit adjustments that conform to the orthodox stream of competitive and monopolistic theory, Chamberlin's forces or tendencies toward equilibrium are of a different order than the orthodox position in the Robinsonian case. The impersonal market adjustments providing the equilibrium results are a mixture of both monopoly and competition. Patents and trademarks, for instance, provide evidence of the admixture of the forces of monopoly and competition:

Each makes a product unique in certain respects; this is its monopolistic aspect. Each leaves room for other commodities almost but not quite like it; this is its competitive aspect.25

There are innumerable examples of the merging into one entity of the forces of competition and monopoly,26 forces that bring about, however, equilibrium results. Thus, the Robinsonian and Chamberlinian models are equilibrium models, though the latter incorporates a more involved concept of tendencies or forces. Product variation and selling effort and the merging of competitive and monopolistic elements in the behavior of firms means a more complex equilibrium model and more complicated equilibrium results.

25 Ibid., p. 62.
26 Ibid., see pp. 63-64, 68-70, 73, 81-82, 102-103, 109, 124, 126-128.
From this point of view, Triffin's model would be an obvious extension of the Chamberlinian model. It represents an equilibrium model, not in terms of the orthodox firm-industry results, but equilibrium in terms of the entire economy of firms and industries. The constants of the Robinsonian and Chamberlinian cases become the variables in the Triffin model. Analogically, price or value theory in the classical-neo-classical stream of theory, in which all other prices were held constant, automatically opened up the way to a general theory that would attempt to make variables out of the other prices, i.e., the constants to become variables in a general theory of equilibrium as in the Walrasian theory.

However, the extension of the equilibrium model to the general case for an entire economy may also be offset by the application of the concept or model to a limited market range. The isolation of the product as an economic variable, the concentration on duopoly, the development of the "kinked" demand curve, might be thought of as the utilisation of an equilibrium model or concept on a highly limited, or restricted scale. In these cases, a few selected variables of


theory, product, demand, price, cost, etc., are singled out for
attention in a model-form resulting in an equilibrium condition for
the selected variables.

Moreover, the equilibrium model that can be developed
theoretically in the direction of general equilibrium or to a particular
equilibrium may be used to develop the theoretical result of dis-
equilibrium. In this case the development of an equilibrium model
does not result in the movement of forces or tendencies to a final or
resting position. Modifications in orthodox tools of theoretical
analysis provide, then, a dis-equilibrium result.

Power and Bargaining Models

There are several important implications methodologically
developing from the use of equilibrium models or concepts in the theory
of monopoloid market analysis. However, such consequences for theory,
empirical work, and policy must be postponed temporarily while
attention is directed to alternative model or concept forms, e.g.,
those that are described as power and bargaining models, organic
models, and open-ended models. Power and bargaining models are
typified in the work of Zeuthen, Fellner, and Galbraith, dealing with


31 Benjamin Higgins, "Elements of Indeterminacy in the Theory of
Non-Perfect Competition", The American Economic Review, Volume XXIX,
Number 3, September, 1939, pp. 468-479. In part, the design of theory
from within an equilibrium model may be modified by changing the
assumptions of behavior, a trend advanced by the work of John von
Neumann and Oskar Morgenstern, Theory of Games and Economic Behavior.
market adjustment of firms. Also, outside of the area of market
adjustments, e.g., in labor economics, power and bargaining models
have considerable application.\textsuperscript{32} In the power and bargaining models
the theorist commits his analysis to pressures and forces. But, in
place of a commitment of the model to the development of equilibrium
results, or tendencies toward equilibrium results, the power and
bargaining model develops conflicts, impasses, and indeterminacies.

As Fellner states the issue,

Present-day value theory recognizes that the conditions of
determinate equilibrium are not satisfied on all markets.
But the main body of value theory is concerned with markets
on which determinate equilibria...can be established.\textsuperscript{33}

However, there are a significant number of markets, characterized by
fewness of firms, in which "determinate equilibria, in the usual
sense, cannot be established for these markets analytically".\textsuperscript{34} Power
and bargaining models thus are aimed not at a final equilibrium
position but develop conditions and results in terms of a bargaining
"range". Moreover, such models generally allow for a variation in the
width of the bargaining range over time, with changes in economic
conditions. Analysis avoids emphasis on results of the orthodox
curves of demand and supply, marginal revenue and costs, etc. Here

\textsuperscript{32}Zeuthen, Problems of Monopoly and Economic Warfare, \textit{op. cit.};
Fellner, \textit{Competition Among the Few}, \textit{op. cit.}; John K. Galbraith,
\textit{American Capitalism, The Concept of Countervailing Power}, Boston:
Houghton Mifflin Company, 1952. For a broad treatment of general
problems in bargaining theory, see Thomas C. Schelling, "An Essay on
Bargaining", The \textit{American Economic Review}, Volume XLVI, Number 3,

\textsuperscript{33}Fellner, \textit{op. cit.}, p. 9.

\textsuperscript{34}Ibid.
the role of "power" plays an important part, supplemented by considerations of strategy, political consequences of bargaining, the existence of coalitions, and other features of bargaining.

Organic Models

Organic models of market adjustments pose difficulties theoretically and methodologically, including the difficulty of describing an organic model or concept. A general explanation of an organic model might be one framed in terms of growth, life and death; evolution and adaptation to an ever-changing economy; and/or a life-process. The Marshallian analogy of a firm resembling a tree in the forest, maturing, and gradually dying, provides one type of organic model. Or, the analogy is modified to include a Darwinian process of mutation possibilities to an ever-changing, complex, institutional framework. An organic model might also be on the order of an evolving economic system in which firms engage, through the activities of the entrepreneurial group, itself an evolving institution, in the process of "creative destruction." 35

However, regardless of the manner in which the elements of an organic model are constructed in theory, the concept or model suggests a host of problems. The mechanistic character of equilibrium models will conflict with a model-type that rejects the influence of the laws of mechanics and runs in terms of biological concepts and laws. 36

35 See Joseph A. Schumpeter, Capitalism, Socialism, and Democracy, op. cit., Chapters 6-9.

36 The controversy between the classical economists and the historical school was in part the product of a reaction to mechanism.
Open-Ended Models

The description of the fourth model or concept in the theoretical analysis of monopoloid markets also poses difficulties. What would be termed an open-ended model may not be sufficiently unique in comparison to an organic concept, since the latter inherently is open-ended.

However, by considering the attitude of theorists towards an incomplete formulation of theory, we might suggest a version of the open-ended model: it would be the development of a simplified theory to which additional relationships can be fitted. The three variable theory to which fourth, fifth, sixth, etc., variables can be added within the framework of the original theory would provide an example. Thus, the distinction between organic models and open-ended models would be the extent to which additional variables do, or do not, follow some

A mechanical or mathematical relationship.

An example of theoretical development along lines of an open-ended model occurs in the thesis offered by J. M. Clark, that economists should endeavor to work out a concept or theory of "workable competition". Basically, the idea is that the nature of competition in individual cases depends on a large number of conditions, that the number of possible combinations of such conditions is large, and that these conditions vary through time. Therefore, economics should attempt to develop a theory sufficient to embrace every aspect of the actual conditions. However, due to the above circumstances, the form of the resulting theory cannot be predicted in advance. We are left, then, with an open-ended model.

The issue of determining the best model or concept on the basis of the preceding examples is a complicated one for methodology, but there are several things that might be accomplished. First of all, at the level of observation, economists need to recognize the existence of a variety of model or concept types used in the analysis of monopoloid markets. Some of the controversy surrounding analysis may be minimized with the recognition that economists form theoretical conditions and conclusions from within different models: equilibrium, power and bargaining, organic, and open-ended. The fact that an equilibrium model may be incapable of dealing with time and change in

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the same way as an organic model would not, then, be a defect in the equilibrium model as such. Similarly, criticism leveled at organic models for the alleged failure to utilize the standard techniques and tools of theory as they have been developed historically would be misdirected. It must be noted, however, that once this type of argument is advanced, a criterion or standard appears. Evaluation moves beyond the level of recognition of various models used in analysis and begins to formulate standards or criteria. In this case, the criterion is that a model necessarily incorporates a way of arranging and organizing data, of restricting the frame of possibilities, and that the model must be judged in terms of its own set of attributes. Given this criterion, theoretical conditions and conclusions might be criticized if they depart from the model.

This would not bring about the settlement of the issue. There is an additional complication to consider. If it is granted that the above methodological criterion is only one of the many possible criteria, then the evaluation might be required to show the supremacy, or on a more modest level the desirability, of adopting this criterion over the others. This leads into an examination of the area of methodological principles and standards, involving a project of formidable proportions as the previous chapters indicated. But, although this approach would lead into methodological problems of a broad nature, with many of them in turn difficult to solve, there would be the advantage that standards of evaluation would receive explicit consideration. To discover and to emphasize the nature of criteria that have been used for evaluation and to analyze their status is a
Theoretical Topics and Problems

The topics analyzed in monopoloid markets, and the theoretical hypotheses, conditions, and conclusions that feature this analysis are exceedingly complex and numerous. Thus, a study proposing to deal with the entire range of topics and hypotheses must in some fashion or another come to grips with this aspect of monopoloid markets. According to theoretical exponents, the complexity of phenomena requires the isolation of strategic variables, and, theory offers the mechanism for coping with complex data. Anti-theoretical critics charge that the meaningful relationships are handled artificially by the simplifications of theory, or are even ignored completely. What deserves attention, then, is the degree to which complexity permeates the phenomena of monopoloid markets. The following presentation of topics, though not exhaustive, illustrates the involved area of analysis in monopoloid markets:

1. Prices
2. Costs
3. Profits
4. Output
5. Efficiency
6. Allocation of resources
7. Capacity
8. Scale
9. Product
10. Entry
11. Market structure
12. Concentrations and mergers
13. Advertising
14. Market performance
15. Income distribution
16. Research
17. Technology and invention
18. Power
20. Decision-making
21. Motivation
22. Government
23. International
24. Elasticity
25. Marginal quantities
26. Economies and diseconomies
27. Large-scale management
28. Determinants of demand
29. Knowledge of demand
30. Fewness of firms
31. Potential competition
32. Non-price features of demand
33. Role of profits
34. Limits to profits
35. Limits to entry
36. Optimum size
37. Expansion of industry
38. Specialization
39. Monopoly output
40. Decreasing costs
41. Trade secrets
42. Rationalization
43. Price discrimination
44. Utility
45. Factor demand
46. Employment
47. Exploitation
48. Flexibility of prices
49. Rigid prices
50. Flexibility of profits
51. Rigidity of profits
52. Agreements
53. Full employment
54. Size of national income
55. Savings
56. Investment
57. Justice
58. Equilibrium
59. Divisibility of factors
60. Definition of an industry
61. Definition of a product
62. Economic growth
63. Growth of firms
64. Segmenting markets
65. Barriers to expansion
66. Raising capital
67. Interest rates
68. Reinvestment of profits
69. Marketing economies and diseconomies
Here a problem of general methodological concern involves classification or taxonomy. Viewed methodologically, the problem of classification or taxonomy can be approached in different ways. However, it might be more helpful to view the general problem in terms of two levels. At one level, classification or taxonomy involves the issue of the methodological principle of classification. Controversy, in the methodological dimension, may then involve the contradictions of two opposing points of view: one requiring that taxonomy is, in principle, possible; the opposing view denying that position. However strong the philosophical arguments at this level may wax, the problem remains a basic one of debate over a methodological principle.

At the other level, the problem of application becomes a source of controversy. At this stage, it is not the question of principle, but the kind of taxonomy to be followed. Thus, economists may agree that classification is, in principle, possible, but disagree over the actual way in which the complex phenomena are to be arranged or organized. An added difficulty may develop at the level of application. Critics of the principle of classification may relax their opposition to classification and advance a practical principle enabling them to engage in a debate over the merits of the application of a given
principle of classification. Therefore, while the issue of the methodological principle of classification may be confined to one level with the opposing positions allowed to conduct their arguments with reference to philosophical matters, controversy breaks out at the level of the application of a principle. This level, however, is perhaps the place where debate should occur, since a principle of classification would then come into contact with the data and the knowledge and judgment of the economist.

Theoretical Hypotheses and Conclusions

Theory provides a means for multiplying possibilities. Thus, in the light of methodology and preceding findings, the idea that theoretical results can be conclusive must be modified.

For example, the technique of marginal cost and marginal revenue in determining the output-price-cost-profit equilibrium of a firm under imperfect or monopolistic competition has been heralded as a powerful theoretical device. Yet, unless we handle the technique with care, its power can be exaggerated. Even if the problems of defining the marginal quantities and developing their empirical counterparts are left to one side, a severe restriction, the marginal cost-revenue adjustment admits to a series of theoretical possibilities. What tends to make the technique powerful in theory, in the sense of obtaining theoretically conclusive results, is the deliberate restraint placed on the number of possible combinations of marginal cost and marginal revenue. Marginal cost may be: positive or negative or zero; rising or constant or falling; fluctuating or stable; continuous or discon-
tinuous; short- or long-run; real or imaginary. For marginal revenue, similar combinations are possible. Thus, a methodological issue for theory is the determination of the combination or combinations of marginal cost and marginal revenue to be selected. Since almost two hundred different combinations of the marginal quantities are possible in the above, theory has a great deal of latitude in selection. Selection then is restrictive in terms of the large number of remaining combinations that are theoretically ignored.

Summary and Conclusions

Many issues of economic theory, when viewed methodologically, arise in the four areas of: scope or magnitude of theory, theoretical concepts or models, theoretical topics or problems, and theoretical hypotheses or conclusions.

Abstraction is a methodological issue allied to problems of the scope or magnitude of theory. Given the existence of alternative positions concerning abstraction: (a) abstraction as an essential characteristic of analysis, (b) the impossibility, in principle, of abstraction, and (c) the conception of an infinite number of abstractions, it would be the task of the economist to determine the extent to which a selected abstraction suits analysis.

An examination of the general direction of the scope or magnitude of theory shows that theory may develop in a lateral dimension, broadened by the inclusion of additional variables or narrowed by the intensive cultivation of a few variables. In addition, the inclusion or exclusion of a time element will affect scope. The combination of
the lateral and time dimensions thus provide theoretical scope with significantly different alternatives, a condition that suggests consequences for theoretical and empirical studies. The broadening of theory introduces rapidly the possibility of numerous and complex theoretical conclusions, at a rate of apparently geometric degree. Moreover, at the empirical level, the broadened scope of theory forces upon empirical investigators a problem of selecting appropriate hypotheses from many conceivable ones, presents contradictory conclusions to offset empirically-tested ones, and seems to encourage the development of special case studies.

The variety possible in theoretical scope is illustrated in the work of selected theorists concerned with monopoloid market analysis. Edward H. Chamberlin, Robert Triffin, William Fellner, Joan Robinson, Lawrence Abbott, Hans Brems, Joe Bain, Fritz Machlup, and others, present different developments of the scope of theory. Variations at this level become part of methodological evaluation.

Similarly, problems develop in the construction of models or concepts for theoretical analysis. In monopoloid market theory, four strategic model or concept forms are often utilized. They include: (1) equilibrium models, (2) power and bargaining models, (3) organic models, and (4) open-ended models. A case study of Joan Robinson's *Economics of Imperfect Competition* illustrates the process by which an equilibrium model is created in strict theoretical terms. However, equilibrium models, while stressing the balancing of forces and determinate results vary in form, illustrated by a comparison between Chamberlin, Robinson, Triffin, and other special types.
Power and bargaining models, typified in the work of Zeuthen, Fellner, and Galbraith, involve the commitment of the model not to determinate results but to conflicts, impasses, and indeterminacies. Organic models avoid the use of laws of mechanics and run in terms of biological concepts and laws. Open-ended models, as in the case of J. M. Clark's "workable competition", involve the addition of variables and the construction of a model reflecting the large number of possible combinations of variables.

Several consequences for the evaluation of theory emerge from a consideration of the possible alternatives. Economists will need to recognize the existence of variety in concept or model forms. This should increase understanding of theoretical controversy resulting from the use of different models or concepts. Specific attention to the criteria of special concepts may also lead to specific attention to the "ordering" of criteria, that is, the supremacy of a system of criteria. The latter suggests increased emphasis on methodology.

The topics and problems of monopoloid markets are numerous and complex. The attempt to introduce a principle of classification or taxonomy involves methodological analysis of two separate issues, the principle of classification and the application of a specific kind of classification.

Finally, once it is granted that theory is a means for multiplying possibilities, the idea that theoretical results can be conclusive must be extensively modified. An economist may view theory as intellectual acrobatics, mental chess, and sterile speculation. He may judge it as the weapon of analysis, a device for clarifying
relationships, and a precision instrument to eliminate fuzzy thinking. However, from a methodological viewpoint, theory has a different appearance. Methodologically, theory would consist of a "mathematical set" of concepts, models, hypotheses, topics, conclusions, and scopes of various forms and structures. Variations would be expected in each "subset." The methodologist, facing the complete set and the series of minor subsets, would then seek to identify their outlines or "limiting characteristics" and to suggest implications that differences entail. But, this would have to be a process conducted within the context of the inherent variety of methodology. Failure to recognize alternatives within methodology itself is almost certain to influence the judgment that may be applied.
Empirical Work in Monopoloid Market Analysis

Customarily scientists advance the thesis that the propositions, hypotheses, or theories of a discipline ultimately must produce observable, measurable results, that science ultimately makes an appeal to facts. The process by which facts are obtained is, however, exceedingly complex. Thus, it is the purpose of this section to call attention to several of the methodological problems affecting empirical research, to illustrate difficulties inherent in such work, and to suggest some methodological implications.

Empirical Suspicions

Some of the problems arising in empirical research have to be explained as the product of empirical "suspicions", the ill-defined doubts concerning the accuracy, reliability, and adequacy of factual investigations. Empirical "suspicions", as they are considered here, might include: lack of faith in the techniques used; bias or partiality; impossibility of reconstructing work independently; interrelationship of sets of facts; empirical findings versus theory; and policy implications.

Lack of faith in techniques of economic research is a source of methodological controversy. In many instances of empirical research, criticism is directed at techniques involving questionnaires, interviews, samples, correlation, etc. An economist who attempts to develop facts of monopoloid markets opens up controversy, not from the
standpoint of what is actually presented, but from the point of view of errors and gaps in techniques. Studies of costs, price behavior, and the application of marginal analysis have been fertile sources of controversy over techniques.¹

Bias or partiality represents another empirical suspicion common to the social sciences. Special interest groups conduct empirical research into issues of monopoloid markets.² The National Association of Manufacturers, The United States Chamber of Commerce, large


²To answer critics who charge that the large firm in the economy "sets" prices, controls markets, and causes inflation, the United States Steel Corporation published a 292-page, slick-paper book, with photographs, charts, tables, and "expert" testimony to prove that the critics err in their charge. See, Steel and Inflation: Facts Vs. Fiction, New York: Public Relations Department, United States Steel Corporation, 1958.
corporations, advertising councils, trade associations, the AFL-CIO, and other groups, engage in empirical studies bearing on monopoloid markets. One may doubt, however, the disinterested nature of their findings. In addition, the economist himself may not be free from personal predilections with reference to selections of facts and policy positions. The economist may have a professional or intellectual interest to represent.

Empirical suspicions also may result from the inability to reconstruct independently the steps of a research study. When secret information, personal interviews, and private conversations are used as basic data in the empirical work, the scientific public finds that access to the same alleged facts is impossible. Thus, the accuracy of the work cannot be checked. The data are isolated from further investigation.

The interrelationship of sets of facts and the connection between empirical discoveries and the theory involve empirical suspicions. Often the suspicion is based on the dichotomy between theory and facts. Rising profits and prices under conditions of a few large firms may be the product of oligopolistic behavior or the result of adaptations of the firms to ebullient demand conditions throughout the economy. Mergers of firms in the economy may be the consequence of the desire on the part of management to secure market control or the result of changing economic and institutional conditions. Thus, facts of monopoloid markets may be suspect for the reason that different types of theory have been applied.

Finally, empirical suspicion may develop from the policy impli-
cations of facts. This would suggest a distinction between harmless
and "dangerous" empirical findings.

One who has engaged in a study of corporation statistics, and
has been caught or has caught himself in serious blunders
while seeking to interpret such statistics, is disposed to be
lenient in criticizing attempts by other investigators to
draw useful inferences from the corporation data. When, how­
ever, such data are used by investigators to support
extraordinary findings, which are presented in a confident
tone, and in a way likely to influence public opinion and to
create in the public mind a conviction that the said findings
rest upon a secure foundation of statistical proof, all
qualified workers in the field are justified in making a
searching examination of the statistical evidence.

When, furthermore, the extraordinary findings apparently
enter into the thinking of important officers of all branches
of the national government—executive, legislative, and
judicial—and may be expected presently to contribute in an
important way to the determination of public policy, it
becomes the clear duty of qualified workers to examine this
evidence. 3

Empirical suspicions, as they have been termed in the above, are
part of the environment of methodological evaluation of empirical
research. In this situation they have both advantages and disadvantages.
They create obstacles in analysis and interfere with methodological
evaluation. They prevent the attempt to settle controversies
involving the facts of monopoloid markets. Where the suspicions are
firmly embedded in the mental outlook or perspective of the economist,
empirical research is constantly on the defensive. Yet, we would not
wish for their disappearance, only some modification in their status.
Where the scientist doubts and suspects is the promise of scientific
discovery. Changes in suspicions thus should not eliminate the spirit

but rather the letter of the criticism. Suspicions need greater
translation into the form of methodological principles and criteria.

Facts

A methodological problem in empirical work is the status and
meaning of a fact. Although some assistance might be derived from a
philosophical inquiry into the meaning of a fact, this will not be
attempted. Instead, the idea is offered that a fact may be viewed in
different contexts, and differences arising in this level shape or
modify evaluation.

From one standpoint, facts are finite, tangible, limited, and
exclusive things, permanent in time, and subject to only one
interpretation. Thus, the view implies that controversy over empirical
studies is not the result of diverse positions concerning an appraisal
of phenomena, but rather the premature or misplaced debate over some­
thing not as yet finally discovered. Moreover, it would not be
possible to debate the facts of an issue, since if one has the facts,
i.e., the "real facts" and not imperfect substitutes, controversy is
impossible. By implication, the view suggests that the only obstacle
preventing the verification of theory is a failure to come up with the
true facts. Also, the view may lead to the claim that if economists
had all the facts they could predict any future event perfectly.

The antithetical point of view incorporates opposing character­
istics. Facts, in this case, are infinite, intangible, unlimited,
non-exclusive, variable in time, and subject to diverse interpretations.
Thus, controversies surrounding empirical studies are the natural
corollaries of the imperfect status of facts. From this point of view, theoretical verification would necessarily be tentative and incomplete. Theories could be disposed of by one compilation of facts but be substantiated by a different set of facts. Moreover, the certain prediction of future events would be impossible since the facts are impermanent.

It is evident that economists clash in their evaluation of empirical work due to the differences present in the conception and understanding of the nature of a fact. It is a mistake, however, to assume that one or the other point of view must dominate the judgment of economists. Empirical evaluation is better performed if the methodological perspective of the economist recognizes both.

This dualistic conception of the nature of a fact should afford some needed tolerance in the evaluation of empirical research. Since it is impossible in economics to create carefully controlled laboratory conditions in which facts appear under the tightest of controls, economists must work with alternative arrangements for developing evidence. That alternative arrangements or forms of empirical study are then available pose problems of evaluating the significance, adequacy, and accuracy of the findings presented. Thus, the dualistic conception may reduce the feeling that facts are poor in terms of the yardstick of laboratory investigations and may increase doubts as to the value of every fact regardless of origin.

Contradictory Theses and Methodological Problems

An issue that has received considerable attention in monopoloid
market analysis is the extent to which economic concentration affects welfare, i.e., the consequences of aggregations of economic units on the economic-social well-being of a society. In terms of theory, conclusions are advanced which claim that economic concentration leads to high prices, restricted output, monopoly profit, excess capacity, wasteful advertising, market restrictions, impediments to entry, barriers to economic growth, the stifling of innovation, unemployment, and the aggravation of instability in the economy. However, theoretical analyses lead also to opposite conclusions for the preceding. Similarly, the economic consequences of concentration in economic activity have included references to political and social consequences: the corruption of political processes, bureaucrati- zation, development of a mass or herd culture, eliminating individualism and inculcating group standards of behavior, and interference with home and family life. Again, however, contrary observations concerning the political and social consequences have made appearances.

Theoretical analysis has the ability to handle internal contradic- tions involving the consequences of economic concentration. This is a consequence of the variety possible in theoretical scope, in models and assumptions, all of which are characteristic of theoretical analysis. Moreover, restricting analysis to the theoretical plane eliminates the necessity of admitting the test of facts. A partial explanation, therefore, of the popularity of theory is that its internal structure is designed to handle built-in contradictions. Theoretical analysis will not collapse from the strain of opposing
conclusions. In this sense it seems desirable to inquire if a similar mechanism operates with empirical studies, to allow for the existence of internal contradictions. According to Professor Mason, this may be a requirement of empirical research in monopoloid markets.

No one who is other than eclectic, methodologically speaking, has any business in the field of business organization. The price theorist is, sooner or later, sure to become lonesome in the exclusive company of his market models. The collector and classifier of census data and price statistics will, at some stage, begin to wonder about the relevance of his aggregates and averages. The assiduous devisor of questionnaires, having drawn the secret of the 'full cost principle' from the conscious and the subconscious of his business subjects, may discover ultimately that there is less inconsistency between this principle and profit maximization than he had once supposed. All of these people have, nevertheless, something to contribute to each other and to the subject under study and, consequently, it is unwise to don prematurely the old school tie of a particular orthodoxy.4

What has happened in the economic analysis of monopoly and bigness since the 1920's has been a movement away from relative certainty in dealing with the problem to one of relative uncertainty.5 On the empirical side progress has resulted largely from "the development of striking theses that have later disintegrated under the corrosive action of factual criticism. The resulting synthesis has usually been much less striking but much more realistic.6 Evidence of this is found in many areas, of which the following is illustrative.

The empirical studies of Gardiner C. Means into the role of the large corporations in the American economy, and the separation of the

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5 Ibid., pp. 2-3.
6 Ibid., p. 3.
functions of ownership and control, carried considerable impact. He claimed that the 200 largest non-financial corporations in the United States controlled between 35 and 45 percent of all business wealth, a striking observation. Equally striking he predicted that the rate of growth in concentration, if maintained at the rates in the past, would result in the control of between 50 and 80 percent of non-financial corporate wealth in the United States by the 200 largest industrial corporations.

Such empirical findings were not left unchallenged. Professor Crum took vigorous exception to them, questioning the "propriety" of excluding financial corporations, the classification of railroads and public utilities with the non-financial concerns, the treatment of asset and income estimates, and the division of the data by size of firm. In reply, however, Means suggested that the former was overly concerned with the impressions the study might make, that Crum had misinterpreted the "average", and that he was parochial. Basically, Means felt that the issue was not the result of statistical inadequacies on his part.
In conclusion, then, one must simply reiterate the fact, which Professor Crum appears to accept, that 200 corporations controlled approximately half of all non-financial corporate wealth in 1929 (or 55 per cent at the end of 1931). This fact stands untouched by his criticism. It is up to the American community to decide on the relevance of this figure to American economic life. Nevertheless, the contradiction would persist since Crum offered the rejoinder that

I emphatically object to Dr. Mean's 'conclusion' that I 'accept' his main point. I do nothing of the sort. It has not been my purpose to prove or disprove his main point—it has been my purpose to show that he...has not proved it by statistics. Whether I have succeeded can be determined by the reader.

But, given the debate, perhaps the contradictions would appear in a different light.

Economic Concentration

The concentration thesis has since been dealt with in different ways. According to one study, "During the past two or more decades there has not been an increase in overall business concentration" in the United States. And, the author adds, "In spite of many allegations to the contrary, there has been no general tendency for manufact-

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12 Ibid., p. 87.
turing industries to become more concentrated. Yet, while this judgment is being made of the extent of changes in economic concentration in the United States, findings that allege the opposite—that concentration has been increasing—are repudiated. Thus, an interesting aspect of the appraisal of the extent of concentration is that opposing conclusions are rejected on the basis of the absence of data and the use of improper concepts.

Because, however, the deficiencies of data in all such estimates are substantial, the best conclusion is that there is no proof of a significant change in business concentration.

A methodological principle is at stake. Is it possible to formulate a judgment either way with reference to the extent of concentration if the facts are unavailable and no proof exists? If we should repudiate an empirical conclusion by rejecting the facts do we have the right to offer a contradictory conclusion? If conclusions are symmetrical to facts, with positive and negative conclusions, the rejection of the factual origin may prohibit a conclusion in either direction. It is necessary to consider questions of methodological principle of this sort, since others have followed attempts to prove that economic concentration has not been increasing, that the economy of the United States remains competitive.

15 Ibid., p. 114.
16 Ibid.
There are further aspects of the methodological problems in concentration studies. Measures and indexes of concentration contain questionable procedures that influence evaluation. The raw data geared into such measures, for one, is variable and allows for different interpretations. A narrow or broad definition of an industry in terms of products will afford different concentration ratios, as will be the case with a broad or narrow breakdown of products within a product group. Also, results will differ depending on the extent to which industry data is based on firm or plant data. Moreover, variety in the magnitude of concentration measures, indexes, and curves occurs because of differences in the definition of income, assets, sales, and employment, and their relationship in a firm or industry.

Thus, the conclusion has been offered that attempts to characterize various sectors of our economy in a meaningful way by simple indexes of concentration, profitability, and monopoly have not been very successful. There is a growing consensus that further progress in developing a meaningful theory of market structures and behavior lies in empirical work designed to test the significance of various hypotheses and to suggest new hypotheses of more relevance.

Cf. E. S. Mason, op. cit., p. 43.


Some students would go one step further after recognizing the methodological problems and reject the question of concentration. In regard to "the much discussed question: How competitive is the economy as a whole?", Stigler states that "Despite the frequency with which dogmatic answers are given to this question, it is doubtful whether any meaningful answer is attainable." 21

Merger Movement and Business Cycles

The merger movement in the American economy shares the difficulties present in the determination of concentration in industry. One empirical worker will discover a relation between the merger movement and the business cycle, that "one cannot help but be struck by the degree to which the merger movement corresponds to the business cycle." 22 Another worker, however, will respond with a contradictory judgment, that the "data do not give strong support to the thesis that merger cycles are timed closely with business cycles." 23 At times, a point of contention between the empirical researchers will be


23 Jesse W. Markham, "Survey of Evidence and Findings on Mergers", in Business Concentration and Price Policy, op. cit., p. 151. J. Fred Weston writes that "Much evidence suggests it is inappropriate to consider that a cyclical pattern exists in the timing of mergers"; and that the "timing of mergers does not appear to exhibit cyclical characteristics". The Role of Mergers in the Growth of Large Firms, Berkeley: University of California Press, 1953, pp. 77, 81.
aggrandized to be used as the single criterion for evaluating research work. Stigler, for example, criticizes merger studies and the computation of concentration ratios on the basis of a "systematic bias" introduced by the failure to consider the effects of imports on an industry or firm and its market position. Such a bias "is very large" and "is therefore sufficient...to vitiate the conclusions". However, after taking into account the effects of imports, another empirical worker concludes that the effects of imports are negligible.

"Turnover" of Giant Firms

Quantitative data has also been used to show that even if the American economy contains giant firms, such firms are part of a dynamic, growing, competitive system. Thus, corporate "turnover" occurs as giant firms succumb to the forces and pressures of dynamic change and the activities of other giant competitors. Yet, critics of the thesis question its legitimacy on grounds such as: (a) turnover is over-stated when firms disappear because of mergers among giant firms, (b) mergers of this type open the list of giants to other firms, but this is not evidence of rivalry, (c) a large part of turnover is due to the secular rise and decline of industries, (d) variations in accounting practices in handling subsidiaries affects the stated size of firms, and (e) firms created by antitrust action are not evidence

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of rivalry bringing new firms into the economy. Some would conclude that the entire framework of a "turnover" study is irrelevant for an understanding of monopoly and competition:

The statistical universe of the hundred or two hundred largest corporations is inappropriate to studies of monopoly and competition, and we may hope that this will be the last study to fall prey to its dramatic irrelevance.

Monopoly and Misallocation of Resources

The conclusion that monopoly results in a misallocation of resources in an economic system is a commonly accepted one in traditional theory. Rarely, however, have efforts been directed to measure the degree to which misallocation occurs in quantitative terms. Thus, the empirically-supported thesis that misallocation of resources in the United States from monopoly totaled only $1.50 per person in the year 1954 is striking. Thus, it was claimed that this study puts the problem of monopoly in a different perspective, that "Our economy emphatically does not seem to be monopoly capitalism in big red

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27 M. A. Adelman, "A Note on Corporate Concentration and Turnover", The American Economic Review, Volume XLIV, Number 3, June, 1954, pp. 392-396; and, George J. Stigler, "The Statistics of Monopoly and Merger", The Journal of Political Economy, Volume XLIV, Number 1, February, 1956, pp. 36-37. For an empirical study attempting to show that there is a "stable group of large firms" in the U.S. economy; that there is not as much "turnover" in the large firms as, for example, Kaplan's work would indicate; and that, therefore, the large firms may yet be considered as dangerous, see: Seymour Friedland, "Turnover and Growth of the Largest Industrial Firms, 1906-1950", The Review of Economics and Statistics, Volume XXXIX, Number 1, February, 1957, pp. 79-85.


letters", and that monopoly "does not seem to affect aggregate welfare
very seriously through its effect on resource allocation". 30

Several critics take exception to the factual allegation incorpo­
rated in the thesis. One argues that the figure of $1.50 per capita
is far "too low". 31 Another would attempt to unsettle the thesis by
questioning almost every facet of the study, that the study involves

all sorts of assumptions with any one of which some would
surely quarrel such as the assumption of unit elasticity for
all industry and all consumer goods; the horizontal long­
term cost curve which is empirically vulnerable in the
context in which it is used. They involve, also, the
statistical difficulties; namely the rather special character
of the Epstein data, the use of industry rather than
individual firm profits figures (which raises theoretical
problems too), the questionable weighting system implicit in
blowing up Epstein's sample to cover total manufacturing, and
finally the question whether the five-year period can dub for
long-run partial equilibrium conditions. As I say, someone
would certainly quarrel with each of these matters, and I
would—quite energetically—with some of them. 32

Other criticisms involve broader questions concerning the conception of
such empirical work, e.g., that it is questionable to use profits as a
measure of maldistribution of capital; that monopoly may maldistribute
other resources than capital which would not show up in the profits
figure; and that there are other less desirable results of monopoly
than resource allocation needing intensive investigation. 33

30 Ibid., p. 87.
31 George J. Stigler, "The Statistics of Monopoly and Merger",
cit., p. 24.
32 Ruth P. Mack, "Discussion", The American Economic Review,
33 Ibid., p. 89.
The degree to which concentration in industry affects the flexibility of prices is another topic for which empirical conclusions are mixed. Some argue that the problem itself may be impossible to solve empirically, that it involves the dilemma "of a choice between an institutional gathering of irrelevant data and a theoretical formulation of unanswerable questions". The general findings of empirical studies directed at the problem have varied, with one indicating that a rough relationship exists between economic concentration and price rigidity, another claiming that there is no such relationship and that rigidity could be interpreted in terms of the variations in the product, and one that price changes are the result of changes in direct costs. Critics of the empirical findings have also emphasized such defects as: improper conception of the measure of price flexibility or inflexibility; too narrow and restricted samples; inadequate price data as in the failure of ELS wholesale price index to show hidden


discounts; and the failure to apply standards in the determination of what is to be included and excluded.\footnote{36}

Of the many criticisms of empirical studies of price inflexibility and concentration, the one that seemingly carries heavy weight is aimed at the concept of a price. Errors arise in price studies when the research worker conceives of price as simply a one-dimensional entity. To the contrary, price is affected by many other factors. On the product side, changes in the quality of the product, multiplicity of style, grade, and size factors, and the continual introduction of new lines and the withdrawal of old lines leads to occasional "broadside price changes", "sharpshooting" price changes, and "tailor-made" prices.\footnote{37} Market complications also affect the meaning of a price, e.g., customer-class prices, a discount structure by geographical structure, quantity of business, method of purchase, and whether standard or private labels are used, represent potential sources of departure from the imagined, "quoted" and objective price. In addition, special practices in the form of cash and credit terms, freight and delivery items, special service charges, advertising allowances,


\footnote{37} Willard L. Thorp, "Price Theories and Market Realities", \textit{The American Economic Review}, Volume XXVI, Number 1, \textit{(Papers and Pro-
container extras, and rental or leasing plans affect the quoted prices used as the basic data in price studies. Thus, complexities of this type challenge the significance of much of our statistical work in its present form—both as depicting price trends and price flexibility. Perhaps nothing can be done about it, and I know that those working with the data are aware of these weaknesses. But list prices certainly give no accurate picture, being merely of significance as a basis on which computations are made. Even list prices with corrections for certain simple discounts are inadequate in many of the cases which have been examined.

The effects of the complexities, however, "can easily be overstressed in discussions of the price rigidity issue". The consequence of the difficulties in carrying on empirical research into the problem of concentration and price inflexibility is that contradictory theses remain in the literature, encompassing the range of: (a) It is concentration, rather than the mere fact of overhead costs, which accounts for the tendency toward price inflexibility; "The degree of concentration in production is not a significant factor in determining price-quantity behavior of manufactured products with reference to the business cycle" and "so-called 'price rigidity' is not in any considerable degree a function of the extent of concentration"; to

38 Ibid., pp. 16-19.
39 Ibid., pp. 19-20.
41 Ralph C. Wood, "Dr. Tucker's 'Reasons' for Price Rigidity", op. cit., p. 666.
42 Willard L. Thorp and Walter F. Crowder, "Concentration and
the claim that economic concentration may or may not lead to rigid prices. Costs

Empirical evidence relating to costs in monopoloid markets is inconclusive. Attempts to develop quantitative data to support cost generalizations have been subject to criticism. Critics have offered different reasons for concluding that it is "both difficult and dangerous to generalize on the basis of scattered and heterogeneous empirical materials..." The reasons advanced include broad questions of structuring empirical studies and specific problems.


Caleb A. Smith, op. cit., p. 229.
of gathering and interpreting cost data. On the broad plane, the critics have mentioned the difficulties arising with the concept of costs, output, and size relationships, that research investigations ignore the inherent complexity of such data and misconceive the nature of empirical facts. This in turn involves specific research problems of defining and classifying the data, developing such a thing as the meaning of capacity, making a distinction between products for firms producing a "product mix", handling the existence of different technologies at different times and between plants, and managing cost data on a firm or plant basis. All of this, moreover, in addition the problems arising in connection with varying accounting practices in industry and the application of straight-line regression in developing cost functions.\textsuperscript{46}

Entry

Recently, attention has been directed at the importance of entry into industry as a structural dimension of markets with the effects of various conditions of entry on market performance explored.\textsuperscript{47} An attempt has been made to show the significance of varying entry conditions on the performance of selected industries with the use of a "cross-sectional" method, that is, an inter-industry framework with


\textsuperscript{47} Joe S. Bain, \textit{Barriers to New Competition (Their Character and Consequences in Manufacturing Industries)}, Cambridge, Massachusetts: Harvard University Press, 1956.
emphasis on entry and performance. 48

The theoretical basis of the study of entry rests on the idea that economists have too long neglected the significance of entry in the structure of markets as a means of explaining and predicting market performance. Thus, Professor Bain claims that "potential" competition may be as strong a regulator of the behavior and performance of firms as actual competition is expected to function as such a regulator in orthodox theory. It is to be understood that "variations in the condition of entry may be expected to have substantial effects on the behavior of established sellers, even though over long intervals actual entry seldom or never takes place." 49

Entry is defined in terms of new capacity introduced into an industry by new business entities, excluding simple reorganizations of established firms and expansion of capacity by existing firms in an industry. 50 There are, then, three basic conditions strongly influencing entry into an industry and shaping market performance, conditions which are identified as: (1) economies of large-scale, (2) product differentiation, and (3) absolute cost advantages. Thus, where entry is easy, the above conditions are absent, that is, no important economies of large-scale operations, no product differentiation advantages, and no absolute cost advantages of established firms

48 Ibid., p. vi.
49 Ibid., p. 4.
50 Ibid., p. 5.
over potential entrant firms. Conversely, where entry is difficult, all three conditions are present to a high degree.

As an accomplished theorist and noted student of markets, Professor Bain "sees" in the theoretical development of entry and performance relationships a system of multiple, conflicting theoretical conclusions. He conceives of theory as a means of providing a "map" of possibilities, or sets of theoretical results. Thus, even where some of the theoretical conclusions are given greater emphasis than others, they must be judged as part of a large number of conceivable theoretical outcomes.

In pursuing this empirical investigation and tentative empirical "testing", Bain exercises considerable caution in advancing claims concerning the findings. He recognizes that the inadequacy of information permits only "fragmentary testing." Several specific empirical biases are also identified as limiting factors, including: (1) the size of the sample of industries used, (2) the composition of

51 Ibid., p. 12. Bain devotes separate chapters to each of the conditions, e.g., Chapter 3, "Economies of Large Scale as Barriers to Entry," (pp. 53-113); Chapter 4, "Product Differentiation Advantages of Established Firms as Barriers to Entry," (pp. 114-143) and Chapter 5, "Absolute Cost Advantages of Established Firms as Barriers to Entry," (pp. 144-166).

52 Ibid., see Tables XIV and XV, pp. 169-170.

53 His Chapter 1, "The Importance of the Condition of Entry," pp. 1-41, affords ample illustrations of the many theoretical possibilities considered.

54 "Limitations of time and of data restrict the sample of industries studied; information on the character and determinants of the condition of entry in these industries is not entirely adequate; available performance data permit only a rather fragmentary testing for the association of the conditions of entry to market performance." Ibid., pp. 42-43.
the sample, (3) the size of the industries, and (4) the degree of high seller concentration present.\textsuperscript{55} Moreover, there is reference to limitations on his findings originating in the use of questionnaires, confidential data, and the time period involved.\textsuperscript{56} In sum, these are elements on the empirical side that reduce the degree of confidence concerning the reliability of his findings.\textsuperscript{57} Taking both the theoretical conception inherent in this study and the empirical doubts surrounding the factual investigation and testing, it seems apparent that the author is somewhat hesitant to push his analysis very far beyond the methodological limitations that have been recognized.

In an evaluation of the success of the study, Professor Heflebower grants that Bain stays within this theoretical-empirical-methodological framework and compliments the skill with which the study is conducted.\textsuperscript{58} Yet, even with this admiration, several weaknesses are suggested: (1) reliance upon a theoretical framework of

\textsuperscript{55}Ibid., pp. 42-46.
\textsuperscript{56}Ibid., pp. 49-51.
\textsuperscript{57}"Our predictions thus are limited, since they are extremely rough and in a significant degree indefinite in many cases. Moreover, they do not touch upon certain important dimensions of market performance." Ibid., p. 180. "But let us emphasize at the outset that the tests are somewhat inconclusive, and that their results are thus not only limited in scope but also tentative in character." (p. 182) "Because of the incomplete nature of the check on our predictions of the association of performance to entry, as well as the limited scope of these predictions, it is not possible to state any definitive conclusions on how the conditions of entry affects the workability of competition." (p. 203). Cf. also, pp. 181, 183, 187, 188-189, 201, and 210.

"comparative statics" that therefore ignores the role of secular and
cyclical movements and the force of the Schumpeterian dynamism of
capitalistic innovation; (2) little attention is directed towards the
range of oligopoly theory except as it relates to entry; (3) the
pressure on firms arising from entry by firms established in other
industries is ignored; and (4) debatable assumptions and conclusions
are used with reference to the average cost of plants of various
scales. 59

The first three defects noted above can be subsumed under the
statement that the framework is debatable. In terms of preceding
discussion, Professor Heflebower seems more interested in theoretical
and empirical development pursued within the framework of organic
model-types. Empirically, he prefers more of an institutional
approach than this attempt to confine the empirical study, as Bain
does, to a single dimension—entry. The fourth defect simply
emphasizes a previous point that empirical studies of costs, scale,
capacity, and so forth, are openly questionable from many directions
and viewpoints.

Several other characteristics of Bain's study are of methodo-
logical interest. First, it is possible that the reliance upon
"potential" competition inherently limits the achievement of clearly
defined, acceptable empirical conclusions. Potential competition
differs from actual competition empirically, for the latter is
conceivably measurable while the former may conceptually remain

immeasurable. Potential competition is "threatened", "future", "impending" competition; in short, future competition that is expected, not realized. The problem for empirical work would then involve the issue: how are economists to identify and to measure that which does not exist? Secondly, potential competition is a highly rational concept in the sense that calculations are possible, at least in theory, as to the behavior of firms which act rationally in terms of their own goals—profits. However, entry of firms may be an irrational move, the desire of other firms to grow large, the desire of new entrants to "try-their-hand", etc. Or, it might simply be stated that in an uncertain world one may expect a great deal of miscalculation as to conditions of entry and consequences of entry into an established industry.

At the policy level, Bain suggests that students of market policy (anti-trust policy) should devote attention to a new design of legislation in order to preserve potential competition. However, some students of the problem of public policy in this area would argue that Bain merely dresses up idiomatically the original intent of the Sherman Act which was to prevent "attempts to monopolize." The advice on "maintaining potential competition" thus would be only a different way of stating that attempts to monopolize should be forestalled. On this matter of seller concentration, there is a revealing passage referring to the emphasis between "entry" considerations and the orthodox concern with concentration:

We have recognized throughout that seller concentration has a concurrent influence—and probably an equally important one—with the condition of entry in determining the worka—
bility of competition in an industry. In other words, both actual and potential competition are important. Existing antitrust policy implicitly more or less recognizes this, although the economist commentators on that policy are inclined to give a preponderant emphasis to the effects of seller concentration, and then to append a few remarks on the problem of entry.60

His position is that "High concentration may be a relatively innocuous phenomenon if entry barriers can be reduced to a moderate level."61

Institutional Approach

It would be possible to present additional cases relating to conditions, behavior, and economic consequences of monopoloid markets as based on empirical studies. This would show the characteristics of controversy indicated above but with reference to other issues such as: the role, size, flexibility, and equality of profits; entry into different types of industry; the success or failure of research and innovation under different market and industrial structures; the contribution of various firm sizes to economic growth; the place of patents in the economic environment; advertising as an economic waste or benefit; the effects of large firms on general economic stability and investment; and alterations in the distribution of income resulting from changes in market structure. An exhaustive investigation along such lines would duplicate problems mentioned previously and emphasize the difficulties involved in evaluating research of this type. Yet, it still would not include the entire range of market

60 Bain, op. cit., p. 217.
61 Ibid., p. 218.
analysis, empirically considered. An important omission would be the overlooking, or ignoring, of institutional elements of markets.

Certainly, economic analysis can, and does, ignore institutional change with its emphasis upon theoretical models and their implications, or as empirical evidence relating to special issues of monopoloid markets is developed. However, to demand strict reliance upon pure theory or upon the results of empirical studies of selected issues as devices for fully interpreting market conditions is not the soundest requirement. Within the economic system changes take place that are not confined to events explainable in terms of elasticities, assumptions, or mathematical formulae, nor to phenomena measurable in terms of dollars, production, and prices.

The theoretical economist may exclude from his analysis any consideration of internal structural changes in firms, such as: the separation of ownership and management, the development of corporate bureaucracy, and the growth and stimulation of the managerial philosophy or ideology. Empirical investigations of consumer behavior may emphasize the size and composition of income, savings, and debt, to the exclusion of the institutional elements modifying tastes and shaping consumer preferences. Trade association activities, the control of mass media, advertising and public relations, political contributions, lobbying, and the infiltration of a "bigness" point of view in government also may be adjudged as non-economic phenomena by theoretical and empirical analysts.

Ideas and insights of this order\textsuperscript{62} into potent and fundamentally

\textsuperscript{62}Since the institutional approach is viewed in broad terms, only
significant changes occurring in the economy may be unsusceptible to
criteria erected by theory and empirical techniques. They may
challenge orthodox arrangements of data and customary ways of viewing
economic phenomena. Yet, to ignore the challenge or to repudiate it
by references to arbitrary methodological criteria would be a mistake.
It is true, as the following illustrates, that it may be difficult to
reconcile the contradictory theses appearing from such studies. But,
this is a problem also arising in theory and empirical studies.

selected references pertinent to the range envisioned can be mentioned,
references that would include: Fundamental changes in the economy;
Kenneth E. Boulding, The Organizational Revolution, (A Study in the
Ethics of Economic Organization), New York: Harper & Brothers
Publishers, 1953; Peter F. Drucker, The New Society, (The Anatomy of
James Burnham, The Managerial Revolution (What Is Happening in the
World), New York: The John Day Company, Inc., 1941; Adolf A. Berle,
Jr., The 20th Century Capitalist Revolution, New York: Harcourt,
 Brace, and Company, 1954; Giant Business and Its Consequences; Robert
A. Brady, Business As A System of Power, New York: Columbia University
Press, 1943; Theodore J. Kreps, "Cartels, A Phase of Business Haute
Politique", The American Economic Review, Volume XXXV, Number 2, Papers
and Proceedings, May, 1945, pp. 297-311; T. K. Quinn, Giant Business:
Threat to Democracy, New York: Exposition Press, 1953; T. K. Quinn,
Giant Corporations; Challenge to Freedom, New York: Exposition Press,
1956; Herrymon Maurer, Great Enterprise; Growth and Behavior of the
Big Corporation, New York: The Macmillan Company, 1955; C. Wright
C. Wright Mills, White Collar, The American Middle Classes, New York:
Oxford University Press, 1955; David Riesman, with Reuel Denney and
Nathan Glazer, The Lonely Crowd; A Study of the Changing American
Character, New Haven: Yale University Press, 1950. Ideology and
Propaganda: Reinhard Bendix, Work and Authority in Industry:
Ideologies of Management in the Course of Industrialization, New York:
John Wiley & Sons, Inc., 1956; Francis X. Sutton, Seymour E. Harris,
Carl Kaysen, and James Tobin, The American Business Creed, Cambridge:
Harvard University Press, 1956; J. D. Glover, The Attack on Big
Business, Cambridge: Harvard University Press, 1954; Thurman W. Arnold,
The Folklore of Capitalism, New Haven: Yale University Press, 1937;
John Ise, "The Futility of Trust-Busting", The American Economic Review,
Volume XXXVIII, Number 2, Papers and Proceedings, May, 1948, pp. 488-
501; Ernest Gruening, "Power and Propaganda", The American Economic
Review, Volume XXI, Number 1, Papers and Proceedings, March, 1931,
Institutional studies have noted that the divorcement of stockholders and managers results in greater "professionalization of management" and better "public service" while others see the rise of an elite class that may lead to tyranny. Or, the idea that changes taking place in the structure of the economy are bringing about greater initiative and individualism, improved innovational drive, may be in conflict with the opposing belief that the opposite results. Opportunities for economic-social advancement may be


Business managers have become increasingly conscious of responsibilities inside and outside their companies, and have increasingly dedicated themselves to study, experiment, and change. This open-mindedness is one reason why large corporations can continue to grow in the U.S. and yet not turn into monopolies..." Herrymon Maurer, Great Enterprise, op. cit., p. 63.

As each of the economic, political, and military domains "has coincided with others, as decisions tend to become total in their consequence, the leading men in each of the three domains of power—the warlords, the corporation chieftains, the political directorate—tend to come together, to form the power elite of America." C. Wright Mills, The Power Elite, op. cit., p. 9. Also, see Robert A. Brady, Business As a System of Power, op. cit.

T. K. Quinn, Giant Corporations, op. cit.
increasing or decreasing. The increased size of firms may be a natural by-product of evolution in the economy or the consequence of a desire on the part of managers to protect markets and to enhance earnings.

Several methodological implications follow from a consideration of the role of the broad-gauged institutional studies. Initially, methodological criteria remains untouched within a special framework. Precision of theory and the methodological criteria implied, will oppose incomplete formulation of empirical work. But, empirical investigations require fulfillment of criteria of a type different from those involved in theory. An institutional approach would possess additional differences in criteria. However, in terms of a general point of view, each framework, or approach, or conceptual device would set checks on the others. An empirical study may not satisfy theoretical criteria, but neither would theoretical works satisfy fully empirical criteria. An institutional study may satisfy even less the criteria of theory and empirical work, yet give a better sense and "feeling" to changes occurring within the economy. What this implies is that interaction among opposing methodological criteria assists in promoting a fuller and more complete kind of analysis. The empirical worker is subject to the test of precision; theorists are required to maintain contact with the world of fact and

66 W. Lloyd Warner and James C. Abegglen, Big Business Leaders in America, op. cit., p. 33.

evidence; and the institutional frameworks demand that fundamental changes and trends be recognized. In the absence of this interaction, economics loses some of its power of analysis.

Summary and Conclusions

The thesis is commonly advanced that the propositions, hypotheses, or theories of a discipline ultimately must produce observable, measurable results, that science ultimately makes an appeal to facts. The process of developing facts is, however, complex. An examination of empirical work in monopoloid market analysis illustrates several methodological problems and difficulties often encountered.

Some of the obstacles to empirical research appear in the form of ill-defined doubts or "suspicions" concerning the reliability of factual findings. Lack of faith in techniques used, bias or partiality, the impossibility of reconstructing work independently, interrelationships of sets of facts, empirical results versus theory, and policy implications associated with empirical results are suspicions that generate methodological controversy. Such suspicions are part of the environment in which methodological evaluation operates. However, it is not urged that the spirit of criticism arising from this source be eliminated, but that the suspicions be translated into the form of methodological criteria and principles.

The nature of a fact is an important methodological problem in empirical research. Several conflicting views are involved, and differences at this point shape the type of methodological evaluation obtained. Instead of viewing a fact as something tangible, finite,
limited, and permanent in time; or, assuming that facts are infinite, intangible, unlimited, variable in time, and subject to various interpretations, the use of a dualistic conception is suggested. As a consequence of the dualistic conception, there should be more tolerance in the evaluation of empirical research and increased ability to identify satisfactorily methodological problems.

Empirical research contains many instances of the existence of contradictory theses based on factual investigations. Thus, a methodological problem of importance is the determination of the nature of the contradictions and a consideration of the means by which they may possibly be eliminated. Comparatively, conflicting theses of empirical work are similar to those in theory. Theory has an internal structure, a built-in mechanism, to cope with the existence of conflict among theoretical conclusions. It appears also that the course of empirical work contains an inner structure to deal with conflicting empirical conclusions, although this may not be admitted by the participants in a controversy.

Examination of empirical work from a methodological viewpoint brings out types of criteria and implicit judgments offered by economists. In this connection, application of a basic methodological principle in evaluation needs questioning. Are economists correct in formulating a judgment either way with reference to topics of monopoloid markets when the factual basis is expressly denied? The topics of economic concentration, merger movements, turnover of giant firms, monopoly and misallocation of resources, rigid prices, and costs provide instances of the range of the problem entailed.
There are specific procedures in empirical research that come under methodological scrutiny. Questioning the raw data and definitions used become identified as special problems of research. Varying specifications in handling a firm, an industry, a product, prices, costs, growth, flexibility, etc., result in contradictory appraisals of the significance of findings.

Yet, an examination of the status of empirical research, the presentation of methodological issues in general and their identification as special problems for unique research efforts, does not indicate the entire breadth involved. Institutional studies have direct bearing on factual findings, and introduce both general and specific methodological problems. Comparing theory, empirical research of selected topics, and institutional studies involves questions of the relationship among them and the degree to which each must fulfill methodological requirements handed down by the others.
Chapter VII

Public Policy in Monopoloid Market Analysis

Methodological perspective has been emphasized in previous chapters as an essential element in the evaluation of theoretical and empirical studies. In the policy plane of economic analysis, perspective has similar significance. Economic policy requires an understanding of the difficulties of theoretical and empirical work in the provision of the information for policy. It also requires an appreciation of the special problems of social action. In a sense, economies has a problem of "confounding" in policy analysis involving the intermingling of methodological problems in foundation work with special issues of policy. Separation of investigation into theoretical and empirical categories offers assistance in reducing policy confounding, but it does not touch important specific issues of policy. Thus, the purpose of this section is to examine some of the broad and specific issues of economic policy and to suggest their implications for policy formulation.

Contradictions in Public Policy

It should not be expected that the policy plane will be free from contradictions and controversy. From the point of view of policy built upon the foundation of theoretical and empirical work, deficiencies and contradictions at the sub-level are transferred to difficulties at the policy level. Analogically, the failure to
construct a sound and consistent foundation affects the structure to be supported. In this case, structural defects in theory and empirical investigations would reduce the acceptability of the policy analysis is to support. Pushed to an extreme, however, the analogical argument may lead to a rejection of all policy considerations. Thus, public policy recommendations may be placed under attack, not for reasons of philosophical or ethical differences of opinion, but solely due to the idea that the imperfect character of the analytical foundation prohibits action.

The presence of contradictions and disagreements over economic policy also results from different ideas of social action. The philosophical outlook of the economist, his Weltanschauung, will be made up, in part, of ideas associated with the extent to which man is able to alter his environment. From one position, public policy may be suspect because it involves tampering with "laws" of nature and society. The argument that man's destiny is determined by the operation of impersonal forces in society, which cannot be resisted by social action generated by man himself, may be used to oppose public policy. On the other hand, the economist may visualize man and his environment in a way that requires deliberate social action to alter and to direct the course of society. For the latter view, social action and public policy are the requisites of maintaining an orderly society.

The broad generalizations concerning likely sources of contradictions in policy serve to underscore the fact that controversy is an inevitable aspect of policy. Economics cannot avoid judgment in
policy, and that judgment will be influenced by ideas that make up perspective. Therefore, it seems necessary to adopt the view that the policy area is essentially one of contradictory elements and the interrelationship of conflicting principles and positions.

There is, of course, some difficulty in postulating that policy in economics that requires the acceptance of conditions of contradiction. In a conventional view of science, contradictions and controversy over facts and theory is anathema to the goal of science of eliminating contradictions. It would then appear that economics becomes unscientific with the denial that such a goal can in fact be achieved at the policy level. Yet, even though it is admitted that the goal of science is the elimination of contradictions, that does not mean that all contradictions are, or can be, eliminated. New ones may appear through additional discoveries and through basic changes in the data of science. Moreover, it has been noted that contradictory theories may exist side-by-side within a science as explanations of identical phenomena. Elimination of contradictions, therefore, while a goal, is not the status of scientific enterprise at any one moment of time. For economics, the absence of contradictory policy judgments may mean the end of any scientific character. Perfect unanimity among economists on an issue of inherent contradiction may mean, not that the discipline has attained the goal of "Science", but that it has become the captive of a single philosophy, or bias.

A Public Policy Perspective

Policy perspective in economics involves an understanding of contradiction arising out of differences in ideas and philosophies.
both scientific and non-scientific. It requires a conceptual framework to cope with contradictions and to furnish a means for making decisions among policy alternatives. In the case of policy for monopoloid markets, the perspective of the economist should include both an understanding of general policy issues and the problems of policy directed specifically towards monopoloid markets. The division between general problems of economic policy and specific problems of monopoloid market policy helps to keep broad philosophies and principles of public action distinct from specific recommendations aimed at malfunctioning of markets. The general problems of policy extend beyond the area of market policy, with ideas broader than a single issue, and with principles from which specific policy may have been deduced. On the other hand, policy is carried out by specific action aimed at some part of the economy, so that full evaluation of policy necessitates working with both.

There can be no guarantee that this conceptual framework would satisfy demands for final, conclusive solutions to policy problems. The policy framework, with a distinction between general and specific policy ideas and suggestions, only affords a different rationale or format. However, if the nature of policy is such that it contains permanent internal contradictions, economics may be better off with a device that isolates major policy principles and philosophies from the specific policy directed at a contemporary problem. The framework might facilitate the discovery of basic policy philosophies and beliefs and simplify their identification. This would be important in those cases where economic policy recommendations represent a mixture
of philosophical preconceptions, economic prejudices, and personal panaceas. It should also be helpful in situations involving a complex of problems of philosophy, ethics, theory, values, and evidence.¹

In addition, the framework might be better suited for dealing with the problem of policy consistency. An economist may have a fundamental policy premise (at the general level) from which he deduces his specific suggestions aimed at a contemporary malfunctioning of the economic system (the specific level of policy), or, the premise may be used as the basis for accepting or rejecting the policies of other economists. Thus, it might be argued that inconsistency develops in policy when a fundamental policy premise conflicts with a specific recommendation. However, the problem of policy consistency is more general than the testing of logical relationships for consistency or inconsistency. The problem involves a questioning of the legitimacy of the test of consistency. It seems likely that policy consistency is, in fact, an impossibility. Hence, the use of the test of consistency would be unrealistic. Posing the issue differently, the only way in which the economist could maintain consistency in a policy position would be to establish such a narrow, restrictive premise and to be so exact in deductions that no basic creative suggestions would be forthcoming. For example, if the principle of laissez faire is rigidly adhered to, and carried to its logical consequences, public policy would degenerate into anarchy, opposition to all governmental

action. Conversely, complete acceptance of economic planning and control would lead logically to complete dictatorship. Thus, perhaps it is the implicit recognition of the difficulty that leads to positions somewhere between the extremes of rigid adherence to laissez faire and the complete acceptance of governmental planning and control. Public policy issues become matters of the "degree" of governmental responsibility and participation in the economic system. This conceptual framework for policy analysis may also provide flexibility in the examination of policy alternatives and enlarged access to imaginative ideas. Monolithic policy positions rooted in narrow policy visions need to be avoided in the formulation of public policy. Stagnation in policy ideas may be more debilitating than short-term disruptions from adopting a unique policy. Under such conditions, it would be desirable to foster the encouragement of policy ideas, the preservation of freedom for advancing the ideas, and a technique for analyzing them. The framework suggested may be of assistance here.

General Approaches to Public Policy

Many problems of economic policy occur at the general level of policy, where the problem of the type of approach applicable to policy involves conflicting viewpoints. Modern welfare economists approach

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2 Lionel Robbins claims that the English Classical economists (Hume, Smith, Ricardo, Malthus, Torrens, Senior, McCulloch, and the two Mills) did not accept dogmatic laissez faire, that they saw many places for state action and intervention. The Theory of Economic Policy in English Classical Political Economy, London: MacMillan and Company, Ltd., 1932, pp. 21, 34-67. According to Robbins, "the Classical Economists were reformers and...the theory of economic policy in
economic policy within a formal framework, confining their attention to conditions of social optimum, and applying logico-mathematical methods in the development of technical conditions of social optima. \(^3\)

Although it is relatively easy to criticize the formalists of modern welfare economics for adopting a narrow and static concept of economic policy, for fostering a method of analysis that is "too theoretical" and non-empirical, for ignoring the element of group action and bargaining relationships in the economy, and for their failure to develop criteria for ethical judgments, that is not the significance of the formal approach to policy at this stage. Rather, the modern welfare economists are of interest because of the way in which they conceive policy problems and their solutions in formal, highly technical terms. \(^4\)

The taxonomic approach to problems of economic policy represents another way in which economic policy is analyzed. \(^5\)

In its bare outlines, the taxonomic approach to policy deals with model

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construction on an extended basis. A system of many different models is established, in which each model differs from the others in respect to the interrelationships among possibly significant economic variables. Different magnitudes of the variables are established and their resulting consequences traced out in terms of established model relationships. In its construction of differing models the taxonomic approach assumes a formal, or theoretical, aspect. Unless empirical functions are built into the models, which is impossible in the case of the large number of models that require hypothetical results, the taxonomic approach remains a formal one. However, while the taxonomic approach has this formal characteristic, a built-in theoretical structure, it provides an outlook for policy issues on a broad plane. It attempts to create a complete policy framework in which all possible policy consequences would appear, in addition to the requirement that the approach remain open-ended to allow for the inclusion of other variables and interrelationships as they are recognized.

A model approach to economic policy appears in the work of Tinbergen. Applying the idea of decision models developed by Ragnar Frisch, Tinbergen suggests that quantitative economic policy might well be explored in terms of the concepts of data, target variables, instruments or political parameters, and irrelevant variables, using quantitative empirical functions expressed in the form of a series of

6 Ibid., p. 75.

Similar to the taxonomic approach, the objective of policy is to determine an optimum policy from the models available, i.e., to determine the optimum solution. With this approach the range of systems of economic policy is extensive, from those with a few targets to complicated target sets, from abstention of controls to detailed regulation, from objective to discriminatory instruments of policy. Moreover, non-economic aspects of policy intrude into the formal structure as important elements.

The reader should not be deceived by the preceding chapters and believe that the formulae tell all about economic policy. Economic policy, like any real activity, has to reckon with many aspects originating from very different realms of life, and hence certainly not only economic viewpoints: institutional, juridical, technical in the widest sense of the word, and psychological...

A further difficulty with the development of decision models arises when policy is established by several policy makers, that is, decentralized policy. Under such circumstances, as in the United States with multiple governmental agencies, the division of responsibilities among the different branches of government, the existence of layers of policy-making authorities from federal to local


\[9\] Ibid., p. 37-38.

\[10\] Ibid., p. 70.

\[11\] Ibid., p. 74. Non-economic factors mentioned by Tinbergen include: legislative principles, public opinion, protection of contracts, psychological element of confidence, cost and delays and numbers affected by policy, absence of data, uncertainty, judgment errors, personal animosities, too rigid norms, personal or institutional inertia, aversion to complicated policy, and the way in which policy is presented. See pp. 74-77.
with degrees of autonomy, and the presence of unions, corporate
corporate entities, collectives, the problem of policy-making becomes exceedingly
complicated.\textsuperscript{12} The likelihood of being able to determine the optimum
policy is therefore reduced.\textsuperscript{13}

A different approach to public policy, one that sheds the frame-
work of logical-mathematical relationships is presented by John R.
Commons.\textsuperscript{14} Instead of waiting for the creation of the models of
policy before considering the non-economic factors affecting policy,
Commons starts with the social process with its conflicts of interest
in individual and collective action. Where the formalists in their
approach to policy would leave to one side collective action by
organized groups, strategy and power relationships, and the working
rules of society, Commons' approach would begin with individual and
social behavior within a society of conflict among groups and
individuals. The legal, psychological, personal, power, strategy
elements, etc., that Tinbergen recognizes as factors affecting the
decision models, complications reducing the effectiveness of policy
models, are the data for policy with Commons.

Thus, one of the basic problems of economic policy at the general
level involves differences in approach and in the methods to be used
within approaches. Given these alternatives, a choice or decision

\textsuperscript{12} Centralization and Decentralization in Economic Policy. \textit{op. cit.}
p. 2, 18.

\textsuperscript{13} Ibid., pp. 50-51.

\textsuperscript{14} John R. Commons, \textit{The Economics of Collective Action}. New York:
issue arises. However, the criteria for the best decision or selection among the alternatives are difficult to establish, since each approach would use special sets of criteria. The result of this is an internal contradiction at the general policy level.

Role of State Action

Contradictory philosophies of central governmental action are influential at the general policy level. Supporters of a laissez faire philosophy of government action defend the freedom of individual action and the functioning of an impersonal market-price system, condemning almost all suggestions of federal action as a threat to personal freedom and liberty. Others conceive of central government activity as the means for preserving the economic system from collapse. Sometimes, however, the contradictions are not framed so succinctly in terms of polar cases of extreme laissez faire or extreme central planning and control, but are disagreements over the degree of policy action taken by central government. Perhaps a typical expression of the philosophy would be that "governmental control is one of our social resources which should be carefully economized."18


16 Herman Finer, Road to Reaction, Boston: Little, Brown and Company, 1946.


18 Corwin D. Edwards, "Can the Antitrust Laws Preserve Compe-
Or, that there has to develop "acceptance by the state, in some form or another, of responsibility for participating in the exercise of economic power". Public policy for a "mixed" economic system in which variety in powers occurs also is stressed.

The competitive system which, it is presumed, the government should try to preserve is not that perfect and universal competition that exists only in the writing of certain economists. Rather, it is the competitive element of a mixed economy that has long contained, and, if competition is preserved, will still contain, large amounts of governmental control, wide differences in the power of business units, substantial obstacles hindering access to markets, monopolized activities under public sanction, and various types of agreed action.

Thus, while granting that extremes in philosophies of public policy for the United States economy account for part of the contradictions at the general level of policy, in other cases the contradictions involve the degree to which government participates in the economy.

Historically, the philosophy incorporated in the Employment Act of 1946 seems to indicate acceptance of an active role for the central government in the economic affairs of the nation. In the interpretation of Sidney Fine, "the ideological conflict between the advocates of laissez faire and the advocates of the general-welfare state have been resolved in theory, in practice, and in public esteem in favor of


the general welfare state",\(^\text{21}\) an interpretation to which other historians subscribe.\(^\text{22}\)

Size and Certainty in Policy

The degree of governmental action within the economy involves many issues at a general level of policy and the opportunity for conflicting positions. The size of public policy is readily subjected to diverse interpretations. Should economic policy be applied on a wholesale scale, or should it be advanced in terms of incremental

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\(^{21}\) Sidney Fine, *Laissez Faire and the General-Welfare State (A Study of Conflict in American Thought, 1865-1901)*, Ann Arbor: The University of Michigan Press, 1956, p. 400. Fine explains the distinction between the philosophies of the policies of laissez faire and the general-welfare state as: "The term 'laissez faire' is employed in this book to embrace the arguments of those who accept government as a necessity but nevertheless wished to see its functions reduced to the narrowest possible limits. They recognized that government must protect life and property and must provide a few common services, such as education, but essentially they viewed the state in negative terms and were loath to have it assume positive duties in the interests of the general welfare. The theorists of the general-welfare state, on the other hand, believed that the state could benefit society by a positive exertion of its powers and that it should therefore act whenever its interposition seemed likely to promote the common well-being. Although they were unwilling, in theory, to set any arbitrary limit to the scope of government action save the ability of the state to serve the general interest, the principal advocates of the general-welfare state wished government to operate within the framework of the capitalist order and to avoid the extremes of both laissez faire and socialism. Of course, not all critics of laissez faire were, at the same time, advocates of the general-welfare state, but insofar as they helped to discredit the let-alone policy, they facilitated the task of those who were urging the state to play a considerably more important role in the market place and in social affairs as a whole."


adjustments within the economy? Should public policy be a matter of formulating general rules of the game for economic behavior within the economic system or a matter of devising ad hoc policy to counteract special patterns of behavior? Views on such issues vary. Hayek, for instance, argues that the implementation of law as a form of policy action requires general, impersonal, and predictable laws. Simons demands a structure of law that is "elaborate, stable, confining", where the "rules of the game" in a competitive society are "definite". Critics of the elimination of uncertainty in policy, in the form of law, argue that rigid, inflexible laws cannot cope with changing problems and circumstances. Economic regulations must be plastic and adaptable. Certainty is as undesirable as it is unattainable. The more explicit the prohibition, the more likely it will be riddled with loopholes.

While businessmen prefer certainty in law, at least as law applies to business activities, the students of public policy would not agree completely. There is something to be said in support of certainty in law, that independent policy can be established within a framework of

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24 Ibid., op. cit., p. 83.


26 Milton Handler, Antitrust in Perspective (The Complementary Roles of Rule and Discretion), New York: Columbia University Press, 1957, pp. 46-47. Also a proponent of uncertainty in law is Corwin D. Edwards, see Maintaining Competition, op. cit., p. 46.

expected interpretation of law. In another sense, the working rules of competitive society, if they are stable and predictable, allow individuals to develop plans taking into account the structure of law. However, when dealing with concentrates of individuals, or in the case of oligopolistic markets, the individuals are not small and numerous. Perhaps predictability is essential when many small decision-making groups are formulating plans, that all would have to operate within the same context; yet, when conditions change to those of only a few, large units, it may be necessary to introduce unpredictability into policy. When the units are large and the numbers small, as in oligopolistic industries, it is difficult not to think of the interests of a single firm linked to the interests of the others. The pattern of price leadership, basing-point pricing, trade associations, pools, cartels, market sharing, and so forth, represent the attempts by the few to create certainty in their market relationships. Public policy, therefore, in dealing with economic concentrates, might be better framed if some uncertainty is deliberately interjected.

Paradoxes of Policy

Another problem having significance at the general level of policy concerns the actual presence of government in the economy and the possibility that social goals will not be achieved because of state action. In other words, public policy may develop paradoxes, that public policy may be a double-edged sword cutting swaths in the economic system in opposite directions. The reliance upon positive action taken by the central government to preserve competition and to
prevent the encroachment of economic power aggregates may result in a reduction in competition. Or, in terms of quantitative monetary policy, instead of the weapons of monetary policy providing the stability in the economy that they are designed to afford, monetary policy may become a disequilibrating force. In the case of competitive-micro-policy, or antitrust policy, the chance that state action may not be able to achieve its goals has been mentioned by various writers from different points of view concerning policy. Simons, for example, claims that "We may recognize, in the almost unlimited grants of power to corporate bodies, one of the greatest sins of government against the free enterprise system." 28 According to Burns,

Although social policy is assumed to have been aimed in general at the maintenance of competition, it has in many ways reinforced the movement away from competition.29 Similarly, Edwards criticises antitrust policy for the "undue breath of exemptions enjoyed by various groups."30

The more complex the system becomes, the more likely it is that policy paradoxes will develop. For monopoloid markets, this can arise in different ways. Corporation laws may facilitate the growth of giant firms to a size contradicting the objective of maintaining a relatively structurally-competitive economic system. Patent laws, designed to encourage invention and to stimulate economic growth and

29 The Decline of Competition, op. cit., p. 9.
30 Maintaining Competition, op. cit., p. 50.
technological change, may be used to eliminate competitors or to bar
entry to an industry. Inheritance laws and tax policy may be
ineffective in checking growing income inequality, particularly if
legislation is created to afford tax dodges by way of foundations.
Exemptions from antitrust legislation, including foreign trade,
agricultural cooperatives, transportation firms, labor unions,
fisheries, insurance companies, banks, etc., may greatly weaken the
strength of the law. Moreover, the government may actively promote
economic concentration in contradiction to the objective of greater
competition through its procurement policies, defense contracts,
disposal of surplus facilities, and special legislation as in the case
of atomic energy. Even the existence of regulatory agencies may
dramatically alter competitive or antitrust policy. Attempts to
protect the public from inferior quality of products and persons, an
assumed desirable objective of policy, may result in barriers to
entry into industry and a corresponding weakening in the competitive
policy. Finally, the devastation of economic instability via

31 For an analysis of existing exemptions under antitrust legis­
lation, see: House of Representatives, Select Committee on Small
Business, Eighty-Fourth Congress, Congress and the Monopoly Problem:
Fifty-Six Years of Antitrust Development, 1900-1956, Washington:
Burns, The Decline of Competition, op. cit., pp. 9-25; Edwards,
Maintaining Competition, op. cit., pp. 50-90.

32 Vigorous exponents of the thesis that government actively
promotes monopoly are Walter Adams and Horace M. Gray, Monopoly in
America: The Government As a Promoter, New York: The MacMillan


34 Franchises, licensing regulations, inspection laws, building
depression and inflation may result in altering or contradicting antitrust policy. Government policy designed to prevent depression and inflation, to stimulate economic recovery from depressed conditions within the economy, may offset directly or indirectly antitrust-competitive policy.

The following case illustrates problems of the above type. The United States made an attempt to stimulate economic recovery during the Nineteen-Thirties by means of the National Industrial Recovery Act that promoted industrial self-government. But, this required a relaxation in the antitrust legislation to enable firms collectively to determine prices and output, wages and working conditions. According to a spokesman for industry, the country in the midst of depression needed a better balance between production and consumption. Due to existing legislation, however, industry would need changes in antitrust legislation to bring about the, in their eyes, necessary codes, etc., may develop into blockades of entry into industry and hence work contrary to policy. For an extended discussion, see the above, pp. 185-248.


36 Henry I. Harriman, "The Stabilization of Business and Employment", The American Economic Review, Volume XXII, Number 1, Supplement, March, 1932, p. 67. Of course, the critics of industrial self-government often wonder about the share that the public would receive during the course of such an "equitable" distribution. Producers in general would prefer to gauge their output to the consuming capacity of the country and divide the volume of such production among the different units of industry on an equitable basis, rather than to continue the present harsh and unremunerative competitive system, but this they cannot attempt today because of the
It is not suggested that the present antitrust laws be repealed, but it is suggested that they be amended to provide that (a) business concerns desiring to enter into contracts for the purpose of equalizing production to consumption and so carrying on business on a sound basis, may make such contracts, but must file them with some governmental authority having supervision over the same, such contracts to continue in force until such governing body finds on complaint or on its own initiative that such contracts are not in the public interest, in which event such agreement shall be abrogated, and (b) business concerns that desire to combine may find out from some governmental authority before the combination is made whether or not such combination is prohibited by our antitrust laws. 

However, this interest of industry in modifying antitrust laws to accomplish economic recovery was scored by the opinion of some economists, a group of whom argued in a petition that what was needed was not the relaxing of antitrust laws, but more vigorous enforcement of the laws and increased regulation and supervision of corporate abuses. Thus, economic instability carries important implications for antitrust policy. The commitment to the objective of maintaining ever-present risk of incurring penalties under antitrust laws which, suitable as they may have been for economic conditions of an earlier day, are not in consonance with the present-day needs of industry.  

Ibid., pp. 67-68. While Harriman believes that industry would benefit from the self-governing arrangements and the relaxation of antitrust laws, he recommends that stabilization policies be different for farmers, e.g., "free" markets and bankruptcy: "the sooner the right to attempt the stabilization of prices by government purchasing of crops is abrogated, the better it will be for the farmer and for the country"; and, "as farms are foreclosed for non-payment of taxes and come into possession of federal, state, or municipal governments, they should be converted into state forests." Ibid., pp. 70-71.  

a fully-employed economy, and the design of aggregative economic policy in the form of monetary and fiscal policy, may strengthen or weaken public policy at the firm and industry level. In summary, increased governmental participation in the affairs of the economy develops problems of policy paradoxes, inconsistencies in goals and techniques of state action.

Political Environment and Personal Elements

The political environment, or the democratic process generating policy, and the personal element in public action are critical problem areas for economic policy. The economist may gain valuable insights into the formulation of policy with theoretical approaches, but abstract goals and idealized action may be upset by forces of political action and the human element in the economy. Conceptually, the actual problems of attaining acceptance of policy through legislative machinery, agreement with public opinion, judicial approval, and proper implementation through agencies and commissions, may be short-circuited by postulating these factors as aberrations, disturbances, or ceteris paribus elements. However, in the practice of policy such factors cannot be dismissed. Conflicting interests and

39 Professor Galbraith suggests that monetary-fiscal policy can be discriminatory, aiding oligopolistic industry and restricting competitive industry to such an extent that (to turn a phrase) "it is possible to imagine that an active and continuing monetary policy is not less effective than, say, the repeal of the antitrust laws". John Kenneth Galbraith, "Market Structure and Stabilization Policy", The Review of Economics and Statistics, Volume XXXIX, Number 2, May, 1957, pp. 124-133, p. 133.

40 Henry S. Commager, Majority Rule and Minority Rights. New York: Peter Smith, 1950, pp. 58-60. Speaking of democracy, Commager writes
differences of opinion are data in a society. Convincing scientific findings may be blocked by the processes of democracy.

Neither political scientists nor economists are highly esteemed as advisers by the public. In economics in particular, this is no doubt partly because what attracts general attention is not the scientific work, theoretical or empirical, but moral and political speculation and preaching. Where economists are agreed, as illustrated by protectionism—and I will add a national legislative minimum wage—the situation is summed up in the adage that free traders win the debates but protectionists win the elections.41

Clearly, public policy within such a system continually has to contend with the diversity of the political structure and the human element, and this is particularly the case with respect to the role of the judiciary in policy. Given judicial review of legislative acts and prosecutions under antitrust policy, courts occupy an important role in the development of policy. Policy does not unfold in a simple, unambiguous fashion, but is influenced by the personalities of judges, their economic philosophies, and corresponding human factors. Handler, for example, writes that

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that: "A good part of our politics, indeed, seems to be concerned with reconciling majority and minority will, class hostilities, sectional differences, the divergent interests of producer and consumer....In small issues as in great, the result is generally a compromise. Democracy, in short, ...furnishes its own checks and balances....But there is this to be said of checks and balances of democratic politics—they are natural, not artificial; that they are flexible rather than rigid; that they can yield to public opinion and to necessity. They do, sometimes, enabling the majority to ride down the minority; they, do far more frequently, enable the minority to delay and defeat the majority. But the responsibility in all this is with the people themselves—where it belongs. Where they indulge their apathy, their carelessness, their blindness, they pay the price, and it is right that they should pay the price. As the fault is theirs, so, too, the remedy."

Those who naively expect a complex jurisprudence to spring full-panoplied from the brain of a lawgiver or who would have the law proceed in a straight line, like a guided missile, from initial premise to ultimate solution, may find this tortuous development uninspired and unrewarding. It takes time in a democracy for opinions to mature and win their place in the market place of ideas. Vital to the process of maturation is the sharp division of disinterested minds. Good doctrine can be forged only in the fires of honest debate. In no branch of the law has dissent played a more significant role than in antitrust. All the jurists whose work we have examined have rendered an incalculable service to the cause of justice, even though, as is inevitable in our system of common law adjudication, many of their thoughts have failed to survive.  

Judges may, in the ideal sense, or as they rationalize their personal decisions, attempt to afford free and equal treatment under the laws, to exercise wise judicial restraint, to provide justice. Yet, economic views may be substituted for judicial doctrine, that time and time again it has been evident that the development of constitutional law has depended as much, if not more, upon the personalities of the justices who have made it as it has upon the force of fixed legal principles or precedents, or upon any compelling word of the Constitution itself.  

The importance of the human element in policy appears in distinctive fashion with respect to the performance of regulatory agencies established to administer legislation, to regulate industry, or to enforce competition. Political appointments, lack of funds for

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42 Antitrust in Perspective, op. cit., p. 28.
the acquisition of trained personnel, absence of status and prestige, expansion of bureaucratic organizations, conflicting realms of authority, function collectively as forces and pressures affecting the attempts to implement policy in pure form. Even a clear mandate from Congress may be difficult to implement due to institutionalized inertias and restraints. In short, policy at the action level rarely if ever occurs in pure form.

In the previous discussion an attempt has been made to develop some of the major problems arising in the formulation of public policy. Of course, in addition to the above problems, there are many issues of specific policy that have had to be ignored, including: the treatment of mergers, corporation laws, consent decrees, resale price maintenance agreements, international industrial relations, trade association activity, discriminatory pricing, geographical pricing formulae, unfair business practices, etc. For the students of market policy, such problems are the substance of policy. Yet, while the analysis of different recommendations and the development of new policy are essential in the study of market policy, it is also necessary that policy be thought of in broad terms with a perspective wider than single policy proposals and their possible merits or inadequacies.

Summary and Conclusions

Methodology may be farther removed from the area of economic policy than the methodologist would care to admit. To depart from examination of the procedures, concepts, and operations of a science,
the familiar territory of methodological examination, may be a drastic step. However, if it is assumed that policy is built upon a foundation of empirical and theoretical analysis, matters of social action become the concern of the methodologist, although the connection may only be an indirect one. Furthermore, there are important issues of public policy deserving examination from a point of view not directly concerned with a special theory, empirical study, or belief.

The point of view or perspective of the methodologist in dealing with economic policy should be a broad one. It should recognize that contradiction is a fundamental characteristic of policy. This contradiction may make an appearance through contrasting philosophies of social action, conflicting ideas, and differing weltanschauung.

The broad perspective of policy, accepting the condition of contradiction, includes a division between general and specific problems of policy. This division should facilitate the separate identification of basic philosophical ideas and assist in the determination of the consistency of policy.

Economists have advanced different types of general policy approaches. For instance, modern welfare economists conceive of policy problems in formal terms. Taxonomists in policy view the construction of policy as a matter of extensive model-building and the evaluation of the consequences of the multiple models. Decision models suggest the development of quantitative economic policy in terms of data, target variables, instruments, with the use of empirical functions expressed in the form of structural equations.
Others, with the work of John R. Commons providing a prototype, would steer away from the formalities of models and emphasize the social process as the proper framework or approach to policy.

The role of state action introduces conflicting concepts and presents problems of reconciling conflict. The range of concepts extends from rigid laissez faire to complete state control, with many remaining between in a middle range for which the issues would involve "degrees" of governmental action.

Size of policy is a debatable issue, with questions arising with reference to wholesale or incremental action. Certainty in policy, another source of difficulty with policy, similarly yields contradictory ideas. Some would favor the development of stable, certain rules of a system. Others would claim that the deliberate injection of uncertainty is a necessity at the policy level.

Issues of this type serve to reinforce the basic theme that policy is inherently an area of built-in contradictory elements. The methodologist is engaged in a fruitless task if he expects to unify the diverse and conflicting elements easily. Perhaps the only way this might be done is to adopt a position that eliminates the differences. But, the evidence of paradoxes and dilemmas of policy will be difficult to overlook. Positive action by the state in one direction may nullify efforts in other directions. And, the growing complexity of a system seems to lead to an increase in the number of paradoxical results. Moreover, when the political environment and personal aspects are examined, many situations are uncovered in which the process of policy making is altered sharply by the political
Thus, the existence of a political-social structure in society militates against well-ordered policy. The review of policy in this light is one that may be disquieting for the view that the only obstacle to policy is the absence of a scientific foundation. Yet, perhaps greater emphasis on the contradictions of policy, and admission of their existence, will replace the feeling that all policy is emotional or value dominated and result in a different perspective towards the issues.
Chapter VIII

Summary and Conclusions

Conventional procedures for correcting or improving economic analysis are not always notably successful. Theoretical refinements, improvements in logical rigor, extensive model-building, additional factual discoveries, new empirical techniques, and the use of interdisciplinary teams for the derivation of policy guides may be unsuccessful activities for the economist if the issues of analysis are imbedded deeply in the corpus of economic science. In this situation, a methodological orientation and emphasis may be essential.

However, it is a seriously debated question to consider if methodological perspective can accomplish what more conventional treatments have not accomplished. On one hand, critics of methodology argue that such activity is sterile and useless, that the real issues of a science are not susceptible to methodological examination and evaluation. Defenders of methodology, to the contrary, claim that it is essential for a scientist to investigate and to understand the fundamental concepts and ideas underlying a discipline. Whatever impact the conflicting points of view might have on methodological work should be recognized at an initial stage in order to neutralize in part that influence.

The scientist who intends to probe fundamentals of concepts, techniques, and procedures in his discipline as a methodologist must be conceptually prepared to deal with an inherently complex subject.
matter. Methodology represents not a series of fixed rules, standards, and principles that need only to be applied to a science, but a complex area of conflicting ideas, beliefs, and criteria. Consideration of several dimensions of the subject is valuable in indicating the conceptual variety involved. Variations occur in the definition and meaning of methodology, the functions of methodology, approach or framework, standards and tests, and in attitudes and preconceptions.

Methodology has been used to refer to a separate science, a logic, a study, and a series of descriptions. It is thought of as a rational-analytical discipline and as an empirical-descriptive study. Methodological functions possibly performable for science include: settlement of methodological controversies, analysis of the foundations of prediction, specification of implicit theoretical laws in a science and the development of their interrelations, coordination of different kinds of activity through the attainment of methodological insight, activity as a methodological critic, and the promotion of mutual understanding among scientists.

Differences in frameworks or approaches used in methodological study suggest added complexity. The existence of historical, descriptive, comparative, and analytical frameworks tends to explain the origin of contrasting insights, inferences, and evaluations. Significant differences also arise in connection with standards, tests, rules, or criteria, and it seems clear that conflicting attitudes and preconceptions share in the development of this complicated subject matter.
Thus, the scientist must concern himself with the conceptual outlook adapted for methodology. In terms of the following, the perspective would admit the existence of three conditions of the subject: direct contradictions, direct agreements, and exclusions of methodological ideas, principles, and positions. On the basis of the concept of the limitless number of combinations of independent methodological ideas, the thesis is advanced that the scientist must perform his methodological examination and evaluation in terms of his discipline. However, this would require understanding of the inherent complexity of the subject, the existence of multiple methodological matrices, and the status of methodology in external fields, as primary resources for dealing with analysis within a given discipline.

Methodological ideas external to economics provide important insights into the viewpoints and philosophies of economists. Physical and natural scientists differ in scientific outlooks or scientific weltanschauung. One group claims that science must have precision of form, contents, and results, that science must be precise and free from ambiguity. Others accept the philosophy of empiricism, the minimization of purely logical form and a priori principles, and with a reduced emphasis on the supremacy of one "general" theory. There are also significant conflicts concerning the place of metaphysics in science, with positions ranging from the charge that metaphysical issues are out of place in physical science to the argument that they are the profoundly disturbing elements in science.

Moreover, examination of the standards, rules, and criteria applicable in the evaluation of scientific hypotheses shows the
presence of variety. Conventional treatments stress language clarity and the reliance upon facts to settle inconsistencies, with theoretical assumptions required to conform to those of established theories. Conversely, some physical scientists would shift attention from conventional tests and criteria to "operationalism", in which the meaning of scientific concepts is found in the operations used and where personal checking is essential. The status of general theory versus dualism in theory introduces an issue for the physical scientists and implies problems for the structuring of economic analysis in the form of a single theory or multiple theories. In addition, questions advanced by physical scientists concerning the validity of mechanical models, the differentiation between continuant and occurrent conditions, and the method of evaluating meaningless questions imply corresponding issues for economics. Finally, the attempt to compare the physical and social sciences introduces a battery of methodological issues for which insight into implicit criteria of evaluation is essential. And, some of this insight evolves in the work, not of the physical scientists, but of other social scientists.

Within economics the status of methodological ideas is subject to conflict. Discussions of methodological issues of economics are conducted in terms of different frameworks and from different points of view. Consequently, methodological conclusions and evaluative comments vary widely. Some economists would argue for a compromise of conflicting points of view against others who stress in most singular fashion the development of a logical position of analysis or a
completely empirical organization of economic propositions. Thus, issues involving the limits of economics, the place of value judgments and ethics, language clarification, the necessity for a general theory, the role of theory and experimentation, and the importance of assumptions and facts are strongly debated and contradictory conclusions obtained.

Methodological issues form a basis for policy disagreements. Concentration upon questions concerning the condition of economic theory and supporting facts influence judgment at the policy level. Moreover, policy becomes involved in other problems external to the domain of strict analytical construction, e.g., interference by special interest groups, inflexibility of an economic system, and conflicting normative positions. Intricate questions of methodological interest are also contained in the test of prediction as applied to economic hypotheses. Thus, for economics, methodological evaluation has to carry the marks of an uncertain, tentative, and controversial basis. Yet, the problems that can be uncovered with methodological study, although not solved in the usual sense of the word, may result in some valuable changes in the construction of economic analysis and bring about significant modifications in some of the ideas of economists.

Monopolied market theory, as it is examined methodologically, includes four problem areas: the scope or magnitude of theory, theoretical concepts or models, theoretical topics and problems, and theoretical hypotheses or conclusions. The scope or magnitude of theory offers difficulties for methodological evaluation, since a
basic characteristic of scope is the existence of varied forms. Theoretical scope may be thought of in two broad dimensions, lateral and time extensions. It is broadened when additional variables are introduced and narrowed when only a few variables are cultivated intensively. The combination of this lateral dimension of scope with a time dimension results in the rapid accumulation, actual and potential, of numerous and complex theoretical conclusions and this in turn alters the nature of empirical investigations.

Similarly, the presence of various model-types or working concepts for theoretical analysis influences the course of methodological evaluation. Equilibrium models, along with power and bargaining models, organic models, and open-ended models as they are utilized in monopoloid market theory involve both different structurings of variables and differing methodological criteria.

The actual problems of monopoloid markets are numerous and vary widely in their relevance to particular theoretical constructions. To organize and classify them introduces methodological issues involving the possibility of classification and the application of different kinds of taxonomies.

Thus, the examination of monopoloid market theory suggests that ideas concerning the finality of theoretical doctrine are openly questionable. Methodologically, theory might be thought of as a "mathematical set" of hypotheses, topics, conclusions, and scopes of various forms and structures, with variations in each of the many "subsets." On the basis of this concept, the partial success of methodological evaluation depends on two factors or abilities: the
identification of the outlines and limiting characteristics of subsets and their corresponding implications, and the realization of the inherent variety in methodology itself when it is applied in evaluation.

Although the thesis is commonly advanced that science must make an appeal to facts, the process of developing facts in empirical research of monopoloid markets is a complex one. Ill-defined doubts or "empirical suspicions" influence judgment concerning the reliability of findings and factual evidence. The lack of faith in techniques, bias or partiality, impossibility of reconstructing studies independently, complicated interrelationships of sets of facts, empirical results conflicting with theory, and policy implications of empirical findings are sources of doubts that generate methodological controversy. In addition, examination of conflicting views concerning the nature of facts indicates how differences at this level shape methodological evaluation. A dualistic conception of a fact affords some needed methodological tolerance.

Empirical work contains conflicting theses dealing with many aspects of market structure, performance, and economic-social consequences. This condition closely parallels the contradictory theses of theory, but the mechanism by which theory deals with inner contradictions differs in empirical studies. The methodological evaluation of factual research shows the variety in criteria and implicit judgments offered by economists. It is doubtful, therefore, if an economist can formulate a judgment either way with reference to issues of economic concentration, merger movements, turnover of giant
firms, misallocation of resources, rigid prices, costs, and other problems, when the factual base is overtly discredited. And, it would seem difficult to accept the attitude or philosophy of empirical researchers that facts easily clinch a case or solve a problem when the specific empirical procedures are methodologically tenuous. The inclusion of institutional studies as a necessity of empirical research further broadens the concept of the validity of factual studies and introduces added methodological considerations.

Although the concern of the methodologist with policy issues in monopolistic market analysis may not be entirely a direct one, there are conditions in policy formulation requiring a broad perspective that methodology furnishes. Contradiction in policy seems to be an automatic concomitant feature of policy, suggesting that the attempts to develop policy consistency must either be rejected or modified significantly. Such contradictions make an appearance in a variety of ways ranging from conflicting philosophies of social action and differing weltanschauung to variations in models, techniques, and ideas of policy construction.

Methodological perspective towards policy is implemented by a separation of general and specific policy problems in order to identify the general from the specific and to confine attention, when necessary, to the implications of general ideas and philosophies. At the general level are found variations among policy approaches. Formalists of policy conceptually organize the data of policy in theoretical form. Taxonomists conceive the construction of policy as a matter of extensive model-building and the determination of possible
outcomes for the multiple models. Decision models follow similar theoretical lines. The significant deviations from the formal, theoretical approaches stress the inner core of conflicts in the social process as the proper focal point of policy.

The role of state action provides another general point of departure among policy students, with conceptual ranges extending from rigid laissez faire to complete state control. Size of policy action, incremental or wholesale, and certainty versus uncertainty are the other places in which general policy issues prevail. Paradoxes and dilemmas of public action are a constant feature of general policy investigation. Positive action by the state in one direction may quickly nullify efforts in other directions, and, the increased complexity of a system increases the likelihood of added paradoxes. The presence of a political environment and the force of personal elements represent added pressures on the development of consistent policy. Thus, economists are faced with a condition in which the probability of exactness in policy decisions must remain low.

In summary, an attempt has been made in this study to approach problems of economic analysis from a position or point of view that deviates from the commonly-accepted or more conventional approaches. Attention was directed at a special set of problems, those arising in monopoloid market analysis, in order to maintain a strand of continuity and to focus emphasis on conditions of the economy of major importance to economists and to the public. Although the problems have not been solved in the customary sense of the word, this is not necessarily a defect in the idea of methodological analysis. In more
competent hands, the methodological perspective developed in this study may provide closer approximation to the actual solutions.
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I, Charles Everett Helppie, was born in Wadena, Minnesota, August 4, 1930. I received my elementary and secondary education in the public schools of Detroit, Michigan, and my undergraduate training at Eastern Michigan College, Ypsilanti, Michigan, which granted me the Bachelor of Science degree in Business Administration in June, 1951. I obtained a year of graduate work in economics at Wayne State University during the year 1951-52. While in residence, I was an Assistant to Professor King Adamson and conducted laboratory sections in economic statistics. In September, 1952, I was appointed an Assistant in the Department of Economics at Ohio State University. In 1953-54 and 1954-55, I was an Assistant Instructor; in 1955-56 and 1956-57, an Instructor in the Department of Economics at Ohio State University. In September, 1957, I was appointed a Lecturer in Business Administration at Bowling Green State University, the position held at the time of the completion of the requirements for the degree Doctor of Philosophy.