THE STRUCTURE OF ECONOMIC THEORY AND THE GOALS OF SCIENTIFIC ANALYSIS

By

ROBERT GEORGE FABIAN

A DISSERTATION PRESENTED TO THE GRADUATE COUNCIL
THE UNIVERSITY OF FLORIDA
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE
DEGREE OF DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

August, 1966

TO MY PARENTS

ACKNOWLEDGEMENTS

The writer would like to express his gratitude to the members of his supervisory committee for the assistance they have given during the preparation of this dissertation. To Professors Clement H. Donovan, Frederick H. Hartmann, and especially to John N. Webb, who supervised the task with much patience and helpful criticism, my sincerest thanks.

TABLE OF CONTENTS

CHAPTER I

INTRODUCTION

Scope of the Dissertation

In what follows, an attempt is made to merge the discussion of methodological issues in economics with a wider framework of scientific discussion. While the paper ranges quite far beyond economic theory proper, nevertheless it stays within a restricted domain: the current status of the deductive pattern of theory in the social sciences. It belongs to economics for two reasons. In the first place, the specific focus of the paper is economic theory. Secondly, it embodies an outlook or set of preconceptions more likely to be found among certain students of economics than among any other group. The paper is, then, one student's apologia for deductive theory in economics, and his reaction to a body of related scientific endeavor not restricted to economics.

The present paper attempts to explore relevant material in the philosophy of science, and establish what are hoped to be enlightening juxtapositions of material drawn from rather diverse sources. While treatment of the main issues raised is hardly expected to be definitive, it is hoped that the exploration will clarify the most significant problems and issues.

Judging from the current literature on the subject, the most significant issues concerning deductive theory in economics may be summarized as follows:

1) Economic theorists have failed to completely abandon their claims to provide deductive proof for norms or value judgments. (Myrdal)

2) While purporting to offer systematic knowledge of the observable world, economics has often compromised its conclusions in deference to ruling ideologies or privileged classes. (Robinson)

3) Many of the concepts and statements found in economic theory assert nothing observable about reality. These empirically vacuous propositions have frequently been the cause of circular reasoning and ideological outlooks often characteristic of economic theory. (Robinson)

4) Too much economic analysis has been concerned with the search for "deeper explanations," the "reality behind observable occurrences," and other metaphysical quests which have dissipated much creative talent in years past. (Samuelson)

5) Much of economic theory confuses judicious abstraction, central to all theory, with empirical falsity. It justifies false assumptions on the basis of correct predictions. (Samuelson contra Friedman)

6) Many economic theorists mistakenly believe that a "theory" is somehow wider than its "conclusions," when in fact they must imply each other mutually in an empirically valid theory. Both "theory" and "conclusions" must be fully empirical. (Samuelson contra Machlup)

These criticisms, almost all of which are found in the most recent literature, show a common source of dissatisfaction. They indicate that modern theoretical economists are displeased with much of economic theory because of its apparent shortcomings as representative of modern empirical science. This dissatisfaction registers in two important ways: regarding the methods of theory construction, and regarding the scope of theoretical investigation. The two sources of dissatisfaction are closely interrelated. Theoretical method is criticized for admitting non-empirical terms and statements. According to criticism six, only empirical entities have any rightful place in theory. It is true that adherence to the imperative of criticism six would eliminate most of the criticisms related to the scope of theory. An economics that admitted only

terms and statements that have an observational base of reference would not be sidetracked into metaphysical speculations, ideological debates, or searches for unverifiable explanation. A fully empirical economics would perforce be restricted to the tasks of short run prediction and description. An important question remains, however. We will spend much time trying to determine if the "fully empirical economics" is required to achieve the ends described.

Indeed, a fully empirical economics in the sense described by the critics in question would be a radically reconstructed economics. Modern economists have, accordingly, developed new and satisfactory branches of analysis. But we are concerned in this paper with the deductive pattern of analysis in economics, a branch of economics with a long and still influential tradition. Its structure does not measure up completely to the strict canons of modern empiricists. Some economists continue to employ certain concepts that are not fully quantifiable, if they can be quantified at all. As a result, the closely interrelated problem of scope is still present, since criticisms one through four result in part from the presence of non-empirical theoretical segments. We will discuss examples of modern analysis in the deductive tradition, which has important intellectual and social consequences quite apart from its empirically verifiable content. (Refer again to criticism six.)

Many economic theorists seem vulnerable to some of our enumerated criticisms, even though they are among the most highly respected professionals in the field. Some of them even violate their own criticisms, if Machlup's rebuttal of Samuelson is valid. Economics is what economists do, as one economist has pointed out. He was stating aphoristically a partial truth of scientific method applicable in many disciplines: that valid methods of theorizing are learned by observing the performance of the best workers. So perhaps it would be best to shun methodological disputes and simply go about our business. And yet the existence of a gradually growing body of professional literature about these matters gives one pause. Every identifiable scientific problem should receive rigorous scrutiny, and problems of methodology are no exception. Careful attention to formal problems of method will insure that serious work will not be impeded by hastily applied principles of scientific method, erroneously believed to discredit the work of the past.

The question of what remains of value in the legacy of traditional deductive theory is regarded here as an important one. It will be examined primarily from two points of view. We will first take up the criticisms which refer primarily to the method of deductive theorizing. In so doing, we will try to evaluate critically the important methodological statements of recent years.

During this stage of our investigation, we will draw rather heavily on writers outside the social sciences. The physical sciences have the best record of empirical discovery, so an examination of their methods seems appropriate. Interestingly enough, many of their problems are similar to those of economics. For example, (the most important example of the paper), there has been considerable controversy in the hard sciences over the role of non-empirical terms and statement-forms in theory. Spurious goals, non-empirical in nature, have been identified by some critics; hyper-factualism and lack of proper cultural perspective have been identified by others. And, bearing out our contention about the importance of methodology, these differences have had some important effects on actual scientific endeavor.

Our second point of view will focus on the historical antecedents of the issues and criticisms we have identified. How and why did non-empirical terms and statement-forms enter economic theory in the first place? Are they present in the hard sciences? The answers to these questions are closely associated with the intellectual milieu of the pertinent period. We identify this early period of science as the era preceding the burgeoning of empirical techniques of analysis, and the breakdown of what we will identify as the rationalist era of scientific thought. The

rationalist era is identified in this paper with scientific goals of a somewhat more ambitious nature (from a philosophic point of view), and also of a less quantifiable nature. We have cited these goals in criticism four. The capacity of scientific theory to pursue these goals, we find, stemmed from the extra-empirical dimension of theory cited in criticism three. Method and scope, it must be remembered, are closely interrelated.

While it might seem more natural to some people to pursue the problem by presenting the historical problem first, it is the judgment of the writer that the current controversies should be brought out in the open first--and most of the current literature deals specifically with method. Also, the question of scope of theory is more complicated, since it turns on the narrower problem of method. Specifically, the wider goals of economic theory are pretty much ruled out under the stringent empirical canons formulated under criticism six. If, on the other hand, we opt for a less rigid empirical methodology, then the problem of scope reappears. We do in fact defend a less stringent empirical position, and we hold that some of the broader social and cultural dimensions of theory are valid concerns for the current deductive analyst. Nevertheless, our criteria for valid theorizing at this level are strongly influenced by the newer empirical requirements imposed on theory. We will discuss at considerable length why we

believe that certain traditional goals of economic theory are still justified within the bounds established by modern empirical methods.

Chapter Summary

Chapter two states a basic methodological problem of economic theory, shared with other theoretical disciplines: the degree of success writers of deductive economics have enjoyed in their effort to produce work which fully measures up to the requirements of empirical science. What does it mean to be an empirical science? Can we not appeal to the "scientific method" for an authoritative criterion? It is contended in this chapter that there is no unambiguous standard against which all scientific theorizing can be evaluated. Popper is quoted to the effect that propositions not falsifiable by empirical evidence must be excluded as scientific propositions. Yet we find Nagel saying that in the deductive pattern of explanation there are found extra-empirical statements which are an essential feature of the theory. Nagel's views on deductive theorizing, one of the most important aspects of the present paper, are summarized in this chapter. We shall maintain throughout the paper, against considerable objections, that extra-empirical terms and statement forms have a valid and vital role to play in modern deductive analysis.

In this chapter the presence of such concepts in economics is pointed out, and the valid criticisms against their improper use is acknowledged. But it is also shown how their presence finds justification in the methodology of deductive theory.

Having indicated our methodological position and its significance, we turn in chapter three to the closely related question of the application of that methodology to economic theory. In the preceding chapter we found general methodological support for extra-empirical segments in theory, but the abuses noted there made us pause concerning their rightful place in economics. These abuses were also noted by P. A. Samuelson, whose ideas on empirical content in theory we take up in this chapter.

The Samuelson-Machlup controversy has been the focal point of the most recent examination of the literature of economics concerning its status as an empirical science. Samuelson argues what we shall call the "strong empirical position," reminiscent of Popper: every term or statement form in theory has empirical significance. The "conclusions" of a theory are empirical, or else insignificant and should be eliminated. The "assumptions" and "conclusions" of a theory imply each other mutually, so that the "theory" and its "conclusions," to the extent that they have scientific status, are empirical.

Machlup argues that a theory is wider than its conclusions, and that there are extra-empirical segments in theory. Reminiscent of Nagel's position that the complicated logical structure of a deductive theory gives it greater explanatory scope than a purely empirical theory, Machlup defends the presence of extra-empirical concepts.

In this chapter we take the position that Machlup is more nearly correct, and try to show that Samuelson's line of reasoning is unduly restrictive.

Chapter four presents a detailed discussion of valid uses of extra-empirical theoretical segments in economics. It is argued that deductive economics has a strong and valid commitment to their use, and that the use conforms to sound methodological principles as expounded by Nagel and corroborated by Machlup. The examples and discussion of this chapter show that criticisms of economics as metaphysics and ideology do not derive their validity from the use of extra-empirical theoretical constructs as such, but only from their invalid application. Hence it follows that correct handling of deductive economics is required, rather than dismantling it via elimination of purely theoretical concepts.

Chapter five reflects the belief that it is important to understand the scientific experience that underlies the methodological differences that have been discussed up to this point. In this chapter it is advanced that the controversies discussed in earlier

chapters result from a carry-over of methods and goals prevalent in scientific thinking prior to the empiricist revolution in science occurring in the second half of the nineteenth century. The chapter describes the changes in thinking, and in the requirements of empirically-valid scientific analysis, which resulted from the empiricist revolution. It also describes, taking the thought of Erwin Schrödinger as a contribution of critical importance, the reasons for believing that traditional methods and goals have greater current vitality than is normally attributed to them.

This chapter is focused on the general background of present-day scientific analysis. Prior to the empiricist revolution, physical science was served by Newtonian mechanics as a paradigm of method. As for the goals of science, these we have described as the goals of rationalism--a coherent theoretical explanation which reflects the grand unity and underlying rational plan behind nature. The comprehensive simplicity of Newtonian mechanics served also as paradigm of goal; every area of science sought reduction to mechanical explanation. Thus were goals and method closely interrelated in the scientific era preceding the empirical revolution.

In chapter six it is contended that the historical experience of relevance to deductive economics is very similar to general scientific experience related in the previous chapter. In economics,

as in physical science, thoughtful people sought to understand the simple rational laws that guided society--laws that were obscured by the welter of confusion and complexity that pervaded the surface of things. Understanding the essence of things was the rationalist goal in economics, and it was Adam Smith who succeeded in filling the need. Holding together the rich tapestry of his great work is a theoretical strand rivaling the comprehensive simplicity of the Newtonian model. Like Newton, Smith achieved his goal by means of a relatively simple axiomatic system possessing deductive powers of great scope.

The chapter is an effort to justify Smith's rationalist theoretical goals by showing how application of Schrödinger's principles, met in the previous chapter, makes possible and desirable their carry-over into the modern, empiricism world in which economists now work. Later on in the chapter the same line of reasoning is applied to modern economists, principally J. M. Keynes. The concluding portion of the chapter is devoted to a significant article by George Stigler. In his article, Stigler shows how the deductive tradition in economic theory has strong continuity with a "main stream" of economists beginning with Smith and the Physiocrats. Over time, according to Stigler, the concepts of succeeding generations of economists have been transformed and assimilated into the main body of economic thought.

Stigler's emphasis on the importance of historical continuity in economic theory substantiates our methodological judgment about the validity of traditional concepts appropriately handled.

In chapter seven an attempt is made to extend the application of the methodological principles developed and applied in earlier chapters beyond economics into the fields of anthropology and psychology. Comparisons are made with the work of economists, demonstrating that their experience, as well as that of physical scientists, has a contribution to make to an understanding of the nature of scientific theorizing. The examples chosen are offered as clarification and substantiation of the principles defended in the present paper. The material is presented at this point because it applies and illustrates both the methodological principles developed in the first portion of the paper and also the lessons drawn from the historical experience of science related in succeeding chapters.

Chapter eight capsulizes the entire paper, drawing together the most significant reflections and conclusions found there.

CHAPTER II

AN EMPIRICAL PRINCIPLE FOR DEDUCTIVE THEORY IN ECONOMICS

It has been observed that in the physical sciences the realm of concepts and the realm of facts and objects are widely separated. As physical theory becomes more powerful and advanced, the gap grows wider and wider. Scientific theories embody both empirical and purely theoretical statements at the same time. Theories depend for their validity and usefulness on the coordinated functioning of both types of propositions. Since the theoretical terms and statements perform solely a logical function, it cannot be asserted that their lack of content renders them empirically false. While this contention is often rejected, at least implicitly by many modern critics of economic theory, it will be contended in what follows that the distinction is valid and important to economic theory, both from the standpoint of its method and its goals.

At the popular level the important distinction is between theory and fact. Curiously enough, theory is always made the handmaiden of fact in such discussions. One will always be challenged as to whether he is stating facts or merely giving his

theory. Theory is merely opinion or guesswork unless it is constructed piece by piece out of facts.

At a more sophisticated level we are shown that theories in economics involve extra-factual material in an essential way. Economic theories contain terms which upon close examination can be shown to lack any connection with factual or empirical reality. Joan Robinson cites Popper's position to the effect that statements incapable of being falsified by evidence are not scientific propositions.[1] Such statements purport to say something about real life, yet we cannot say in what respect the world would be different if they were not true. "The world would be just the same except we would be making different noises about it." What is the logical status of such a position? ". . . it will roll out of every argument on its own circularity; it claims to be true by definition of its own terms."[2]

These non-empirical concepts are asserted to have "no scientific content," yet are held to "express a point of view" which give direction to scientific investigation.[3] Since these concepts lack content in and of themselves, but rather give direction to investigation, they are assessed according to the implications they lead to, for example in social policy.[4]

---

[1]Joan Robinson, Economic Philosophy (Chicago: Aldine Publishing Company, 1962), p. 3.

[2]Ibid. [3]Ibid., p. 51. [4]Ibid., pp. 72, 75.

Lacking content, their meanings can be altered to adjust for unpleasant implications.[5] The outcome is that "economics . . . has always been partly a vehicle for the ruling ideology of each period as well as partly a method of scientific investigation."[6]

But do the abstract concepts of economics ever relate to the realm of empirical fact, as in the physical sciences? Only quite indirectly, according to Robinson and like-minded critics. The reason is that in the tradition of deductive theorists or system-builders in economics, abstract concepts are related to norms, not empirical fact.

> Even when the claim is not explicitly expressed, the conclusions unmistakenly imply the notion that economic analysis is capable of yielding laws in the sense of NORMS, and not merely laws in the sense of DEMONSTRABLE RECURRENCES AND REGULARITIES OF ACTUAL AND POSSIBLE EVENTS.[7]

Could not the establishment of norms be buttressed by maintaining a sound factual basis for the norms? Unfortunately, facts and concepts cannot stand in this relationship when norms rather than description of regularities are to be established. The reason is "the logical impossibility of deriving positive political conclusions from mere premises of facts." Value premises devoid of content can be regarded as irrefutable, hence, objective. Consequently, "For the

---

[5] Ibid., p. 55. [6] Ibid., p. 1.

[7] Gunnar Myrdal, The Political Element in the Development of Economic Theory trans. Paul Streeten (Cambridge: Harvard University Press, 1961), p. 4. Italics in original.

sake of scientific 'objectivity' the fundamental normative principles must be formulated in such a way that they have no [empirical] content; whereas they can be given content only by the underhand insertion of taut premises, that is to say, of concrete valuations derived from other sources."[8]

In summary, metaphysical concepts, i.e., those without empirical content, function either to suggest significant empirical questions or provide the logical basis for "objectively" established norms. If their purpose is to suggest empirical hypotheses, they can in principle remain outside the theory. Metaphysical concepts become an integral part of theory when they purport to establish norms objectively. Myrdal traces the latter function of non-empirical concepts to

> the normative-teleological way of thinking, traditional in the social sciences, and, indeed, programmatic in the philosophy of natural law upon which they were founded. . . . The norm . . . acquires an air of being founded upon the 'nature of things.' This precisely is the circular reasoning inherent in the philosophy of natural law.[9]

While it is true that economists have long held that theory should remain value-free,[10] they have generally been unsuccessful in eliminating values from their work.

---

[8] Ibid., p. 18. [9] Ibid., p. 20. [10] Ibid., p. 2.

Myrdal and Robinson both agree that explicit value judgments are necessary if practical hypotheses are to be formulated and scientifically tested. They only insist that it be explicitly recognized that values cannot be objectively ascertained, and that policy suggestions based on them are contingent. They are also agreed that while economics has been plagued by the unscientific use of metaphysical propositions, nevertheless many byproducts have emerged which have been scientifically useful.[11]

Up to this point the discussion would not lead one to believe that economic theory has been very successful in bridging the gap between the realm of concepts and the realm of facts. So many of the key concepts of economic theory--value, utility, income--can be shown to be partially or completely empty of empirical content upon close examination. And yet these concepts have survived in an era when economic theorists are more interested than ever before in doing valid empirical work. Are these economists "talking at cross-purposes;" have they failed "to clear the decaying remnants of obsolete metaphysics out of the way before [going] forward."?[12] The thesis to be advanced and argued here is that these non-empirical concepts, which hold their place in modern economics, do have a role to play even in modern, empirically-oriented

---

[11] Ibid., p. 32.

[12] Robinson, pp. 146, 147.

theory. Their part is not simply a heuristic one, but also an organic part of the theoretical structure.

The Deductive Pattern of Economic Analysis

The extent to which non-empirical concepts have a valid analytic purpose depends in large measure upon what approach to economic theory is under consideration. Fels distingusihes four principal approaches to economic theory: inductive, deductive, econometric, and historical.[13] Throughout the present paper, we will be speaking about the deductive theoretical method, described by Fels as "the building of models, whether literary or mathematical, by making simplifying assumptions and deducing their logical conclusions.[14, 15] The elements of Fels' definition apply equally well to the larger theoretical systems. The deductive approach is useful today to the extent that it contributes to the description or explanation of historical or empirical events. The valid use of the deductive method must be distinguished from theory which had as its purpose the objective derivation of norms. The difficulty with the modern use of the deductive approach in economics, and the source of so much

---

[13] Rendigs Fels, American Business Cycles: 1865-1897 (Chapel Hill: University of North Carolina Press, 1959), p. 4.

[14] Ibid.

[15] Fels' categories are not mutually exclusive. "Deductive" economics certainly does not exclude inductive reasoning. The category merely serves to emphasize that the theoretical end-product has a form from which hypotheses can be deduced logically and tested empirically.

controversy, is the survival of non-empirical ("metaphysical") terms in their structure. Must the deductive approach to economic analysis be abandoned altogether, on the grounds that nothing valuable and substantial can be deduced about social life from a few oversimplified assumptions? Or, can it be salvaged by eliminating from models elements which cannot or do not correspond to the hard data of real life? Writers such as George Soule[16] tend toward the <u>former</u> position, while writers such as Samuelson (as we shall see) seem to favor the <u>latter</u> solution. A third possibility warrants more consideration than it has received in the literature of social science. It is that non-empirical elements survive in deductive theory for sound methodological reasons.

A word more about the present status of deductive theory in modern economic analysis is in order. Arthur Burns says:

> The ground covered [by the N. B. E. R., representing the inductive approach] has been smaller, but the findings have been supported by evidence . . . the habit of insisting upon evidence is spreading, and today evidence less often means deduction from untested premises. Economic models continue to receive hopeful attention; but mere logical consistency or aesthetic appeal now count for less, and performance under tests for more, than a generation ago.[17]

---

[16] George Soule, <u>Ideas of the Great Economists</u> (New York: New American Library, 1952).

[17] Fels, pp. 12-13.

The limitations of deductive theory are especially acute in a number of different applications. In discussing the limitations of the traditional theory of the firm, George Katona says that "an analytical framework that considers a few factors only, and always the same few factors, can hardly be sufficient."[18] Katona argues that the theory is essentially inadequate to account for actual decisions made inside the business firm. Cyert and March stress the unrecognized organizational complexities as well as the oversimplified assumptions about decision-making cited by Katona.[19] It is well known that examples such as these could be multiplied. Business cycle analysis is another area in which the theoretical model is probably not the best analytic device. Duesenberry points out that business cycles are greatly influenced by the institutions and structure of the economy, and since they have been changing rapidly, each cycle is to a considerable degree unique.[20] Fels[21] also argues that the deductive approach is probably better suited to problems not so strongly influenced by the pressure of external events.

---

[18]George Katona, Psychological Analysis of Economic Behavior (New York: McGraw-Hill Book Company, Inc., 1951), p. 237.

[19]Richard M. Cyert and James G. March, A Behavioral Theory of the Firm (Englewood Cliffs, N. J.: Prentice-Hall, 1963), p. 11.

[20]James A. Duesenberry, Business Cycles and Economic Growth (New York: McGraw-Hill Book Company, Inc., 1958), p. 6.

[21]Fels, pp. 14-15.

These remarks are intended to identify certain areas where deductive models are less likely to achieve success in explaining empirical events. We will give closer consideration to these examples when we discuss the empirical principles of deductive theory. Fels summarizes the current use of deductive analysis in the following way.

> The clarification of logical relations contributes to immediate understanding, assists in framing limited hypotheses for testing, provides stepping-stones for building better theories, and . . . gives historians hints at what to look for. A model must be the starting point for econometric structures; if there is justification for proceeding with econometric work, there is *ipso facto* justification for model building.[22]

The Principle of Correspondence

The deductive theorist in any branch of knowledge faces the problem of linking empirical fact with his theoretical structure. Following Nagel[23] we shall call this the problem of correspondence. In applying the principles of Nagel's analysis, we shall accept the judgment of Myrdal and Robinson about unwarranted ideological intrusions into economic analysis. We shall, however, take a different position concerning the empirical status of deductive theory embodying the concepts they attack. Specifically, we shall argue that there are sound methodological reasons for the presence of non-empirical terms and concepts in empirically-oriented deductive theory.

---

[22] *Ibid.*, p. 15.

[23] Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation* (New York: Harcourt, Brace and World, Inc., 1961).

Even the most casual glance at the output of economic theorists is enough to convince anyone that there are at least two categories of statements to be found: the factual and the purely logical. In this latter category are contained statements which are obviously not intended to assert anything about the real world, even though words are used which have factual content in other contexts. Machlup reminds Samuelson of this[24] by listing Samuelson's basic propositions or "assumptions" in an international trade model: "1. There are but two countries, America and Europe. 2. They produce but two commodities, food and clothing. . . ." These statements cannot be called metaphysical, nor can they be called counterfactual; they are purely logical and have nothing to do with the things they name.

In his chapter, "Experimental Laws and Theories,"[25] Nagel gives careful consideration to the logical aspect and the factual aspect of deductive theory, and the relationship between the two. Observation is both the ultimate point of departure and the final test of scientific thought. Nevertheless, it is possible to distinguish between experimental laws, which "invariably possess a determinate empirical content," and theoretical laws, which do not relate to anything that has been or can be observed.[26]

---

[24]Fritz Machlup, "Professor Samuelson on Theory and Realism," The American Economic Review, Vol. LIV, No. 5 (September, 1964), pp. 733-736.

[25]Nagel, Chapter 5.

[26]Ibid., pp. 79-80, 83.

Theoretical laws can be tested because there are rules of correspondence which state the empirical meaning of certain of their key terms. Consequently, the empirical content of the theoretical law can be tested. But the theoretical law itself, in the absence of rules of correspondence, cannot be tested directly, because by itself, a theoretical law has no empirical content. The meaning of theoretical laws is determined solely by the logical function they fulfill within the context of the theory. Their meanings are not determined independently by empirical fact; they change as the logical structure of the theory changes. Knowledge about experimental terms, on the other hand, does not depend upon the law in which they are found, but has an empirical existence independent of the law. While their appearance in a law increases their operational significance, nevertheless such empirical terms survive even if the law itself must be refined or rejected.[27] Some examples might add clarification.

The term "atom" has acquired new meanings, from the theories of Democritus to Dalton to the present day. In any theory, the meaning of "atom" derives from its place in the theory, which is designed to provide testable hypotheses. Verified hypotheses give credence to the theory, but the experimental properties do not reflect in every way the

---

[27] Ibid., pp. 84-85.

theoretical properties. Similarly, the term "unconscious" did not begin to take on distinct reference to mental qualities until rather late in Freud's writings, by which time some empirical corroboration was available. Even then, some of his followers (e.g. Adler) were unconvinced, and denied the (empirical) existence of the unconscious. In economic theory, the term "utility" has for a long time been important to the theory of consumer demand, yet its empirical interpretation has changed a number of times.

The deductive theory derives its empirical meaning by way of the experimental laws which are its logical consequences. The experimental law is said to be "explained" by the theory when it, along with any number of other experimental laws, is a logical consequence of the theory. The "explanatory power" of the theory is greater the more of such experimental laws it encompasses or brings to light. Since the terms of a theory (or the model in which it is expressed) are only implicitly defined by the structure of the theory, the theorist must designate, at least implicitly, the empirical counterpart of the terms in the theory. Many theoretical terms in physical theory have functioned for a long time before empirical correspondence was established. It is not to be expected of a successful theory that all its theoretical terms will attain such correspondence.[28] For a variety of reasons this should

---

[28] Ibid., p. 98.

not be disturbing. For one thing, because of the relatively great complexity of theories, theoretical terms are often linked with more than one experimental concept.[29] By judiciously associating certain terms with experimental concepts, leaving others defined only implicitly, the theory can be made to account for a large and diversified number of experimental laws. Tieing each theoretical term down to a single experimental concept would greatly limit the range of applicability of the theory and probably distort an understanding of its content and implications. Nagel illustrates the importance of flexible use of rules of correspondence as a means of greatly increasing the scope of a theory.

> We have already noted the success of Newtonian theory in explaining the laws of planetary motion, of freely falling bodies, of tidal action, of the shapes of rotating masses . . ., laws dealing with the buoyancy of liquids and gases, with the thermal properties of gases, and much else.[30]

Empirical data are associated with, not identified with, theoretical terms. As a rule it is not possible to make them equivalent and interchangeable. It cannot be said that the theory uniquely implies the experimental data with which it is associated, nor that its explanatory power is by any means exhausted by the association.[31]

---

[29] Ibid., p. 99. [30] Ibid., 89.

[31] Ibid., pp. 97-98.

The Principle of Correspondence and the Explanatory Scope of Theories

"Unverifiable mental constructs . . . are almost indispensable for an understanding of the observed relations between perceivable characteristics, yet, they themselves cannot be made accessible to the senses . . . ."[32] Schrödinger's comment is consistent with Einstein's belief in "perfect laws in the world of things existing as real objects, which I try to grasp in a wildly speculative way."[33] To demand a precise empirical basis for every statement appearing in theory is to fall prey to what Rapaport calls "hyperfactualism," a position long dead in the physical sciences.[34]

A theory is a more complicated statement than a simple experimental law. This is one of the features which gives the theory greater explanatory power; a theory typically possesses many more potential applications to different classes of observational phenomena. Nagel makes the point very well. He says ". . . theoretical notions are not tied down to definite

---

[32]Erwin Schrödinger, "On the Peculiarity of the Scientific World View," What is Life? and Other Scientific Essays (Garden City, N. Y.: Doubleday and Company, Inc., 1956), pp. 193, 194.

[33]Albert Einstein, letter to Max Born, quoted in "Einstein's Statistical Theories," Albert Einstein: Philosopher-Scientist, Vol. 1, ed. Paul Arthur Schilpp (New York: Harper and Row, Publishers, 1959), p. 176.

[34]Anatol Rapaport, "Various Meanings of 'Theory'," Politics and Social Life, ed. Nelson W. Polsby, Robert A. Dentler, Paul A. Smith (Boston: Houghton Mifflin Company, 1963), p. 81.

observational materials by way of a fixed set of experimental procedures, and . . . because of the complex symbolic structure of theories more degrees of freedom are available in extending a theory to many diverse areas.[35]

Nagel gives the following analytical summary of the components of deductive theory, which we have been discussing. Such a theory involves:

> (1) an abstract calculus that is the logical skeleton of the explanatory system, and that "implicitly defines" the basic notions of the system;
>
> (2) a set of rules that in effect assign an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment; and
>
> (3) an interpretation or model for the abstract calculus, which supplies some flesh for the skeletal structure in terms of more or less familiar conceptual or visualizable materials.[36]

Summary

In this chapter we have defined our task to be an examination of the methods and scope of deductive analysis in economics. The major problem with deductive theorizing to be identified in recent years is the presence of empirically-empty terms and statement-forms. The presence of non-empirical concepts has been blamed for the intrusion of ideological and metaphysical conclusions into economics.

---

[35] Nagel, p. 89. [36] *Ibid.*, p. 90.

It has been pointed out in this chapter that the criticisms of theory are justified, but that they do not necessarily follow from the presence of the concepts against which objections are raised. On the contrary, there are sound reasons for the presence of non-empirical concepts in deductive theory, as Nagel explains. From a very formal point of view, a deductive theory has no empirical content, except that designated by appropriate rules of correspondence. Because these rules may be altered for different applications of theory, and because not all theoretical terms require rules of correspondence, deductive theories have great explanatory scope. Indeed, they have greater explanatory scope than theories whose terms are tied to empirical entities on a one-to-one basis. As Machlup might put it, the "theory itself" is wider in scope than any particular set of its "conclusions."

We turn in the next chapter to an examination of Paul A. Samuelson's exposition of the "strong empirical position": that every scientifically-valid term and statement-form must possess empirical significance.

CHAPTER III

AN ALTERNATE EMPIRICAL PRINCIPLE FOR DEDUCTIVE ECONOMIC THEORY

In a recent widely-discussed article,[1] Paul A. Samuelson defines his central task to be a refutation of the following statement distilled from Friedman, which he calls the "F-Twist":

> A theory is vindicable if (some of) its consequences are empirically valid to a useful degree of approximation; the (empirical) unrealism of the theory "itself," or of its "assumptions," is quite irrelevant to its validity and worth.[2]

The criterion "empirical unrealism" applies to every proposition in a theory, for, according to Samuelson, every such proposition has factual import. This inference is derived from the following parts of Samuelson's argument. An axiom system taken as a whole, or theory ("B") is identical with its complete set of consequences ("C").[3] B is identical to the minimal set of assumptions ("A") which give rise to it. Identifying C- as a subset of C, and A+ as a wider set of assumptions, we have $A+ \supset A \equiv B \equiv C \supset C-$. That Samuelson canonizes only that part of C which has empirical validity establishes the fact that every

---

[1]Paul A. Samuelson, "Problems of Methodology-Discussion," Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2 (May, 1963), pp. 231-236.

[2]Ibid., p. 232. [3]Ibid., p. 234.

theoretical proposition has or must have a factual basis, in his view. Any A+ known not to possess empirical validity must be rejected; any A+ not known to possess empirical validity may be forgotten about, at least for the time being.

The crucial implication is clear: there exist no theoretical propositions defined only by the logic of the analytic framework; each statement is capable of direct evaluation by appeal to empirical fact.[4, 5] Since postulates are both logically and empirically identical with conclusions, a theory cannot in any sense be said to be "wider" in scope than the theory.[6, 7] It is important to note that Samuelson's formal conception of scientific theory is fundamentally different from Nagel's, the position we defend in the present paper, in one fundamental point. Nagel conceives a deductive theory to be <u>completely empty</u> of empirical content <u>until</u> rules of correspondence are established, associating certain segments of the theory with observable entities. The theory, together with its rules of correspondence, is then capable of generating testable hypotheses.

---

[4] <u>Ibid</u>., p. 235.

[5] Herbert A. Simon makes the same point when he says "If . . . F is a valid theory, it must be because it follows from empirically valid assumptions. . . . " <u>Ibid</u>., p. 230.

[6] Machlup, <u>The American Economic Review</u>, Vol. LIV, No. 5, p. 733.

[7] <u>Above</u>, p.

Samuelson's formal conception of a theory entails a statement completely empirical from the start. Terms or statement-forms in the theory lacking empirical content are superfluous. Let us call Samuelson's principle the "strong empirical position" in contrast to the "weak empirical position" adopted from Nagel in the previous chapter. We may now go on to support our choice of methodology.

No attempt is made in this chapter to find fault with the logic of Samuelson's strong empirical position. Samuelson's position is believed, however, to be inconsistent with the methodology adopted in the previous chapter, following Nagel. Having made a case for our "weak empirical methodology," it is desirable to show in some detail how and why it differs from what is judged to be its most important rival. Accordingly, we shall introduce into Samuelson's argument the distinction between theory as a purely logical construct and theory as an empirical tool. Samuelson's logic, unassailable on his own grounds, breaks down when the distinguishing element between the two methodologies is inserted into his position. We conclude that the differences between the two methodologies are significant ones. Samuelson is justified in pointing out the logical equivalence among elements <u>within</u> the theory. So long as the theory is still at the pre-scientific level of a set of mutually related propositions involving terms that are only implicitly defined, the <u>logical</u> equivalence

holds: $B \equiv C$. But what about the other half of the argument--the empirical half? Does $B^* \equiv C^*$ hold? (where * denotes empirical or non-logical). The proof that the latter identity holds depends on the assumption that $B = B^*$ and $C = C^*$. These are certainly not <u>identically</u> equal, nor are they necessarily equal by any commonly accepted standard of theoretical analysis. Nothing Samuelson says demonstrates anything illogical about assumed inequalities. Yet these equalities must be shown to be necessarily true if it is to be established that Samuelson has proved his argument.

Let us assume then that $B \not\equiv B^*$, in accordance with the large volume of theory and methodology which suggests it. In general, $B \supset B^*$, B^* being associated in some way with elements of B- (in Samuelson's notation) by means of correspondence rules. $C \supset C^*$, where in a scientifically active theory, the range of C^* is unknown.

An Examination of Strong Empiricism

Viewing a theory as an operational tool, it cannot be said at any given time that the conclusions C of a theory are entirely known. This is always true of a currently active theory; the "better" the theory the more it is true. So $A+ > C \not\rightarrow A+$ superfluous, as Samuelson says. For an actual theory, his identity cannot in principle be written. Samuelson does make one qualifying reservation to the effect that A+ is not superfluous. He is willing to suspend judgment on A+ because perhaps new evidence, C+ will justify the presence of A+.[8] If so, A and C will imply each other mutually.

---

[8]Samuelson, <u>Papers and Proceedings of the American Economic Association</u>, Vol. LIII, No. 2, p. 234.

Furthermore, we arrive at the conclusion that the identity $B \equiv C^*$ does not hold. This identity does not do justice to the difficulties involved in establishing empirically relevant theoretical analysis. In general, B and C^* do not enjoy the same factual significance. $B = C^*$, which is consistent with Samuelson's argument, is actually a special case of the type of theory under consideration, where every term in the theory or model has empirical content. But in such cases it is more proper to speak of <u>laws</u>, rather than theories with explanatory capacity as understood in this paper.

$B \supset B^* \rightarrow B$ is broader than justified by empirical knowledge, according to Samuelson. (B here corresponds to B+ in Samuelson's notation). Thus there are γ_i terms $\subset$ B not equivalent to identifiable terms in C^*. Samuelson says eliminate such γ from B+ by Occam's Razor, because they cannot be shown to have the same factual correctness as those γ_j related to C^*. But such use of Occam's Razor would destroy the <u>internal</u> validity of the theory; no longer would $B \equiv C$. $B - \gamma_i \not\leftrightarrow C$. The reason the internal validity of the theory would be destroyed is that the non-empirical segments of B are necessary to the integrity of the logical structure, irrespective of empirical content. The strict formal distinction between logical structure and empirical content of deductive theory, while submerged by the strong empirical position, is vital to the weak empirical position.

Matters are actually "made worse" in terms of the strong empirical position. $B \not\rightarrow C^*$; we need rules of correspondence R_i to establish a scientific relationship between theory and empirical conclusions. We need $B + R_i \leftrightarrow C^*$. Rather than stripping B down to its empirical minimum, it is necessary to complicate and qualify it still more.

Permitting the condition $B \supset B^*$ destroys the empirical purity of theory as described by Samuelson. It creates all the difficulties noted by various authors connected with the establishing of correspondence rules.[9] But, we must admit, the condition creates a danger aptly noted by Samuelson: "In practice it leads to Humpty-Dumptiness."[10] It introduces the temptation to produce slight-of-hand tricks by playing with the meaning of words. A concept once understood to possess empirical content is shown to be empirically empty or false. Yet it may retain its place in theoretical structure on the grounds that it is essential to the logic of the system, even though it is no longer supposed to assert anything empirical one way or the other. Or, perhaps a concept will be introduced as a purely theoretical term, defined only implicitly by the logic of the system, even though it is known that the concept would lead to logical contradiction or absurdities if viewed empirically. Marginal utility illustrates the first use. The community indifference curves of international trade theory

---

[9] Nagel, Structure of Science, pp. 93-94, footnote 3.

[10] Samuelson, Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2, p. 236.

illustrate the second use. (These cases are essentially the same; they simply illustrate different ways in which non-empirical concepts creep into economic theory.)

We are reminded of Robinson's criticism that economic theory lacks agreed-upon standards of disproof, so that it is difficult to reach general agreement concerning the scientific merits of theoretical work. We appear to be making matters worse by providing the theorist with a readily invoked defense for any work which proves to be empirically fruitless. No attempt is being made to minimize the problem. We only wish to show that reasoning away the non-empirical aspect of deductive theory is equivalent to dispensing with deductive theory itself, as practiced successfully by many economists.[11] Done in the name of eliminating metaphysical explanation, it would in principle eliminate also the form of scientific explanation peculiar to deductive analysis.

Machlup on Strong Empiricism

It follows from the principles of weak empiricism that since not every proposition in a theory stipulates something about observable reality, consequently, it cannot be asserted that propositions which do not enjoy empirical status are empirically false. Not all methodologists accept this principle, even those

---

[11] Machlup, The American Economic Review, Vol. LIV, No. 5, p. 733. See also Samuelson's reply, ibid., p. 736.

who adhere to it in their own theoretical work. Even Machlup does not fully grasp the principle in his cogent criticism of Samuelson on this very point.

> Perhaps both Samuelsons make a distinction between a theorem and a theory, meaning by the former a proposition deduced from counter-factual assumptions and postulates, and by the latter a proposition stipulating something about observable reality ... The bulk of economic theory ... is based on counterfactual assumptions, contains only theoretical constructs and so operational concepts. . . .[12]

While the writers disagree with each other, they both accept the dichotomy rejected here. Machlup seems tacitly to assume that statement-forms are counter-factual if they are not factual.

This is not a mere semantical disagreement, since we cannot insert "non-empirical" for counter-factual into Machlup's argument and get agreement. Clearly, this would not satisfy Samuelson, and it would also put more restrictions on valid theory than Machlup appears willing to impose. Yet the substance of his argument is closer to weak empiricist principles than Samuelson's. Note especially his statement that "the postulated relationships (which constitute the theory)" are not by themselves sufficient to make the theory empirically relevant. He presents a regrettably brief and somewhat unclear procedure for establishing empirical correspondence[13] which asserts that C* and B are not equivalent because extra-logical considerations are essential.

---

[12] Ibid., p. 735.

[13] Ibid., p. 733.

Samuelson's Strong Empiricism in Light of His *Foundations*

Samuelson's position is not completely opposed to the present argument, which is apparent when attention is directed to his *Foundations of Economic Analysis*. Samuelson's purpose in *Foundations* is generality, or what amounts to a variety of theoretical explanation. His methodological principle[14] is that of pursuing the structural analogies which can be found in diverse areas of economic theory. Let us pursue his argument far enough to see the relationships. Were Samuelson interested only in relating unknowns to pertinent empirical data, he would be satisfied with a descriptive formulation such as the following: $\psi_i = g^i(\alpha_1, \cdots, \alpha_n)$, $(i = 1, \cdots, n)$, which he gives as the set of equations expressing the relationship of the parameters to the unknowns. The whole process would be on firm empirical ground, and not open to the charge of a meaningless search for empty generality or explanation. What would be obtained is a "final functional relationship between our unknowns and parameters."[15] Such a system of relationships would be open to direct empirical check, fully capable of refutation. One might suspect that this is the empirical equivalence between B and C sought by Samuelson.

---

[14] Paul A. Samuelson, *Foundations of Economic Analysis* ("Harvard Economic Studies"; Cambridge: Harvard University Press, 1947), Vol. LXXX, p. 3.

[15] *Ibid.*, p. 12.

Yet he rejects this approach to theory as bare formalism "containing no hypothesis upon the empirical data."[16] "Indeed, it may be pointed out that these resulting functions between unknowns and parameters could have arisen from an infinity of possible alternative sets of original equations."[17]

"So what?," we might have expected Samuelson to respond. "Scientists never 'explain' any behavior, by theory or by any other hook. Every description that is superceded by a 'deeper explanation' turns out upon careful examination to have been replaced by still another description. . . . "[18] Samuelson does not do complete justice to this more fundamental problem. Indeed, he holds that the "explanatory" element is indispensable to economic theory. What does this mean? It is not sufficient to merely state $x^{o} = g\ (t^{o})$, that a variable (perhaps a behavioral one) will take a different value depending on the magnitude of some parameter. All this function tells us is that there exists an equilibrium value of x for each value of t. Perhaps it implies that between these particular variables there is some stable relation which can be discovered statistically. But Samuelson wants to know "What is the nature of the dependence of our variable upon [our] parameter?"[19] For example, "Will an

---

[16] Ibid., Italics added. [17] Ibid., p. 11.

[18] Samuelson, The American Economic Review, Vol. LIV, No. 5, p. 737.

[19] Samuelson, Foundations, p. 15. Italics added.

increased unit tax result in a larger or smaller output?"[20] To answer this type of question it is necessary to specify functional relationships among the unknowns of the theory: $f^i(\alpha_1, \cdots, \alpha_m, \alpha_1, \cdots, \alpha_m) = 0$, ($i = 1, \cdots, m$). This restricts the number of solutions to the function $\gamma_i = g^i(\alpha_1, \cdots, \alpha_m)$ by specifying the way in which the unknowns are related to each other. The equations $f^i(\cdot)$ are the equilibrium conditions for the theory in question. Samuelson shows, pp. 15-16, how we can by specifying $f^i(\cdot)$, show how unknowns vary with changes in each of the parameters, something which couldn't be done without specifying equilibrium conditions.

The equilibrium conditions, $f^i(\cdot)$, may be said to constitute the explanatory aspect of deductive theory, in a special sense of the term. It resembles explanation in Nagel's sense in that it provides a logical structure which postulates interrelationships among unknowns which bear no obvious or self-evident relationship to empirical data.[21] Furthermore, the logical aspect of the theory takes precedence over the new data in the sense that partial empirical failures here and there, while definitely damaging, are by no means necessarily fatal to the theory.[22] While the equilibrium equations $f^i(\cdot)$ put restrictions on the explicit solutions $g^i(\cdot)$, by

---

[20] Ibid.

[21] Ibid., p. 11.

[22] Samuelson, The American Economic Review, Vol. LIV, No. 5, p. 737.

so doing they define the nature of dependence stated in the explicit solutions. Observation plus statistical analysis might reveal the explicit relationships, as already pointed out, yet it takes a theory to postulate how departure from such and such a position will be felt on the other critical variables in the matrix. Restrictiveness and comprehensiveness of coverage are thus seen to be two sides of the same coin. The larger the matrix of equilibrium conditions becomes, the more restrictions are introduced. But at the same time more and more variables are made endogenous to the theory. Hence the theory takes on greater explanatory power at the same time. No one expects a perfect theory, as Samuelson readily admits. A certain amount of empirical falsity is thus tolerated. Presumably, the greater the successes or positive gains derived from generalizing a theory, the greater the amount of empirical falsity will be tolerated (though never viewed as a merit.) To the extent that this is true, explanation plays an important part in deductive theory, according to Samuelson's scheme. Comparison of economic theory with theory in the physical sciences suggests that explanatory systems in economics tend to require a greater amount of elaboration to achieve a given amount of explanation. Perhaps for this reason discrepencies between B and C* will always be tolerated, even when these become fairly significant.

There is an important sense, however, in which Samuelson's views on explanation differ fundamentally from Nagel's. In Foundations, pg. 12, Samuelson in effect says that we start with assumptions A and by deductive reasoning reveal to ourselves the implications B contained in A. "We may bring to explicit attention certain formulations [C] of an original assumption which admit of possible refutation (confirmation) by empirical observation." In making this translation into a "different language" we leave B unaffected, adding or subtracting nothing from its empirical content. This appears to be a justifiable reading of the Foundations passage in view of Samuelson's later remarks. Samuelson achieves explanatory capacity by constructing a sufficiently large matrix of endogenous variables. Yet he retains this generality even though he insists that every term appearing in his theory possesses empirical content. Nagel, on the other hand, insists that some terms remain implicitly defined, so that the logical framework can be made to correspond to various empirical requirements, as needed. According to Nagel, completely specifying a model empirically unduly limits the possible range of applicability of the theory which underlies the model.

Samuelson aspires to generality along with complete empirical specification by systematically exploiting the formal similarities or structural analogies which recur again and again in economic analysis. " . . . essentially the same inequalities appeared again

and again, and I was simply proving the same theorems a wasteful number of times." Use of these analogies becomes the "fundamental principle of generalization" in economics for Samuelson.[23]

Let us give some additional consideration to this aspect of Samuelson's thought. He quotes Hertz in support of his own methodological position: "All of Maxwell's theory boils down to the simple question of whether the observable measurements on light and waves do or do not satisfy Maxwell's partial differential equations."[24] But we might respond things like "waves" are not observed; they are only accepted as real because certain aspects of observable reality can be "explained" in terms of waves, which are not known to exist outside the mathematical framework which defines them implicitly. Their reality is not observed, it is postulated by a coordinating definition.

Agreed, Samuelson would say. " . . . physicists didn't know or much care what it was that was waving in Schrödinger's equation, a probability or what not, so long as the facts of refraction and emission could be described well by this mnemonic model.[25] It is merely convenient to talk about waves, or helpful to think in those terms. The point is that if contemporaries of

---

[23]Samuelson, Foundations, p. 3.

[24]Samuelson, The American Economic Review, Vol. LIV, No. 5, p. 737.

[25]Samuelson, Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2, p. 232.

Schrödinger continued to think literally about wave images suggested by Maxwell's work they would be F-twisting: insisting upon the reality of some imaginary entity in hopes of clinging to a comforting "deeper explanation." In fact, Maxwell, Newton, or Schrödinger never intended any such thing.[26]

Accordingly, it is better to deal exclusively with functional relationships which can describe how a matrix of unknowns is affected by the change of a parameter, just as Schrödinger's mnemonic model can describe the facts of refraction and emission, than to retain an unreal "grasp" of a theoretical system. This is very good advice. But in emphasizing these points, Samuelson emphasizes the dangers and disadvantages of models for theories, which are the principal vehicles for attaching empirical content to a theoretical structure B. Physicists typically embed their theories in a model, and then associate certain of the latter's terms with observables, such as waves with refraction and emission. Often they are careful to point out that this model with its coordinating definitions is not identical with the theory. But even here, Samuelson emphasizes, for example, that physicists don't care "what is waving" when they consider Schrödinger's wave equation. Presumably it could be this or that or nothing at all. The task of theory is complete when functional relationships have been written down which describe observational data. What then

---

[26] Ibid., p. 232. Samuelson, The American Economic Review, Vol. LIV, No. 5, p. 737.

becomes of $B \equiv B^*$, $C \equiv C^*$, which are essential to Samuelson's strong empiricist position? Whereas we might have expected Samuelson to stress the requirements of identification of all theoretical terms with empirical counterparts, thus preserving the identity of theory and conclusions, instead he provides examples which seem to stress the opposite course. What seems to emerge is that models, with all their colorful imagery and capacity to satisfy philosophical yearnings for deeper explanation, have distracted economists, as well as physical scientists, from pursuing sounder goals. The distinction between theoretical terms and empirical counterparts appears to be overshadowed by an emphasis on a minimal set of theoretical statements (e.g. equations) necessary to predict or describe observable occurrences. There is no inconsistency in this shift of emphasis (which also occurs in Samuelson's 1963-1964 work), but we can begin to see a distinction in Samuelson between the logical framework and the empirical counterpart, i.e. between B and B^*, C and C^*. This is important, because we have differentiated strong and weak empiricism by the presence or absence of this distinction. We shall pursue this distinction further in the following section on analogy in science.

The Role of Analogy in Deductive Theory

Samuelson has found that formal analogies play an important role in deductive theorizing.[27] In analogies of this kind, "the

---

[27]Samuelson, Foundations, p. 3.

system that serves as the model for constructing a theory is some familiar structure of abstract relations, rather than, as in substantive analogies, a more or less visualizable set of elements which stand to each other in familiar relations."[28] In consequence of this approach,

> the new theory is not only assimilated to what is already familiar but can often be viewed as an extension and generalization of an older theory which had a more limited initial scope. From this perspective an analogy between an old and a new theory is not simply an aid in exploiting the latter but is a desideratum many scientists tacitly seek to achieve in the construction of explanatory systems.[29]

Another statement, also drawn from Nagel in a different context, is worth considering here.

> [One view states that] a theory is a compendious but elliptic formulation of relations of dependence between observable events and properties. Although assertions of a theory cannot be properly characterized as either true or false when they are taken at face value, a theory can be so characterized insofar as it is translatable into statements about matters of observation.[30]

In chapter six of his Structure of Science, Nagel develops at considerable lengths the advantages of applying analogy in scientific theory, especially in conjunction with substantive models. Diverse domains of experience are much more easily assimilated intellectually when familiar systems are employed in

---

[28] Nagel, Structure of Science, p. 114.

[29] Ibid. [30] Ibid., p. 118.

the study of strange occurrences. Apprehensions of similarities between old and new are frequently the starting points for important advances in knowledge.[31]

Formal analogy has been a very fruitful tool in physical theory when based upon the science of mechanics. Numerous branches of physical inquiry have utilized the formal characteristics of the differential equations of mechanics in the construction of theories.

> But this indicates no more than that diverse subject matters exhibit structures of relations that are abstractly or formally indistinguishable. It does not signify that what is distinctive of the corresponding theories for each of these domains is exhaustively rendered by the <u>formal</u> structure of the theory.[32]

What Nagel means by this, of course, is that the theory remains in the pre-scientific stage until coordinating definitions are supplied, linking the theoretical terms to empirical data. Once this is done the theory can be asserted to say something definite; until then it says nothing about the real world. Indeed this is the property of the formal analogy which makes it such a valuable tool of scientific research; as pre-scientific B it has no empirical contrast whatever. $B^{*}=0$. This property permits the same theory B to say many things about many topics depending on the nature of the coordinating definitions supplied.

---

[31] <u>Ibid</u>., p. 108.

[32] <u>Ibid</u>., p. 165. Italics in original.

Nagel makes this point in terms of a helpful distinction: "it is not in consequence of mathematical form [that axioms] are to be viewed as the premises of distinctive science. We must . . . examine the kinds of terms the axioms relate, in order to ascertain the characteristic features [of an explanation]."[33] Samuelson does not choose to recognize this distinction. He says:

> 2. A reader of Friedman might be forgiven for lapsing into thinking that the thing called B has consequences (call them C) that somehow come after it or are implied by it and (sic) are somehow different from it.
>
> 3. That same reader might be forgiven for thinking that just as B has consequences C that come after it, it also has some things which are somehow antecedent to it called its 'assumptions' (and which we can label A).[34]

Samuelson is not denying by this that the various theories possessing the same formal characteristics are distinct theories. It is agreed that the equations are not a theory B until they are related to the real world. But, following this line of thought, it is preferable to recognize explicitly that the logical framework and the empirically applied theory are distinct. It is this distinction that gives the formal analogy its special advantages. For example, the same set of equations, constituting a theory, B, has wide applicability in

---

[33] Ibid., p. 166. Italics in original.

[34] Samuelson, Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2, p. 234.

say, economics, because it can be applied to numerous economic problems whose solutions are formally similar. Yet, aside from the structural similarity of the solutions, we are dealing with distinct theoretical applications, B_1^*, B_2^*, etc.

Having considered Samuelson's use of formal analogies in economic theory, we conclude that he does, at least in practice, make a distinction between B and B*. In any event, the most crucial difference still remains. On the one hand, Samuelson's strong empirical position demands that every term or statement-form have factual content. Or, from a slightly different vantage point, it demands that only the minimal set of theoretical statements (e.g. equations) be admitted in the description of any category of phenomena. The weak empirical position of this paper is less strict on either count.

Some Applications

Samuelson's methodology helps put normative problems into a proper relationship with economic theory. For example, full employment is basically a normative term, although semantically it appears to be empirical. We all agree that full employment in and of itself is a good thing, but there is wide disagreement how much of this good thing should be tolerated when other normative goals are considered. Recognition of this fact leads to the prescription that it is undesirable to attempt to supply a coordinating definition

for this term. There are no generally acceptable criteria for an empirical definition of full employment, because exact consensus on competing goals cannot be reached.

Samuelson shows us how to eliminate this concept from analysis. His purpose is to establish a logical construct correctly exhibiting the patterns of mutual interdependence found in the real world. The patterns of interdependence are provided by his equilibrium equations $f^i(\cdot)$. These equations are totally devoid of normative content. Quite properly, in the view of the modern writers cited, normative judgments must be made extra-theoretically; they are prior to any application of the theory, and are quite necessary to any such application. In addition, the theory generates no necessary or teleological outcomes, because limits on the attainable range of values of the unknowns is purely a technical or logical matter.

The normative connotations of full employment can be eliminated, then. Clearly the theory must show what combinations of values the matrix must acquire for "full employment" to be attained, consistent with any reasonable suggested figure. It must clarify the difficulties of attaining these values, show the implications of the alternative suggested value, or point out why attainment of certain values is impossible. Only such a theory can have any policy value. Without these properties, the theory

is merely an exercise, or at best a step along the way to empirically significant analysis. The argument could be repeated for any pseudo-empirical concept which embodies normative considerations of importance to the society: price-level stability, growth, etc. We will see in the next chapter, however, that not every theoretical term can be eliminated in this way.

Summary

In this chapter, we have contrasted in some detail two important and representative methodological positions. Following Samuelson, we have described his "strong empirical position," and shown how it differs from the "weak empirical position" derived from Nagel in the previous chapter. Two major differences were pointed out. Samuelson makes no distinction between the underlying logical structure of a theory and its empirical counterpart. He insists these are equivalent, just as a "theory" is equivalent with its "conclusions." In contrast, we have followed Machlup in holding that a theory is wider than its conclusions, in the sense described in the previous chapter. The other major difference between the two positions is that Samuelson insists that every term and statement-form must have empirical significance in a theory. Following Nagel, the weak empirical position permits

selective use of correspondence rules to identify some of the theoretical concepts with empirical entities. Partial correspondence is another reason behind our belief that a theory is wider than its conclusions, having as a consequence many "degrees of freedom," as Nagel puts it.

Our discussion of formal analogy in deductive theory led to the suggestion that perhaps in practice Samuelson does make practical use of the distinction between a theory and its empirical counterpart. In any event, Samuelson continues to insist on complete, rather than partial, empirical correspondence.

In the next chapter we shall show, through a rather detailed discussion of significant examples from economics, why we have chosen to support the weak empirical position as a useful and valid methodology for deductive economics.

CHAPTER IV

THE CHOICE OF PRINCIPLES: EVIDENCE FROM THE LITERATURE

Keynesian Liquidity-Preference

Keynes' General Theory of Employment, Interest, and Money[1] is a good example of a theory which possesses great empirical significance when viewed in its entirety, even though a number of its terms taken singly have dubious factual significance. Keynes' theory exemplifies Nagel's proposition that theoretical concepts possess empirical significance not in isolation, but rather by virtue of their being component parts of a total theory.[2]

Keynes' General Theory stresses three fundamental relationships: the consumption function, the marginal efficiency of capital, and the liquidity preference schedule. A number of fundamental parameters are also contained in the system, such as the quantity of money in circulation and the marginal propensity to consume. The competing principles of strong vs. weak empiricism suggest two important questions. Does each of these terms and relationships possess empirical content in its own right, following Samuelson, or

---

[1]John Maynard Keynes, The General Theory of Employment, Interest, and Money (New York: Harcourt, Brace and Company, [1936]).

[2]Nagel, Structure of Science, p. 202.

must we rely upon the context of the theory as a whole to supply the empirical meaning? Must we, at best, suspend judgment about the inclusion of a concept not (yet) proved to be subject to empirical measurement, or are other criteria ever offered by economists to justify their use?

Dudley Dillard holds that the importance of the liquidity preference schedule is that it introduces money into the theory of output. Specifically, it permitted an explanation of how investment could be checked before "the interest rate" fell to zero. Liquidity preference was injected into the system to supply a missing logical link in a theory which, taken as a whole, accounted for experience unrecognized in received doctrine.[3] Dillard is well worth quoting at length on the question of the empirical content of the liquidity-preference concept.

> [Such concepts] do not involve the discovery of new laws, principles, or phenomena previously unknown, but are new contrivances or inventions which previously did not exist either known or unknown. Liquidity-preference reveals no new truth. It is a device for focusing the analysis on relations between aspects of known experience. The relevant test of such concepts is one of usefulness, not of validity in the sense of correspondence to experience. Any test of validity must be in operational terms after the meaning has been established, presumably in relation to the whole system of which individual concepts are a part.[4]

---

[3]Dudley Dillard, "The Theory of a Monetary Economy," Post-Keynesian Economics, ed. Kenneth K. Kurihara (New Brunswick, N. J.: Rutgers University Press, 1954), p. 9.

[4]Ibid., p. 10, footnote 9.

Elsewhere Dillard points out that shifts in the schedule are more important than movements along the schedule,[5] and that the interest rate is a somewhat unrealistic link between money and the level of output. But the crucial fact is that the essential link is forged. Furthermore, it interjects expectations into the theory as they must be, and as they could be only in a monetary theory of output. It also shows that while the origins of depressions can be monetary in nature, yet the cure cannot be solely monetary (as widely believed prior to the General Theory).[6] This practical insight is embodied theoretically in the liquidity trap.

It is clear that Dillard regards the liquidity-preference schedule as a theoretical term, lacking empirical content outside of the context in which it appears. Not everyone shares this judgment.[7] Tobin provides an argument in support of an empirical liquidity-preference schedule. The Tobin-Warburton-Fellner

---

[5]This may be counted as a weakness of the system because it implies that exogenous changes are more influential than the functional relationships actually stated in the theory.

[6]Dillard, "The Theory of a Monetary Economy," Post-Keynesian Economics, p. 20.

[7]James Tobin, "Liquidity Preference and Monetary Policy," Readings in Fiscal Policy, ed. Arthur Smithies and J. Keith Butters ("The Series of Republished Articles on Economics"; Homewood, Ill.: Richard D. Irwin, Inc., 1955), Vol. VII.

discussion that followed Tobin's original article[8] emphasizes the difficulties of providing firm empirical support for the concept. But regardless of the empirical significance of liquidity-preference as an isolated concept, the critical point is to observe Tobin's skillful handling of the concept as a theoretical term. To a considerable extent it could be said without injuring his discussion: "perhaps it is impossible to pin down liquidity-preference empirically. Maybe we will never know the shape of the schedule. Nevertheless, many statements about monetary policy can be shown to imply certain liquidity-preference schedules which we know are highly unrealistic, whatever the 'real' schedule may be like." The value of Tobin's discussion, therefore, doesn't turn solely on his belief in the empirical content of the term, or on his support of that content. Much of its value depends on his skillful handling of the term as a logical concept or theoretical term implicitly defined by the theoretical system taken as a whole. Tobin's theoretical work, therefore, is offered as an example of analysis conducted along lines which may be regarded as an alternative to Samuelson's methodological approach. It seems fair to argue that Samuelson is unduly restrictive in insisting on his particular interpretation of the empirical content of deductive economic theory. We turn now to an additional instance of theory which supports this contention.

---

[8]William Fellner, "Monetary Policy and the Elasticity of Liquidity Functions," Review of Economic Statistics, XXX (February, 1948). Clark Warburton, "Monetary Velocity and Monetary Policy," Review of Economic Statistics, XXX (November, 1948). James Tobin, "A Rejoinder," Review of Economic Statistics, XXX (November, 1948).

The Classical Theory of International Trade

Referring to the classical theory of international trade, Ellsworth states

> we should note that the classical economists were more interested in showing the gains from international trade than in explaining its mechanism. Their theory served adequately to show the effects of trade upon welfare, but it had serious shortcomings [in other respects]."[9]

The weakness referred to hinges upon the labor theory of value, which is a cornerstone of the theory. From the standpoint of formal theory the results depend upon the labor theory of value. How are we to evaluate the theory in view of this weakness? (Nothing said here should be interpreted as denying that it is a weakness.)

One interpretation states that if the axioms or postulates of a theory are discredited, then the theory itself is discredited. Now, it may be asked, what has been more discredited than the labor theory of value, postulated by the comparative advantage theory of international trade? But if the "theory" B, (which includes the labor theory of value as a postulate) is identical with its consequences C, it follows that B and C are both invalid, and must be rejected.

---

[9] P. T. Ellsworth, The International Economy (3rd ed.; New York: the Macmillan Company, 1964), p. 69.

This is a much more stringent condition than imposed by syllogistic logic, in which empirically valid conclusions can be drawn from false premises. The reason for the stricter condition is to eliminate trivial and fruitless theories from consideration. As Nagel points out, "It is always relatively easy to invent an arbitrary set of premises which satisfy the logical conditions for deductive explanations; and unless further restrictions were placed on the premises, only a moderate logical and mathematical ability would be required for explaining any fact in the universe without leaving one's armchair."[10]

Let us look at the classical theory of international trade and its significance from an historical point of view. It was the dominant theory from the time it was proposed by Ricardo in the 1830's until modern value theory was inserted into the theory in the 1930's by Ohlin.[11] The labor theory of value itself, however, had been superseded long before Ohlin's contribution. Samuelson's commentary under the circumstances is: "There can be no factual correctness of C so defined that is not also enjoyed by B. The minimal set of assumptions that give rise to B are identical with B, and if A is given this interpretation, its realism cannot differ from that of the theory B and consequence C."[12]

---

[10]Nagel, Structure of Science, p. 43.

[11]Bertil Ohlin, Interregional and International Trade ("Harvard Economic Studies"; Cambridge: Harvard University Press, 1957), Vol. XXXIX.

[12]Samuelson, Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2, p. 234.

Rejection of the theory is well-grounded on this interpretation. Yet the success of the theory, related in Ellsworth's statement, cannot be minimized. People were given a convincing explanation of the error of mercantilist views, which dominated European thinking for two and one-half centuries.[13] It is as unfair to accuse the writers of making naive counterfactual statements (no significant differences in factor endowments, homogeniety of factors), as it is to accuse Samuelson of "believing" the "assumptions" of his factor-price equalization model.[14]

Note, for example, Ricardo's statement "It will appear then, that a country possessing very considerable advantages in machinery and skill, and which may therefore be enabled to manufacture commodities with much less labour than her neighbours, may, in return for such commodities, import a portion of the corn required for its consumption, even if its land were more fertile, and corn could be grown with less labour than in the country from which it was imported."[15]

---

[13]Ellsworth, p. 38.

[14]Samuelson, The American Economic Review, Vol. LIV, No. 5, p. 734.

[15]David Ricardo, The Works and Correspondence of David Ricardo, Vol. I: On the Principles of Political Economy and Taxation, ed. Piero Sraffa (Cambridge, England: Cambridge University Press, 1962), p. 136.

As happens in most seminal theoretical works, Ricardo anticipates extra-theoretically numerous key ideas explicitly incorporated in later theoretical work. That they do not succeed in doing the entire job at once should not be held against them, especially if we concede them the right to define their own task. Suppressing considerations of the "mechanism" which we have been brought up to consider important enabled the classical theorists to demonstrate the gains from trade and the advantages of specialization.[16]

Of course, to be a genuine theory, B must have some authentic empirical content. Otherwise it would run the risk of being entirely arbitrary. The classical theory at least made an attempt to identify the major element of cost, implicitly saying that the other costs are relatively insignificant. While this judgment hasn't stood up over the years, it provided one of the pillars for a theory which greatly advanced knowledge. The theory also ignored relative resource endowments, even though this is one of the elements of a more satisfactory theory of trade. The theory is not completely satisfactory is quite distinct from its being unacceptable methodologically. This interpretation appears to be appropriate and consonant with the methods tacitly employed by economists past and present, as well as by physical scientists.

---

[16]Ellsworth, p. 69.

Sir Donald Mac Dougall's study, cited in Peter B. Kenen,[17] has provided an empirical test of the classical theory. Results are sufficiently good to justify the assertion that the deductive explanation of comparative advantage has an empirical basis. This assertion is further justified because the theory has shown considerable capacity for expansion and generalization in empirical directions without having its basic structure altered.[18] The theory begins to break down only when attempts are made to overburden the logical framework by giving explicit consideration to the internal adjustment process, which is best left implicit, or suppressed from empirical view.

The Law of Diminishing Returns

The well-known discussion between J. H. Clapham and A. C. Pigou[19] over the law of diminishing returns as a valid economic concept provides a vivid illustration of the empirical principles we are discussing. Professor Clapham asserts that the law cannot be made to fit the actual facts of particular industries, that nothing useful would be accomplished even if it were possible,

---

[17]Peter B. Kenen, International Economics ("Foundation of Modern Economics Series"; Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1964), pp. 16-17.

[18]Ellsworth, pp. 67-68.

[19]J. H. Clapham, "Of Empty Economic Boxes," Readings in Price Theory, ed. Kenneth E. Boulding and George J. Stigler ("The Series of Republished Articles on Economics"; Chicago: Richard D. Irwin, Inc., 1952), Vol. VI. A. C. Pigou, "Empty Economic Boxes: A Reply," Readings in Price Theory. Clapham, "The Economic Boxes: A Rejoinder," Readings in Price Theory.

and that the law could not be tested for realism except after the fact, that is, in practice it must remain a historical exercise. Finally, the law of diminishing returns cannot in practice be separated from other factors of great importance which completely obscure its workings.[20]

Pigou's response illustrates the type of reasoning we have been applying. Referring to the law of diminishing returns as one of many "empty economic boxes" identified by Clapham, Pigou says:

> Dr. Clapham appears to hold that, provided, as boxes, they cannot be filled, it is self-evident they can serve no purpose, of this kind [the construction of a realistic economic science.] In that I venture to suggest that he is mistaken, that he has, in fact, misunderstood altogether the nature of the work he is belittling.[21]

Pigou goes on to explain how the concept of diminishing returns is essential to an understanding of the relationship between aggregate output and changes in unit costs. He says that it is not the concept itself that is important, but its strategic function in the solution to the great problem of economic value.

---

[20] Clapham, "Of Empty Economic Boxes," Readings in Price Theory, pp. 126-128.

[21] Pigou, "Empty Economic Boxes: A Reply," Readings in Price Theory, pp. 133-134.

> To take the categories of increasing and diminishing returns out of their setting and to speak of them as though they were a thing that could be swept away without injury to the whole corpus of economics is a very perverse proceeding . . . These boxes, as he calls them, are not merely boxes; they are also elements in the intellectual machinery by which the main part of modern economic thought functions. . . . They are an organic and inseparable part of that machinery.[22]

Clapham's rejoinder, "I see no perversity in criticizing part of a theory,"[23] has a large following, at least by implication, as we can see in many prevelent criticisms of economics today.

Utility Theory

In this section, we shall try to show that much of the dissatisfaction with the utility concept centers around its empirical status in economic theory.

Frank Knight points out certain abstract parts of economics which "are no less practical than concrete-descriptive or applied economics but are less directly related to immediate problems."[24] The study of value and distribution have been basic to modern economics since its inception, and the way in which they have been understood has greatly influenced all branches of economic theory.

---

[22] Ibid., pp. 134-135.

[23] Clapham, "The Economic Boxes: A Rejoinder," Readings in Price Theory, p. 140.

[24] Frank H. Knight, "Economics," On the History and Method of Economics (Chicago: The University of Chicago Press, 1956), p. 19.

(For example, recall how abandonment of the labor theory of value permitted the scope of international trade theory to be extended.) One of the key occurrences on the road to a modern theory of value was the invention of marginal utility. At first, theorists believed they could measure the quantities of satisfaction people acquired from the use or the acquisition of goods. In time, however, it became apparent that the whole idea of marginal utility was based upon a very dubious form of psychology long since abandoned by professional psychologists. Economists came to realize that all promise of giving empirical content to the concept of marginal utility had vanished. Yet to abandon the concept would have done violence to the logical structure, and economic theory is full of terms which economists have been reluctant to give up for that very reason.

It would be possible for economists to treat consumer demand in a completely empirical way. Relationships could be

> treated as observational and the generalizations . . . inductive. . . . Economists, however, are prone to go 'behind' the observable demand and income curves to a more 'ultimate' magnitude: utility. The reasons for this are several. It is consistent with (admittedly unsophisticated) psychological experience. It yields predictions about consumer behavior. It permits the analysis of a significant range of welfare problems that are meaningless in purely observational terms.[25]

---

[25] George J. Stigler, The Theory of Price (rev. ed.; New York: the Macmillan Company, 1952), p. 68.

Knight continues, "It is now generally agreed . . . that the economic theories had better use the notion of a maximum without trying to say what is maximized--much as the physicist speaks of matter or mass in terms of the way it is measured without trying to define its nature."[26]

But bad psychology is bad psychology, it might be protested, and there is no excuse for justifying it simply because it leads to convenient results. Yet the economist is thoroughly justified in replying that he is not offering a theory of psychology, but a theory of economics. It is asking far too much of a theorist in any branch of knowledge to organize his concepts in a way which would lead to fruitful lines of inquiry in other disciplines where subject matter overlaps. It is sufficient for the economist to disavow any implications which might seem to flow from his theory, contrary to the conclusions of competent specialists. It is wholly gratuitous to criticize economic theory because it is bad psychology. Some concepts unavoidably overlap several disciplines. They will have a different status in each theory, depending on the objectives of the theory. Utility has a primarily logical role in economic theory, as Stigler and Knight point out. From the point of view of psychology, on the other hand, the term covers terrain that must receive more explicit attention. The utility concept thus has nothing relevant to say from the point of view of psychological theory.

---

[26]Knight, p. 20.

It is impossible to avoid the use of purely theoretical terms because just about every subject has been approached from another point of view, with a different theoretical emphasis. And one cannot make statements about a given matter which are equally acceptable from all possible theoretical points of view. Such statements would need to possess such all-inclusive generality that they would probably be hopelessly vacuous. Hence, any genuinely significant theory is inherently open to criticisms of distortion of reality by people who insist on verifying the wrong parts of the theory. Unfortunately for economics the inclination is strong because these theories are stated in more or less familiar terms, and touch on areas charged with great moral significance to the lay audience.

Utility theory has also come under severe criticism because of its alleged circularity. It is said to be circular because it asserts nothing significant about individual behavior.[27] People desire goods because they possess utility, and we know that they possess utility or else people wouldn't desire them. Utility maximization is established the same way. The fact that a person chooses goods in a certain combination indicates that it is the best combination, for otherwise he would have chosen another.

---

[27]Robinson, p. 47.

Reduced to these terms, and applied to the behavior of particular individuals, the theory is nothing more than an empty rationalization; it tells us that people behave the way they do. Utility maximization, like profit maximization, has been faulted on two counts. Being vacuous and circular, it asserts nothing at all. Being based on discredited psychology and having unsavory ideological connotations, it asserts what is known to be false. At one time or another these "assumptions" of economic theory get the worst of both worlds. Again, the point is that utility theory is not intended to further our knowledge of individual attitudes and behavior. Tastes and preferences, after all, are always exogenous variables in economic theory. Utility theory is to be regarded as empty of empirical content. But this does not mean that economic theory would be no worse off without it. The pervasive use of the concept in virtually every branch of economic analysis attests to this. Utility theory is an important part of the explanation of <u>collective</u> behavior. Terms <u>implicitly</u> referring to individual behavior are included in a purely formal way.

Empirical correspondence is not even attempted except in connection with logical conclusions related to the average behavior of large numbers of people. As with the profit maximization hypothesis (to be examined) it is necessary to relegate utility to the realm of implicit definition. Any other course would be unjustified and out of keeping with the goals of economic analysis.

C. G. Jung states that scientific knowledge of the individual is in a sense a contradiction in terms.

> . . . it is not the universal and the regular that characterize the individual, but rather the unique. He is not to be understood as a recurrent unit but as something unique and singular which in the last analysis can neither be known nor compared with anything else. At the same time man, as member of a species, can and must be described as a statistical unit; otherwise nothing general could be said about him . . . . If I want to understand an individual human being, I must lay aside all scientific knowledge of the average man and discard all theories in order to adopt a completely new and unprejudiced attitude.[28]

Jung is pointing out that much data about the individual is necessarily jettisoned in the construction of theories of human behavior. These theories, when properly interpreted and competently applied, can contribute greatly to knowledge. But he counsels at considerable length against the easy mistake of overextending the application of these theories into the wrong domains. That problems of interpretation arise in psychological theories of human behavior may be taken to justify our modest interpretation of an economic theory of human behavior. Jung points out that psychological theories, whose goal is understanding of human motivation and action per se, are inherently vacuous in certain crucial aspects, and will

---

[28] C. G. Jung, The Undiscovered Self, trans. R. F. C. Hull (New York: The New American Library, 1957), p. 18.

cause serious bias if these limitations are not respected. We may conclude that economic theory, whose goals are quite different, is not necessarily deficient if it is empirically vacuous with respect to portions that bear implicitly on human behavior. (Schrödinger's essay, "On the Peculiarity of the Scientific World-View," already referred to, is perhaps one of the most eloquent testimonies on the deliberate and necessary elimination of important aspects of understanding in order to achieve scientific knowledge.)

We turn now to a brief comparison of classical utility theory and indifference analysis. Leftwich says: "Some economists find distasteful the quantifying of utility and the principle of diminishing marginal utility. Nevertheless, the [classical] utility theory . . . and the indifference curve theory . . . reach the same results."[29] According to the present analysis, is there any ground for rejecting the traditional approach? It is true that at one time utility was thought to be the very area where empirical results could be achieved. Yet the end of classical utility's career as an empirical concept did not necessarily destroy it completely. Nor did it mean the end of every piece of analysis based on it. Put briefly, classical utility could justifiably survive as a theoretical term. This is essentially what happened when economists moved from cardinal utility to ordinal

---

[29]Richard H. Leftwich, The Price System and Resource Allocation (1st ed.; New York: Rinehart and Company, Inc., 1955), pp. 67-68.

utility. It was discovered that many of the theorems of economics based on utility theory could survive even if the empirical content of the key concept was weakened. Yet Hicks argued that even ordinal utility is unnecessary for the logical integrity of these theorems. Quite rightly, therefore, he set out to eliminate the concept from economic analysis. According to his indifference analysis, anything even suggesting quantitative utility is disallowed. The analysis is built solely upon an assumed scale of preferences.[30] This application of Occam's razor in itself constitutes a theoretical improvement according to Samuelson's approach. A more concise theory results, and what is more important, it can be applied to a wider range of problems, as is evident to anyone familiar with the literature.

We conclude that there is no particular advantage in retaining theoretical terms that stand little chance of acquiring empirical content. Such terms add little to the generality of the theory as an explanatory instrument. Explanatory capacity comes to a theory in part because of its characteristic of being a complicated and lengthy statement. As such it can be modified by additional restrictions or be used as the basis for more models than could a highly concise statement. But classical utility as a theoretical term doesn't seem capable of extending the generality of theory in

---

[30] J. R. Hicks, Value and Capital (2nd ed.; London: Oxford University Press, 1946), p. 18.

this way for the reason given. (Classical utility theory is being compared with the more concise indifference analysis, not with the alternative of no theory at all, to which it is far superior.)

The Theory of the Firm

Understanding of the traditional theory of the firm also benefits from an application of the empirical principles developed in this paper. The most frequently heard criticisms are easily summarized. On the one hand, the theory embodies highly over-simplified, if not altogether counterfactual assumptions concerning motivation. By ignoring multiplicity and diversity of motives, the theory gives a false impression on this score, if it says anything at all. On the other hand, the "firm" of the theory of the firm bears practically no resemblance to actual business organizations.

> It has no complex organization, no problems of control, no standard operating procedures, no budget, no controller, no aspiring 'middle management.' To some economists it has seemed implausible that a theory of an organization can ignore the fact that it is one.[31]

Furthermore, in simpler versions of the theory at least, perfect knowledge of the future is assumed on the part of decision-makers.

---

[31] Cyert and March, A Behavioral Theory of the Firm, p. 8.

George Katona expressed a criticism of the theory of the firm in terms of the use of the ceteris paribus assumption. Basic to the theory, of course, is the function stating an inverse relationship between price of firm output and quantity demanded by the consumer public. Impounded in ceteris paribus by this function are the vital consiserations of uncertainty, expectations, effects on consumer and rival firms, etc. In short, the theory "is based on a mechanistic psychology according to which one item in the psychological field can be changed without affecting other items in the same field."[32] There is no such "one-to-one correlation between a given stimulus and a given response."[33] Yet it is upon such a mechanistic psychology that the validity of the function under consideration depends. Empirical research reveals that the psychological assumptions implicit in the traditional analysis of business decision-making are false.[34] A fundamental empirical fact is uncertainty on the part of the businessman as to the consumer response to his price-output-product decisions. There is a fundamental psychological reason for this uncertainty, which is systematically ignored by the traditional theory. It is the essential interrelation of consumer motivation and expectations as part of a complicated and dynamic psychological field which generates this uncertainty.

---

[32] Katona, Psychological Analysis of Economic Behavior, p. 221.

[33] Ibid., p. 237. [34] Ibid.

Cyert and Grumberg[35] make another attack on the theory of the firm, which is levelled against the use of cet.par. Although it is different in nature from Katona's criticism, it is convenient to consider them together. Cet.par. is used so extensively in economics in order to reduce the great complexity of the real world to proportions manageable by theory. What economics loses by achieving manageability is capacity for prediction. Since it is impossible to determine whether cet.par. is ever fulfilled in actual practice, the authors argue, the validity of the hypothesis remains forever in doubt. Strictly speaking, economic laws are untestable for this reason.[36]

These remarks provide merely the smallest sampling from a large and well-known body of criticism of the traditional theory. Yet enough has been said to indicate how for some purposes simplification can be tantamount to distortion and falsification of theory. If the theory is interpreted as an explanation of the way an individual firm makes its day to day decisions, handles its organizational problems, defines its own goals, etc., then it can be said that empirical analysis has pretty well discredited the theory. Furthermore, if the theory is so interpreted, then the psychological implications that Katona finds embedded in the theory become

---

[35] R. M. Cyert and E. Grunberg, "Assumption, Prediction, and Explanation in Economics," A Behavioral Theory of the Firm.

[36] Ibid., p. 301.

empirically important. In other words, if we say that the traditional theory of the firm should illuminate organization problems, details of decision-making, goal formation, and the like, then the theory is genuinely deficient as criticized. For example, the theoretical statement that the sole goal of the firm is to maximize profits is then a seriously deficient empirical statement. Likewise, the implicit assertion that firms have no organizational problems (since the theoretical firm is not an organization) is gross empirical invalidity.

We have already seen, however, that the failure of a theory in one direction of inquiry does not preclude fruitful and valid applications in other directions. Cyert and March make the following distinction:

> . . . much of the controversy is based on a misunderstanding of the questions the conventional theory of the firm was designed to answer. The theory of the firm, which is primarily a theory of markets, purports to explain at a general level the way resources are allocated by a price system. To the extent to which the model does this successfully, its gross assumptions will be justified. However, there are a number of important and interesting questions relating specifically to firm behavior that the theory cannot answer and was never developed to answer, especially with regard to the internal allocation of resources and the process of setting prices and outputs.[37]

---

[37] Cyert and March, p. 15.

We may now examine the theory of the firm from this distinct point of view. We will then have a more complete picture of how the logical structure of a theory delimits the nature and range of its application, and determines how coordinating definitions should be supplied for the theory. We shall see that the unreality of the traditional theory of the firm stems from the fact that certain of its terms are given coordinating definitions, or reified, when they should be left implicitly defined. Properly selected rules of correspondence can preserve the empirical validity of the theory within its proper scope of application.

Modigliani explains two different ways in which micro theory is made to correspond to observation. On the one hand, he states that "normative theory" is concerned with the internal problems of the business manager. The goal of "positive economics," on the other hand, is the "understanding and explaining how our economic system works."[38] Modigliani is referring to such things as the impact of different market structures on output and income distribution in the community as a whole. He is interested in the "theory of markets" referred to by Cyert and March. As a consequence of their different goals, these economists will make a different choice

---

[38]Franco Modigliani, "Managerial Economics-Discussion," Papers and Proceedings of the American Economic Association, Vol. LI, No. 2 (May, 1961), p. 159.

> as to which features of the situation are essential and must be incorporated in the model and which can be neglected. Here the positive economist is likely to concentrate on the elements which are common to many agents and to neglect what seem idiosyncratic aspects of the problem. . . . these features may have to play a critical role in the managerial economist's model.[39]

This is the kind of problem which very frequently faces economists. Some of them, like Modigliani, concentrate on the problems of positive economics; some, like Hitch and McKean, work in the area of normative economics.[40] They develop theories and models appropriate to their own set of problems. Now, positive economic theory can be applied to the internal problems of the firm. It is developed in terms of a language which makes statements about firms, decisions, and the like. Yet we have seen how this extension of coverage tends to falsify important parts of the theory, because the language of the original theory takes on meanings it was never intended to possess. By the same token, the normative economist has some grounds for moving into the domain of positive economics. This might entail improving the "behavior inputs" of the normative economist's model. Hitch and McKean appeal to evidence that the behavior of business managers does not conform to the propositions

---

[39] Ibid., p. 158.

[40] Charles J. Hitch and Roland N. McKean, "What Can Managerial Economics Contribute to Economic Theory?" Papers and Proceedings of the American Economic Association, Vol. LI, No. 2 (May, 1961), pp. 147-148.

of positive theory to support this contention.[41] Modigliani's reply should be easy to anticipate: "For my part, I am somewhat skeptical about the chances of developing alternative postulates which are capable of broad applicability and yet are operational enough to lead to precise verifiable implications."[42]

Since the two related theories have different goals, their common theoretical terms have a different status. Profit maximization is a good example. For the normative economist, interested in "how the system works," the basic motivation is properly left without coordinating definition. At this level of abstraction it would hopefully amount to a neglect of what seem "idiosyncratic aspects of the problem which--he hopes--will tend to wash out under aggregation or will at worst show up as random components in his model."[43] Thus, the positive economist considers the profit maximization hypothesis as merely one of the organizing elements in the marginal calculus of his theory. ". . . striving to achieve with given means a maximum of ends [is] the so-called <u>economic principle</u>," and marginalism is the logical process of 'finding a maximum'. . . ."[44] The theory cannot be

---

[41] <u>Ibid.</u>, pp. 149-150.

[42] Modigliani, <u>Papers and Proceedings of the American Economic Association</u>, Vol. LI, No. 2, p. 158.

[43] <u>Ibid.</u>

[44] Fritz Machlup, "Marginal Analysis and Empirical Research," <u>The American Economic Review</u>, Vol. XXXVI, No. 4 (September, 1946), p. 519.

evaluated by taking any of its propositions in isolation; "It is necessary to know precisely what the theory says, what it implies, and what it intends to do."[45] Marginalism is the essential feature of the theoretical framework, and "profit maximization and marginalism are so closely connected that it is hardly possible to make any use of marginalism except for the purpose of determining the output and price that will yield maximum profits."[46] It should be clear that profit maximization is a different sort of concept in the positive theory than it is in the normative theory. In the former, it is only defined implicitly; no attempt is made to endow it with an independent existence by establishing empirical correspondence for it. Profit maximization does not enjoy the status of an empirical law, which can survive unaltered the theory in which it is embedded. The concept has an altogether different status in normative theory. There it becomes the very focal point of empirical research. In normative theory, analysts seek to establish laws which are significant independently of any theory in which they might appear, or at least good working empirical hypotheses. This is the distinction that Modigliani seems to have in mind when he voices skepticism over the suggestion of Hitch and McKean that managerial economics can contribute more realistic and empirically valid basic assumptions to the positive theory.

---

[45] Ibid., p. 520.

[46] Katona, p. 215.

There is another advantage in using the simple motivational assumptions implicit in positive micro-theory which is abundantly clear to all theorists. It enables the theorist to make precise statements about economic activity. He does not for a moment attach great empirical significance to this precision; what the theorist gains is an ability to make concise statements--manageable intellectual tools possessing great flexibility and considerable generality. Stigler acknowledges the diversity of entrepreneurical motivation, but stresses the difficulties of strengthening the theory by including them. Frequently they possess no agreed upon meaning and would only detract from the clarity of the conclusions drawn from the theory. The desire for security, or the goal of "fair profits" are examples. Stigler metnions others: "to be his own boss, to maintain a customary standard of living, to obtain economic /or/ political power, etc."[47]

> There is no objection in principle to these alternative goals, but in their present undeveloped state they are seldom useful in general analysis . . . unless they are developed in content and their scope of operation and strength are approximately determined, they improverish rather than enrich economic analysis.[48]

---

[47]Stigler, p. 148.

[48]Ibid., p. 149.

Summary

This chapter has been devoted to an examination of a number of concepts and theories which illustrate the application of the methodological principles that were developed in the previous chapters. There is no universal agreement in theoretical science concerning valid principles of deductive theorizing. Consequently, the choice of principles must be somewhat eclectic in nature. The examples given in this chapter justify our defense of the methodology distilled principally from Nagel, and called in this paper the "weak empirical principle."

We have seen how the concepts of diminishing returns and liquidity preference are vital to the logical frameworks of the theories in which they appear, even though their empirical content, when viewed in isolation, is highly questionable. These important examples illustrate the principle that concepts should be judged, not in isolation, but rather within the entire theoretical context in which they appear. The discussion of utility theory shows how a concept can acquire new and different significance in theoretical analysis. Originally treated as an empirical term, utility has retained its position in theory as an implicitly defined term,

important to the logical structure of theory. Rules of correspondence can change over time, in accordance with new applications and developments.

The discussion of the theories of the firm and international trade make clear the principle that the valid application of an abstract theoretical framework depends on the very careful formulation of correspondence rules. Reification of the wrong concepts leads to erroneous and unjustified conclusions, which frequently cause theories to receive unjust criticism. Proper formulation of correspondence rules, on the other hand, greatly facilitates proper use of theory and helps reveal the legitimate range of theoretical application.

Each of the examples discussed in this chapter reveals why it is important to realize that non-empirical terms and statement-forms have a legitimate and vital role to play in the deductive form of economic theory.

In the next chapter, an attempt is made to examine the historical background to the controversies over the empirical principles of deductive theory. We shall see that the outcome of the discussion is still a matter for debate in the physical sciences. Nevertheless, we shall find considerable support for the principle developed in the previous chapters, as well as important implications concerning the scope of deductive theory.

CHAPTER V

PROBLEMS IN THE HISTORICAL DEVELOPMENT OF DEDUCTIVE THEORY

We have explored a number of aspects of economic theory which can best be understood against the backdrop of broader scientific or philosophical issues which have received the attention of the scientific community at large. When we are faced with the serious charge that much of the work done by economists in the deductive tradition is invalid, we are prompted to examine the developments in scientific thought which suggest the criticism. The issues we are interested in are, as always, the nature of the connection between experimental data and the theoretical representation of that data. Specifically, what effect has the greatly expanded emphasis on empiricism had on the position of the *a priori* method of analysis in scientific theory? Of equal importance is the closely related question, to what extent have the goals of scientific analysis changed?

Operationalism

P. W. Bridgman has presented an influential statement concerning the empirical content of deductive theory. Bridgman's

basic premise is that theory (physical theory at least) achieves validity by being restricted to a description of actually performed physical operations. Physical concepts formulated according to knowledge gained by experiment or observation will never require fundamental revision, but only extension to keep them coextensive with current knowledge.[1] It is possible to define physical concepts without references to observation or experiment, but not without running the risk of being contradicted empirically. For example, concepts may be defined

> in terms of properties, as is so often done in mathematics, and then experiment with the structures we may erect in terms of such concepts to see whether the concepts are useful. We still have operational meaning for our concepts, but the operations are mental operations, and have no necessary physical meaning.[2]

Consequently, according to Bridgman, physicists have come to accept the operational convention which he describes as standard procedure in physical theory.

Every scientifically sound conclusion must have an experimental basis, according to Bridgman. Yet, he does not go on to assert that scientific theories must be based entirely on an empirical foundation. Scientific theories are convenient devices for integrating concepts, enormously expediting the problem of dealing with diverse complicated situations.[3] Any such device, be it verbal or mathematical, is

---

[1]P. W. Bridgman, The Nature of Physical Theory (New York: Dover Publications, 1936), p. 9.

[2]Ibid., p. 11. [3]Ibid., p. 29.

incapable of reproducing experience with fidelity.[4] The conceptual frame itself is inherently lacking in correspondence with material objects. Establishing correspondence involves getting outside the system of language by observation, use, and verification.[5] And Bridgman does not insist that the entire system of integrated concepts be empirically grounded by such outside references. This is particularly clear in his discussion of applied mathematics. He says " . . . let us suppose that I am presented with a set of equations by the theoretical physicist, which he tells me contains the theory of the phenomenon in question." The application of the theory requires more than the formal theory itself. Application requires a "text" telling what the significance of the equations is and how to use them. Certain of the symbols must be linked up with the physical facts of the experimental process so that

> numbers obtainable by the phys[illegible]tion stipulated in the text satisfy [illegible]on when substituted into it. Not only must the text describe the nature of the measurement, but it must also specify the connection between the different symbols in the equation.[6]

Let it first be noted that Bridgman states that the set of equations may be said to contain the theory, but that a supplementary text must be supplied to make the theory operational. Correspondence between theory and reality involves " . . . going outside the system

---

[4] Ibid., p. 19. [5] Ibid., p. 20. [6] Ibid., p. 59.

of the mathematical theory and assuming an intuitive knowledge of the language of ordinary experience."[7] Thus the theory is not identical with its conclusions, according to Bridgman's view. Let us next note that Bridgman rules out any complete correspondence between the theory and the phenomena to be described. To demand one-to-one correspondence would entail a misguided

> . . . conviction of the organic similarity of mathematics and physical experience. In fact the mathematical structure has an infinitely greater complexity than the physical structure with which it deals . . . except for a few isolated singular points [we] relegate the entire mathematical structure to a ghostly domain with no physical relevance.[8]

The basic theory, with its value as an efficient calculating device, and the conclusions drawn from it, are separate and distinct entities. They are linked together by a rulebook or text (usually not explicitly written down) which functions to distinguish between implications that have physical counterparts, and implications that seem to assert conclusions known not to occur physically. The more advanced physical theory has become, the greater the proportion of the formalism that must be rejected. In view of this trend, it would probably seem incorrect to Bridgman to assert that the parts of the formal theory lacking empirical significance must be jettisoned or merely tolerated.

---

[7] Ibid., p. 60. [8] Ibid., p. 116.

Nagel brought out a similar aspect of physical theory when he stated that the complexity of theory enables it to describe a wider range of phenomena than would be possible if every theoretical term had a physical counterpart. Nevertheless, Bridgman's distinct views on correspondence are worth discussing. If the " . . . enormously greater wealth of possibility among the structures of mathematics than in physical models which we can visualize"[9] is to be exploited, then the theorist must not concern himself with attaching empirical significance to each segment of his theory. Indeed he does well not to attempt to erect an idealized physical model for his theory. Physical models themselves bear but imperfect correspondence with real physical systems, so nothing is lost by abandoning them in favor of the much more flexible mathematical theories. The theory itself is then quite separate from the empirical relationships which it establishes. Correspondence between the formal theory and its physical conclusions may be established by " . . . any sort of arbitrary correspondence."[10] This is what Hertz meant when he said that "a belief in Maxwell's theory of light meant nothing more and nothing less than that the observable measurements agreed with the partial differential equations of Maxwell." There is no question of attributing physical significance to the internal structure of the theory.[11]

---

[9] Ibid., p. 62. [10] Ibid., p. 63.

[11] Samuelson, Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2, p. 232.

We may conclude that Bridgman considers physical theory to be an extremely flexible instrument. He makes a very clear distinction between the logical framework of a theory and its empirical counterpart. Bridgman points out that rules of correspondence must supplement the theory in the form of an accompanying "text" which tells how the theoretical logic applies in practice. That correspondence need only be partial, consistent with our principle of "weak empiricism," is recognized by Bridgman's observation of the infinitely greater complexity of the mathematical theory than the physical structure with which it deals. The theorist is free to indicate the factual meaning of his theory as he sees it by supplying appropriate coordinating definitions.

Rationalism and Empiricism

A number of writers have characterized the *a priori* tradition of scientific theory as rationalism.

Edgar Zilsel explains that rationalism, or classical empiricism, maintained an unusually strong hold on investigators in the physical sciences from the time of Hobbes in the seventeenth century until Maxwell in the nineteenth.[12] The guiding idea of classical empiricism is the belief that all significant aspects of reality can be subsumed under a single principle. According to Hobbes, mechanical explanation

---

[12]George de Santillana and Edgar Zilsel, *The Development of Rationalism and Empiricism* ("Foundations of the Unity of Science," Vol. II, No. 8; Chicago: The University of Chicago Press, 1941).

is the key. "All processes consist in movement."[13] Hobbes distinguished sensations such as color and taste from objective qualities. The latter, not being capable of mechanical explanation, were regarded as part of the subjective world. Since they couldn't be reduced to processes consisting of motion they were not part of the real world; they have only a sort of symbolic or subjective relationship to that aspect of reality amenable to scientific analysis.[14]

Variants of Hobbes' philosophy were debated by writers such as Locke and Berkeley, but the crucial distinction between the "real" qualities, such as motion, and the merely "apparent" ones, such as color, became firmly embedded in scientific thought when classical mechanics received its ultimate formulation by Newton. The unimagined success of Newton's formulation provided what was accepted as a complete verification of duality in nature between the real and the apparent. The very comprehensiveness of Newtonian theory seemed to vindicate the distinction. If the world is actually constructed in a way which can be investigated scientifically, then certainly we could hope for no more successful discovery than Newton's. Nor could one hope for a more convincing confirmation of that fond hope. It was natural, therefore, to approach all problems in terms of Newtonian framework, believing that if they could be explained, here was the

---

[13]Edgar Zilsel, "Problems of Empiricism," The Development of Rationalism and Empiricism, p. 65.

[14]Ibid.

theory to explain them. Many important successes followed. To abandon the belief that nature is capable of comprehensive explanation by unified theory because of a few seemingly unimportant phenomena which couldn't be made to fit would indeed have been a faint-hearted retreat from the great strides so recently accomplished. Acceptance of the duality of nature was a much more acceptable alternative, especially in view of the philosophical background which supported such a view. Indeed, the new science could be viewed as a scientific verification of these philosophical views. Furthermore, there was a long tradition of religious thought which distinguished in greater or lesser degree matter and spirit, soul and body. Various religious systems were built around this type of dichotomy. The enormous success of mechanical explanation was wedded to the theological belief in ultimate spiritual realities.[15] Never before had the two diverse worlds, the temporal and the spiritual, both so very important, especially the latter, been so intimately fused into one grand world-view. Each sphere served and complemented the other. It is small wonder that scientists were so confidently committed to sounding out their entire understanding of nature according to the mechanical plan. The simple, yet comprehensive Newtonian model was accepted as a reflection of the master-plan according to which nature was believed to be constructed. A theory of the temporal was accepted as a model for the spiritual; a model of the "outer world" reflected the "inner world."

---

[15] Ibid., p. 68.

Immense successes were secured by the application of mechanical models, and as is characteristic of rationalism, the tendency was to push towards the "illuminating miracle of unity" which research so warmly promised.[16] The subsuming of all nature under the extensive mantle of mechanical explanation had become the goal of science. Ultimate explanation entailed the demonstration that all nature operates according to mechanical principles. Thus the "inner world" which was eventually, in that happy day, to be fully mirrored by the grand, all-embracing mechanics that would penetrate every realm and area of experience.

The realm of human experience was no exception. People hoped to attain a comprehensive theoretical grasp of human affairs as well; social, cultural, economic activity would one day hopefully receive interpretation equally as penetrating as those given the physical world. It is not surprising, then, that rationalist principles were so widely accepted.

For three centuries[17] mechanical explanation continued to be a fruitful approach to scientific investigation. Originally, the ideal had been unified deduction from central principles common to all problems. Later, explanation took the form of mechanical analogies

---

[16]George de Santillana, "Aspects of Scientific Rationalism in the Nineteenth Century," The Development of Rationalism and Empiricism, p. 46.

[17]Zilsel, The Development of Rationalism and Empiricism, p. 93.

of various kinds.[18] So long as it remained successful, it continued to provide the necessary explanation for the "second world behind experience."

Yet it did not remain successful forever.

The Decline of Rationalism

> In the second half of the nineteenth century important physical discoveries resulted in the breakdown of the mechanical theories of light, electricity, and magnetism. As philosophy, since the period of Galileo, had been influenced by physics to a higher degree than by any other empirical science, this physical revolution also reshaped philosophical thinking and the analysis of knowledge.[19]

This happened in the 1860's when Maxwell wrote down his equations for the electromagnetic field. At first he had recourse to a mechanical model, but as time went on mechanical analogies became increasingly unconvincing. Despite the dangerous philosophical consequences, scientists learned that they were better off without cumbersome mechanical models. "If equations, wherever they are derived from, present all the observable facts in the simplest way possible, they may very well fulfill the task of science better than theories attempting to reveal a 'real' world behind the phenomena."[20] Kirchoff, Helmholtz, and Mach wrote in detail about the implications of the decline of

---

[18] de Santillana, The Development of Rationalism and Empiricism, pp. 22-23.

[19] Zilsel, The Development of Rationalism and Empiricism, p. 89.

[20] Ibid., p. 90.

mechanism. When, in 1905, Einstein published his first fundamental paper on relativity, the philosophy based on mechanistic principles was totally undermined. The problems of philosophy debated during the past three centuries came to be regarded as pseudo-problems.[21] "Economic description" of observed regularities, given mathematical formulations suitable to the solution of problems of current interest to practicing physicists, became the realistic, if less grand, concern of scientists. "Analysis of experience shifted from ideas to statements."[22] Abstract words, useful syntactically, were substituted for abstract ideas, with no more concern for the underlying support of qualities. Modern empiricism "restricts itself to analyzing the methods by which statements are tested and verified. . . ."[23]

There occurred a shift in scientific goals towards verification of statements of empirical regularities. The decline of rationalism eventually had a profound effect upon our attitudes towards reliable knowledge of human affairs as well. Sooner or later, social scientists would have to learn the great lesson taught by the decline of rationalism. They would have to learn that the mere possession of simple yet satisfying and comprehensive systems of explanation was not sufficient, that the only reliable knowledge was that which could somehow be verified by observation.

---

[21] Ibid., p. 93. [22] Ibid., p. 70. [23] Ibid., pp. 66, 67.

Significance of the Decline of Rationalism

Perhaps modern physical science had turned down a path which led away from the interests of the wider community. "Comprehensive philosophical reinterpretations of the import and nature of physical science" had been tried a number of times, and were found to be "highly dubious" in value.[24] The question naturally arises, does the experience of the natural sciences lead necessarily to the conclusion that scientific investigation is misguidedly seeking solutions to pseudo-problems if it purports to retain a dimension of meaning beyond the proximate problems with which it deals? To say that logical deduction from judiciously chosen first principles is inadequate as scientific procedure is not the same as saying that theory has no meaning beyond the immediate subject matter. Indeed, all hope of reaching an ultimate philosophical benchmark is gone. The great lesson of the decline of rationalism is that faithful and continued deference to new empirical facts is a continuous requirement.

According to classical empiricism, the necessity of observation reached a terminus when the theoretical synthesis was achieved; from then on scientific activity was largely an *a priori* operation. Modern empiricism, on the other hand, entails a never-ending interplay

---

[24]Nagel, *Structure of Science*, pp. 337-338.

between observation and rational construction. The touchstone for fruitful theory is the anticipation of unsuspected empirical regularities and the subsuming of previously unaccountable data. The theory is defunct when it ceases to be the instrument of this ongoing process. This is far different from the old ideal which strived for an empirical plateau where the view is complete, and no further deference to fact is necessary. In the old view, the touchstone of a good theoretical synthesis is the attainment of a final formulation which does not stand in need of further winnowing to conform to new data, but rather could be counted on to subsume all significant fact, at least by analogy, and would furthermore give a satisfactory philosophical picture. The philosophical picture, or explanation of underlying reality was, under the old ideal, the touchstone for theory. This is evidenced by the fact that data which did not conform to the "objective" criteria established by the theory (color or taste for the mechanist of Hobbes' day) were branded as "subjective" qualities not appropriate for theoretical treatment. The principles so impressively established by the grand theory became the criteria or touchstone by which the significance of facts were judged. Thus the Platonic duality of the real vs. the merely sensational grew up to rationalize the choice of significant facts.

Modern empiricism insists on a complete reversal of emphasis. Facts are never assessed according to how well they fit the theory--the theory is judged as to how well it handles the facts. The only theory

that becomes terminal with respect to data is a sterile theory. The philosophical tidiness of the theory is never accepted as a touchstone for its completeness or validity, and is never allowed to stand in the way of reformulation. In the next chapter, when these remarks are applied to the social sciences, we say at this juncture that a theory which, in accordance with procedures identified with classical empiricism, allows the philosophical overtones to dictate over facts, has degenerated into ideology.

None of these considerations are particularly controversial. But neither are they conclusive. They do not establish a particular philosophical orientation, nor do they signify that all philosophical interpretations or intentions must be abandoned. Of course, there have been interpreters who have argued each of these alleged implications. Yet, consider if we have discovered any reason for excluding a deliberate and conscious philosophical or cultural dimension from scientific theory, so long as that theory satisfies all the requirements of modern empiricism, and so long as the philosophical or explanatory dimension does not impede the progress toward solution of problems near at hand. If the validity of the philosophical dimension were admitted, than that dimension or its plausability, would have to be tested by the ongoing ability of the theory to perform its empirical tasks. The decline of the early form of rationalism does not imply that wider concerns are spurious, it

merely shows that the early investigators had an incomplete method of testing all the objectives of their theory.

The wider concerns of modern rationalism have been shared by a number of important writers. It is well known that Einstein, one of the original contributors to quantum theory, parted company with his colleagues when they carried the theory in directions which he found distasteful. "In our scientific expectation we have grown antipodes," Einstein wrote to Max Born. "You believe in God playing dice and I in perfect laws in the world of things existing as real objects, which I try to grasp in a wildly speculative way."[25] Born states that Einstein wished, in his later years, to preserve the causality of classical physics and to relegate the statistical phase of investigation to a secondary position in the theoretical formulation. Born concedes Einstein's right to defer to philosophical considerations, since he bases them on his "fundamental results" which "stand like a rock." He concludes significantly: "That his opinion in this matter differs from mine is regrettable, but it is no object of logical dispute between us. It is based on different experience in our work and life."[26]

---

[25]Max Born, "Einstein's Statistical Theories," Albert Einstein: Philosopher-Scientist, p. 176.

[26]Ibid., pp. 176-177.

Rationalist Goals in Modern Science: Schrödinger's Testimony

Erwin Schrödinger is another modern theoretical scientist who holds the wider goals of modern rationalism, and he has written very effectively about their place in modern science. Much can be found in Schrödinger to support the view that the theorist has the right, even the responsibility, to make his contribution meaningful to the community as a whole. As we have seen, asking whether or not cultural concerns fall within the province of scientific theory is to raise all the problems connected with rationalism and classical empiricism. Metaphysical difficulties intrude themselves largely when theorists take seriously the cultural implications of their work. Real progress is impeded and arbitrary distinctions and pseudo-problems appear. Such difficulties are removed when theory is regarded solely as a tool which is most efficient when designed only for the most specific empirical purposes. It is not too difficult to recognize a very troublesome dilemma here. Schrödinger recognizes it, and his manner of dealing with it is most interesting.

On the one hand, Schrödinger holds physical theory to be much more than an efficient calculating device. He is opposed to those who say: "Let us only have differential equations or other

mathematical procedures and a recipe for deriving from them and from a set of actually performed observations all statements about all future observations of which foreknowledge is in principle at all possible. [All else is only metaphysics.]"[27] That nature is comprehensible is a fundamental tenet of the Western world-view. For reasons both technical and philosophical he insists that "what matters to us is essentially the picture which is eventually or at any given step obtained; we are interested in the shape itself of the interconnexions."[28] The picture that Schrödinger envisages is to become "ever more distinct, lucid and clearly understood in its interrelations." This goal would be "utterly destroyed" if we were forced to omit everything that could not be empirically ascertained.[29] As classical empiricists realized, and as their critics have pointed out, the "comprehensive picture" is a frame of reference in itself, and admits of no direct empirical verification. Yet Schrödinger holds certain of the essential goals of rationalism, despite the severe attacks they have been subjected to.

Schrödinger acknowledges that a scientific view requires that an immense amount of human knowledge saturated with cultural meaning be renounced. All the small segments of nature, all that is unique

---

[27] Schrödinger, "On the Peculiarity of the Scientific World-View," What is Life? p. 195.

[28] Ibid., p. 191.

[29] Ibid., p. 197.

in nature, disappears from a scientific view.[30] Science is also

> lacking in everything and anything the meaning of which lies exclusively in its relation to the conscious, perceiving and feeling self. I am referring to the ethical and aesthetic values, values of any kind, anything that refers to meaning and purpose of the course of events. For everything that enters into this world-picture willy-nilly takes the form of a scientific proposition; and as such it becomes false.[31]

Science becomes the "Masque of the Red Death" because, when one looks behind its impersonal formulations, one finds that it says of human values--nothing at all.[32] It is well to remember, therefore, that, despite the wider concerns of a theorist, a scientific theory will always seem remote from most human experience, whether he be Mach, Bridgman, a rationalist or logical empiricist.

Schrödinger makes a clear distinction between a highly abstract science and a science which has abandoned its cultural frame of reference. "For the spirit is never the object of science. But the sciences are a product of the spirit in which they are conducted . . . [and] are meaningless outside their cultural context."[33] Schrödinger says that scientific constructs presently being erected are "destined eventually to be framed in concepts and words that have a grip on the

---

[30] Ibid., pp. 202-203.

[31] Ibid., p. 227. Italics in original.

[32] Ibid., p. 217-218.

[33] Schrödinger, "The Spirit of Science," What Is Life? p. 232. Schrödinger, "Are There Quantum Jumps?" What Is Life? p. 133.

educated community and become part and parcel of the general world-picture."[34] Schrödinger here stresses concepts and patterns of thought rather than the naive reifications that plague every theoretical discipline. He accuses modern physical scientists of frequently settling for convenient calculating devices which are useful for problem solving but run the risk of severing physical science from the best of scientific and philosophical contributions of the past, as well as from meaningful cultural connections. Modern writers are all too ready to condemn the obfuscation wrought by the three centuries of rationalism and old-fashioned empiricism, and to eulogize the progress brought by the rejection of these principles. The very great significance of the impact itself on the general Western world-view makes it easy to overlook and take for granted. It should also be noted that, since modern critics of rationalism do not deny the philosophical and cultural impact of science in the past, they are hardly in a position to quarrel with Schrödinger's judgment about the future.

In our discussion of rationalism and logical empiricism we have already anticipated Schrödinger's resolution of the two strands of our problem: empirical validity and "cultural significance." Schrödinger deals with the problem most explicitly in the section called "Prediction--touchstone or goal?"[35] There he says, "Fore-

---

[34]Schrödinger, "Are There Quantum Jumps?" What Is Life? p. 133.

[35]Schrödinger, "On the Peculiarity of the Scientific World-View," What Is Life? pp. 190-192.

telling, predicting, observations is only a means for us to ascertain whether or not the picture that we have framed is correct."[36] Schrödinger, of course, accepts the empirical test as the only way of ascertaining the reliability of knowledge. A theory which fails to give accurate descriptions and correct predictions from exact experimental data must be abandoned. But beyond the purely observational functions, the theorist should be permitted to examine other significant relationships.[37] To this end the theorist should have free rein to introduce "unverifiable mental constructs" which establish a connection between the observed data and the cumulatively developing scientific tradition.[38] Accordingly, he must retain a free hand in going beyond the realm of observation to freely construct whatever concepts he needs for a satisfactory theory. His goals and criteria differ from Bridgman's, but together they share the opinion that a theory is wider than the empirical content of its assumptions and conclusions.

The classical empiricist lacked the empirical touchstone; for him the picture was both touchstone and goal. Schrödinger remains a rationalist in his fundamental concern; he has not gone the full route with modern logical empiricists by making prediction both touchstone and goal of scientific theory.

---

[36] Ibid., p. 191. [37] Ibid.

[38] Ibid., pp. 192-193. Schrödinger, "Are There Quantum Jumps?" What Is Life? pp. 132-133.

Summary

In this chapter, we have attempted to interpret the historical development of deductive theorizing, with primary emphasis on the physical sciences. We have seen how extra-empirical concerns recognized in modern economic theory were once very prominent in scientific analysis. Pursuit of these so-called rationalist goals was stimulated by the great success of the simple, yet comprehensive Newtonian theory. During the nineteenth century, however, Newtonian mechanics failed in competition with newer, non-mechanical theories to incorporate the growing diversity of empirical knowledge. In consequence, the rationalist goal of a unified picture of nature was seriously undermined. Henceforth, scientific progress would be understood to be a tentative process depending upon empirical verification each step of the way.

Despite the great new emphasis on observation, certain scientists have retained their insistence on scientific formulations fully consistent with, but not restricted to, empirical verification. These extra-empirical goals, defended by Schrödinger and others, have had great importance to early and contemporary economists. Consequently, a valid methodology consistent with their aims is highly significant.

The methodology suggested by Schrödinger is in large measure consistent with the wider concerns of earlier theorists. Schrödinger's insistence on a unified physical picture leads him to advocate the use of free mental constructions, many of which are not empirically verifiable. Pursuit of wider, extra-empirical goals is justified so long as the work is fully consistent with empirical data. In other words, prediction is the touchstone for theory formulated with wider goals in mind.

Schrödinger's methodology resembles the "weak empirical principle" adopted in this paper. Adoption of the principle widens the scope of theory beyond the more narrow confines prescribed by the "strong empirical principle." While this broadening of theoretical scope recalls the possibility of the theoretical abuses discussed at the beginning of the paper, nevertheless it can be seen that these abuses do not flow from the presence of nonverifiable constructs as such.

Bridgman's principle of operationalism corroborates the position that not all segments of theory must be verifiable. His insistence on the necessity of an interpretive "text" for the theory, and his prominent distinction between the logical framework and its empirical counterpart are consistent with the empirical position of this paper, even though Bridgman does not share any interest in extra-empirical goals.

In the next chapter, we shall consider some examples from the literature of economics that illustrate the application of rationalist goals and principles just examined. In the following chapter, an attempt is made to show that deductive theory can have legitimate goals that extend beyond the scope mapped out by logical empiricists.

CHAPTER VI

THE THEORETICAL SYSTEM IN ECONOMICS: A REASSESSMENT

The Changing Role of the Theoretical System

Early economic analysis was often "of magnificent cast, ambitiously attempting to analyze the growth and development of entire economies over relatively long periods of time--decades or even centuries."[1] Dubious assumptions and "inspired over-simplification" were characteristic. Later, with the advent of marginalism and positivism, only "obviously" justifiable assumptions were made.[2] These assumptions constituted the empirical base of economics, the remaining task being a logical or syntactical one. Economic thought became more "colorless," to use Veblen's expression. More recently, as analytic techniques have become more refined, "the approach is so much more cautious that the problems handled must in general be considerably more humdrum and pedestrian."[3] No longer do economists discuss the "magnificent" topics of classical analysis. No longer, apparently, do they feel free, like the classical economists, to construct panoramic views

---

[1] William J. Baumol, Economic Dynamics (2nd ed.; New York: The Macmillan Company, 1959), p. 13.

[2] Ibid. [3] Ibid., pp. 13-14.

of society in its economic aspect, thereby meeting the need of their lay contemporaries to understand the forces which seemed to shape their destinies. No longer is "wild speculation" and "free construction," beyond suspicion as a technique of analysis. Nagel has expressed current feeling in these terms:

> there is no one 'big thing' which, if known, would make everything else coherent and unlock the mystery of creation. In consequence, many of us have ceased to emulate the great system-builders in the history of philosophy. In partial imitation of the strategy of modern science, and in the hope of achieving responsibly held conclusions about matters which we could acquire genuine competence, we have tended to become specialists in our professional activities. We have come to direct our best energies to the resolution of limited problems. . . .[4]

Many writers believe that a major consequence of the new preeminence of empiricism in scientific analysis has been the displacement of the cultural focus which pervaded early economic analysis. Its broad, imaginative constructs defy empirical corroboration. Furthermore, the universal premises which form the basis of interpretive systems lack the capability of generating the specific, testable statements so characteristic of empirical hypothesis.

---

[4]Ernest Nagel, Logic Without Metaphysics and Other Studies in the Philosophy of Science (Glencoe, Ill.: The Free Press, 1956), p. 4.

Accordingly, Nagel goes on to say

> [It is] my impression that the majority of the best minds among us have turned away from the conception of the philosopher as the spectator of all time and existence, and have concentrated on restricted but managable questions, with almost deliberate unconcern for the bearing of their often minute investigations upon an inclusive view of nature and man.[5]

Nagel is generally satisfied by the displacement of the broad rationalist goals by the narrower concerns of modern analytic philosophy, although he recognizes the influence of basic intellectual commitments.[6]

The goal of the system-builder--the "rationalist ideal" which was the inspiration of every writer who attempted to interpret current events to the community at large--was by its very nature alien to the observer's skill. Basically, "the spiritual attitude of a given generation of economists is . . . in good part a special outgrowth of the ideals and preconceptions current in the world about them."[7] Details of observation were secondary in importance to their "spiritual attitude,"[8] their primary concern. The rationalist goal was a logically articulated conceptual framework which exhibited the relationship of observable events to the ideals and preconceptions of the writer. The

---

[5] Ibid. Italics added.

[6] Ibid., pp. 4-5.

[7] Thorstein Veblen, "The Preconceptions of Economic Science," What Veblen Taught, ed. Wesley C. Mitchell (New York: The Viking Press, 1947), p. 42.

[8] Ibid.

theoretical process always involves great abstraction. Yet for economists of the rationalist era, the preconceptions and ideals provided were the criteria for legitimate abstraction. Which observable things were important and which were to be neglected as atypical was decided by the particular conceptual framework that reflected the ideals of the writer. For example, which facts were central in an explanation of capitalism was different for Adam Smith and Karl Marx, since their basic attitudes towards capitalism were radically different. The rationalist frame of reference was not only the goal of the writer, but also the touchstone by which he sifted and sorted the essential facts from the non-essential. Beyond this stage of analysis, the writers were not "moved to push their analysis of events or their scouting of phenomena."[9]

The Intrusion of Ideology

If the theorist is a particularly effective spokesman for his generation, such as Marx, his writings may become immune from attack by contradictory events, since it is not contradiction of theory by events that counts, but contradiction of ideals and preconceptions. As long as attitudes remain intact, no degree of historical contrariety of events will shake the theory, since it is attitudes that are both

---

[9] Ibid., p. 113.

touchstone and goal of theory.

> . . . having once been accepted and assimilated as real, though perhaps not actual, [the theory] becomes an effective constituent in the inquirer's habits of thought, and goes to shape his knowledge of facts. It comes to serve as a norm of substantiality or legitimacy; and facts in some degree fall under its constraint. . . .[10]

When a theory attains the status of orthodoxy, it serves the popular need to understand the forces influencing the lives of individuals. Orthodox theory becomes ideology because of the fear of permitting sound judgment and responsible scientific knowledge to undermine the comfort of knowing one is right in his understanding of events of importance to him. Theory becomes the servant that shores up ideology. The very existence of ideolgy is a testimony to the cultural importance of social theory.

Adam Smith's Economic System

Some writers have been successful in combining the rationalist explanatory goal with the modern empirical goal. Viner[11] and Veblen both emphasize Adam Smith's success at this synthesis of theoretical purposes. Smith's rationalist views were his "accepted ground of finality--the ground to which all his speculations on human affairs

---

[10] Ibid., p. 108.

[11] Jacob Viner, "Adam Smith and Laissez Faire," Essays in Economic Thought: Aristotle to Marshall, ed. Joseph J. Spengler and William R. Allen (Chicago: Rand McNally and Company, 1960).

converge."[12] His philosophic goal was a "detailed application to the economic world of the concept of a unified natural order, operating according to natural law, [which], if left to its own course [would produce] results beneficial to mankind."[13] This was Adam Smith's unified picture of reality--his rationalist ideal.

Smith was highly modern in the other major aspect of his work. "In no instance does Smith rely heavily upon his assertions as to the existence of harmony in the natural order at large to establish his immediate point that such harmony exists within the specific range of economic phenomena which he is at the moment examining."[14] He was very much interested in the special technical problems of economics requiring expert knowledge. And he was fully aware of the need to bring empirical evidence to bear on specific problems.

Viner finds a highly rationalist emphasis in Smith's Theory of Moral Sentiments and a more highly developed empiricist concern in the Wealth of Nations. He interprets Smith's more mature, empirically-oriented work as a great improvement over the earlier book.

---

[12]Veblen, What Veblen Taught, pp. 79-80.

[13]Viner, Essays in Economic Thought, p. 306.

[14]Ibid., p. 313.

Yet Viner does not try to reason away the rationalist dimension of the Wealth of Nations. Nor does he, or Veblen, censure Smith for this aspect of his work. Viner explains clearly why the continued presence of philosophic interests in the Wealth of Nations does not compromise its scientific merit. Commenting, in effect, on Smith's faithfulness to knowledge gained from observation as a scientific touchstone for his systematic thinking, Viner says

> There is a longstanding feud between sweeping generalization and run-of-the-mill factual data, and when Smith brought them together he did not always succeed in inducing altogether harmonious relations. But . . . it is to his credit that when there was sharp conflict between his generalization and his data, he usually abandoned his generalization.[15]

Adam Smith always attempted to make his generalizations dependent upon knowledge carefully drawn from observation. According to Viner

> Smith's argument for the existence of a natural harmony in the economic order, to be preserved by following the system of natural liberty is, in form at least, built up by detailed inference from specific data and by examination of specific problems, and is not deduced from wide-sweeping generalizations concerning the universe in general.[16]

---

[15] Ibid., p. 317.

[16] Ibid., p. 313.

Let us imagine, for the moment, that Smith's works are designed to present his ideas in two separate stages. Consider the Theory of Moral Sentiments, as a tentative and highly speculative social theory. Many of the propositions upon which it rests are untestable, even though they are critically important. At this stage, however, he merely presents an interpretation of society which, if correct, will have implications for the right conduct of human affairs. We may defer judgment about Smith's social theory until he has given us reliable evidence for its underlying validity, and for its inherent superiority over other prominent competing views of society. In the Wealth of Nations, Smith remains quite faithful to the line of thinking he has set out earlier, but only as a guide to investigation. While Smith is guided by his philosophy in choice of subject matter, he is not blinded by that philosophy in his interpretation of what he sees. His method is very largely one of testing by objective observation. This is the change of focus which distinguishes the Wealth of Nations from the Theory of Moral Sentiments.[17] In carrying out this empirically-oriented development of his philosophy, Smith discovered numerous exceptions to his original orientation. Viner enumerates the "wide divergence between the perfectly harmonious, completely beneficent order of the Theory of Moral Sentiments and the partial and limited harmony in the economic order of the Wealth of Nations.[18]

---

[17] Ibid., p. 308.

[18] Ibid., p. 316.

Smith's work provides a good model for theory which would operate at the rationalist's level of explanation and the empiricist's level of description. It is primarily "a tract for the times, a specific attack on certain types of government activity which Smith was convinced, on both a priori and empirical grounds, operated against national prosperity. . . . "[19] Smith was out to attack abuses which existed in his own society, and to substitute a coherent and plausible alternative view of society, which, while not the final word, would at least be superior both as philosophy and as empirical science to theories in vogue at the time. Smith's work reminds us of Schrödinger's question: "Prediction, touchstone or goal?" Smith, like Schrödinger, seems to answer: explanation the goal, empirical analysis the touchstone.

J. M. Keynes' Economic System

Adam Smith provided the first great tableau of economic society, as Heilbroner points out.[20] J. M. Keynes has provided the latest. His General Theory has had the same basic kind of impact on theory and on society as the Wealth of Nations. Keynes produced his work during the last great crisis of capitalism, the depression, which raised basic questions concerning the belief in impersonal forces as the fundamental arbiters of social development. Many writers

---

[19] Ibid., p. 319.

[20] Robert L. Heilbroner, The Worldly Philosophers (New York: Simon and Schuster, 1953), p. 27.

had spoken of capitalism as if it developed according to some internal logic of its own. Smith, Marx, Schumpeter, Veblen, all wrote in this vein, some optimistically, some predicting the disintegration of capitalism. Yet they shared the common characteristic of seeing the capitalist economy moving in some discernible direction.

Keynes, on the other hand, saw no internal logic inherent in capitalism, directing it along established lines of development. To him, capitalism was headed no place in particular, at least not of its own accord. Keynes' basic explanatory principle was that the economy responds to forces within the control of the society which has chosen capitalism, which alone is responsible for its economic condition.

Earlier system-builders had all viewed their explanatory task in a fundamentally different light. Following the traditional rationalist mode of explanation, they sought to construct the mirror which would reflect the orderly, rational underpinnings of society. They sought to discover the inherent laws governing society in its economic aspect. Denial of such forces would have appeared to them as a denial of the explanatory aspect of theorizing. No one before had thought to make Keynes' basic idea the central element of an explanatory system.

Keynes, however, took the position for the first time that order in society was not to be found--it had to be created. In so doing, he eliminated the last vestige of teleology from economic theory. This was a great explanatory accomplishment. It opened the door to a confident search for methods of managing the economic affairs of a society by means acceptable to it. The randomness that Keynes found inherent in the capitalistic system has proved to be the key to modern social policy. Randomness becomes flexibility and scope for human control over society in its economic aspect--a social interpretation never before advanced in an explanatory system. Viewed as an explanatory system in the rationalist tradition, Keynes is intellectually more revolutionary than Marx. Marx's teleological system was conventionally similar to earlier explanatory systems by comparison with the intellectual style of Keynes.

For those predicting the end of capitalism, the depression could easily have been the final chapter, being as it was the culmination of so many diverse developments. But the depression did not fulfill the prediction of the downfall of capitalism. It destroyed the credibility of the optimistic systems, but failed to vindicate the pessimistic ones. Yet, there being no better explanations, people adhered to the best old ones in sight. They embraced either the Marxist view, or the older equilibrium theories. But such basic outlooks could survive only as ideologies; neither had sufficient historical justification to sustain them reasonably.

The feeling that something was lacking in classical theory as a body of knowledge having adequate relevance in the culture which relied upon it had been articulated by many economists from Malthus forward, but without affecting the basic, influential viewpoint of the society. Joan Robinson has pointed out that Keynes stated his theory in a polemical way because of the deep-seated, highly ingrained influence of ideas which grew out of classical theory, and the need to get his own ideas into the main stream of social thought. A theory was required to give direction to the nebulous dissatisfaction and bewilderment engendered by the depression. An intellectual construct as imposing as the one currently prevailing had to be presented.

Keynes found the deductive framework, embodying careful observation, statement of functional relationships, and ingenious free construction, along with numerous side comments, ideal for his purposes. Purely empirical theorizing about disaggregated particulars would not have been sufficient if his theory were to serve the social purpose he had set for it. Furthermore, the theory would not have been nearly so seminal in mapping out an analytic program influential to this day. Nothing but the freely constructed, broad deductive framework possessing only partial empirical correspondence, would have sufficed.

The Keynesian system has been successful for a number of reasons. It is scientifically sound because it has made testable predictions

and has shown great capacity to generate fruitful empirical hypotheses. It has been corroborated because the trial of its most basic policy implications have proved effectual. It has remained plausible in view of historical events. It is a basically attractive explanation because it entails a conviction of human influence over events of major importance to society. The Keynesian system has been successful as a supplement or successor to competing systems because of its equally impressive intellectual stature.

While Keynes radically altered the course of economic thought from the inherent lawlike nature of the physical sciences and previous economic systems, nevertheless, it has the basic characteristics of theory in the rationalist tradition. It gives a fundamental interpretation of society to the community, and it continues to offer empirical hypotheses which meet with considerable success. The historical success of the theory as a reflection of society and as an empirically predictive tool have provided ample touchstone for the underlying explanation embodied in the Keynesian system.

Internal Autonomy of Theoretical Development

George Stigler has described another dimension of the development of economic analysis that is closely related to the foregoing discussion.[21] Stigler holds that the major problems facing the theoretical

---

[21]George J. Stigler, "The Influence of Events and Policies on Economic Theory," Papers and Proceedings of the American Economic Association, Vol. L, No. 2 (May, 1960), pp. 36-45.

economist have been pretty clearly identified for nearly two hundred years. During this period of time economic theory has changed, not so much in the problems with which it deals, but rather in the techniques with which they are handled. The body of economic theory has grown in a cumulative fashion, each generation benefiting from the contributions added by former generations. True, a number of economists have remained outside the main stream of economics and still exerted great influence. Nevertheless, their work has had little importance for the development of economic theory except insofar as it has added to or exposed the shortcoming of the "ruling" theory. In the aggregate, each influential economist contributes to the strengthening of what Marshall called the "engine of analysis."

Theoretical changes do not come from major new events or problems, according to Stigler.

> The dominant influence upon the working range of economic theorists is the set of internal values and pressures of the discipline. The subjects for study are posed by the unfolding course of scientific developments.[22]

A new development in the theory of equations is likely to have more impact on economic theory than the decline of an economy or the rise of a Hitler. The reason is simply that most of these great world events are quite routine from the point of view of economic theory.[23]

---

[22] Ibid., p. 40.

[23] Ibid., pp. 38-39.

(Stigler hedges his argument somewhat in connection with Keynes' General Theory. Possible exceptions such as this, however, do not seem to affect the weight of his argument.)

The universal problems of economic analysis provide the focal point, and the internal analytical pressures of the discipline set the style of treatment the problems will receive. Widely differing philosophical views have had their impact on society. But what has survived of the theories which embodied these views has been determined by the success of these theories in advancing the analytical and empirical solutions to the basic problems named by Stigler. A theory becomes a part of the main stream of economic analysis on the basis of its narrowly defined economic contributions, not on the basis of its wider implications.[24]

The success or demise of an economic concept is necessarily a complex event. For example, cardinal utility was displaced because (1) measurable utility lacks the support of psychological theory, (2) because empirical measurement proved impossible, (3) the concept has proved weaker than ordinal utility or indifference analysis as a theoretical term. The latter have much greater theoretical import. Another example is the wages fund doctrine, which disappeared because it was empirically discredited, because its ideological implications became unacceptable, and because it had no place in the marginal analysis, a conceptual framework with vastly richer theoretical import. Thus, a concept may fall for several reasons: if it becomes unsatisfactory as

---

[24]This "main stream" may be taken to coincide with the "family tree of economics" printed in the inside back cover of Samuelson's Economics.

a cultural interpretation, if it fails when considered as an empirical statement, or if it does not survive as a theoretical construct in a wider theory which requires its presence logically.

The last criterion is the most decisive one. For even though empirical validity is the ultimate touchstone of deductive theory, only parts of the theory require empirical correspondence, not each concept separately. Concepts which are introduced originally for philosophical or for purely empirical reasons frequently survive as logical segments even when they fail in their original purpose. Stigler makes a similar point:

> Even where the original environmental stimulus to a particular analytical development is fairly clear, as in Ricardo's theory of rent, the profession soon appropriates the problem and reformulates it in a manner that becomes increasingly remote from current events, until finally its origin bears no recognizable relationship to its nature or uses.[25]

It is characteristic of economic theory to build cumulatively in response to contributions of successive generations of writers. Frequently, the contributions lose much of their original identity before their final position is determined. Nevertheless, the contribution of each economist is measured in good part by what he has contributed to the "engine of analysis." Smith's explanatory and empirical contributions are still very important, but his

---

[25]Stigler, Papers and Proceedings of the American Economic Association, Vol. L, No. 2, p. 41.

continued relevance and vitality is also due in no small measure to the concepts that have been absorbed into the analytic makeup of economists. Much the same can be said for Keynes. It cannot be predicted what form Keynesian concepts will have in the future, but they will be of greatest use to the extent that they have provided economists with tools that can be modified for their own purposes.

Economic analysis, Stigler shows, has strong roots in the past. It has developed primarily in response to pressures and potentialities generated internally, and in considerable measure independently of external events. A great deal of current economics, especially analysis in the deductive tradition, has been fundamentally conditioned by the work of earlier writers. Even the most original writers, such as Marx and Keynes, owe a great deal to the intellectual mold developed by pioneers like Smith, Malthus, and Ricardo. That current economic analysis still retains strong traces of the rationalist goals of early writers is not surprising, considering the strong link with the past that Stigler has identified. The empirical techniques of deductive theory have tended generally to retain a position for the traditional contributions by conforming to the weak empirical principle described in earlier chapters. The attributes of economic analysis described by Stigler owe their continued relevance to the development of empirical technique, and economics is probably fortunate for it.

Summary

Examples have been provided in this chapter which illustrate the diversity of explanatory levels found in the theoretical systems of economics. Sound methodology demands that the fundamental explanatory level be the empirical, in that description and prediction of observable regularities is the only means of obtaining unbiased, interpersonal judgment about a theory. Empirical data must be regarded as basic because it is the only standard that exists in any sense "outside the observer." The "decline of rationalism," spoken of at the beginning of the chapter is closely related to the elevation of the empirical standard, or touchstone, for the evaluation of theoretical work.

In our discussions of the theories of Adam Smith and J. M. Keynes, we argued that empirical predictions need not be the sole function of theory. Following Schrödinger's line of reasoning, we held that an extra-empirical level of explanation is a legitimate theoretical goal. A theoretical system can be structured so as to embody a particular cultural outlook of potentially great consequence. Such an outlook may become accepted if it is plausible on its own merits. Yet, acceptance of a cultural viewpoint becomes ideology when the responsible theory fails as a tool of prediction and an interpreter

of observable historical events. Schrödinger would say that the explanatory level of the theory had lost its empirical touchstone. Marxian theory is open to this interpretation.

The great economic systems of the past and their extraordinary influence on society testify to the importance of the explanatory dimension of economic theory, both for good and ill. That these theories have had important empirical value suggests that the two levels of explanation are compatible. That they are compatible depends in large measure on the fact that economists have had recourse to what has been called the weak empirical principle in this paper. In other words, the scope or goals of theory depend in part on the theoretical structure employed. The strong empirical principle, explained and advocated by Samuelson and others, would limit the scope of theory to more exclusively empirical matters. This they believe would be a good thing; the writer disagrees.

Additional reasons for disagreeing with the strong empirical principle stems from the nature of the historical development of economic theory. Stigler explains that economics owes much to its historical antecedents. In a variety of ways, economics has inherited a set of logical constructs and concepts that have only partial empirical correspondence. Their place in economic theory, admittedly shifting over time, has found a secure place in economics.

Too much would be lost in eliminating them, especially in view of the sound methodological principles, and experience, behind them.

In the next chapter, we shall attempt an application of our principles to certain areas of theory in anthropology and psychology.

CHAPTER VII

PROBLEMS OF DEDUCTIVE ANALYSIS IN ANTHROPOLOGY AND PSYCHOLOGY

Problems of Theory in Physical Anthropology

Physical anthropology provides a notable example of a field which began with a very close attachment to observation and measurement, eschewing elaborate theoretical constructs. "The assumption seems to have been that description, . . . if accurate enough and in sufficient quantity, could solve problems of process, pattern, and interpretation."[1] Classification of specimens according to increasingly elaborate schemes of measurement was the means by which physical anthropologists sought understanding of primates and human races. Understanding progressed rapidly for a period of time, but eventually began leading to contradictory results.[2] The physical anthropologists had been dealing with what were to become the parameters of later research.[3] For example, suppose

---

[1]S. L. Washburn, "The Strategy of Physical Anthropology," Physical Anthropology: 1953-1961, ed. Gabriel W. Lasker, Yearbook of Physical Anthropology (Cordoba, Mexico: Instituto de Investigaciones Historicas Universidad Nacional Autonoma de Mexico, 1964), Vol. 9, p. 2.

[2]Ibid.

[3]Samuelson, Foundations, pp. 10-12.

that $\alpha_1, \cdots, \alpha_m$ represent m dimensional skull characteristics attributed to α-type man, $\beta_1, \cdots, \beta_m$ the skull characteristics of β-type man, and so forth. Early strategy dictated greater and greater metrical and morphological refinement, making m larger and larger, and also swelling the number of human types. Diminishing returns could easily be predicted however: "Weidenreich (1946) objected to adding the blood groups to the traditional anthropological characters, on the ground that this would make the theoretical total of races 92,780.[4]

Suppose, for example, the skull characteristics of Java man, χ_J are $(j_1, j_2, \cdots, j_m)$, where j_i indicate a certain range of measured values for each characteristic. So $\chi_J = (j_1, j_2, \cdots, j_m)$, and so on, for other human types. Refinement was extended further and further, until Weidenreich was prompted to observe that if blood types were added, for example, j_n for Java man, then the number of human types would approach a hundred thousand.

A discussion of Samuelson's anticipates the physical anthropologist's dilemma. He says, "it may be pointed out that these resulting functions between unknowns and parameters could have arisen from an infinity of possible alternative sets of original equations.[5] In our example, $\chi_J = (j_1, j_2, \cdots, j_m)$ is the "resulting function," j_i are parameters, and χ_J is an "unknown" specimen identified as Java man by the function. The "set of original equations" will be discussed shortly.

---

[4]Washburn, Physical Anthropology: 1953-1961, p. 10.

[5]Samuelson, Foundations, p. 11.

Samuelson is saying that x_j could be the result of an indefinitely large number of combinations of j_i. Furthermore, increasing the number of parametric observations could swell the number of human types to unmanagable proportions, as Weidenreich found to be true in practice. Samuelson continues: "It is hardly enough . . . to show that under certain conditions we can name enough relations (equations) to determine the values of our unknowns. . . . In fact, our theory is meaningless in the operational sense unless it does imply some restrictions upon empirically observable quantities, by which it could conceivably be refuted."[6]

Here is where our "set of original equations" comes in. That set is "a specified set of relationships imposed upon the unknowns by assumption or hypothesis."[7] Samuelson says that a set of restrictive assumptions is needed from which observable properties can be deduced. The deductions are then compared with observation to check the validity of the theory. The purpose of such a theory is twofold. It eliminates Weidenreich's problem by restricting the range of matters to be considered by the anthropologist. It provides a framework from which testable hypotheses can be deduced.

The investigator who would gain these advantages, however, must commit himself in advance to the construction of a conceptual framework whose logical consequences are not known in advance to be empirically

---

[6] Ibid., p. 7. Italics added.

[7] Ibid. Italics added.

correct. He must face up, in other words, to the italicized portions of Samuelson's remarks. Success in following Samuelson's program "will hinge upon the degree to which factors relevant to the particular investigation at hand are brought into sharp focus."[8] This is the essence of simplifying abstraction. The manner in which selected variables are brought together determines the generality and capacity for generating verifiable hypotheses of the theory. "Within the framework of any system the relationships between our variables are strictly those of mutual interdependence.[9] Mutually interdependent relationships mean that a change in the value of any variable or parameter in the theory has logically determinate implications concerning the reactions on the part of all other variables. Of course, laboratory manipulation of variables makes no more sense in anthropology than in economics. However, economists have devised any number of substitutes for the laboratory, and similar opportunities probably abound in anthropology as well. This point will be touched upon again in the brief comments on archaeology, which follow shortly.

Before attempting to apply this line of reasoning to anthropology, it will be helpful to look at some early attempts to extend theoretical work beyond classification. A number of concepts were developed to permit a theory of evolution to be formulated on the basis of existing empirical work. One of these is orthogenesis, which, according to

---

[8] *Ibid.*, p. 9. [9] *Ibid.*

Boyd, holds that organisms evolve as if guided to certain goals along apparently predetermined lines.[10] Washburn identifies nonadaptability as another of these concepts. Inadaptability entails that many important physical characteristics are the result of independent development, whose study provided final knowledge of a given type. Irreversibility is another basic assumption about evolution.[11] It is basic to the view that evolution proceeds as if directed along preconceived lines to some established goal. These assumptions permitted a theory of evolution to be developed on an empirical base restricted to the kind of measurement and classification already outlined. " . . . it allowed conclusions to be drawn by a few rules based on little evidence."[12] Conclusions could be drawn on the basis of final outcomes, without regard to the infinite variety of ways in which these outcomes resulted. It was sufficient to discover what had happened; the facts could speak for themselves. If it were thought necessary to provide an "explanation," one need merely say that the process came about as if such and such principles were operative.[13]

---

[10]William C. Boyd, "The Contributions of Genetics to Anthropology," Anthropology Today, ed. A. L. Kroeber (Chicago: The University of Chicago Press, 1953), p. 488.

[11]Washburn, Physical Anthropology: 1953-1961, pp. 8-9.

[12]Ibid., p. 9.

[13]Neither Washburn nor Boyd use this "as if" construct in their formulation of the principles of the evolutionary mechanism. It is consistent with their interpretation, however, and will serve to explain the status of such propositions in modern theory.

Uses of the 'as if' construction in economics and physical science will help substantiate this interpretation. Schrödinger says "The chemist used the valency stroke for building models of complicated molecules. It is an instance of the famous 'as if.' It represented very real facts of observation. For a long time the physicist could not supply any explanation of the mechanism of the chemical bond. Then, in quick succession two were given. . . ."[14] Friedman speaks of a billiard player performing as if he were capable of rapidly making extremely complicated mathematical calculations. Excellent predictions may be expected from this prediction, even though the assumption is known to be false.[15] Friedman also gives the example of trees on which "the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives. . . ."[16] Friedman asks if these hypotheses are rendered unacceptable simply because they are known to be false. He believes not, because their predictability is not affected by the falsity of the assumptions.

---

[14]Schrödinger, "Are There Quantum Jumps?" What Is Life? p. 44. First italics added.

[15]Milton Friedman, "The Methodology of Positive Economics," Essays in Positive Economics (Chicago: The University of Chicago Press, 1953), p. 21.

[16]Ibid., p. 19.

That these hypotheses are at best preliminary to a fully articulated theory is illustrated by the anthropological concept of orthogenesis. That each can be greatly improved by designing a theory based on relevant underlying variables is also clear. Perhaps 'as if' propositions can be justified in the early stages of theory construction, as the theorist feels his way along. But it seems unjustified to use them in defense of a fully articulated theory, as Friedman does in his examples.

At about the same time that the traditional methods of physical anthropology had become barren of useful results, modern theories of race and evolution began to be formulated in genetics, paleontology, and zoology.[17] These modern theories showed great promise for application in anthropology. "The anthropologist may simply adopt the new evolutionary point of view, and his task is primarily one of adapting to this intellectual environment and devising techniques suitable to his particular needs."[18] Innovations introduced from the related sciences provided an entirely new approach to the organization of data. A number of concepts, such as selection, adaptation, and mutation, called disposition terms by Hempel, were introduced.[19] They do not denote directly observable characteristics,

---

[17]Washburn, Physical Anthropology: 1953-1961, p. 3.

[18]Ibid.

[19]Carl G. Hempel, Fundamentals of Concept Formation in Empirical Science ("Foundations of the Unity of Science," Vol. II, No. 7; Chicago: The University of Chicago Press, 1952), p. 28.

but assert relationships which will hold under certain specifiable circumstances. Specification of the circumstances is equivalent to the framing of testable hypotheses; the terms become associated with empirical data in their applications. These concepts provided the basis for the "original set of equations," so to speak, i.e., a simple set of restrictive hypotheses which would organize observational data into simple highly suggestive categories giving promise of great generality.

The work being done in related fields suggested that there existed an underlying mechanism responsible for producing the evidence that had been presented for their inspection, and that this mechanism must be investigated and applied, rather than completely bypassing it with the old set of 'as if' statements which seemed consistent with evidence susceptible to direct observation. Thus, paleontologists who "were impressed by the seemingly unerring way in which an organism marched from an undifferentiated beginning towards a definite goal"[20] could no longer appeal to the simple principle of orthogenesis as an explanation, or rather as the name given to the explanation which the facts seemed to provide for themselves. Having been introduced to the concepts of mutation and selection, they felt it necessary to account for the facts in these terms, since they were given good reason to believe that mutation and selection were part of an underlying mechanism responsible for the facts.

---

[20]Boyd, Anthropology Today, p. 488.

Research revealed that the tendencies observed by paleontologists could be explained in these terms, and that other conclusions based on earlier reasoning were incorrect. For example, it was realized that the modern horse has evolved along many lines over a long period of time, rather than following one course of development.[21] Genetic principles proved their superiority over orthogenesis in evolutionary theory. Similarly, introduction of the concept of adaptation discredited the conclusion that "man could not be descended from the ape, as this would reverse the [irreversible] course of evolution."[22]

Rounding out the anthropological picture of evolution according to the Samuelson program would probably require researchers to introduce additional disposition terms and logical concepts to provide a complete and consistent picture capable of accounting for the data and suggesting new hypotheses. Indeed, new concepts were brought in by anthropologists to explain the evolutionary process. It is difficult to discover purely theoretical terms in anthropological theory--terms possessing no meaning outside the theory in which they appear. It is noteworthy that the once highly useful concepts of orthogenesis, non-adaptability, irreversibility, and the like were discarded once their empirical base was undermined. They did not survive as purely theoretical terms, or as terms whose newly discovered

---

[21] Ibid., p. 489.

[22] Washburn, Physical Anthropology: 1953-1961, p. 8.

limitations made clear the limited range of applicability for the theory in which they appear. Washburn describes the importance of culture in guiding the process of selection.[23] Any complete picture of the evolutionary process must contain cultural variables in addition to the genetic ones contributed by related disciplines. The concepts drawn from related fields such as genetics have made a strong contribution in advancing physical anthropology beyond the purely descriptive stage. Yet Washburn does not believe this is enough, because some problems of evolution are unique to man. Insofar as evolution is unique to man, its understanding requires that the cultural dimension receive explicit consideration; it must be wedded to knowledge drawn from the life sciences.

> If we are to understand the process of human evolution, we need a modern dynamic biology and a deep appreciation of the history and functioning of culture. It is this necessity which gives all anthropology unity as a science.[24]

Washburn is pointing out that human evolution is a subject matter for social science. Physical anthropology, as a social science, must give unity to diverse explanatory strands if a coherent theory of human evolution is to be developed. Again we are reminded of Samuelson's set of basic functional relationships, characteristic of deductive analysis, which imparts unity and operational significance to theory.

---

[23] Ibid., p. 14. [24] Ibid., p. 24.

They have been replaced by concepts which describe the disposition of physical forms to develop according to their functions in the organism (adaptability), the internal laws of their development (genetic principles), response to environment (selection), etc. Observational data alone no longer gives final answers.[25] New schemes of measurement and classification with greater theoretical import have become necessary.

Theory in Anthropology and Economics: Parallels and Contrasts

In this section, certain parallels and contrasts between theory in anthropology and economics are suggested. Consider a subsistence culture in which the predominant occupation is deriving a living from the habitat. Suppose that a new, domestic food plant is introduced, permitting agricultural workers to produce a surplus crop. For the first time the society will be able to specialize and diversify the functions of its members. Based on numerous cases from which the anthropologist can generalize, the sequence of events can be predicted. For example, the priesthood will grow in size and in importance in response to increased emphasis on religion. Out of this group, political specialists will emerge, meeting the needs of the more complex society. Furthermore, the society will aggress upon any of its weaker neighbors who still lack the means to produce a differentiated society. Such would be a dynamic theory of cultural

---

[25] *Ibid.*, p. 8.

change, having a well-established empirical base and fully capable of disproof because of new evidence. It applies to the real world and is a very useful tool for prediction. Is it based totally on observational data? The theory purports to tell us that given a permissive disturbance to the society, the persons involved will respond in such and such a way--religiously, politically, militarily. Such theory has proved itself as an instrument possessing generality and predictability. That a culture will in all probability respond according to certain predictable patterns suggests that certain general statements about motivation, attitude formation and the like, are possible. It is perhaps a moot point whether or not behavioral assumptions of various sorts are not actually implicit in the type of theory under consideration. Individuals inclined to deny the charge feel as they do because they are opposed to making assumptions of any kind not directly verifiable by observation. Furthermore, behavioral assumptions have their locus in the individual. Linking cultural development in causal fashion with assumptions about individual behavior would probably compound unverifiability with unrealism.

Other individuals would be inclined to make a virtue of implicit behavioral assumptions, on the grounds that these are precisely the restrictive assumptions required for the "set of basic equations" described by Samuelson. Machlup, says

> Social phenomena are defined as results of human action, and all human action is defined as motivated action. Hence, social phenomena are explained only if they are attributed to definite types of action which are "understood" in terms of the values motivating those who decide and act. This concern with values--not values which the investigator entertains but values he understands to be effective in guiding the actions which bring about the events he studies--is the crucial difference between the social sciences and the natural sciences. . . . The social scientist . . . is not doing his job unless he explains changes . . . by going back to . . . decisions. . . . Explanation in the social sciences regularly requires the interpretation of phenomena in terms of idealized motivations of the idealized persons whose idealized actions bring forth the phenomena under investigation.[26]

Were an observer trained in the methods of economics to analyse the problem, he would be likely to construct a model for the culture, comprising the basic set of behavioral statements stressed by Samuelson and Machlup. He would build into his model those characteristics that a researcher plans ahead of time to investigate in the field: familial, linguistic, political, economic, etc. Then, taking the culture at some initial position, he would examine the effects on his model produced by introducing some exogenous disturbance. Given the functional relationships postulated among the variables, it would be possible to examine the effects on each of the variables, and on the system as a whole as it developed. The model would be constructed so as to react to a disturbance in ways known through

---

[26] Fritz Machlup, "Are the Social Sciences Really Inferior?" The Southern Economic Journal, Vol. XXVII, No. 3 (January, 1961), p. 176.

observation to generally occur--for example a period of disorganization followed by eventual reorganization of the society. Having postulated a fairly complicated set of interrelationships, the investigator could derive a large number of testable hypotheses from it, since the model being utilized implies that the results came about by virtue of the postulated interaction of variables.

This type of analysis would most probably involve a certain amount of purely implicit theorizing to impart internal consistency and operational significance to the model. The model would likely involve certain propositions implicitly at variance with knowledge derived from other disciplines. These implicit propositions would have to be carefully noted by the investigator lest he derive hypotheses from the logic of his model which nonetheless attribute empirical content to statements where none is intended.

The investigator would also attempt to compare the properties of his model culture with an identical culture having perhaps only one difference. This would entail specifying one variable in a different way. By tracing through the effects of an exogenous disturbance on this alternate system, the theoretical significance of the variable in question could be estimated. The working of the model would no doubt indicate that the culture as a whole is much more critically dependent on stability in certain segments than others.

Exploration of the properties of the model would suggest alterations as its implications were compared with actual experience. Analysis could proceed along lines of dynamics or comparative statics, although the former would probably be preferred. Theory developed along these lines would not conflict with the desire for a patient accretion of empirical knowledge out of which tentative generalizations are directly drawn.

Problems of Theory in Archaeology

Problems concerning the relation of theory to data have also risen in archaeology. For example, William H. Sears complains of certain archaeologists who

> actually present some of the wildest reconstructions. The reader is suddenly confronted with a wholly imaginative detail that could not conceivably be substantiated from any known archaeological data with any currently available techniques of reconstruction or interpretation.[27]

Scientific investigation, on the other hand, calls for "fully documented reconstruction and interpretation." Extrapolations should be made solely on the basis of documentable facts.[28]

---

[27] William H. Sears, "The Study of Social and Religious Systems in North American Archaeology," Current Anthropology, Vol. II, No. 3 (June, 1961), p. 230.

[28] Ibid.

The question of what an archaeologist can legitimately regard as a "fact" seems to be a central one in Sears' article. The "ordinary survey" which produces maps and surface collections of sherds, configurations of artifacts, positions of layers and the like, are all hard observational data.[29] Yet the realm of fact also includes inferences which can be drawn directly from the data when the latter are viewed as the consequence of a specifiable function. For example, "excavation and analysis of a mound as a fossilized ceremony rather than as a repository for pots, sherds, and bones, will allow at least partial reconstruction of the ceremonialism and interpretation of it in social terms."[30] Data must be gathered with an eye towards the use to which it will be put, because the amount of data that could be gathered is almost infinite, and otherwise much valuable data will be lost in even the most meticulous excavation. Since inferences are so intimately connected with observational data, they may be regarded as empirical data themselves. The trick is conceptualizing the data-gathering stage sufficiently so that the most relevant data are preserved. While viewing a mound as a fossilized ceremony rather than a mere found object entails some prior theorizing, full correspondence with empirical data is assumed. For example, inferences on family size will correspond to a group of observational data including size of rooms in dwellings.

---

[29] *Ibid.*, pp. 225, 227.

[30] *Ibid.*, p. 227.

If the data collecting process is modified to permit a wide range of inferences to assume the same status as hard data, then much more knowledge can be gained about such things as social structure, religious structure, and religious and cultural patterns.[31] Nevertheless, this goes only part of the way toward fulfilling the program Sears has mapped out. The amount of information than can be extracted directly from these empirical data is limited. "The classes of evidence and classes of cultural reconstructions and interpretations referred to here are . . . interrelated. . . ."[32] " . . . reconstruction can only come from joint analysis of several lines of evidence."[33] "Ceremonial reconstruction" is made possible by empirical investigation (excavation) according to an existing functional analytic framework. Social reconstruction and interpretation[34] requires a system of interdependent functional relationships.[35] This is recognized (with a different emphasis) in the following passage.

> Regularities in the processes of cultural development cannot be determined until the cultures are reconstructed in some degree. Reconstructions of form, and sequences of forms, are impossible without the establishment of properly documented, clearly understood units, and their equally clear and well-documented arrangement in space and time. Without this foundation, <u>interprelations at more complex levels</u> are apt to be so erroneous as to invalidate any generalizations based on them.[36]

---

[31] <u>Ibid</u>., p. 230. [32] <u>Ibid</u>., p. 231. [33] <u>Ibid</u>., p. 229.

[34] <u>Ibid</u>. [35] <u>Ibid</u>., p. 229, 231.

[36] <u>Ibid</u>., p. 224. Italics added.

The emphasis here is on cultural reconstruction out of properly documented parts, as opposed to wild reconstruction based wholly on imagination. Specifications are apparently not permitted until completely justified by empirical knowledge. The resulting theory must be constructed so as to embody only implications with prior empirical support. On the other hand, it might be prudent to steer a middle course in the attempt at social reconstruction and interpretation. There are disadvantages in making interpretations only on the basis of observational data without prior identification of postulated interrelationships. An example would be the statistical correlations between estimated population size and social, religious and political structure,[37] or correlation between community plan and lineage structure.[38] The trouble is, "these resulting functions between unknowns and parameters could have arisen from an infinity of possible alternative sets of original equations."[39] That is, an infinity of theoretically conceivable social, religious and political structures could have given rise to the observed (or inferred) parameter values. Hence the need to specify the underlying functional relationships in a theory. Working with a framework formulated on the basis of partial empirical knowledge, the investigator could hope for empirical verification of many of his hypotheses <u>after</u> they have been formulated, and he would probably always have to be satisfied with a

---

[37] <u>Ibid</u>., p. 224. [38] <u>Ibid</u>., p. 227.

[39] Samuelson, <u>Foundations</u>, p. 11.

theory which falters somewhere along the line, even if he doesn't make a virtue of these deficiencies.

Any researcher willing to go this far might permit the introduction of some purely theoretical terms if they should prove useful in elaborating a theory, and if they are understood to be exactly what they are--purely logical concepts. A little "wild reconstruction" of this sort might have some merit.

Disputed Questions of Method in Psychoanalytic Theory

We turn now to a consideration of Freudian psychoanalytic theory in order to give additional application to the position we have been developing regarding the deductive pattern of scientific theory. In this section we shall see that psychologists have disputed strongly over the question: is it valid to construct a theory which embodies numerous empirically empty statement-forms, or must scientific activity be restricted to the consideration of theories and hypotheses composed solely of empirical statement-forms?

B. F. Skinner has argued[40] that science must restrict itself to the analysis of explicit, quantifiable categories of observable events. Concepts which do not permit quantification of events, or which obscure "the many specific properties of the circumstances themselves," are at best pre-scientific.[41] In psychology, the use of systems constructed

---

[40] B. F. Skinner, "Critique of Psychoanalytic Concepts and Theories," The Validation of Scientific Theories, ed. Philipp G. Frank (New York: Collier Books, 1961), pp. 110-121.

[41] Ibid., p. 117.

out of empirically-empty categories obscures "important details among the variables of which human behavior is a function and [leads] to the neglect of important problems in the analysis of behavior as a primary datum. . . .[42] Skinner objects to the use in psychology of such concepts as "forces, processes, organization, tensions, systems, and mechanisms,"[43] or 'libido,' 'cathexis,' . . . 'instinctive or emotional tendencies,' etc.[44] "These all imply arrangements or relationships among things, but what are the things so related or arranged?"[45] "Perhaps these metaphorical notions might have been helpful in getting psychoanalytic theory on its feet," says Skinner, just as "'essences,' 'forces,' phlogistons,' and 'ethers'" were useful in the dawn of other sciences.[46] Yet Skinner denies them any place in modern theory.

Skinner explains why a concept like instinct, which lacks the quantifiable dimension he speaks of, is no more respectable than the phlogiston concept. Apparently every statement-form in a theory must be "real"-either empirically real or else allegedly real in some way beyond experience.

> No matter what logicians may eventually make of this mental apparatus, there is little doubt that Freud accepted it as real rather than as a scientific construct or theory. One does not at the age of seventy define the goal of one's life as the exploration of an explanatory fiction.[47]

---

[42] Ibid., p. 120. [43] Ibid. [44] Ibid., p. 116.

[45] Ibid., p. 120. Italics in original.

[46] Ibid., p. 112. [47] Ibid., p. 111. Italics added.

Skinner's equation of scientific construct with explanatory fiction reveals to us that he does not admit the presence of implicitly defined statement-forms having only, but crucial, logical importance. When Freud talks of libido, instinct, available quantities of psychic energy and so on, and when we find these to be unquantifiable, then these things must be part of some "pattern of an inner explanation of behavior."[48] But, he continues, "the pattern of an inner explanation of behavior is best exemplified by doctrines of animism. . . ."[49] Now animisms are invented to account for the complicated behavior of complicated organisms, so "inner-determiners" are invented, such as "'demon,' 'spirit,' 'homunculus,' or 'personality.'"[50] It is no wonder that Skinner wishes to do away with much of what he finds objectionable in Freudian theory.

Freud, says Skinner, was aware of environmental and genetic determinants of behavior.[51] But being accustomed to "the traditional explanatory system . . . he was unable to eliminate the pattern from his thinking."[52] Freud has, despite himself, created a merely pre-scientific system if he is required to make each segment of his theory quantifiable, and if all non-quantified segments must be interpreted to assert something literally real. If, on the other hand, valid methodology permits a theory to be essentially a logical construct, having no factual content whatever, except what it is endowed with by virtue of the coordinating definitions given the theory,

---

[48] Ibid., p. 113. [49] Ibid. [50] Ibid.

[51] Ibid. [52] Ibid.

then Freud is justified in erecting his elaborate theoretical scheme. He may give it all the explanatory scope he is capable of, and then give sufficient rules of correspondence to provide his theory with empirical content. For example, Freud need only tell us a certain amount about what aspects of human behavior are explainable in terms of the id, ego, and superego. If the logical structure of his theory is powerful enough to account for and predict wide ranges of human behavior, then the theory is successful. He needn't verify every segment of his theory--only enough of it to make it operational over a wide range of phenomena. The remaining uncoordinated statement-forms serve (for the time-being at least) only a logical function. Freudian psychoanalytic theory fits the description of a system characterized by a logical framework of wide scope and incomplete, but far-reaching empirical correspondences. Freud regarded himself primarily as a psychological theorist; psychoanalysis was for him primarily a systematic theory of personality.[53] Hall's readable survey of Freudian theory outlines the manner in which Freud made his basic concepts an integral part of his psychological system. Else Frenkel-Brunswick acknowledges that many of Freud's basic concepts, such as unconsciousness, id, superego, or repression "refer only indirectly, and not completely at that, to observable data. . . ."[54]

---

[53]Calvin A. Hall, A Primer of Freudian Psychology (New York: The New American Library, 1954), pp. 6, 19.

[54]Else Frenkel-Brunswik, "Confirmation of Psychoanalytic Theories," The Validation of Scientific Theories, p. 96.

Hall says "The names, id, ego, and superego, actually signify nothing in themselves. They are merely a shorthand way of designating different processes, functions, mechanisms, and dynamisms within the total personality."[55] Frenkel-Brunswik goes on to say:

> Freud seems to have proceeded rather directly to the building of a hypothetical theoretical structure, with interpretation lagging somewhat behind; in the definition of such theoretical constructs as superego, ego, and id, the major emphasis is on their structural relationships to one another rather than on their relationships to observation.[56]

In his introduction to Freud's History of the Psychoanalytic Movement, Philip Rieff says:

> This fact or that takes on importance only from the analytic line along which the facts are strung out. . . . The first lesson to be learned from [The History of the Psychoanalytic Movement] has little to do with the empirical content of the argument but rather with its theoretical form. Against his own cautionary advice, and against the prejudice of the time, Freud reasserted the primacy of theory in any debate on the meaning of the facts.[57]

In like manner, Egon Brunswik stresses the great "resolving power" of psychoanalytic theory in "conceptually bringing together an apparently chaotic variety of behavioral features relevant to life."[58]

---

[55]Hall, pp. 34-35. [56]Ibid., p. 104.

[57]Philip Rieff, "Introduction," The History of the Psychoanalytic Movement, and Other Papers [by Sigmund Freud], ed. Philip Rieff (New York: Collier Books, 1963), pp. 13-14.

[58]Egon Brunswik, The Conceptual Framework of Psychology ("Foundations of the Unity of Science," Vol. I, No. 10; Chicago: The University of Chicago Press, 1952), p. 58.

Here again we see the stress of systematization over empirical testing and measurement. Finally, we quote Freud himself concerning his method of theorizing. Notice that Freud stresses the importance of skillful conceptualization, and the idea that statement-forms acquire empirical content as the theory progresses, rather than being empirical statements from the beginning. Freud says

> /Definitions in science/ are in the nature of conventions; although everything depends on their being chosen in no arbitrary manner, but determined by the important relations they have to the empirical material-relations that we seem to devine before we can clearly recognize and demonstrate them. . . . Progressively we must modify these concepts so that they become widely applicable and at the same time consistent logically. Then, indeed, it may be time to immure them in definitions. . . . The science of physics furnishes an excellent illustration of the way in which even those 'basic concepts' that are firmly established in the form of definitions are constantly being altered in their content.[59]

Freud is using the term "definitions" in two senses here. (They are underlined for reference convenience.) In the first case he speaks of implicit definitions; statement-forms are welded into the theoretical structure devoid for the time-being of empirical content, but giving the theory potential for empirical explanation. In the second case Freud speaks of the proper time for supplying a term or statement-form with coordinating definitions

---

[59]Sigmund Freud, "Instincts and Their Vicissitudes," cited by Frenkel-Brunswik, The Validation of Scientific Theories, p. 96.

or rules of correspondence, thus assigning it specific empirical content. In the last sentence quoted, Freud explains that the implicitly defined "basic concepts" of the theory take on new empirical meanings as knowledge progresses and new coordinating definitions are required. Frenkel-Brunswik gives us an example. She states that the concepts of consciousness and unconsciousness at first called mainly "for a specification of their relationships within the over-all formal model."[60] At this stage it was desirable to avoid "confusion concerning mentalistic reification."[61] Accordingly, Freud proposed the use of abstract-looking symbols Cs and Ucs for the two concepts. In Freud's later writings, however, the terms "take on a distinct reference to mental qualities."[62] The terms were given empirical content because of subsequent empirical evidence for a coordination between unconsciousness on the one hand and id and superego on the other hand.[63]

Our brief discussion of contentions among psychologists regarding psychoanalytic theory has indicated that Freud was, despite Skinner's remarks, a highly conscientious and sophisticated theorist. We have argued that Freud's method of theorizing exemplifies very strongly the theoretical method we have been defending in this paper. Having noted the important parallels between Freudian theory and the deductive pattern of explanation we have been supporting, we conclude

---

[60] *Ibid.*, p. 97. [61] *Ibid.* [62] *Ibid.*, p. 98.

[63] *Ibid.*

that the entire weight of the argument of the present paper bears against Skinner's position. Freudian theory cannot be written off as pre-scientific theorizing whose continued influence "has prevented the incorporation of psychoanalysis into the body of science proper."[64] Indeed, Frankel-Brunswik's entire essay shows a striking similarity of viewpoint and emphasis with the present paper. Throughout her essay she stresses the value of concepts which refer indirectly and incompletely to observable data, the extraordinary explanatory capacity thus made possible, and also the broader aspect of cultural interpretation which grows out of the framework as constructed by Freud.

Frenkel-Brunswik informs us of the need for greater formalization of psychoanalytic theory--the need for a "more systematic differentiation between basic assumptions and their derivations."[65] Rieff is very outspoken in his criticism of the current condition of the psychoanalytic movement in its theoretical aspects. The rich legacy of Freud is not being developed, in his opinion. It is pertinent to speculate why the systematic possibilities of Freudian theory are not being more vigorously exploited by efforts to add rigor and formalism to his groundbreaking and seminal work. Rieff argues that psychoanalysis has departed from Freud's original conception of a theoretical system of psychology,

---

[64]Skinner, The Validation of Scientific Theories, p. 120.

[65]Frenkel-Brunswik, The Validation of Scientific Theories, p. 103.

and has become simply a method of psychotherapy.[66, 67] The theoretical aspect has been eclipsed, partly because psychoanalysts receive an educational background unsuitable for the development of theoretical acumen. Perhaps also the influence of empirical principles characteristic of Skinner has discredited a mode of theorizing so full of statement-forms difficult to give empirical significance.

Freudian and Keynesian Systems Compared

There are other reasons which may help explain the absence of a strong theoretical tradition along Freudian lines. We will try to bring out these reasons by comparing the psychoanalytic tradition with the Keynesian one in economics. Freud's work continued until the late 1930's. It had only lately begun to take on characteristics which lend themselves to systematization. As formal theory, it is held to be deficient in a number of respects,[68] but it is incomparably original and seminal.

Much the same could be said about Keynes, but with a crucial difference. Two generations of the best theoretical economists have cast and recast Keynesian theory into various rigorous molds; they have extended the theory into new areas of application; they have generated and tested innumerable empirical hypotheses of increasing complexity and ingenuity, reasoning along Keynesian lines; they have produced great masses of statistical series as an outgrowth of

---

[66] Rieff, The History of the Psychoanalytic Movement, p. 29.

[67] Hall, p. 19.

[68] Frankel-Brunswik, The Validation of Scientific Theories, pp. 100-101.

theoretical extension and replication; they have gradually evolved competing schools of thought which have but remote connection with the theoretical taproot, but whose development has assured the continued vitality of economic theorizing.

Why has economics a more active tradition of exploiting the knowledge derivable from the deductive pattern of explanation, even though psychology possesses such a rich base? Perhaps it is because economics lacks two very important advantages native to psychology. One advantage, peculiar to experimental psychology, is the possession of a large and varied opportunity for laboratory experimentation. Laboratory experimentation enables psychologists to work in close proximity to empirical data. Psychologists have less need to fall back on speculative theorizing characteristic of economists. The second advantage, peculiar to Freudian psychologists, is their opportunity to concentrate their energies on clinical application of their principles, without having to labor long over the formal theoretical development of those principles.

Economists, by contrast, lack the advantage of laboratory experimentation and must fall back on systematic theory out of necessity to a much greater extent than psychologists. Secondly, the application of economic principles has proved to be much less of an immediate possibility than in psychoanalysis. We have, then, another reason for the greater theoretical propensity among economists than among psychoanalysts.

Yet, for all their disadvantages, economists have acquired a skill at speculative model building and judicious abstraction matched by workers in few other disciplines.[69] One can speculate that the rich possibilities for systematic theorizing provided by Freud would be farther advanced if psychoanalysts had somehow acquired the speculative instincts of the economist.

Summary

In the present chapter we have reviewed some of the problems concerning the structure and empirical content of theory in selected areas of anthropology and psychology. An attempt has been made to bring the experience of economic analysis to bear on these questions. In both physical anthropology and archaeology the need for simplified theoretical frameworks has been identified. Physical anthropology has greatly benefited from the adaptation of concepts from related fields, such as genetics. In archaeology, the advantages of conceptualization prior to actual excavation are recognized. In each area, the demand for a strong observational basis at every stage of investigation has been retained.

Machlup's contention that theory in the social sciences must be based upon restrictive assumptions about human behavior was advanced as a possible means of rounding out theoretical models from which testable hypotheses could be deduced. Restrictive behavioral hypotheses,

---

[69] William J. Baumol, "What Can Economic Theory Contribute to Managerial Economics?" Papers and Proceedings of the American Economic Association, Vol. LI, No. 2 (May, 1961), p. 143.

as Samuelson has pointed out, commit the theorist to one type of explanation for his data out of an infinite number of possible explanations. The validity of the assumptions and the choice of variables is assessed by the validity of resultant conclusions when compared with observation. The theorist must be willing to commit himself in advance to certain postulates not already possessing empirical validation. The benefits derived are a large number of testable hypotheses and a highly versatile explanatory-predictive framework. While many anthropologists seem to disagree, it is advanced in this chapter that "advance committments" as described here, do not violate canons of sound empirical investigation.

The controversy over psychoanalytic theory highlights as well as any other example in this paper the issues currently debated over the methods of deductive analysis. Skinner strongly opposes Freud's resort to a theoretical framework whose every segment cannot be tested by observed behavior. His absolute rejection of Freud's style of theorizing is important because Freud's theory, by his own testimony and that of his interpreters, is similar, even in details, to the methodological position supported throughout this paper.

The concluding chapter, which follows, condenses the most important arguments and conclusions in a brief restatement.

CHAPTER VIII

A BRIEF RESTATEMENT

Early Applications of Deductive Theory

If we look back to the early period of modern science, we observe the deductive pattern of explanation to be one of the most prevelent devices employed in the search for knowledge about man, society, or nature. Newton's theory of motion, the economics of Smith and Ricardo, all in varying degrees sought to explain their subjects by means of hypotheses derived logically from central principles related in theoretical systems that bore on amazingly diverse ranges of activity. We have told how the success of these systems earned them prestige that influenced many succeeding generations of investigators as they sought to extend their understanding into new and uncharted areas. In the wake of the first modern scientific discoveries came a natural philosophy whose goal was the encompassing of the entire range of experience under one, or a few, comprehensive systems. This faith in the power of abstract reasoning was richly rewarded. The Newtonian model proved very successful in yielding accurate predictions concerning the movements of material objects under the influence of a variety of identifiable forces.

Whatever limitations might subsequently be revealed in the deductive pattern of explanation, it had certainly proved that judicious abstraction and logical reasoning is a powerful instrument for gaining knowledge. There was another important but less favorable consequence, however. Data of experience that showed no capability of systematic explanation was likely to be deemed unimportant. Empirical verification became less important than achieving a comprehensive explanatory framework.

The primary goal of natural or social philosophy seemed to be the attainment of comprehensible order out of the apparent chaos of everyday existence. Knowledge of particulars were important, not so much in themselves as in their role as building blocks of the final intellectual edifice. Facts were important insofar as they contributed to the principles which were to form the system. In the natural sciences, moreover, deduction from these principles had always seemed adequate to account for a wide range of facts. New factual discoveries within the established scope of a natural science had not, and were not expected to challenge the validity of the science within that scope.

For the social sciences, an ideal model for the attainment of knowledge was provided. Since all of nature was believed governed by rational laws, it seemed to follow that similar laws governing society awaited discovery. New facts would constantly turn up, but none would be expected to challenge the established theory of social science.

The establishment of a comprehensive deductive framework revealing the rational mechanism guiding social activity became the goal of theoretical activity, therefore. The social scientist could rest his case with a framework which generated basic understanding of specific hypotheses consistent with the most basic beliefs characteristic of the era in question. Veblen and Viner have explained how Smith and other great figures of the classical era regarded the attainment of a complete "mirror of nature" as the point where they sensed the fulfillment of their task, despite their outstanding ability to observe specific events. Nagel has told of the attempts of early-modern physical scientists to incorporate all areas of the physical world into the mechanical model of Newton. Santillana and Zilsel have clarified the pervasive characteristics of scientific activity in general during the early period: the construction of a mirror of nature which, once attained, would constitute the final chapter of theoretical activity. So long as basic new data contradictory to existing theoretical systems in physical science were not unearthed, the rationalist goal was secure. Physicists could continue to reduce all physical knowledge to Newton's mechanical model.

The Period of Transition

But new data did appear. When knowledge of electromagnetic phenomena and the discontinuous nature of subatomic particles were discovered, the rationalist goals were undermined. It became a

basic premise that new facts would contradict and undermine established rational systems designed to reveal the rational underpinnings of nature. Data had to be sought, not simply to provide new instances of verification of known hypotheses, but to test again and again the ability of theory to predict adequately and accommodate new facts. Empirical knowledge took on a new and greatly expanded position in the theoretical world view, and this event constituted a revolution in scientific activity.

The same revolution had to take place in the social sciences. As late as the 1920's Alfred Marshall could assure his contemporaries that the major theoretical groundwork in economics had been laid, and that all that remained was to round out the picture by fitting in much of the remaining detail. But too many challenges to the prevailing attitude toward society, inherited selectively from preceeding generations, had been accumulating, and a major depression laid to rest once and for all the notion that human behavior is subject to laws from which can be deduced the course of economic events. From that time forward, a theoretical system was only as good as its ability to suggest and correctly interpret recurring events in a world which refused to stand still at the behest of any particular philosophical orientation.

The summary treatment given to these issues in the present paper hardly does justice to the complexities involved. Nevertheless, a major intent of the paper will have been achieved if it is realized

that the empirical revolution through which the entire field of scientific investigation passed has raised crucial difficulties with deductive theory as a method of scientific investigation. One difficulty is that deductive theories formulated during an era when empirical analysis was less important, and still subject to severe methodological limitations, embodies numerous segments devoid of empirical content, and not adaptable to quantification. In the view of many modern critics, this fact alone has made much of the older theoretical work obsolete from a scientific point of view. A closely related difficulty springs from the revolution in method just described; it has to do with the goals of scientific analysis. Deductive theory of the early modern period sought to reveal the underlying rational plan believed to lie behind all nature--animate or inanimate. Yet the very concept of such unity in nature is now recognized as a notion essentially beyond empirical demonstration. Furthermore, to contrive a mirror for the alleged rational structure of nature, it has always been necessary to weave into the analysis a number of logical and metaphysical strands having no empirical function whatever. It follows then, according to these critics, that the long sought-after goal of unified explanation must be rejected if the new, empirical canons of reliable knowledge are to be adhered to as methods.

Both of these criticisms seem to imply that the deductive pattern of explanation must be jettisoned or radically overhauled to be useful,

that the older work is obsolete scientifically, and that the vestigial elements currently present in scientific analysis must be eliminated. They also imply that the goals of scientific analysis are essentially the modest ones of generating testable empirical hypotheses rather than the rather pretentious one of discovering the nature of ultimate reality.

In brief, the empirical revolution raised very fundamental problems for both the natural and social sciences. A considerable literature has grown up in the philosophy of science in response to these problems. The impression is gained, however, that the response has been considerably more extensive and systematic with respect to the natural sciences. This is, perhaps, somewhat surprising, since the social sciences are just as crucially affected. In any event, it is believed that conceptualizing the problem in a manner such as is attempted in this paper facilitates the use of the entire literature for the benefit of problems in the social sciences.

Continuity of Analytic Technique in Economic Theory

These considerations are especially important to deductive economics, because modern economic theory shares with its predecessors much of their methods, terms and techniques, and also the same goals to a remarkable extent. Deductive systems based on a small number of axiomatic statements about human motivation, behavior, institutions,

and the like, are still prominent. The work of Smith, Ricardo, Malthus, Marx, Marshall, Schumpeter, and Keynes come to mind immediately. Terms and concepts such as profit maximization, the "firm," diminishing returns, utility, may be mentioned. Maximization and marginalist notions have retained their central theoretical positions in many areas where their direct empirical verification has been more or less abandoned. Furthermore, the basic goal of making understandable and interpreting for each era the apparently chaotic whirl of economic activity has motivated each generation of economists. We have seen how Adam Smith did it, how Marx and Keynes did it, and how extremely important has been the cultural impact of their theories. Today, much current debate at both the popular and scholarly level turns on the cultural orientation of various economic theories. There are numerous instances of frenzy distilled from the cultural views embedded in the theories of many an academic scribbler.

It would, upon a moment's reflection, be difficult to deny that economics retains a strong relationship to its forerunners of the early modern era. We have seen how George Stigler has explained the special importance of cumulative development in economic theory. Stigler suggests that the major problems of economic theory were sketched out by Adam Smith, and that theoretical progress has been accomplished by the work of successive generations of economists building upon the most useful work of their predecessors. The passage of time often obscures the original intention of the innovator as new contexts and

techniques are developed, but seminal theoretical innovations retain their lasting influence. This "internal autonomy" of theoretical economics is more fundamentally important than the great historical events and current policy problems that absorb the transitory attention of each generation.[1] We have in this paper assigned particular importance to the perennial nature of key economic concepts, especially insofar as they carry over into the era when empirical content has taken on such heightened importance. We have found numerous examples of concepts believed at their inception to contain vital factual significance, and believed to establish once and for all the direction of future investigation. Utility theory is perhaps the outstanding example. It purported to establish the key empirical entity to be defined and counted--the unit of pleasure--and it postulated the philosophy or psychology which essentially explains human motivation. The early ambitions of utility theory have been discredited; its philosophical explanation and its empirical program have been rejected by modern researchers. Yet we find repeated and pervasive references to total utility, marginal utility, and the like in modern economic theory. Economists do not abandon it because it is such a convenient analytic device. A concept that once had far-reaching implications concerning human nature and society is now a cog in what Marshall termed the economic engine of analysis. Utility is only one such concept that has undergone this Stiglerian evolution in economic theory.

---

[1] Leo Rogin, The Meaning and Validity of Economic Theory: A Historical Approach (New York: Harper and Brothers, 1956). In this book, Rogin expresses an opposing viewpoint.

In the minds of many writers, there is a contradiction between the modern requirements for reliable empirical knowledge and the traditional goals and techniques of deductive economic theory. The metaphysical preconceptions and over-ambitious goals of the early era are inappropriate to modern science. They hold that one important reason why economics has lagged behind the hard sciences is because economists cling so tenaciously to so many soft concepts.

We have examined the thesis that modern empirical methods have made necessary a drastic revision of theoretical goals. This thesis implies that economists must distinguish between economic philosophy and economic science. What was once part of the same intellectual effort--the making of philosophy and the making of science--must now be recognized as two separate and distinct activities. The bifurcation of activities has been made necessary by the immensely more complicated task of properly handling facts.

To anyone who has read Joan Robinson's Economic Philosophy there is little room for doubt that economic theory is uniquely qualified as a vehicle of ideology. The whole book is a documentation of the fact that economics "has always been a vehicle for the ruling ideology of each period as well as partly a method of scientific investigation."[2] Consequently, "it is necessary to clear the decaying remnants of obsolete metaphysics out of the way before we can go forward."[3]

---

[2]Robinson, Economic Philosophy, p. 1.

[3]Ibid., p. 146.

Robinson's work is one of the best available criticisms of economic theory from the standpoint of its ideological content. Myrdal's The Political Element in the Development of Economic Theory is a highly erudite treatise that makes a similar important point: that a great deal of traditional economic theory has purported to establish norms scientifically. The great fallacy inherent in the attempt to establish norms scientifically is the belief that moral principles can be deduced from axioms or self-evident truths. Suffice it to say that Myrdal has made his point; no time need be taken to argue that such activity is a blemish on the record of economic theory, early or contemporary.

We must grant the points that Robinson and Myrdal have made; economic theory offers many examples of work which falls far short of acceptable standards of scientific analysis. At the same time, we know that early theorists did a great deal of outstanding work in relating facts to analytic systems of analysis, even when viewed by modern standards. Critics of the traditional analysis, with its particular combination of goals and methods, will say that the early economists achieved scientifically valid results in spite of their over-ambitious goals. Our argument indicates that the deductive tradition in economic theory need not suffer a basic reduction in interpretive scope, that some of the finest modern work successfully combines modern methods with traditional goals, and that the Stiglerian tendency of cumulative and sometimes rather insular development of economic theory has sound methodological justification.

Traditional Goals and Modern Methods: Resolving the Problem

Especially pertinent to the discussion is the work of Erwin Schrödinger, discussed briefly in a previous chapter. In a particularly important way, Schrödinger's views on the development of scientific theory are similar to Stigler's. Schrödinger places great stress on historical continuity in the development of physical theory. He views with considerable misgiving the tendency among numerous contemporary investigators to cut themselves off from their theoretical predecessors. These theorists concentrate their attention on problems of current and immediate interest, working quite independently of any "main stream" or theoretical tradition. They do so in the belief that approaching new and rather unique problems with complete freedom to innovate maximizes their ability to achieve formulations that successfully predict new findings and account conveniently for known data. Their preoccupation with problems of current empirical interest is fully consistent with the belief that modern science must operate exclusively within the rather limited sphere of immediate empirical objectives. Certainly, the limited objectives of these analysts does not replace deductive theory with isolated low-level hypotheses. What it does is restrict the goals or scope of the deductive system to immediate empirical requirements.

What is important about Schrödinger's position is his belief that traditional theoretical goals need not be rejected in order to properly comply with empirical exigencies. He sees in physical theory the same continuity of development that Stigler finds in economics. What's more, he sees great value in preserving this continuity. Schrödinger says "all science is bound up with human culture in general, and . . . scientific findings, even those which at the moment appear the most advanced and esoteric and difficult to grasp, are meaningless outside their cultural context."[4] Any theoretical science which neglects its connection with the rest of mankind will eventually atrophy.

Schrödinger believes that it is all too easy for scientists to forget the great importance that scientific activity has had in forming the general Western world-view. The most basic goal of science remains quite similar to what it has always been, in Schrödinger's view: "a comprehensive picture of the subject under investigation, a picture which becomes ever more distinct, lucid and clearly understood in its interrelation."[5]

Schrödinger preserves the traditional, or "rationalist" scope of scientific theory in the face of the empirical revolution and the concomittant drift from these goals on the part of many scientists. We saw, in our first encounter of Schrödinger's ideas, that he subordinates any particular empirical finding to the theory which

---

[4]Schrödinger, What Is Life? pp. 132-133.

[5]Ibid., p. 197.

predicted it or accounts for it; the primary goal of scientific theory is the cumulatively developing comprehensive picture of nature that the scientist is able to evolve. Hence the need for continuity of development rather than splintering theory so as to handle most conveniently each particular empirical problem as it arises. But the comprehensive theoretical picture must be validated by the only test which can be reliably applied to findings of any kind--empirical verification. Scientific explanation in the contemporary sense stands on an altogether different footing than before; the scientist is never permitted to rest his efforts with the assurance that his job is finished, simply because he has an impressive and comprehensive axiomatic system that yields results that he and his generation approve of. Empirical testing has become the touchstone of any explanatory system.

For the logical empiricists, prediction is both touchstone and goal of scientific investigation. That is, there is no theoretical goal beyond producing empirical hypotheses, and their validation is the factor by which the theory is evaluated; that is, it is the touchstone. For the rationalists of the early modern period, comprehensive explanation--the "picture"--was both touchstone and goal. That is, the empirical or predictive goal was of secondary importance, and did not enter into the validation of the theory in any essential way. For Schrödinger, the modern rationalist, the "picture" is the goal, prediction is the touchstone. In other words, no matter how impressive the logical structure of the theory, it must be rejected in the absence of empirical validation. The "picture"

must come under intense and continuous examination, just as much as the specific research conclusions reached within the larger framework. The scientists' comprehensive picture of nature must remain just as tentative and contingent as empirical hypotheses derived from specific scientific investigations.

We have seen many examples drawn from economics and the physical sciences of how theoretical structures have become established in pursuit of extra-empirical goals. It is perhaps a moot point which goals are more important: the empirical, predictive ones, or the extra-empirical ones. But there can be no conclusive criticism of theorists who pursue extra-empirical goals in their theoretical orientations, so long as the empirical aspect of their work remains sound. According to this line of reasoning, the economist is free to retain the goal of revealing to his contemporaries the cultural dimension of economic activity.

Deductive Analysis and the Problem of Ideology

We have discussed ways in which a theoretical system becomes an ideology. It was asserted that ideologies are prone to spring from deductive theories not because of defects inherent in the deductive pattern of explanation, but because acceptance of the "explanatory" aspect of these theories is not subordinated to the continued empirical acceptability of the theory. A theory which has no empirical moorings at all is a purely metaphysical construct and has

no scientific value whatever. A good explanatory system is vulnerable on empirical grounds, though not necessarily concerning every proposition contained in the theory. The early scientific theories of the modern era had less defense against ideological encroachment than theories postdating the empirical revolution, because they only had to be impressively comprehensive and satisfy a few eclectically chosen facts. In contemporary analysis, the explanatory model, if it exists at all, is embedded in a predictive theory. All aspects of the structure stand or fall together. There is much in Marx, for example, that permits empirical verification, such as the proposition that real wages never rise in the long run. That Marxian economics still stands as an explanatory system even though substantial portions of it are discredited empirically attests to its ideological power. If Marxian theory contained no capacity to explain factual knowledge or predict future observable events, it would be merely a metaphysical system. Such is not the case. If it succeeded as an ongoing empirical tool, its interpretive dimension would merit the strong attention it receives. Such is apparently not the case, although it has made valuable contributions. That the Marxian system receives wide allegiance at the explanatory level by vast aggregations of persons incapable of caring about its empirical validity attests to its power as an ideology. Prior to the empirical revolution, all that could be done with a Marx was erect a competing system of equal

magnitude--one which could command equal respect as an explanatory system and embody the "right" outlook. People of these generations, even to some extent the most honest and learned of men, were much less conditioned as a matter of intellectual habit to systematically appeal to the developing course of day-to-day mundane events to test the validity of theoretical systems. It would be an unfair and distorted oversimplification to push this statement too far; nevertheless it is more characteristic of the rationalist era than our own, and our own era does not have a remarkably outstanding record. Even under the best circumstances, it will undoubtedly take a long time for a scientifically critical attitude toward the interpretive content of theoretical systems to become part of the intellectual equipment of the community at large. Yet the need for this achievement is never likely to disappear, and if Schrödinger's conviction about the impact of science upon our fundamental world-view is correct, then there is reason for optimism.

A theory may fail at the explanatory level while enjoying success at the predictive level. Einstein and Schrödinger have made this criticism of quantum theory. But Schrödinger, the modern rationalist, has taught that the converse is not admissible. Theory cannot offer explanation without predicting. Prediction is the essential touchstone. Samuelson makes the point that "explanations" are sooner or later replaced by descriptive theories that illuminate an even wider area. Whether or not Samuelson has succeeded in proving his contention that the search for "deeper explanations" is a futile

quest, his remark emphasizes the contingent nature of scientific theory at any level it may operate.

The Empirical Basis of Modern Deductive Method

We turn now to a summary of the distinctive features of the deductive pattern of explanation which adapt it to the tasks and properties we have been describing. Another passage from Schrödinger will serve to show the close connection between the scope and goals of scientific theory on the one hand, and the methods of deductive analysis on the other hand. Schrödinger says that if we are interested in a comprehensive picture, "the coherence would be utterly destroyed if we felt bound by pangs of conscience to omit all that is not directly ascertained or cannot, if so desired, be confirmed by sense perception; if we felt bound to formulate all propositions in such a way that their relations to sense perceptions were immediately manifest."[6] From this proposition we can determine an important characteristic of the deductive form of analysis. Schrödinger puts it this way, reasoning analogously from the "historical" sciences, such as archaeology. "[There] will remain, from the standpoint of science, an unverifiable mental construct created to establish a correlation between all the encountered records and remnants and to assign to them their proper place within the larger context of the history of civilization."[7]

---

[6] Ibid.

[7] Ibid., p. 193.

It is the presence of unverifiable mental constructs, the forms they take and the functions they perform that will occupy us in this section of our summary. Economic theory has many unverifiable constructs, as every critic has pointed out. Moreover, these constructs frequently have the most real appearance. Amounts of utility achieved by persons whose only goal in life is to maximize pleasure derived from incomes paid by firms whose only goal is to maximize profits by hiring homogeneous factors of production whose contribution diminishes at a well-defined margin constitutes a bewildering array of unreality to lay and professional critics of economic theory alike.

In his chapter "Experimental Laws and Theories" Ernest Nagel states " . . . some of the most impressively comprehensive explanatory systems of the physical sciences are notoriously not about matters that would ordinarily be characterized as 'observable'. . . ."[8] Nagel does not mean to suggest that scientific theories "tend in the limit to become empty of all content as their range of application becomes more inclusive." What he means is that "a theory seeks to formulate a highly general structure of relations that is invariant in a wide variety of experimentally different situations. . . ."[9] Within the general framework the theorist can make a very wide variety of special restrictive assumptions in order to apply the theory to numerous diverse observable situations. A theoretical law, or theory, may

---

[8]Nagel, Structure of Science, p. 79.

[9]Ibid., p. 104.

therefore be said to be wider than any particular observable data that it predicts or accounts for. This neither confirms nor denies the proposition that a theory is wider than all possible observable events. It merely means that the events that a theory can predict or account for, i.e., "explain," are likely to be so diverse that no particular set of restrictive assumptions will permit the theory its widest range of application. In other words, it is impossible to pin each term in a theory down to one particular empirical application without unduly restricting its application or "explanatory power." We may go still further. A theory may possess terms or statement-forms for which no empirical association is provided. These terms or statement-forms are necessary in order to elaborate the structure of the theory to give it logical completeness and empirical comprehensiveness.

There is no unique way in which a comprehensive theory may be articulated, so the particular logic structure will owe much to the special objectives and idiosyncrasies of its inventor, as Hertz pointed out. In practice, theories are seldom spelled out in complete formal detail. It is more typical to find many of the logical steps omitted. Instead the theory is stated in terms of a model suitable for a particular application of the theory. For this reason it frequently happens that more attention is paid to the empirical segments of a theory as applied to some particular problem than to the underlying logical framework which more properly constitutes the

theory itself. It is common for uncritical persons to overlook the fact that such models for theories usually contain terms or statement-forms which give names or make statements about things which, strictly speaking, have never been observed. Nagel discusses many such examples from the physical sciences.

In itself, a theory is nothing but an "abstract relational structure." Within the logical framework which is the theory, there are a number of nonlogical terms and statement-forms which have no meaning except as derived from their place within the theory. The only meaning these terms have is given to them by the theory itself; the terms are said to be implicitly defined.

> . . . insofar as the basic theoretical terms are only implicitly defined by the postulates of the theory, the postulates assert nothing, since they are statement-forms rather than statements (that is, they are expressions having the form of statements without being statements), and can be explored only with the view to deriving from them other statement-forms in conformity with the rules of logical deduction.[10]

Implicit definition of terms is obviously not sufficient. A theory possessing only implicit definitions would have no contact with the results of experimental science. And yet the hallmark of a good scientific theory is its ability to subsume a large and diverse measure of empirical data. Empirical knowledge differs from "theoretical knowledge" in that it is valid independently of the means used to

---

[10] *Ibid.*, p. 91.

discover or interpret it; connection established between empirical regularities and theories. "If the theory is to be used as an instrument of explanation and prediction, it must somehow be linked with observable materials."[11]

In order to make his theory an instrument of prediction, the theorist must specify, at least tacitly, what segments of the theory are to be regarded as the counterpoints of empirical data. For example, a line in the spectrum may be associated with the jump of an electron from one permissible orbit to another in the atom. The theoretical notion of electron jump is thus associated with the experimental notion of spectral line. Nagel gives the name "rules of correspondence" or "coordinating definitions" to the association of unobserved concepts with observable data. Which segments of the theory are given rules of correspondence depends upon the particular application being given to the theory at the time. As experimental knowledge progresses, old coordinating definitions are dropped and new ones supplied. Or perhaps a new coordinating definition is given to a term in the theory which had long remained only implicitly defined. The flexibility and versatility that scientific theory derives from its structure is one of its outstanding characteristics. The terms "atom" and "electron" are good examples in the physical sciences. From the time of Democritus or Dalton to the present day the concept "atom" has changed many times. Many times the term has been stripped of past connotations to be incorporated in fresh

---

[11] *Ibid.*, p. 93.

theoretical contexts having but remote connections with previous theories. When Samuelson quoted Hertz to the effect that the only meaning "electron" had was the meaning given it by the logical position of the symbol in Maxwell's electromagnetic equations, he was acknowledging the fact that electron was at that time a purely theoretical term. It was only later that the term acquired associations with experimental data, such as provided by Hertz, Millikin, Thomson and others.

Abuses of Deductive Method: Real and Imagined

Such are the distinctive features of scientific theorizing that we believe to be especially important in evaluating the methods and goals of deductive economic analysis. Earlier in this chapter we reviewed certain criticisms of economic analysis from the standpoint of theoretical goals. We may now summarize the significance of these criticisms with respect to theoretical method. At the simplest level is the popular criticism of economics, which holds that it is too far removed from the real world and describes activity that simply can't be found to occur in actual life. Economics is, like other social sciences, vulnerable to this level of criticism because it has developed rather esoteric and technical meanings for familiar words which are applied to familiar subjects. The result is doubly disconcerting to the layman, who feels that his own common sense must certainly be worth more. At a different level, Robinson's criticisms

of non-empirical, hence metaphysical, terms and statements, have been reviewed. We concluded that part of her criticism is based essentially on methodological principles which we have chosen to reject. Robinson, for example, seems to place too much emphasis on the metaphysical side of concepts like utility and Keynesian liquidity preference. Utility theory, as she so skillfully points out, has had its metaphysical abuses. But it is not a piece of metaphysics simply because it has been shown to be devoid of empirical content. Robinson nowhere discusses the function of the concept as an implicitly defined, uncoordinated term in the logical structure of the theory. She seems to adopt a much more stringent empirical criterion: only propositions that are capable of being falsified by evidence have any place in the empirical sciences. Liquidity preference is likewise for her a metaphysical device. She sees it as a useful concept to "get the theory on its feet" but not as essential to the theoretical structure. This doesn't go far enough. As Dillard pointed out, liquidity preference is vital to Keynes' theoretical framework, aside from its ultimate empirical status. By means of it, Keynes introduced the vital idea that monetary activity can have important effects upon income and employment, a revolutionary departure from classical theory. It also has provided the basis for empirical and theoretical research that continues to this day. And yet the concept is laconically stated and deeply embedded in the fundamental theoretical structure.

Our review of the well-known Clapham-Pigou discussion very clearly illustrates the same issues. Clapham argued that the law of diminishing returns is a clearly labeled box nicely arranged with a lot of other boxes on the shelves of economic theory. Open the box, however, and nothing is inside. This is the trouble with much of economic theory, argued Clapham. His point seems clear enough. Diminishing returns is neatly ensconced in the frame, but doesn't really contribute anything. Clapham, in effect, was adopting the methodological position advocated by Robinson: only empirical propositions are legitimate candidates for empirical science. Pigou's reply is fully consistent with the methodological position taken in this paper. He admitted the practical impossibility of clearly identifying an example of diminishing returns in actual operation. The real world is too cluttered up with technological advancement and other obfuscating events to permit positive identification. Yet the concept is so vital to the underlying logical framework of microeconomic theory that its removal would be destructive. Its value as an implicitly defined statement-form is sufficient to justify its presence in the theory. It is the theory as a whole, rather than individual segments within the theory, that must have valid empirical significance. Clapham's rejoinder that he felt justified in criticizing <u>particular parts</u> of a theory is a reiteration of his more stringent empirical position.

The work of Cyert and March,[12] and Modigliani[13] illustrate the fact that the rules of correspondence given economic theory in one application will not be suitable for every application of the theory. Cyert and March are primarily interested in developing a theory of the firm which takes into account such things as its complex organizational structure, lines of authority, complex objectives, and the like. To these ends the traditional theory of the firm contributes little understanding. There is no organizational structure in the traditional "firm," and it has only one uncomplicated motive. The traditional firm doesn't exist except in the mind of the economist; real world firms simply don't act the way the traditional theorist says they do. Traditional theory does not direct its attention to the day-to-day problems of the business manager. Yet Cyert and March are not merely engaging in facile popular criticism when they point these things out; they realize that traditional theory is designed to answer different sorts of questions--questions relating to economy-wide problems such as resource allocation under different market structures. For theory with these objectives, it is appropriate that the "firm" be simply an implicitly defined entity in the theory. Modigliani draws essentially the same distinctions and conclusions as Cyert and March.

---

[12] Cyert and March, A Behavioral Theory of the Firm.

[13] Modigliani, Papers and Proceedings of the American Economic Association, Vol. LI, No. 2, p. 158.

A new and differently structured theory is needed to answer the different set of questions. (The two types of theory--normative and positive, as they are sometimes called--are not actually this separate and distinct in practice. Each has a considerable amount to say about the primary concerns of the other, as is amply testified to in the literature.)

Rejoinder to Major Critique of Modern Deductive Method

The criticisms of Clapham and Robinson concerning the empirical requirements of theory foreshadow Samuelson's critique. Samuelson has expounded what we have called a "strong empirical position" with regard to theory. He explains in some detail his position that a theory has no room for statement-forms lacking valid empirical content. A theory is a completely empirical instrument, with respect to its assumptions on the one hand, and its conclusions on the other hand. As a matter of fact, Samuelson disparages the distinction between a theory and its conclusions; these are really identical. The conclusions of a theory cannot enjoy any empirical significance not already possessed by the theory itself, and conversely. If a theory entails certain empirically invalid conclusions, then the theory, or at least the invalid portion of it, must be jettisoned. Only the minimal set of assumptions giving rise to the complete set of empirically valid conclusions has justification in a scientific theory. Everything else must be stripped away by Occam's razor. It may be that apparently superfluous assumptions will justify themselves in time. We may therefore suspend judgment on them for the time being.

Since a theory is a completely empirical instrument in Samuelson's view, it is only to be expected that he emphasizes his aversion to inflated "explanatory" pretentions on the part of scientific theory. Explanation in the sense rejected by Samuelson means any alleged significance beyond the ascertainable observable events summarized or predicted by the theory. Samuelson stresses the conviction shared by modern logical empiricists that any goals ascribed to scientific theory beyond the description of observable regularities is not only futile, but detrimental to the progress of knowledge as well.

In support of his position, Samuelson cites the work of such distinguished scientists as Mach, Pierce, Hertz and Bridgman, some of whom have been reviewed very briefly in this paper. In his most recent contribution to the discussion, Samuelson cites the work of Ernest Nagel, used so extensively in this paper, as support for his position.[14]

We have stated our views of Samuelson's position and have rejected his "strong empirical principle" in favor of a "weak empirical principle." There is one critical difference between these two positions. Whereas Samuelson insists upon empirical content for every statement-form appearing in a theory, we follow Nagel in making an essential distinction between the factual

---

[14]Paul A. Samuelson, "Professor Samuelson on Theory and Realism: Reply," The American Economic Review, Vol. LV, No. 5 (December, 1965), pp. 1164-1172.

consequences of a theory and the completely extra-factual logical framework that forms the basis for empirical investigation. If, following Nagel, we look carefully into the formal properties of deductive theory, we find that the theory, standing by itself, is a pre-scientific instrument of analysis. It has no empirical content, and none can be inferred from it. Rules of correspondence must be introduced to enable the analyst to draw factual conclusions, C^*, from the logical framework, B. When rules of correspondence, Ri, are added to the underlying frame, B, we get $B + Ri \rightarrow B^*$, a theory with some specified empirical content. That the set of coordinating definitions, [R], can be specified in many different ways for the theory B, is what gives B so many "degrees of freedom," i.e., so many different modes of application among diverse empirical situations. It is what makes a theory "wider than its conclusions," as Machlup put it. Samuelson makes no distinction in his recent writing between B and B^*; he admits only B^*. We have examined how the introduction of the distinction forces us to abandon Samuelson's logic, which we find valid only for the case he has identified, i.e., no distinction between B and B^*.

Why do we distinguish between Samuelson's strong position as opposed to our weak one? The answer is that for Samuelson, all statement-forms $x_i \subset B$ must possess empirical content. Following Nagel, we maintain that only some $x_i \subset B$ need coordinating definitions such that $B + R_i \rightarrow B^* \leftrightarrow C^*$. The weak empirical position gives greater

generality to theory than does Samuelson's latest methodological statement.[15] The reason is that there is an almost indefinitely large set of $[B + R_i]$ which yield B^* of different characteristics applicable to widely differing circumstances. B is invariant; B^* takes on many different forms. But while B itself once formulated is invariant under its many diverse specifications, nevertheless B may be formulated in a number of different ways, each way possessing promising empirical possibilities. Which B is best can be determined only by the ultimate touchstone--empirical performance. And there is little to be said on these methodological grounds against ulterior (extra-empirical) motivation determining a theorist's choice of theoretical structure, B.

In a way we are back where we started: it appears as though one's choice of methodology is determined in large measure by one's attitude about the nature and purpose of theorizing, and how many pitfalls one is willing to risk in pursuit of one's values. There is no one unique scientific method against which all scientific endeavor can be measured. Different methodologies will be developed to formalize the explanation of different methods of research. The choice that any individual or school makes must in the last analysis be an eclectic one. Our choice of methodological principles reflects

---

[15](As we saw earlier, in Foundations, Samuelson explains how the use of analogy permits considerable generality. Yet his distinction in Foundations between the logical structure of theory and its empirical counterpart appears to be missing in his most recent writing.)

the belief that the extra-empirical goals so boldly worked for by earlier workers are still important goals today. It reflects the belief that the returns accruing to economists working within well-established theoretical traditions are so high that their work will continue to stubbornly resist critical onslaughts based on different methodologies, as has happened in the past. Samuelson simply throws out too many babies with the bath. The Stiglerian tradition of deductive theorizing is a prominent one, and justifies the eclectic choice of methodology made here. There is much modern work being done in economics that closely follows the orientation of Samuelson and the logical empiricists. The diversity is evident in the physical sciences and the rest of the social sciences. There is nothing unhealthy about this diversity; it is simply myopic for any one camp to proclaim--my theory of theories above all others. It is reasonable to expect diversity of theoretical techniques to continue in the future. But there seems to be no conclusive reason to write off the traditionally-oriented deductive system as a thing of the past. Much of the most important work in economics, as elsewhere, has been of that form. It is reasonable to expect future recurrences of important new systems to reflect developments in analytical technique, social organization and outlook.

Deductive Analysis in the Social Sciences

When the social scientist formulates the restrictive behavioral assumptions and the interdependent functional relationships that constitute deductive theory, he has constructed a theoretical structure which is operationally significant. Operationally significant theory is theory from which can be deduced empirical hypotheses capable of being refuted. Both the generality and the operational significance of a deductive theory depend on its postulated behavioral relationships. Hence, a distinguishing characteristic of the social sciences is their commitment to explanation of events in terms of motivated behavior. The influence of motivated behavior is coextensive with the scope of explanation in the social sciences. When physical anthropologists realized the need to bring together into a unified theory of evolution the basic variables drawn from the life sciences and the science of culture, human evolution became part of the social sciences proper. At that point also, human evolution became a subject capable of treatment in terms of deductive analysis. The experience of the economist provides one suggestion as to how this may be accomplished.

There is nothing obvious that problems of evolutionary theory faced by the physical anthropologist are formally similar, for example,

to problems of national income theory faced by the economist. Yet, we have seen that basic to each is an understanding of motivated behavior which in turn is fundamentally influenced by the cultural milieu of the community being investigated. Motivated behavior is "understood" in the social sciences in terms of the values believed to guide the actions which have brought about the events being studied. When Machlup points these facts out, he is asserting something that is by no means obvious. Yet it is perhaps one of the most fundamental postulates of deductive analysis in the social sciences, because it is the basis for any belief in the efficacy of deductive theory.

Economics became a social science in the modern sense when it discarded teleological reasoning more or less characteristic of theoretical systems prior to Keynes. The reason, of course, is that teleological reasoning retains a fundamental commitment to the discovery of laws external to and beyond the control of human agents. The transition was slow in coming perhaps because of the powerful attraction of the mechanistic systems of the physical sciences. Yet the new orientation has provided a fully promising alternative orientation.

The economist's experience with deductive analysis is strikingly germane to psychoanalytic theory as well. The controversies reviewed in this paper are parallel to the general issues in a fundamental way. It seems natural, especially for one trained in economics, to glean from discussion among psychologists that Freudian theory is especially

well suited to formalization along deductive lines, much as Keynesian theory has been formalized. The testimony of psychologists suggests that systematization of Freudian theory has not been carried forward to any great degree. If this is true, then it is reasonable to speculate that theoretical psychology is less rich for the absence of theorists experienced in the methods of deductive theory.

That the deductive pattern of explanation is basic to scientific method without regard to disciplinary boundaries has been a basic premise of the present paper. Examination of examples of deductive analysis in various fields has revealed many formal similarities of theoretical structure and intention. Even the problems and controversies have been similar. The brief survey of theoretical problems and methods in anthropology and psychology have confirmed our belief in these similarities.

BIBLIOGRAPHY

Books and Monographs

Baumol, William J. Economic Dynamics. 2d ed. New York: The Macmillan Company, 1959.

Boulding, Kenneth E. Economic Analysis. 3d ed. New York: Harper and Brothers, 1955.

_____., and Stigler, George J. (eds.). Readings in Price Theory. "The Series of Republished Articles on Economics"; Chicago: Richard D. Irwin, Inc., 1952, Vol. VI.

Bridgman, P. W. The Nature of Physical Theory. New York: Dover Publications, 1936.

Brunswik, Egon. The Conceptual Framework of Psychology. "Foundations of the Unity of Science," Vol. I, No. 10; Chicago: The University of Chicago Press, 1952.

Cyert, Richard M., and March, James G. A Behavioral Theory of the Firm. Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1963.

Davenport, Herbert Joseph. The Economics of Enterprise. New York: The Macmillan Company, 1925.

Duesenberry, James A. Business Cycles and Economic Growth. New York: McGraw-Hill Book Company, Inc., 1958.

Ellsworth, P. T. The International Economy. 3d ed. New York: The Macmillan Company, 1964.

Fels, Rendigs. American Business Cycles: 1865-1897. Chapel Hill: University of North Carolina Press, 1959.

Frank, Philipp G. (ed.). The Validation of Scientific Theories. New York: Collier Books, 1961.

Friedman, Milton. Essays in Positive Economics. Chicago: The University of Chicago Press, 1953.

Fromm, Erich. May Man Prevail? An Inquiry into the Facts and Fictions of Foreign Policy. Garden City, N. Y. : Doubleday and Company, Inc., 1961.

Hall, Calvin A. A Primer of Freudian Psychology. New York: The New American Library of World Literature, Inc., 1954.

Heilbroner, Robert L. The Worldly Philosophers: The Lives, Times, and Ideas of the Great Economic Thinkers. New York: Simon and Schuster, 1953.

Hempel, Carl G. Fundamentals of Concept Formation in Empirical Science. "Foundations of the Unity of Science," Vol. II, No. 7; Chicago: The University of Chicago Press, 1952.

Hicks, J. R. Value and Capital. 2d ed. London: Oxford University Press, 1946.

Joergensen, Joergen. The Development of Logical Empiricism. "Foundations of the Unity of Science," Vol. II, No. 9; Chicago: The University of Chicago Press, 1951.

Jung, C. G. The Undiscovered Self. Trans. R. F. C. Hull. New York: The New American Library, 1957.

Katona, George. Psychological Analysis of Economic Behavior. New York: McGraw-Hill Book Company, Inc., 1951.

Kenen, Peter B. International Economics. "Foundations of Modern Economics Series"; Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1964.

Keynes, John Maynard. The General Theory of Employment, Interest and Money. New York: Harcourt, Brace and Company, [1936].

Knight, Frank H. On the History and Method of Economics. Chicago: The University of Chicago Press, 1956.

Kroeber, A. L. (ed.). Anthropology Today. Chicago: The University of Chicago Press, 1953.

Kurihara, Kenneth K. (ed.). Post-Keynesian Economics. New Brunswick, N. J.: Rutgers University Press, 1954.

Lasker, Gabriel W. (ed.). Physical Anthropology: 1953-1961. Yearbook of Physical Anthropology. Cordoba, Mexico: Instituto de Investigaciones Historicas Universidad Nacional Autonoma de Mexico, 1964, Vol. 9.

Leftwich, Richard H. The Price System and Resource Allocation. 1st ed. New York: Rinehart and Company, Inc., 1955.

Mitchell, Wesley C. (ed.). What Veblen Taught: Selections from the Writings of Thorstein Veblen. New York: The Viking Press, 1947.

Myrdal, Gunnar. The Political Element in the Development of Economic Theory. Trans. Paul Streeten. Cambridge: Harvard University Press, 1961.

Nagel, Ernest. Logic Without Metaphysics and Other Studies in the Philosophy of Science. Glencoe, Illinois: The Free Press, 1956.

_____. The Structure of Science: Problems in the Logic of Scientific Explanation. New York: Harcourt, Brace and World, Inc., 1961.

Ohlin, Bertil. Interregional and International Trade. "Harvard Economic Studies"; Cambridge: Harvard University Press, 1957, Vol. XXXIX.

Polsby, Nelson W., Deatler, Robert A., and Smith, Paul A. (eds.). Politics and Social Life. Boston: Houghton Mifflin Company, 1963.

Rieff, Philip. (ed.). The History of the Psychoanalytic Movement, and Other Papers [by Sigmund Freud]. New York: Collier Books, 1963.

Robinson, Joan. Economic Philosophy. Chicago: Aldine Publishing Company, 1962.

Rogin, Leo. The Meaning and Validity of Economic Theory: A Historical Approach. New York: Harper and Brothers, 1956.

Russell, Bertrand. The Scientific Outlook. New York: W. W. Norton and Company, Inc., 1931.

Samuelson, Paul A. Economics: An Introductory Analysis. 6th ed. New York: McGraw-Hill Book Company, Inc., 1964.

_____. Foundations of Economic Analysis. "Harvard Economic Studies"; Cambridge: Harvard University Press, 1947, Vol. LXXX.

de Santillana, George, and Zilsel, Edgar. The Development of Rationalism and Empiricism. "Foundations of the Unity of Science," Vol. II, No. 8; Chicago: The University of Chicago Press, 1941.

Schilpp, Paul Arthur. (ed.). Albert Einstein: Philosopher-Scientist. New York: Harper and Row, Publishers, 1959.

Schrödinger, Erwin. What Is Life? and Other Scientific Essays. Garden City, N. Y.: Doubleday and Company, Inc., 1956.

Smithies, Arthur, and Butters, J. Keith. (eds.). Readings in Fiscal Policy. Homewood, Ill.: Richard D. Irwin, Inc., 1955, Vol. VII.

Soule, George. Ideas of the Great Economists. New York: The New American Library, 1952.

Spengler, Joseph J., and Allen, William R. (eds.). Essays in Economic Thought: Aristotle to Marshall. Chicago: Rand McNally and Company, 1960.

Sraffa, Piero. (ed.). The Works and Correspondence of David Ricardo. Vol. I: On the Principles of Political Economy and Taxation. Cambridge, England: Cambridge University Press, 1962.

Stigler, George J. The Theory of Price. Rev. ed. New York: The Macmillan Company, 1952.

Walsh, W. H. Philosophy of History: An Introduction. New York: Harper and Row, Publishers, 1958.

Articles

Baumol, William J. "What Can Economic Theory Contribute to Managerial Economics?" Papers and Proceedings of the American Economic Association, Vol. LI, No. 2 (May, 1961).

Born, Max. "Einstein's Statistical Theories," Albert Einstein: Philosopher-Scientist, Vol. I, ed. Paul Arthur Schilpp. New York: Harper and Row, Publishers, 1959.

Boyd, William C. "The Contributions of Genetics to Anthropology," Anthropology Today. Ed. A. L. Kroeber. Chicago: The University of Chicago Press, 1953.

Clapham, J. H. "Of Empty Economic Boxes," Readings in Price Theory. Ed. Kenneth E. Boulding and George J. Stigler. "The Series of Republished Articles on Economics"; Chicago: Richard D. Irwin, Inc., 1952, Vol. VI.

_____. "The Economic Boxes: A Rejoinder," Readings in Price Theory.

Cyert, R. M., and Grunberg, E. "Assumption, Prediction, and Explanation in Economics," A Behavioral Theory of the Firm. Richard M. Cyert and James S. March. Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1963.

Dillard, Dudley. "The Theory of a Monetary Economy," Post-Keynesian Economics. Ed. Kenneth K. Kurihara. New Brunswick, N. J.: Rutgers University Press, 1954.

Dirac, P. A. M. "The Evolution of the Physicist's Picture of Nature," Scientific American, Vol. CCVIII, No. 5 (May, 1963).

Fellner, William. "Monetary Policy and the Elasticity of Liquidity Functions," The Review of Economic Statistics, Vol. XXX, No. 1 (February, 1948).

Frenkel-Brunswik, "Confirmation of Psychoanalytic Theories," The Validation of Scientific Theories. Ed. Philipp G. Frank. New York: Collier Books, 1961.

Friedman, Milton. "The Methodology of Positive Economics," Essays in Positive Economics. Chicago: The University of Chicago Press, 1953.

Garb, Gerald. "Professor Samuelson on Theory and Realism: Comment," The American Economic Review, Vol. LV, No. 5 (december, 1965).

Hitch, Charles J., and McKean, Roland N. "What Can Managerial Economics Contribute to Economic Theory?" Papers and Proceedings of the American Economic Association, Vol. LI, No. 2 (May, 1961).

Knight, Frank H. "Economics," On the History and Method of Economics. Frank H. Knight. Chicago: The University of Chicago Press, 1956.

Lerner, Abba P. "Professor Samuelson on Theory and Realism: Comment," The American Economic Review, Vol. LV, No. 5 (December, 1965).

Machlup, Fritz. "Are the Social Sciences Really Inferior?" The Southern Economic Journal, Vol. XXVII, No. 3 (January, 1961).

_____. "Marginal Analysis and Empirical Research," The American Economic Review, Vol. XXXVI, No. 4 (September, 1946).

_____. "Problems of Methodology-Introductory Remarks," Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2 (May, 1963).

_____. "Professor Samuelson on Theory and Realism," The American Economic Review, Vol. LIV, No. 5 (September, 1964).

Massey, Gerald J. "Professor Samuelson on Theory and Realism: Comment," The American Economic Review, Vol. LV, No. 5 (December, 1965).

Modigliani, Franco. "Managerial Economics--Discussion," Papers and Proceedings of the American Economic Association, Vol. LI, No. 2 (May, 1961).

Nagel, Ernest. "Assumptions in Economic Theory," Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2 (May, 1963).

Pigou, A. C. "Empty Economic Boxes: A Reply," Readings in Price Theory. Ed. Kenneth E. Boulding and George J. Stigler. "The Series of Republished Articles on Economics"; Chicago: Richard D. Irwin, Inc., 1952, Vol. VI.

Rapaport, Anatol. "Various Meanings of 'Theory'," Politics and Social Life. Ed. Nelson W. Polsby, Robert A. Dentler, Paul A. Smith. Boston: Houghton Mifflin Company, 1963.

Richfield, Jerome. "the Scientific Status of Psychoanalysis," The Validation of Scientific Theories. Ed. Philipp G. Frank. New York: Collier Books, 1961.

Rieff, Philip. "Introduction," The History of the Psychoanalytic Movement, and Other Papers [by Sigmund Freud]. Ed. Philip Rieff. New York: Collier Books, 1963.

Samuelson, Paul A. "Problems of Methodology--Discussion," Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2 (May, 1963).

_____. "Professor Samuelson on Theory and Realism: Reply," The American Economic Review, Vol. LV, No. 5 (December, 1965).

_____. "Theory and Realism: A Reply," The American Economic Review, Vol. LIV, No. 5 (September, 1964).

Schrödinger, Erwin. "Are There Quantum Jumps?" What Is Life? and Other Scientific Essays. Garden City, N. Y.: Doubleday and Company, Inc., 1956.

_____. "On the Peculiarity of the Scientific World View," What Is Life? and Other Scientific Essays.

_____. "The Spirit of Science," What Is Life? and Other Scientific Essays.

Scriven, Michael, "Notes on the Discussion Between E. Frenkel-Brunswik and B. F. Skinner," The Validation of Scientific Theories. Ed. Philipp G. Frank. New York: Collier Books, 1961.

Sears, William H. "The Study of Social and Religious Systems in North American Archaeology," Current Anthropology, Vol. II, No. 3 (June, 1961).

Simon, Herbert A. "Problems of Methodology--Discussion," Papers and Proceedings of the American Economic Association, Vol. LIII, No. 2 (May, 1963).

Skinner, B. F. "Critique of Psychoanalytic Concepts and Theories," The Validation of Scientific Theories. Ed. Philipp G. Frank. New York: Collier Books, 1961.

Stigler, George J. "The Influence of Events and Policies on Economic Theory," Papers and Proceedings of the American Economic Association, Vol. L, No. 2 (May, 1960).

Tobin, James. "A Rejoinder [to Warburton]," The Review of Economic Statistics, Vol. XXX (November, 1948).

_____. "Liquidity Preference and Monetary Policy," Readings in Fiscal Policy. Ed. Arthur Smithies and J. Keith Butters. Homewood, Ill.: Richard D. Irwin, Inc., 1955, Vol. VII.

Veblen, Thorstein. "The Preconceptions of Economic Science," What Veblen Taught: Selections from the Writings of Thorstein Veblen. Ed. Wesley C. Mitchell. New York: The Viking Press, 1947.

Viner, Jacob. "Adam Smith and Laissez Faire," Essays in Economic Thought: Aristotle to Marshall. Ed. Joseph J. Spengler and William R. Allen. Chicago: Rand McNally and Company, 1960.

Warburton, Clark. "Monetary Velocity and Monetary Policy," The Review of Economic Statistics, Vol. XXX, No. 4 (November, 1948).

Washburn, A. L. "The Strategy of Physical Anthropology," Physical Anthropology: 1953-1961. Ed. Gabriel W. Lasker, Yearbook of Physical Anthropology. Cordoba, Mexico: Instituto de Investigaciones Historicas Universidad Nacional Autonoma de Mexico, 1964, Vol. 9.

BIOGRAPHICAL SKETCH

Robert George Fabian was born July 17, 1938, in Chicago, Illinois. In June, 1956, he was graduated from Bishop Noll High School, Hammond, Indiana. In August, 1960, he received the degree of Bachelor of Arts from the University of Notre Dame. In August, 1962, he received the degree of Master of Arts from the University of Illinois, where he served during his last year of residence as research assistant. In September, 1962, he enrolled in the Graduate School of the University of Florida. He worked as a graduate assistant in the Department of Economics until June, 1964. He worked as interim instructor in the Department of Economics at Florida Atlantic University for one year beginning in August, 1964. Since that time he has worked as interim assistant professor at Florida Atlantic University, while he has pursued his work toward the degree of Doctor of Philosophy. Mr. Fabian is a member of the American Economic Association.

This dissertation was prepared under the direction of the chairman of the candidate's supervisory committee and has been approved by all members of that committee. It was submitted to the Dean of the College of Business Administration and to the Graduate Council, and was approved as partial fulfillment of the requirements for the degree of Doctor of Philosophy.

August, 1966

Dean, College of Business Administration

Dean, Graduate School

Supervisory Committee:

Chairman