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A SEARCH FOR THE GROWTH OF ECONOMIC KNOWLEDGE: POPPER AND METHODOLOGICAL PROGRESS

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A SEARCH FOR THE GROWTH OF ECONOMIC KNOWLEDGE:
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To the librarian:

This thesis ~~is not~~ is to be regarded as confidential

Glenn R Hueckel
Professor in charge of the thesis

In memory of my father,

Donald H. Stanley

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Looking back over the years involved in this task, I realized that many people contributed to this enterprise. Though a complete recording is impossible, a few must be explicitly mentioned.

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GLOSSARY*

A periphrastic study in a worn-out poetical fashion
Leaving one still with the intolerable wrestle
With words and meanings. The poetry does not matter.

(T. S. Eliot, Four Quartets. Burnt Norton, 1935)

It is impossible to dissociate language from science or science from language, because every natural science always involves three things: The sequence of phenomena on which the science is based; the abstract concepts which call these phenomena to mind; and the words in which the concepts are expressed. To call forth a concept a word is needed; to portray a phenomenon, a concept is needed. All three mirror one and the same reality.

(Antoine Lavoisier, Traite' E'limentaire de Chimi, 1778)

This glossary is provided to define, as unambiguously as possible, a few key words. To rationally discuss any subject, the author must attempt to employ an unambiguous, intersubjective language which is consistent with the likely interpretation of his readers. In return, the language used by the critic and the content of the criticism must correspond to that of the original author. Otherwise, the discourse is beside the point, a meaningless conversation. English contains a multiplicity of ambiguities and biases. Yet, it can be meaningfully employed, if there is some understanding between the reader and the author. Towards this end, the glossary is a short step.

* The reader who is familiar with the writing of Karl Popper might find this glossary unnecessary. I have attempted to employ a vocabulary consistent with his.

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The following definitions are denoted for words single quoted in the text.

basic statement: a single instance of the occurrence of some phenomenon. An 'observation.' Ex: the price of unleaded gasoline at Standard of West Lafayette is $1.24.9 on April 1, 1980. Positivists refer to basic statements as photocol sentences. A 'basic statement' must be expressed in an inter-subjective language and be capable of being ascertained, at least in principle, by anyone. Though we may consider them solid, they are not immutable. 'Basic statements' will always contain some degree of convention and anthropomorphic bias.

conjecture: a hypothesis, a guess, any statement about some general state of affairs. 'Scientific conjectures' concern phenomena, potentially 'observable,' and they must be 'falsifiable.'

corroboration: an unsuccessful attempt to empirically 'falsify' a 'theory' or statement.

empirical content: the set of 'basic statements' or 'facts' which would 'falsify' (not correspond to) a particular 'theory' or set of statements.

fact: equivalent to an accepted 'basic statement.' To establish the 'facts,' 'observation' and inter-subjective agreement is necessary.

faith: a belief that cannot be 'tested' (some would say "proved"). These beliefs are 'metaphysical,' 'subjective' second world notions.

false: any statement which does not correspond to the 'facts,' not 'true.'

falsify: a 'theory' or statement is 'falsified' when it does not correspond to the 'facts.'

falsifiable, testable, and refutable: capable, in principle, of being 'falsified.'

formal theory: a mathematical or logical theory. 'Formal theory' has no 'observable' consequences. It cannot be 'tested' or 'falsified,' thus, not 'scientific.'

initial conditions: singular existential statements that have the same form as 'basic statements.' 'Initial conditions' are used to specify the free variables of a 'theory.' A 'theory' has no "predictions" and is not 'falsifiable' without 'observed' and measured 'initial conditions.'

knowledge: the information gained from 'testing.' Quantitatively, it is the measure of the set of statements and 'theories' offered for discussion that do not correspond to the 'facts.' Yet, it will be used in a more common manner. 'Facts' are part of our knowledge, 'factual knowledge.' Ex: we 'know' that Galileo's law is 'false.'

metaphysics: any statement that is not 'testable,' an 'irrefutable' statement or 'theory.' One may have 'faith' in 'metaphysics' or may rationally argue for or against a 'metaphysical theory.' But, 'metaphysical' statements can neither be proved nor 'falsified.'

objective: a statement or perspective is 'objective' if it can be stated in inter-subjective language and if, in principle, others come to the same conclusions under the same circumstances. 'Objectivity' is the quality of the 'facts,' 'basic statements,' and 'theories.' 'Facts' are objective in the sense that they are independent of what anyone wishes to believe. Yet, this 'objectivity' is consistent with the 'fact' that 'facts' are the product of man. The concept of classical objectivity is stronger, for it requires the complete independence of man. Ex: a table is 'objective.' The collection of properties, called 'table,' are independent of anyone's beliefs or perceptions. Yet, a table is not objective in the classical sense, for it is a product of man without whom it would not exist or be a meaningful concept. 'Facts' are like tables; they are produced by man but, once created, are independent of his beliefs.

observation: the act of recording the occurrence of some phenomenon. (It is a primitive term.) To 'observe objectively' is the first step in establishing the 'facts.' But, 'facts' may be transformations and conjunctions of 'objective observations.'

observational theory: a 'theory' concerning the process of 'observation.' All 'observations' are 'theories' at the metalevel. For example, the operation of the eye is a 'theory' of optics. What we see depends, at least in part, upon the observation 'theory' of the eye's operation.

rational criticism, rational debate, rational demonstration, rational discourse, rational discussion: a reasonable and sensible argument, discourses that are consistent with the Greek tradition. Such a discussion can never establish proof or certainty, but it may provide good reasons for or against some proposition or position.

reality: a world of 'objectivity,' independent of human beliefs. Like 'facts,' man may create 'reality,' but once created, it is autonomous to him.

refute: equivalent to 'falsify.'

science: the field of human endeavor that creates 'knowledge.' 'Science' requires 'testability' along with 'rational discourse.'

subjective: not 'objective.'

test: a comparison of a 'theory' or statement with the 'facts.' Testing requires that the 'theory' be 'refutable,' that 'observation' and measurement of the 'initial conditions' be made (e.g., if the law of demand is to be 'tested,' we must measure the price change and check if other prices are constant), and that the results could, in principle, be replicated by anyone.

theory: a 'testable' statement about some general state of affairs. Mathematical theories are not 'theories.' 'Theories' must be 'falsifiable.' 'Theories' contain a set of primitive propositions (sometimes called "assumptions"), derived statements or implications, and rules of correspondence. These latter statements equate some theoretical terms to 'observable' phenomena. Rules of correspondence must be stated or implicitly obvious; otherwise, the 'theory' is not 'falsifiable,' thus theory.

truth: the correspondence of some statement to the 'facts.' 'Truth' cannot be proved but only demonstrated. Logical truth-values need not be 'true'; unless they employ this correspondence theory of 'truth,' their truth-value is only convention. See Tarski (1956) for a rigorous treatment of the correspondence theory of 'truth.'

useful: the property of having value towards some end. 'Useful' cannot be defined independent of some specific normative (or evaluative) criterion. 'Usefulness' is a teleological concept.

ABSTRACT

Stanley, Tommy D. Ph.D., Purdue University, December 1982. A Search for the Growth of Economic Knowledge: Popper and Methodological Progress. Major Professor: Glenn R. Hueckel.

"What is truth? What is knowledge? How can knowledge be identified? How can the growth of knowledge be enhanced? What are the relationships among epistemology, methodology, and the history of science? What is the proper relation between theory and fact? How can alternative philosophies and methodologies of science be adequately evaluated? How should scientific theories be appraised? Which methodology provides the best means for the production of knowledge? What have been the methodologies of economics, and are they adequate? Can a methodology that sees only practical success be practically successful?" are some of the questions discussed by this thesis.

In general, we present a critical analysis of the philosophy of science, the methodology of science, and a few problems of economics. Although the methodologies of economics are found insufficient, Popper's falsificationism can provide an adequate epistemological foundation for economics and a technology for its growth.

More specifically, we present a rational refutation of Friedman's "methodology of positive economics," a response to Boland's "critique of Friedman's critics," and a resolution to the "assumptions debates." First, we find Friedman's essay to be "hopelessly ambiguous" and thus "economics as you like it." When interpreted in its best light, as

instrumentalism, Friedman's methodology is inadequate.

(1) "Predictive success" is an inadequate regulative principle, since it requires the immutability of the facts and a valid method of induction.

(2) Therefore, instrumentalism has no basis upon which to predict the success of an accepted theory.

(3) Instrumentalism cannot produce practical utility.

(4) Since Friedman's methodology reduces all economic inquiry to the "art of political economy," it cannot supply the knowledge of "what is" or "what should be" that practical application demands.

(5) Any claim of "confidence," "success," or practical value from a purely instrumental science presupposes the possession of the same knowledge that science seeks to discover.

(6) Friedman's "methodology of positive economics" is inconsistent, since instrumentalism is but the "art of political economy."

Is this not a rational refutation of the "methodology of positive economics"?

PROLOGUE

"Any path is only a path, and there is no affront, to oneself or to others, in dropping it if that is what your heart tells you .... Look at every path closely and deliberately. Then ask yourself, and yourself alone, one question .... Does this path have a heart? If it does, the path is good; if it doesn't, it is of no use."

- Carlos Costaneda

The Teaching of Don Juan

"For myself, I am interested in science and in philosophy only because I want to learn something about the riddle of the world in which we live, and the riddle of man's knowledge of that world. And I believe that only a revival of interest in these riddles can save the sciences and philosophy from narrow specialization and from an obscurantist faith in the expert's special skill and in his personal knowledge and authority; a faith that so well fits our 'post-rationalist and post-critical' age, proudly dedicated to the destruction of the tradition of rational philosophy, and of 'rational thought' itself."

- Karl Popper

The Logic of Scientific Discovery
p. 23

Achilles: But I will catch you sooner or later -- most likely sooner.

Tortoise: Not if things go according to Zeno's paradox you won't. Zeno is hoping to use our footrace to show that motion is impossible, you see. It is only in the mind that motion seems possible, according to Zeno. In truth, Motion Is

> Inherently Impossible. He proves it quite elegantly.
>
> - Douglas R. Hofstadter
>
> Gödel, Escher, Bach: An Eternal Golden Braid, p. 29

The most terse statement of much contemporary philosophical thought might be:

S_1: All generalizations are false.

S_2: Yet, some generalizations are necessary for rational thought.

The conjunction of these two statements forms the syllogism, S_1 and S_2 $\rightarrow S_3$: Some false statements are necessary for rational thought. Although the conclusion, S_3, is logically impeccable, many would find it inconsistent or, perhaps, simply false. From whence comes this unfortunate result? Does the fault lie in one of the "assumptions"? Or, is logic itself at fault? Are we mistaken to perceive a weakness in this set of statements? Or, is the problem merely a reflection of the poverty of language in expressing valid statements? This introductory exercise acquaints the reader with the level of discourse employed in this thesis, and introduces a mode of analysis that is both necessary and sufficient to grapple with methodological issues.

Let us first examine the "assumptions" of this syllogism. S_2 can be seen as a harmless proposition. The only potential problem concerns the definitions of "generalization" and "rational thought." Let "rational thought" include any reasoned inquiry, including scientific theories, logic, mathematics, and empirical statements of fact. In a correspondingly, general manner, we take generalization as assertions about properties that hold for all members of a particular class.[1]

I assert that S_2 is a "true" statement and cannot be rationally refuted. Some might respond that this is merely an assertion and that a "proof" is required before one has the right to make any such statement. Yet, such a "proof" would be impossible and unnecessary.

What form would a potential "proof" take? Would it show that all the specific instances that logically emanate from S_2 are themselves "true"? Though a "proof" constructed in this manner would indeed be valid, it is most unlikely that such a "proof" could be found.[2] S_2 is itself a generalization that applies to an infinity of specific cases (uncountable infinity, at that). Thus, any "proof" that attempts to build from an infinity of specific instances to the general is humanly impossible and doomed to failure.[3] Finitely many specific validations do not an infinite generalization make.

Still we might ask, Can the "truth" of one of these specific instances be ascertained? Logically, S_2 states that there exists at least one generalization in every system of rational thought. Therefore, we could investigate some specific system of rational thought, say Newtonian physics, to determine if a generalization is present. Such an effort might get lucky, and quickly find an obvious generalization. For example, "for every action there is an opposite and equal reaction." But what if our inquiry is not so fortunate? If after considerable effort no generalization is found, can we logically conclude that none exists? No, existential statements (i.e., statements that have the logical form, there exists ...) can never be refuted. It would always be possible to argue that the investigator simply did not look in the right place or did not search long enough. Thus, there is an asymmetry involving the "verification - falsification" of existential statements.

One observation may be sufficient to verify the "truth" of an existential statement, but no number of observations can serve to falsify such a statement.

In sum, we can conclude that both the "proof" and the "disproof" of S_2 are impossible, if we limit our inquiry to specific instances implied by S_2. Though both the critic and the advocate would be blocked from this approach to "proof," it is not the only one.

We might instead heed the wisdom that says: "Never consider a statement in isolation. Always evaluate a statement as part of a larger system of thought." Because S_2 is among the more general propositions, the larger system of thought must be the general theory of knowledge itself (or, if you prefer, epistemology). But here, we will not avoid the grounding of S_2 on some other assertion.

The advocate of S_2 might formulate his epistemology along the following lines.[4]

E_1: All knowledge concerns theories.

E_2: All theories are universal statements.

Where, universal statements have the logical form, for all ...
If then "rational thought" is taken as a subset of knowledge or is required to contain knowledge, it follows indubitably that "rational thought" must contain some generalizations. Yet, all that is accomplished by this effort is to shift the burden of "proof."

Again, we would fail in an attempt to establish the "truth" of E_1 and E_2 by a consideration of specific instances. Since E_1 and E_2, like S_2, are meant to apply to many cases, infinite in number, establishing the "truth" of finitely many specific instances could never result in the "proof" of the general statement. Yet, E_1 and E_2 have a different logical character than S_2. They are stated in the form of a universal statement (as evidenced by "all") and as such can potentially be falsified. Falsification of either E_1 or E_2 requires only the discovery of one specific instance in which E_1 or E_2 is "false." Again, a logical assymmetry exists.[5] Thus, the conjunction of E_1 and E_2 is in some sense superior to S_2, for it allows the critic to "disprove" them by the discovery of a counter-example. But, E_1 and E_2 cannot be "proved." Therefore, we are no closer to our goal of "proving" S_2.

The effort to place S_2 in a larger system of thought is itself subject to criticism. The critic may not accept the broader system, E_1 and E_2, and formulate an alternative system. For example,

> A_1: All "true" knowledge concerns only direct and immediate perception of the essential nature of specific events and objects.

If A_1 is accepted as "true," then S_2 would certainly be "false"; for, if knowledge concerns only specifics, (A_1), then generalizations are not necessary ($\sim S_2$). The critic's alternative, however, could be falsified by finding a counter-example. If I assert that Newton's inverse square law constitutes knowledge and that it is a general statement which has nothing to do with direct or immediate perception, then A_1 would be falsified. Yet, the critic could dogmatically defend A_1 by *ad hoc* rationalizations of every potential counter-example or by raising A_1 to the level of the definition of knowledge. If either tack is consistently maintained, then the critic's system is irrefutable and thus tautologically true. Such tactics only confound the original problem; and any insulation from criticism can only retard the growth of knowledge. Anticritical stratagems are responsible for "false" knowledge and for the degeneration of the growth of knowledge, not, as some think, the lack of "proof."

Perhaps the "best" argument for the veracity of S_2 is simply to assert it and to describe where and how it holds. Rational thought requires the existence of some generalization. This may be seen in science where all scientific theories contain at least one statement which is held to be true for an entire and well-specified class of phenomenon. Science requires generalizations. Otherwise, theories would have no practical application. Theories that hold only for some members of a class could not be safely or logically applied to future or different individual members of that class. Such an unfortunate state of affairs could hardly be called knowledge or rational thought, unless, of course, we are able to identify and define precisely those members of the class for which the statement holds.

This argument for S_2 could easily be extended to the fields of mathematics and philosophy. Yet, the "proof" would still not be provided. The search for the "proof" of fundamental propositions is, at best, unnecessary and, at worst, falsely fosters a dogmatic adherence to one's own system of thought. All that is necessary is to be open to criticism. If a proposed statement is "wrong," someone else will provide a counter-example. One's only responsibility is to be open to criticism and to make fundamental changes in theories or ideas when suggested by reason.

To return to the central issue of this exercise, I simply assert that S_2 is true and that any falsehood derived from S_1 and S_2 is attributable to the statement, S_1. S_1, "All generalizations are false," has an obvious problem. It is self-referential. If you accept the message continued in S_1, then S_1 is false, since it too is a generalization. Or, if you believe that S_1's message is false, then S_1 may be true.

Obviously, something is "wrong" or, better said, inconsistent.

S_1 is similar to Epimenides' paradox or the liar paradox. Epimenides was the Cretan who said, "All Cretans are liars"; and the liar received his name by saying, "This statement is false." These problems of self-reference have been the source of both concern and insight for mathematics and logic.[6] The potential of self-reference to cause logical "mischief" convinces some mathematicians that self-reference must be eliminated from rational thought. This may be clearly seen in Principia Mathematica. In the theory of types, Russell and Whitehead employ an elaborate hierarchy of classes to avoid self-reference. Yet, their efforts cannot be deemed successful, since their method of destroying self-reference also creates its own type of self-reference.[7]

Others see self-reference as harmless or benign. Self-reference need not be perverse or paradoxical, for example, simultaneous equation models. A simultaneous equation model postulates that some variable is a function of some set of variables which are in turn functions of the first variable. Thus, these models are self-referential, at least indirectly. But no paradox ensues as long as the rank-order conditions of identification are satisfied. In fact, a measure of self-referenciality could be defined as the "deficit" rank of the associated matrix. There are many other examples of harmless self-reference, many of which occur in ordinary conversations while others occur in this thesis. An intelligent person might even be so bold as to suggest that intelligence requires self-reference.[8] Otherwise, would not human intelligence be only an implicit system of equations in reduced form?

How can we resolve the paradoxical self-reference in statement S_1? The most direct resolution is simply to ignore the self-reference. This is the way ordinary conversation normally proceeds. But any such circumvention is equivalent to adding a qualifier to S_1 that S_1 applies only to some restricted set of statements in which S_1 is not a member. Yet, this procedure is, at best, ad hoc and contrary to the bold spirit of the original statement of S_1.

Alternatively, we could construct a hierarchy of levels for rational discourse. Tarski's theory of truth and Russell's and Whitehead's theory of types demonstrate that some type of hierarchy is necessary to avoid paradox and ambiguity.[9] In fact, the existence of multiple levels of discourse is required for reasoned analysis.

To show this necessity, let us attempt to stay on the level of "facts." "Facts" are descriptions of "observed reality." Of course, these descriptions depend upon our observational apparatus, but for the current discussion it is assumed that the "facts" are unambiguous and unproblematic. At this level we cannot speak of the "truth" or "falsity" nor any similar quality of some proposition.[10] A description of "fact" either is or is not. It cannot be said to be "true" or "good" or "efficient" or .... Such notions go beyond the level of mere description and must be stated at some higher level.[11] Or, is "truth" a quality that can be directly observed, like perceptual blue? Try it. For example,

"It is true that the sky is blue."

If this statement is stated entirely on the level of "facts" (by using only object language), then it is at best redundant. Or, the "It is true" part raises the question that factual description may not be "facts." Thus, ambiguity or inconsistency enters.

The important point is that "truth" and other interesting properties to the philosopher and scientist are necessarily of a higher level. "Truth" speaks of an interrelationship between different statements of "fact" or between "fact" and "theory." To speak of "theories" or interrelationships of "fact" more than an object language is required.

Thus, I see at least four levels of discourse that are necessary for rational inquiry to speak unambiguously and without antinomies.

1. The level of "facts." Here words or symbols are used only to describe events and objects.
2. The level of "theory." Where words, symbols, logic, or mathematics are used to "explain" and interrelate "facts." Here "facts" can be analyzed.
3. The level of "methodology." Where words, symbols, logic, or mathematics are used to compare, construct, interrelate, or "evaluate" "theories." Here "theories" can be analyzed.
4. The level of "philosophical inquiry." Where all manner of tools (e.g., dialectic analysis) may be employed to compare, construct, interrelate, and "evaluate" "methodologies." Here "methodologies" may be analyzed.

Each level is logically distinct from the others. Whenever a specific level is being discussed the discourse must be conducted on a higher level (for short, the meta-level).[12] I do not claim these four levels are unique for rational discussion. Nor are they, alone, sufficient for all potential topics of rational discourse. I only claim that a multiple-level system of thought and communication is necessary for rational discourse.

Returning to the original problem, we have "established" that S_1 is responsible for the uncomfortable result, S_3. But now we may resolve the paradoxical self-reference of S_1. To be precise, one could reformulate S_1 as:

S_1': All generalizations$_2$ are false$_3$.

Where the subscripts denote the level of discourse.

That is, the generalizations to which S_1' refers are generalizations on the second level, while the statement, S_1', is itself on the third level.[13] This interpretation eliminates self-reference and frees S_1' to be a potentially "true" or "meaningful" statement.

But, S_1' still contains a problem of interpretation. What is "false"? How are we to interpret the meaning of "falsity"? We could define "truth" in its strictest sense. A statement is 'true' if and only if we can be absolutely certain of its "truth," and can "prove" it so.[14] Then 'false' could be defined as any statement that is not 'true.' By the former brief discussion of the problem of induction and the unprovability of generalization we can assert that S_1' is a "true" statement. But here English provides us with an ambiguity, for "truth" and 'truth' are being used in different contexts. This ambiguity is harmless if we are careful to distinguish the level of discourse in which truth is used. Yet, a more literary route is available. Normally, English permits the distinction between the concepts of "truth" and "provability." Gödel even proves that these concepts remain distinct in the formalized system of Principia Mathematica.[15]

To make certain that we faithfully recognize this distinction, S_1' may again be reformulated.

S_1": All generalizations$_2$ are tentative$_3$.

Where tentative on the third level is defined as not totally (once and for all) "provable."

Thus our statement system and syllogism becomes:

S_1": All generalizations$_2$ are tentative.

S_2": Some generalizations$_2$ are necessary for rational thought.

$\therefore$ S_3": Some tentative statements$_2$ are necessary for rational thought.

Where all statements are made on the methodological level.

One might still argue that I have not "proved" [S_1"]. To this criticism, I must agree. Yet, I must reiterate that the major point of this exercise is to establish the "fact$_3$" that such meta-level generalizations are "unprovable." Still the critic might respond that I should develop a philosophical or methodological system in which S_1" and S_2" would logically result. To that criticism, I also acquiesce. It is a central purpose of this thesis to develop just such a philosophical and methodological position. The first substantive chapters of this paper are devoted to precisely this task. It is only hoped that the arguments presented are sufficient for an expanding, rational discourse.

Notes

[1] At this stage, no limits are placed upon the type of properties considered; only that the property in question must be somehow ascertainable. Later it will be argued that truly interesting properties are all a subset of the set of refutable propositions.

[2] This task would involve the creation of a deductive system in which S_2 and the set of all its logical implications are equivalent.

[3] This may be called the problem of induction, and more is said about it in Chapter 2.

[4] Karl Popper's epistemology is similar to this example; see Popper [1959], p. 59.

[5] The astute reader may notice that the asymmetry here is the converse of the former. The reason is simple. The logical form of a universal statement is the negation of some existential statement, and vice-versa.

[6] For an intuitive discussion and survey of self-reference see Hofstadter [1980], pp. 17-23, 438-586, and 684-720.

[7] Gödel is credited for discovering the paradoxical nature of Principia Mathematica. See Gödel [1965] and Hofstadter [1980], pp. 15-24, 75-87, and 438-464.

[8] See ibid for an insightful consideration of intelligence, self-reference, and tangled hierarchies.

[9] See Tarski [1956].

[10] Tarski defines a formal language for this purpose, sometimes called the "object language."

[11] Tarski defines a meta-language for this purpose, and with both levels of language he develops a "formal" correspondence theory of truth.

[12] For convenience, ordinary English will be used throughout this thesis. Yet, to avoid ambiguity, the reader need be careful to identify the appropriate level of discourse, usually levels 3 and 4.

[13] Given the original context of S_1, it is not clear that we are forced to choose levels 2 and 3. Levels 3 and 4 or 2 and 4 might also reflect the original context of S_1, but certainly not 1 and 2.

[14] The definition of 'truth' presented here is not completely adequate. For the current discussion, the reader is asked to employ the ordinary meaning of "truth."

[15] See Gödel [1965].

CHAPTER I

INTRODUCTION

> Exposed as a bore, the methodologist cannot take refuge behind a cloak of modesty. On the contrary, he stands forward ready by his own claim to give advice to all and sundry, to criticize the works of others, which, whether valuable or not, at least attempts to be constructive; he sets himself up as the final interpreter of the past and dictator of future efforts.
>
> - Roy Harrod, Economic Journal, 1938

Typically, methodological essays begin with an apology and a rationalization. The former is an admission of the tedium, paucity, or pedantry of methodological inquiry. Yet such honesty rarely inhibits the author from asserting some good or practical reason for his detour into methodology. Contrary to popular opinion or personal preference, there is no need to justify methodological or philosophical inquiry, and we shall give none. It would be just as reasonable to demand a justification for each omission of a philosophical analysis. Nonetheless, it may be useful to clarify the purpose and motivation behind this foray into economic methodology.

As a student of economics, questions about the content, implication, value, and meaning of economic theories naturally arise. In our case, the shadows of these questions merged with specific problems associated with research on the "causes" of inflation and unemployment. The problems that emerged appeared to be more of a methodological or epistemological nature than any simple deficiency of theory. This thesis is, then, a personal pilgrimage to a better understanding of economic theory and its knowledge content. Although few, if any, new truths are discovered along the way, the traveling is itself quite profitable to those with requisite patience.

At the most general levels, our questions concern "What is knowledge?" "What is methodology?" "How might knowledge be increased?" Traditionally, the subject of knowledge has been discussed as epistemology. Epistemology is the branch of philosophy that deals with what we know or can learn. Specifically, what constitutes knowledge? How might we identify knowledge or recognize its growth? Although universal agreement on such questions will probably never be obtained, it is possible to state some reasonable, though tentative, solutions. The next two chapters of this thesis investigate a number of epistemological questions and answers.

More specifically, we are interested in the special character of scientific knowledge. Does scientific knowledge differ from other ways of knowing? Do its claims have privilege? Or, is scientific knowledge of greater substance? It is unnecessary to assert that scientific knowledge is somehow "better" or "worse" than other types of knowledge (for example, logic, mathematics, beliefs, intuition, gestalt ...). It is sufficient to recognize its separate existence and its growth. We take these "sufficient conditions" as philosophical axioms that need no further discussion. We simply believe that scientific knowledge is important enough to demand that any adequate epistemology must account for its existence and its growth. The central questions of our inquiry are: "What characterizes scientific knowledge?" "How can scientific knowledge be identified?" "How can its growth be enhanced?" The next two chapters will also deal with these questions.

The last question concerns methodology. Methodology attempts to promote the growth of knowledge. It consists of a set of rules or conventions that may provide the means for learning. The choice of methodology can be considered a practical endeavor, since "good" methodology is one that succeeds in promoting the growth of knowledge. Yet, even successful methodology will not make scientific advance inevitable. Methodological prescriptions are usually quite general, designed to guide the practicing scientist but not to make his choices mechanical.

The goal of methodology is the production of knowledge. Its only basis is epistemology. Methodology cannot be judged by the actual practice of scientists but only by its consistency with an adequate epistemology or logic of scientific knowledge. If its rules and conventions reflect a sound epistemology, then it must be deemed adequate. The explicit adoption of a "good" methodology will not guarantee "good" scientific theory. Advances in science may occur through luck, "bad" methodology, or imagination. Nonetheless, "good" methodology must contain sound advice for scientists and enhance the opportunities for scientific growth. Adequate methodology is characterized more by the erroneous reasonings and justifications that it prohibits than by the certain accumulation of knowledge that it forces. Thus, any methodology that can be reasonably seen to promote knowledge and is consistent with an adequate epistemology must, likewise, be deemed adequate.

To reject a methodology because the history of science shows that scientists have violated its rules is not unlike rejecting the "maximization principle" because businessmen are observed not to equate at the margin. Even if it were known that businessmen grossly violate the "maximization" of profits, it would not diminish the value of the "maximization principle" as a guide to better business practice. In fact, the less "realistic" the "maximization principle" the greater is its value; for in scarcity lies value.

Yet many believe that a methodology must somehow reflect the actual practice of science or must analyze science's methods as they are employed. The interrelationship of epistemology, methodology, and the history of science is a complex phenomenon which few philosophers or scientists understand. As Blaug succinctly characterizes it,

> The puzzle is this: to believe that it is possible to write a history of science "as it actually happened" without in any way prejudging the distinction between "good" and "bad" science, without any prior notions of sound scientific practice, is to commit the inductive fallacy in the writing of intellectual history .... In short, all statements in the history of science are methodology laden.
>
> On the other hand, it would seem that all statements about the methodology of science are likewise history laden. To preach the virtues of <u>the</u> scientific method, while utterly ignoring the question of whether scientists now or in the past have actually practiced that method, is surely arbitrary .... We appear, therefore, to be caught in a vicious circle, implying the impossibility both of a methodology-free, totally descriptive historiography of science and an ahistorical, purely prescriptive methodology of science. From this vicious circle, there is, I think, no real escape.[1]

However, there is an escape from this vicious circle as long as we do not demand that it lead to some concrete, unique, and certain answer.

Science and its knowledge need not be viewed in only one way. Different aspects of science afford different perspectives. Epistemology focuses upon the logic of science: that is, how scientific knowledge and its growth can be identified and characterized. While methodology is prescriptive, answering how scientific knowledge can be promoted. Still there is a third way to view science--how science has actually grown. This third perspective is usually associated with the history of science, but, as Blaug aptly "observes," the history of science is itself based upon some theory (the theoretical nature of all observation is discussed in greater detail in the next chapter). Are we not trapped in a circle? Only if we choose so.

The way out requires us to first recognize this third perspective as a theory of scientific knowledge. That is, a theory which explains how science actually grows. Viewed in this manner, we might then "test" our theory of scientific knowledge. "How?", you might ask. "Why, against the facts," answers reason. However, "What are the 'facts' of scientific knowledge or its growth?" is a question with no simple answer.

These "facts" are no simple descriptions of observable phenomena. They are "judgments" that particular scientific theories constitute growths of knowledge. Such "facts" are on the third level of discourse (recall the Prologue), since they must talk about theories to appraise actual growth of knowledge. Thus, a theory of scientific knowledge is on the fourth level (that of philosophical inquiry), for these theories explain and relate third level "facts." Any "test" of a theory of scientific knowledge would be more tenuous than the usual tests of scientific theories, for the former is a "test" between "theory_4" and "facts_3" and the latter is between "theory_2" and "facts_1."

As such, there are major problems confronting a test of a theory of scientific knowledge. The first concerns the need to appraise the "facts_3" rather than merely observing them. These "facts_3" are assessments that some scientific theory is a growth of knowledge or, in some sense, is "better" than another theory. While these appraisals need not be made solely by reference to personal values or philosophy, they are likely to be more "normative" and problematic than usual scientific observations. Thus, the assertion of the "$fact_3$" of some scientific growth needs to be made more carefully and sparingly. If they are confined to theories for which there is all but universal agreement, ones which have passed the test of time, then our appraisals are not likely to be grossly in error. On the other hand, if every new development, each application of a new technique, or each extension of an accepted theory is proclaimed to be a "$fact_3$" of knowledge, then many errors are likely to result. For it is not a "$fact_4$" that all or even most scientific developments are, themselves, increases in our knowledge. To minimize the judgmental nature of the "$facts_3$," we should consider only the "best" and most obvious examples as the "$facts_3$" of scientific growth.

The second problem explicitly addresses the potential for circularity. It is all too easy to see only the type of scientific growth that we wish to see. That is, there is always the possibility that one will interpret the historical record according to one's theory of scientific knowledge. Here again, this circularity can be minimized by confining our assertion of the "$facts_3$" to only a few "best" examples. In such cases, we will find all but universal agreement on the "factness" of these increases in knowledge. If many different people with different values and theories of scientific knowledge come to the same appraisal, then we can be reasonably assured that this appraisal is independent of the theory of scientific knowledge under test. If different viewpoints see the same "facts," then we can be reasonably confident that the "facts" have some independent status and are not just the reflection of our original perspective. Thus, the problems associated with using the history of science as fact can be minimized when we restrict our view to the most obvious cases of scientific growth. As a consequence, consistency between theory and "$fact_3$" of scientific knowledge can be a reasonable requirement for a philosophy of science.

Some readers may be wondering how we can talk about evaluating theories of scientific knowledge or philosophical positions. After all, are not these theories on the fourth level of discourse? Would not their discussion require a fifth level? Since we have not defined a fifth level of discourse, these questions are apt. But, do we need to construct a fifth level? To this, the answer is yes and no. Philosophical inquiry will at times lead us to higher levels, for example, whenever we question our philosophical ideas. Yet, we have no real need to define a fifth level of discourse, and, in any case, its construction would accomplish nothing.

"Meaning" is like "truth." It cannot be "nailed down" once and for all. If we add a fifth level to talk about the fourth, we would need to add a sixth level to talk about the fifth, and so on. There is no natural end or limit to rational inquiry. Any attempt to establish the exact "meaning" of our concepts will inevitably lead to an infinite regress. There is simply no way to avoid ambiguity or potential antinomy in our language. For this reason, we are satisfied with only four levels of discourse, where the fourth level may be considered infinitely elastic and can speak about itself.

Does this then make all our efforts at rational inquiry senseless? We think not. All that is required is to state one's position as clearly as possible by staying well within the shades of meaning of his words and by avoiding destructive self-reference. In such a manner rational discourse can be made as clear as the issue demands. Although philosophical inquiry will always contain some ambiguity and the potential for paradox, its self-destructive nature can be circumvented and sensible argument can proceed. Particularly, if we are aware of the potential problems of language and are not too concerned about precise "meanings." Only the demand to be absolutely precise or to establish one's position causes any real problems.

How then can we appraise alternative philosophies of science? First, we accept the fact that such views cannot be established with certainty. There will always remain room for doubt. However, some reasonable minimal standards can be used to make our choice. For this purpose, we see three general standards.[2]

The first is that a philosophical position must provide an adequate epistemological basis. To be adequate, an epistemology must be able to identify knowledge and its growth. Our epistemological views must be sufficiently well characterized to identify what and when we know. Otherwise, one could reasonably question what is being said about knowledge. Furthermore, an adequate epistemology must be capable of providing answers to the questions it poses. Each philosophical position poses certain questions. A "good" or adequate epistemology should be able to answer its own questions. Or what does it accomplish?

Next, a philosophy position should provide a basis from which a methodology can be derived. Common sense and a sound epistemology are sufficient to construct a methodology which can help promote the growth of knowledge. If an epistemology cannot be used to formulate guidelines for the production of knowledge, one could question its usefulness. Of what use is a philosophical position that cannot be applied? For example, take the philosophical view that all knowledge is belief and that all beliefs are equal. This view is not practical. It cannot give us any guidance about how to increase our knowledge, whether it is "true" or not.[3]

Finally, an adequate philosophy must be capable of explaining scientific knowledge and its growth. Although an epistemological position is not itself a theory of scientific knowledge, it should have the potential to be interpreted as such. This requirement reduces the arbitrariness of a given philosophical position and provides a means to answer the naive realists' interrogatives. For the realists can always ask, "Your epistemological position sounds fine, but is knowledge, in fact, as you characterize it?" Or, "is your position consistent with the manner in which science has actually grown?" When judged as a theory of scientific knowledge, we only demand that our theory be consistent with the "better" and most accepted examples of historical scientific growth.

Will these criteria lead us to some unique philosophy? Will they firmly establish some given position? No, these requirements are quite weak. Many philosophical positions could satisfy them. What then is the point? We only claim that these standards can provide a rational way to appraise alternative philosophies of science. It is hoped only that they reflect what might reasonably be expected of such positions. Choice of philosophical systems need not be completely arbitrary nor based solely upon opinion. Good reasons for or against a philosophy can be given. Although these dialogues may never lead us to the Truth or some other final solution, they may discover some of the errors and give us a reasonable foundation from which to build our towers of knowledge. But just as any building or work of man, it may, at some point, become necessary to build anew when the foundation can no longer support the weight of the rising structure. Thus, we have no more reason to despair of philosophical or scientific inquiry than any other construction of man. Although, like man himself, none may reach some finality, all may be useful or beautiful in their own way.

Is not this thesis about economics? What relevance is there to all this talk about philosophy, some may be asking. While the central issue to this thesis is economic knowledge, it cannot be separated from the broader question of the epistemology or philosophy of science. But is economics a science? Although we do not believe that this is a relevant question, economists have long claimed that their discipline is a science.[4] The questions, "What should we call economics?" or "What has been the methodology of economics?", pall when compared respectively to the questions, "What is the knowledge content of economic theories?" or "How can we encourage the growth of economic knowledge?" Generally, we wish to investigate whether economic methodology is sufficient to provide economic theory with epistemological content or whether there are better means for the production of economic knowledge. When viewed in this manner, questions concerning economic knowledge cannot be divorced from epistemology or the philosophy of science.

Our central focus is the methodology of economics, past and present (Chapter 4). A few economic methodologists are taken as representative, instead of attempting the impossible task of discussing everything that economists have said about methodology. Our objective is to identify the philosophy of science that is reflected in economic methodologies and to analyze the adequacy of their epistemological bases. Before we have any chance in succeeding at this task, we must come to a better understanding of epistemology and the philosophy of science. It is the role of Chapters 2 and 3 to provide the necessary philosophical perspective. These chapters present a brief sketch and critical analysis of the philosophy of science.

Yet, in the course of our discussion, many specific economic problems are raised and, we think, answered. For example, one of the motivating questions behind this research topic concerns the status of "maximization" in either consumer or production theory. It is our conclusion that "maximization" is not a theory at all, and has no knowledge content. It is merely an arbitrary methodological convention that economists have adopted. In general, we find that the manner in which economic theories are stated or held make them irrefutable. As such, L. von Mises' forthright epistemology of economics is probably accurate (see section 4.2). Economic theory is the <u>logic</u> of action and deed. Therefore, it does not address questions concerning what, in fact, happens in the economy. We also discuss specific problems concerning specification, index numbers, and supply and demand analysis.

An alternative way to view this thesis is as a reply to Boland's recent defense of Friedman's essay, "A Critique of Friedman's Critics."[5] Currently, Boland has the last word concerning the much debated "Methodology of Positive Economics."[6] While Boland raises a number of good points and provides the best single interpretation of Friedman's essay, his perspective raises more questions than are answered. In our discussion of Friedman's methodology (section 4.4), we attempt to meet all of Boland's requirements for an adequate criticism of Friedman's essay, and we show the methodology of positive economics to be completely lacking. If nothing else, this thesis presents a thorough criticism of Friedman's methodology and finds a suitable alternative, Popper's falsificationism (the subject of the next chapter). Although philosophical analysis is never complete, just as economic analysis, we believe that the philosophical position developed here can serve as a secure foundation for economic knowledge and suggests many ways that economic theory can be improved and its methodological breaches shored up.

What more can be asked?

Notes

[1] Blaug [1980], pp. 33-34.

[2] Implicit in the following discussion is that we are referring only to philosophies of science. We make no claims that the standards appropriate for the philosophy of science apply to broader philosophical issues - for example, ethics or ontology.

[3] Our second requirement concerning methodology may be redundant with our first, concerning epistemology. That is, we accept the possibility that all adequate epistemologies are capable of providing an adequate methodology. Our example does not contain a sound epistemology, either. Yet, we shall maintain the distinction between our first two requirements, if for no other reason than to clarify the relation between epistemology and methodology.

[4] For example: Senior, Cairnes, J. N. Keynes, Robbins, and Friedman. These economic methodologists and their assertions about economic science are the subject of Chapter 4.

[5] Boland [1979].

[6] Friedman [1966].

CHAPTER 2

SCIENTIFIC KNOWLEDGE: POPPER'S PHILOSOPHY OF SCIENCE

> "It is open to every man to choose the direction of his striving; and also every man may draw comfort from Lessing's fine saying, that the search for truth is more precious than its possession."
>
> - Albert Einstein[1]

The philosophy of science is the discipline in which methodologies of science are developed, discussed, and criticized. It concentrates upon the logic of the scientific enterprise rather than the description or the evaluation of scientific theories. Philosophers of science are more apt to prescribe rules for "good" scientific conduct rather than to recount and account for the history of science.

What is 'science'?* What is the aim of 'science'? Can 'scientific' progress be identified? These are but a few questions brilliantly discussed and answered by philosophers of science, in particular, Karl Popper. Sir Karl Popper is not the only philosopher to significantly contribute to science and epistemology. Yet, Popper's philosophy provides the most suitable vantage from which to view the philosophy and methodology of science. In this chapter, Popper's view of science is developed and discussed. In the subsequent chapters, other philosophies are discussed and analyzed. The purpose of the current discussion is to

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*Words in single quotes are defined in the glossary.

establish a framework in which the philosophy of science may be 'rationally discussed' and the methodologies of economics may be analyzed.

Sir Karl Popper is one of this century's most influential and pragmatic philosophers. He is reputed to have destroyed logical positivism and to have been the official opposition to the Vienna Circle.[2] The requirement that 'scientific theories' must be 'testable' is the result of Popper's philosophy. T. W. Hutchison introduced Popper's ideas to economics in 1938, and lip service has been paid to his methodological prescriptions ever since.[3]

2.1 The Problem of Induction

Until Popper's contribution, it was generally believed that 'science' distinguished itself by its empirical method, a method which was thought to proceed from 'observations' to 'theory' by induction.[4] Popper, following Hume, convincingly argues that induction is logically invalid[5] - as we briefly asserted in the Prologue, p. 2. Induction requires that a general or universal conclusion can result from a finite number of singular statements ('basic statements') which represent 'observable' states of affairs. The problem of induction is not concerned with the 'truth' content of the 'basic statements' (though this is also an important question), but only whether 'truth' can be transferred from singular instances to universal statements. Simply because we observe the sun rising every twenty-four hours does not imply that the sun need rise within the next twenty-four hours.

The solution to the problem of induction is currently seen as obvious, though negative. There cannot exist any rule or method which can render induction logically valid. For any potential procedure, one can always construct a counter-example.[6] Hume's rejection of induction combined with his previous rejection of apriorism, the notion of absolute givens, led him to irrationalism. For how could rational men believe in regularities which had no logical foundation?[7] Thus, 'science' had to either establish an _a priori_ valid method (such as the one provided by Kant, as we shall see in section 3.3) or to admit its irrationality and lose its sanctity.

Popper finesses the whole problem by the restatement: "Can the 'truth' or 'falsity' of universal statements ever be justified by 'basic statements'? Answer: "_Yes, the assumption of the truth of test statements sometimes allows us to justify the claim that an explanatory theory is false_."[8] Thus, Popper recognizes the logical validity of _modus tollens_, not B implies not A, and the logical falsity of reverse _modus pollens_, not A implies not B (given that A implies B).[9]

This solution to the problem of induction can also be seen as a solution to the epistemological foundation of 'science.' If we carefully state and seriously attempt to 'refute' our 'conjectures,' then we have a rational though tentative basis for our beliefs in regularities and 'theories,' as we developed in the Prologue. Rational confidence must be commensurate with the effort devoted to the attempted 'refutations' of our 'theories.' By circumventing the problem of induction, Popper creates a new foundation for 'science,' 'conjecture' and 'refutation,' and an evolutionary epistemology.

2.2 The Problem of Demarcation - Science or Pseudo-Science?

Popper's original philosophical question was this: What distinguishes 'science' from 'pseudo-science'? Or, alternatively stated, What can establish the 'scientific' status of a 'theory'? This question was motivated by the recognition of fundamental differences between Einstein's theory of relativity and Marx's theory of history or structuralism, in general.

What impressed Popper about Marx's theory of social science was that it seemed to explain everything. Once one becomes initiated to his system of thought or learns the language, verifications are everywhere.[10] The world becomes a series of classes, class conflicts, or social and institutional conventions designed to preserve the capitalistic power structure.

Einstein's theory has just the opposite quality. The theory of relativity predicted certain 'observable' phenomena which seemed quite improbable in light of the background knowledge of the day. Instead of explaining all conceivable results of 'observations,' the theory of relativity is incompatible with the results of most potential experiments. The power of Einstein's theory lies more in what is forbidden than in what can potentially be explained. For example, nothing can travel faster than the speed of light; observers in different inertial reference frames cannot obtain different measures of the speed of light; Galileo's, Kepler's, and Newton's laws of motion do not describe the motion of physical objects; and many other similar formulations are forbidden by Einstein's theory.

These orbservations led Popper to the formulation of a 'scientific' demarcation rule, "the criterion of the scientific status of a theory is its falsifiability, refutability, or testability."[11] Tautologies or any 'irrefutable theory' are members of 'metaphysics' or 'pseudo-science' not 'science.'[12] However, 'metaphysical theories' are not insignificant or meaningless. They guide 'scientific' inquiries by provoking questions and problems and by providing, implicitly or explicitly, values needed to judge the importance of 'scientific' problems and their tentative solutions. In fact, all 'science' starts with 'metaphysics' and myth - there is no alternative. Popper summarizes:

> These considerations led me in the winter of 1919-1920 to conclusions which I may now reformulate as follows:
>
> 1. It is easy to obtain confirmations, or verifications for nearly every theory - if we look for confirmations.
>
> 2. Confirmations should count only if they are the result of risky predictions; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory - an event which would have refuted the theory.
>
> 3. Every "good" scientific theory is a prohibition"; it forbids certain things to happen. The more a theory forbids, the better it is.
>
> 4. A theory which is not refutable by a conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice.
>
> 5. Every genuine test of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability: some things are more testable, more exposed to refutation, than others; they take, as it were, greater risks.
>
> 6. Confirming evidence should not count except when it is the result of a genuine test of the theory; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory. (I now speak in such cases of "corroborating evidence.")
>
> 7. Some genuinely testable theories, when found to be false, are still upheld by their admirers - for example by introducing *ad hoc* some auxiliary assumptions, or by re-interpreting the theory

> *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering its scientific status. (I later describe such a rescuing operation as a "conventionalist twist" or a "conventionalist stratagem.")[13]

Popper is a falsificationist, but one should not declare him a naive falsificationist. He is aware that 'science' sometimes (if not always) proceeds with anomalies (i.e., apparent 'refutations'). And, it is often wise to grant temporary immunity to a new theory until, at least, its potential can be fully understood. But, if the aim of 'science' is to seek 'truth,' we must eventually revise our 'theory' or the 'observational theory' in a manner which explains all well 'corroborated' anomalies.

Popper is not an absolutist. 'Theories' always remain 'conjectures'; there is no certainty or even probability of their 'truth' - "tentative" as we concluded in the Prologue. However, the 'theory' which has yet to be 'refuted' has a privileged status over alternative 'theories' with equal or lesser 'empirical content' that have suffered 'refutations.' This privileged status is conferred by the simple fact that we have yet to learn of the 'falsity' of our "best" 'theories,' while we 'know' that the comparable but unsuccessful rivals are not 'true.'[14] We need not bury nor forget 'refuted theories.' They may be 'useful,' economical approximations, applicable to many practical problems. But they are not 'true.'

2.3 Popper's Science[15]

The distinguishing characteristic of science may be its 'falsification,' but how is this accomplished? What are the natures of 'theory' and 'fact'? How might we "justify" or 'test' these elements of science? Popper's view of science provides a thorough answer to these questions, though not without some surprises.

Popper divides statements according to their logical and synthetic qualities.[16] Logically, statements may be categorized by their level of applicability. This division creates universal statements that apply to all members of some class and singular statements that apply only to a specific element of a class. Or, we might distinguish between singular statements that assert some quality of 'observable' phenomena ('basic statements,' 'initial conditions,' and 'facts') and universal statements that assert some quality of the relationship among 'observable' phenomena ('theories,' 'conjectures,' and hypothesis).

'Scientific theories' are <u>strictly</u> universal statements since they are unbounded by space and time.[17] Strictly universal statements have an infinity of singular consequences. This is the reason why induction cannot justify a 'scientific theory.' No matter how certain one is of the "truth" of a finite number of singular statements, it requires a "leap of faith" to infer any strictly universal statement. There is no logical rule of inference for this "leap of faith"; thus, inductive generalizations can always be in error.

It is worthy to note the logical relation between 'scientific theories' and existential statements (i.e., there exists ...). For example, consider the 'theory' that asserts, "All planets have circular orbits." The logical negation of this 'theory' is, "There exists some planet whose orbit is not circular." Yet this "circular theory" is equivalent to the denying existential statement, "There does not exist some planet whose orbit is not circular." Thus, a 'theory' does not assert the existence of any phenomenon, but instead prohibits the existence of some phenomena. In this case, any statement about planets with orbits that are not circular is inconsistent with the "circular theory." If one such existential statement is held to be "true," then our "circular theory" would be "falsified." But, as we saw in the Prologue, existential statements cannot be 'falsified' and are therefore not 'scientific.'[18]

In contrast to 'theories,' we have empirical statements, the most important of which are 'basic statements.' A 'basic statement' is a singular existential statement that has the form, "'There is a so-and-so in region k' or 'such-and-such an event is occuring in region k.'"[19] From 'basic statements' derive all 'scientific fact.' 'Facts' provide the empirical content against which 'theories' are 'tested.'

Yet, the salient characteristics of 'scientific' activity do not concern its constituent elements alone but how these elements are interwoven. The essential quality of 'theory' is 'falsification,' and 'falsification' describes and explains the relations among 'theories,' 'basic statements,' and other elements of 'science.' Alone a 'theory' implies no 'basic statement'; thus, 'basic statements' are not sufficient for a 'refutation.' At a minimum, 'initial conditions' are also needed. Like 'basic statements,' 'initial conditions' are singular existential statements concerning 'observable' phenomena. The role of 'initial conditions' is to relate a 'theory' to a 'basic statement' by providing the 'theory' with sufficient information. For example, if we wish to "predict" the future movement of two physical objects using Newtonian dynamics the following is required.

Initial conditions:

Object 1 had position, x_1, at time, t_0.

Object 1 had velocity, v_1, at time, t_0.

Object 1 had mass, m_1, at time, t_0.

Object 2 had position, x_2, at time, t_0.

Object 2 had velocity, v_2, at time, t_0.

Object 2 had mass, m_2, at time, t_0.

It is now time, t_1.

Only after supplying these 'initial conditions' could Newtonian dynamics have any 'testable' implications or "predictions." The 'basic statements' implied by the conjunction of this 'theory' and these 'initial conditions' are:

Object 1 has velocity, $v_1(t_1)$.

Object 1 has position, $x_1(t_1)$.

Object 2 has velocity, $v_2(t_1)$.

Object 2 has position, $x_2(t_1)$.

Where all velocities and positions are exact numbers when they are calculated by the substitutions of the 'initial conditions' into Newton's equations.

Perhaps 'initial conditions' are required to 'test' a 'theory,' but when can we conclude that a 'theory' is 'refuted'? Before a 'theory' is 'refuted,' it must first be 'falsifiable.' A 'theory' is 'falsifiable' only if it, along with 'initial conditions,' divides the set of all possible basic statements into two non-empty sets: the set of all 'basic statements' that are inconsistent with the 'theory' (called "potential falsifiers") and the set of all 'basic statements' that are not inconsistent with the 'theory.'[20] Now, can one conclude that if a "potential falsifier" is 'observed' then the 'theory' is 'falsified'? No, replies Popper, that would be too easy.

> We shall take it as falsified only if we discover a reproducible effect which refutes the theory. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated. This kind of hypothesis may be called a falsifying hypothesis.[21]

To clarify 'refutation,' let us return to a previous example. The 'theory,' "All planets have circular orbits," cannot be 'falsified' by any four observations of some planet, say Pluto. And, this is true even if the observations are inconsistent with the geometry of a circle. This methodological prohibition eliminates the possibility of rejecting a "true" 'theory' merely because one or a few observations are made in error. To reduce the probability of a Type I error, a "falsifying hypothesis" must be formulated and confirmed. For example, a "falsifying hypothesis" is: "Pluto has a non-circular, elliptical orbit with parameters equal to such-and-such." If this elliptical theory is "true," then the original circular theory must be "false." In 'science,' we can never be certain of the truth of a "falsifying hypothesis," yet we can 'test' it. If, after a "suitable" amount of time and observation, all observations of Pluto are consistent with the elliptical theory, we may rationally 'falsify' the circular theory by this 'well-corroborated' "falsifying hypothesis."[22] If, however, further study reveals significant inconsistency with the elliptical theory, it must be withdrawn and the circular theory maintained until a different 'well-corroborated' "falsifying hypothesis" can be found.

The role of the "falsifying hypothesis" is very important; yet it is not overly restrictive. All that is eliminated is the use of unique events and irreplicable phenomena in 'scientific' inquiry. Any 'observation' that is reproducible, in principle, can be stated and used as a "falsifying hypothesis." Yet, much is gained by employing a "falsifying hypothesis." For, if anyone is not convinced of an alleged 'falsification,' he may repeat the experiment or devise a new experiment that can 'falsify' the "falsifying hypothesis." If no "falsifying hypothesis" is ever provided, seemingly contradictory evidence leads only to unresolved controversy or to a numbers game concerning the relative weights of evidence.[23]

"Falsifying hypotheses" are not necessarily rival 'theories.' They may be less general, less interesting, and of less content than the 'theory' in question. In the former example, the elliptical theory is much less general than the circular theory, since the former applies only to the planet Pluto while the latter applies to all planets, both known and yet to be discovered. Notice also that the circular theory has more content. Circles are special cases of ellipses; thus, they forbid many more types of orbital behaviors.

'Falsifiable theories' are not all equal. 'Theories' are "better," <u>ceteris parabus</u>, the more vulnerable they are to 'testing.' The "better" 'theories' take greater risks and have more 'empirical content.' 'Empirical content' refers to the set of 'basic statements' that are inconsistent with the 'theory.' As the set of "potential falsifiers" becomes larger, the more the associated 'theory' forbids, and the greater is the 'theory's' 'empirical content.'[24] 'Theories' with large 'empirical content' are 'scientifically' interesting and deserve the preponderance of scientific attention and 'testing.' If, in addition, the 'theory' with the greatest 'empirical content' passes at least as many 'tests' as its rivals, then it may be considered the "best" 'theory,' at least tentatively.

To fully understand the concept of 'empirical content' and 'testability,' the dimensionality, d, of a 'theory' must also be considered. Dimensionality is related to the amount of information required to 'refute' a 'theory.'[25] It may be roughly defined as the maximum number of 'observable' parameters that can be specified without the possibility of causing an inconsistency with the 'theory.' The number of parameters or measurements that are contained in the 'initial conditions' is equal to d. A 'refutation' is only possible if all d parameters are 'observed' and measured plus one associated with a "potential falsifier." The smaller d is, the greater the 'empirical content,' and the more 'testable' is the 'theory.'

In our example of Newtonian dynamics, eight measurements are required to specify the 'initial conditions,' two each for time, mass, position, and velocity. Thus, the dimensionality of the two-body problem is eight. Without specifying at least nine such measurements, Newton's theory could not be questioned, for it is consistent with all conceivable 'facts' that have a composition of eight or fewer measurements. If we could find a 'theory' whose explanation is as rich as Newton's but has a lower dimensionality, it would be better 'testable' and have more 'empirical content.'[26] Such a 'theory' would be more interesting and would provide a better explanation of the 'facts' if 'corroborated.'

'Theories' can only be 'falsified' by other 'theories' (or "falsifying hypotheses"). Neither 'theory' need be "proven" nor held as certain. Science proceeds by clashing one 'theory' against another. The resulting heat of this friction burns only the fuel of error. The only losers in the theory contest are erroneous beliefs and 'theories.'

There is no part of 'science' that is certain or untouchable from criticism. Yet, falsificationism allows us to learn from our errors. By eliminating some error, we can approach 'truth.'

Surely Popper does not suggest that we have to make decisions in science without any firm basis. Are not the empiricists correct to believe that our 'observations' and 'facts' are of tough mettle? Does not Popper use 'facts' and 'basic statements' as fixed and infallible referees of the theory contest? No, Popper has consistently maintained the tentative, hypothetical character of 'facts' and argued against extreme forms of empiricism.[27]

Our "empirical basis" of 'science' is composed of currently "accepted" 'basic statements.'[28] These 'facts' are singular existential descriptions of 'observable' events. 'Observable' is a primitive concept involving states of affairs that can be inter-subjectively tested and communicated. 'Observation' is theory-laden. Some type of 'observational theory' is required to meaningfully organize our perceptions and our scientific measurements. Without some type of 'observational theory,' 'observations' would be no more than an unlabeled computer tape for which no computer program has been designed to read or process. Acceptable 'basic statements' must be capable of being tested by anyone; they cannot be merely the result of one person's isolated perceptions.

Which 'basic statement' do we finally accept? Is it one for which we can find universal, inter-subjective agreement? Or, is it one for which we can find some final foundation? No, there is no ultimate end to the process of 'observation' nor is there any finite upper bound for the numbers of steps involved in accepting 'basic statements.'

> For any basic statement can again in its turn be subjected to tests, using as a touchstone any of the basic statements which can be deduced from it with the help of some theory, either the one under test, or another. This procedure has no natural end. Thus, if the test is to lead us anywhere, nothing remains but to stop at some point or another and say that we are satisfied, for the time being.[29]

An accepted 'basic statement' may be considered as an infinite sequence whose range is contained in some open set in which the limit points do not lie.[30] Yet, these accepted 'basic statements' are the only 'scientific facts.'

What if scientists cannot agree on which 'basic statements' to stop?

> (T)hey will simply continue with the tests, or else start all over again. If this too leads to no result, then we might say that the statements in question were not inter-subjectively testable, or that we were not, after all, dealing with observable events. If some day it should no longer be possible for scientific observers to reach agreement about basic statements this would amount to ... a new 'Babel of Tongues' ... (and) the soaring edifice of science would soon lie in ruins.[31]

Everything involving 'observations,' 'basic statements,' or 'facts' is provisional. Even the 'tests' of the 'facts' are not final. Anyone who doubts an accepted 'fact' may simply replicate the associated experiment. Or, any 'basic statement' may be 'tested' that is implied by the conjunction of some 'theory' and the 'fact' in question.[32] If the anticipated effects are not 'observed,' the researcher is forced to reject the 'fact' at least tentatively. If, on the other hand, a "falsifying hypothesis" is proposed and is 'corroborated' by all the 'facts' or 'basic statements' in question, then the 'theory' is 'falsified.' Further testing would then be necessary until an explanation or agreement is reached. Or, if researchers refuse to settle their differences, the Tower of science would fall and scientific knowledge would recede into the dark clouds of its origin, myth, magic, and metaphysics.

'Facts' are not "written in stone." They are as hypothetical as tentative, and as fallible as 'theories.' But, is it not these 'facts' that are used to 'falsify' 'theories'? If both are so uncertain, is not any alleged test only an illusion? Could not any perceived inconsistency between 'theory' and 'fact' be blamed upon the 'observational theory'?

> In point of fact, no conclusive disproof of a theory can ever be produced, for it is always possible to say that the experimental results are not reliable, or that discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding .... If you insist on strict proof (or strict disproof) in the empirical science, you will never benefit from experience, and learn from it how wrong you are.[33]

Thus, to prevent a collapse of 'science' and to guarantee the growth of 'scientific knowledge,' we must establish some rules for the process of 'science.' Popper is not only aware of this problem, but also provides us with many explicitly stated methodological rules to fill the gap. To mention only two:

> (1) The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game.
>
> (2) Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop out without "good reason." A "good reason" may be for instance: replacement of the hypothesis by another which is better testable; or the falsification of one of the consequences of the hypothesis.[34]

All of Popper's rules for science are at the methodological level of discourse. They do not prescribe which specific 'theories' or 'facts' to accept, but only which methods and attitudes should be adopted by the scientific community. These methodological rules give 'science' a conventional character. Popper develops methodological rules for the entire process of science, but for now let us concentrate upon a few rules concerning 'fact.'

(1) Only singular existential statements can be 'basic statements.'[35]

(2) 'Basic statements' must have a reproducible effect to qualify as a "falsifier."[36]

(3) Stray 'basic statements,' those made by chance or which are logically disconnected, cannot be acceptable 'basic statements,' but only 'observations' that result from the 'testing' of 'theories.'[37]

(4) Only singular 'basic statements' can be "accepted" by agreement not universal statements.[38]

These methodological rules and the necessity of accepting by agreement some 'basic statements' give 'fact' a conventional character. This conventional nature of 'facts' endows them with an anthropomorphic bias. But this is as it must be.

All components of science are hypothetical and tentative. All is open to doubt, criticism, and test. Though many would see this as a weakness, this open, uncertain character of science is all that guarantees its growth. The objects of scientific inquiry can be justified only by subjecting them to criticism and testing. Although there is no ultimate justification, the more severe the 'tests' the more concrete are the results. Popper summarizes the quality of 'scientific facts' with a metaphor.

> The empirical basis of objective science has thus nothing "absolute" about it. Science does not rest upon rock-bottom. The bold structure of its

> theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or "given" base; and when we cease our attempts to drive our piles into a deeper layer, it is not because we have reached firm ground. We simply stop when we are satisfied that they are firm enough to carry the structure, at least for the time being.[39]

2.4 The Growth of Scientific Knowledge

Some would ask, "How can we speak of the growth of 'knowledge,' when 'knowledge' is never certain?" Others would respond, "If 'knowledge' were certain, how could it grow?" The aim of 'science' might be 'truth,' but how would we 'know' that we have obtained it?

'Science' grows by imaginative and bold 'conjectures' or 'theories.' Not all 'conjectures' connote progress. A 'theory' is progressive if it

> contains the greater amount of empirical information or content; which is logically stronger; which has greater explanatory and predictive power; and which can therefore be more severely tested by comparing predicted facts with observation. In short, we prefer an interesting, daring, and highly informative theory to a trivial one.[40]

Content is the measure of what a 'theory' forbids, 'empirical content' is the class of 'basic statements' which would 'refute' the 'theory.' Content is inversely related to probability. We do not search for probable 'theories,' for they have little content, but bold improbable 'theories.' For example, the 'conjecture' that an object released above the ground will fall to the earth has less content than the 'conjecture' which states that the time taken for this fall is given by the equation, $t = (2s/g)^{1/2}$. The first 'conjecture' is more probable given ignorance (for example, a uniform prior probability distribution), but it also contains less 'empirical content' since fewer 'observable facts' are inconsistent with it. Thus, 'science' grows by explaining more and by forbidding more.

A 'theory,' t_2, may be said to correspond more closely to the 'facts' than theory t_1 if, ceteris parabus:

(1) t_2 is more precise than t_1, and the additional precision is not 'refuted' by testing.

(2) t_2 explains a greater range of phenomena than t_1.

(3) t_2 passes attempted 'refutations' which t_1 does not.

(4) t_2 suggests new experimental tests not suggested by t_1, and t_2 passes some of these tests.

(5) t_2 unifies previously unrelated problems.

In each case, t_2 has more 'empirical content' than t_1, or excess 'empirical content.'[41]

Though there are no fixed characteristics which define the better 'theories,' history (of the physical sciences, at least) provides examples of a few general qualities. The "best" 'theories' usually begin with a simple unifying idea, connecting previously unrelated concepts. They are independently 'testable.' They explain the same phenomena as their predecessors yet lead to new unexpected predictions. And, a "good" 'theory' needs to pass some 'tests' which its rivals fail.[42] This last characteristic of a "good" 'theory' may be necessary to classify a 'theory' in the highest ranks of 'science,' but it is too strong to demand immediate fulfillment. A 'theory' satisfying the earlier criteria can still be interesting and important.

In general, there are no fixed rules for the game of 'science.' The only rules which are always justified are the ones which promote 'critical debate.' Popper's many methodological rules are designed for just this purpose; the most important of which is, perhaps, "We decide that, in the case of a threat to our system, we will not save it by any kind of conventionalist strategem."[43] Conventionalist strategems include the introduction of *ad hoc* hypotheses, the modifications of definitions, and the accusation of incompetence of the experimenter or the theoretician whose work threatens the *status quo*.[44] Any rule which inhibits 'critical discussion' will also retard the growth of 'scientific knowledge.' Those qualities mentioned may be helpful in identifying interesting and potentially progressive theories, but a dogmatic adherence to these or any set of *a priori* norms may eliminate progressive 'theories' before they have been subjected to or improved by 'critical debate.' The critic who is interested in the growth of'knowledge' should not merely reject 'theories' but point out their deficiencies and offer suggestions for their fortification. The final judge of any 'theory' will be its resilience to severe empirical 'testing' and its ability to incorporate substantive 'rational criticism.'

How can we clearly identify the status of a 'theory' relative to its rivals? Popper employs the concept of verisimilitude (truthlikeness) which may be considered the "truth" content minus the "falsity" content of a 'theory.' Though Popper axiomatizes verisimilitude,[45] he does not believe that it can be meaningfully measured in practice. Instead, a summary of the 'critical debate' can be used to compare alternative 'theories.'

> How do you know that the theory t_2 has a higher degree of verisimilitude than theory t_1? ... I do *not* know - I only guess. But I can examine my guess critically, and if it withstands severe criticism, then this fact may be taken as a good critical reason in favour of it.[46]

Popper uses illustrative examples drawn from the history of science to demonstrate the workings of falsificationism.[47] One should not consider these examples as proofs nor 'tests' of Popper's methodology (for reasons that are explained in the Introduction). Yet, if the "better" historical paradigms of scientific progress are not consistent with a theory of scientific growth, one is compelled to suspect the theory.[48] Here we are discussing Popper's falsificationism as a <u>theory</u> of knowledge and the growth of scientific knowledge - conjectures and refutations. Falsificationism as a normative methodology need not be consistent with past science. Popper's favorite stories speak of the large advances in physics. For example, both Galileo and Kepler developed important physical laws of motion. Both of these paragons of 'scientits' seemed to discover their laws by induction. Kepler's inferences were drawn from the careful 'observations' of Tycho Brahe, while Galileo used his own experimentation. But the appearance that their 'theories' are merely the sum of these 'observations' is 'false.' Each had a preconceived hypothesis which was compared to the data. Kepler began with a 'meta-physical theory'; and he attempted, in vain, to interpret Tycho's 'observations' in the light of this theory. Each interpretation was 'refuted.' Finally, Kepler formulated three laws, not to his liking, which could not be 'refuted' by the 'facts,' and these were hailed a great 'scientific' achievement.

Galileo's story is similar. Galileo began his inquiries with the laws of the traditional authority - Aristotle. Galileo's first step was always to design and perform an experiment which would 'refute' Aristotle's physics. This was easily accomplished in his study of falling objects. But Galileo did not stop there; he too formulated a law which could not be 'refuted' by his experiments.

Both these men of 'science' used a method of trial and error. Though 'observations' preceded the discovery of these laws, induction could not have provided them. If it was induction, it was an induction which can best be described: one, two, infinity; where the step between two and infinity requires a liberal application of intuition and/or wisdom and, perhaps, "luck."

The next giant step in the growth of 'scientific knowledge' was taken by Newton. Newton's 'theories,' along with those of Einstein, may be considered the best examples of 'scientific' progress. Newton boldly 'conjectured' (though he would not have used the word) very different laws of motion, unifying the terrestrial laws of Galileo and the celestial laws of Kepler. Newton's laws could have been neither deduced nor induced (as many believe) from the laws of his predecessors. Newton explained but corrected the laws of Galileo and Kepler. Newton's laws also could not be 'refuted' by their 'observations,' and yet they predicted phenomena which neither mentioned nor explained. And, of course, much of this excess content was later confirmed (i.e., not 'refuted').

Einstein's theory was an even bolder 'conjecture' than Newton's. It was radically different in its fundamental world-view, and Newton's view was, at the time, considered by many to be the absolute 'truth.' Einstein specified several predictions of his 'theory' which were vastly different than those implied by Newtonian physics. He said that these predictions were crucial and stated that he would reject his 'theory' if they proved wrong. Einstein's audacity did not stop there. He declared his 'theory' 'false,' though closer to the truth than Newton's, and insisted that any 'true theory' must be a unified field theory satisfying a number of minimal requirements. To this end he spent the rest of his life. Thus, 'science' grows.

As a theory of the actual growth of scientific knowledge, Popper's view - conjectures and refutations - may be "tested." As a methodology, it cannot. Methodology does not claim that its rules or canons are actually used by scientists nor that they need be used. Methodology only claims that these rules may help promote knowledge, if scientists are guided by their wisdom. The only "test" of a methodology is a "test" of its epistemological or philosophical foundations and its consistency with these foundations. We can 'rationally criticize' the ability of a methodology to reflect a reasonable epistemology, but we cannot "falsify" it by reference to the history of science. On the other hand, we can "test" a theory of scientific knowledge and its growth if we are careful to recognize the inherent differences of such a "test."

When dealing with actual cases of scientific growth, these "facts" are on the third level of discourse - recall the Prologue - since they must appraise some 'theory' as constituting a growth of knowledge. "Facts$_3$" of scientific growth must be stated at the third level or higher since they must speak of and appraise actual scientific theories. Thus, a theory of scientific knowledge is on the fourth level of philosophical inquiry since it speaks of these "facts$_3$." The "test" of such a high level theory is more difficult and problematic than 'tests' of scientific theories, and it is more difficult to reach some reasonable agreement concerning the "facts$_3$" of actual scientific achievement. Because the necessary appraisal of these "facts$_3$" is more elusive than required for ordinary scientific 'facts,' only the best examples of scientific progress (such as the examples of physics which we have mentioned) can be considered "facts$_3$."

When such "facts_3" of the growth of scientific knowledge are compared to Popper's conjectures and refutations, we assert that they will be found consistent. That is, Popper's conjectures and refutations are an adequate explanation of the growth of scientific knowledge, and the "facts_3" will only "corroborate_4" this assertion. A comprehensive exploration of all the evidence - the history of science - is beyond the scope of this thesis. Thus, we can only assert our appraisal and be open to any 'rational criticism' (some of which will be discussed in the next chapter). We are not asserting that Popper's view of science is consistent with all or even the preponderance of the history of science; nor do we believe that an adequate theory of scientific knowledge needs to describe most of what has actually passed as science, as some philosophers and historians of science wish. All that is necessary is that such a theory be consistent with the "facts_3." These "facts_3" can consist only of our "best" historical examples of scientific growth, ones for which enough time has elapsed for the wisdom of hindsight to dominate our appraisals. Only if such "facts_3" are used carefully and sparingly can they add to the 'rational discourse' of theories of knowledge.

In sum, 'science' seeks truthful explanations of the interrelation of phenomena. Bold, new 'conjectures' with excess 'empirical content' characterize 'scientific' progress. No simple set of rules can establish the merit of one 'theory' over another. The status of any 'theory' and the progress of 'science' must be judged in the light of the 'critical debate.' In this light, explanations which seem more truthlike, or potentially more progressive, deserve the preponderance of 'scientific' activity. But 'falsified' or less progressive 'theories' need not be forgotten. They may serve as barometers of our progress and may be used for applied problems if their limited range of application is seriously considered.

2.5 Philosophical Realism[49]

All science and thought begins with 'metaphysics' or philosophy. It is our philosophy which defines what we should learn by conferring differential values upon our problems and questions. 'Metaphysics' and philosophy suggest new problems, stimulate discussion, and provide the basic framework of all inquiry. As Einstein expresses it:

> Time and again the passion for understanding has led to the illusion that man is able to comprehend the objective world rationally, by pure thought, without any empirical foundations -- in short, by metaphysics. I believe that every true theorist is a kind of tamed metaphysicist, no matter how pure a "positivist" he may fancy himself. The metaphysicist believes that the logically simple is also the real. The tamed metaphysicist believes that not all that is logically simple is embodied in experienced reality, but that the totality of all sensory experience can be "comprehended" on the basis of a conceptual system built on premises of great simplicity.[50]

But, 'metaphysics' and philosophy are not 'science,' nor are they subject to the same criteria of validity.

Popper is a 'metaphysical' realist. That is, there exists a real world (in fact, three) which we may discover.

> I will point out that, without taking the words 'worlds' or 'universe' too seriously, we may distinguish the following three worlds or universes: first, the world of physical objects or of physical states; secondly, the world of states of consciousness, or of mental states, or perhaps of behavioural dispositions to act; and thirdly, the world of <u>objective contents of thought</u>, especially of scientific and poetic thoughts and works of art.

> Thus what I call "the third world" has admittedly much in common with Plato's theory of Forms or Ideas, and therefore also with Hegel's objective spirit, though my theory differs radically, in some decisive respects, from Plato's and Hegel's. It has more in common still with Bolzano's theory of a universe of propositions in themselves and of truths in themselves, though it differs from Bolzano's also, my third world resembles most closely the universe of Frege's objective content of thought.[51]

Popper argues that most of the traditional and contemporary epistemology is irrelevant, since it is concerned with the second world manifestations of the third world. Only the study of the third world (containing the 'objective' content of 'theories,' problems, problem situations, 'critical debate,' 'conjectures' and hypothesis) is relevant to epistemology.

> In upholding an objective third world I hope to provoke those whom I call "belief philosophers": those who, like Descartes, Locke, Berkeley, Hume, Kant, or Russell, are interested in our subjective beliefs, and their basis or origin. Against these belief philosophers I urge that our problem is to find better and bolder theories; and that critical preference counts, but not belief.[52]

Though created by man, the third world is autonomous to man. Many of the third world entities are the unintended by-products of man's rational thought, just as flood control is an unintended by-product of a beaver's dam. The first and second worlds are affected by the third, and the third evolves by the changing problem situations and intervention of the second. The interrelations of the first world (as discussed in physics) are part of the subject matter of the third world. But, the intervention of the second world is required to 'test' the validity of our third world constructs against the 'observed' processes of the first world. Popper's view may be considered Neo-Platonic, where the world of Forms and Ideas is lowered to the status of human products and is neither fixed and absolute nor perfect.

Popper's theory of realism is 'metaphysical' only in the sense that it can neither be proved or 'refuted.' Since Descartes' statement, cogito ergo sum, philosophers are quick to state that any assertion beyond this is pure supposition. But every theory, 'metaphysical' or not, is the subject of 'rational criticism' - the only other alternative is dogmatism. Because all theories attempt to solve problems, they can be 'critically discussed' in light of their problem situation. In this manner, Popper argues for realism and against determinism, idealism, irrationalism, voluntarism, and nihilism.[53]

Realism only requires that there exists an independent 'reality' which does not merely depend upon human beliefs or perceptions. Although human constructs may be necessary for the understanding and interpretation of 'reality,' realism asserts that 'reality' will exist whether or not we have the words to describe or explain it. The discovery and explanation of this 'reality' is the aim of science; and it is the final judge of our 'scientific theories.' Later the philosophical position of realism will be critically discussed. But for now, I simply assert that realism is a tenable philosophy that provides an adequate foundation for an efficacious methodology of science. To many practical men, the reliance on 'metaphysical realism' may be uncomfortable, but as George Bernard Shaw is reputed to have said of his age, "It's fine when you consider the alternative."

2.6 Popper's Tangled Hierarchy

The aim of 'science' is 'truth.' 'Science' seeks objective explanations of 'reality.' Though these goals may never be obtained, progress can be made towards them. The growth of 'knowledge' requires bold, imaginative 'theories' that are always open to criticism and 'refutation.' Criticism and severe empirical 'testing' is all that is necessary for the growth of 'scientific knowledge,' the rest will follow naturally. Falsificationism can lead scientific inquiry toward 'truth.'

Yet, as always, questions remain. Is Popper's system of philosophy and science consistent? Is this a sound foundation for methodology and epistemology of science? How are philosophy, methodology, science, theory, and fact interrelated? Do Popper's views unify and explain the complexities of philosophy and science?

At the most general level, Popper's philosophy is realism and fallibilism. Fallibilism refers to the philosophical position that the products of man will never be certain nor immune to error.[54] The demand for certainty, the quest for infallibility, or the insistence of positive justification can only compound our inevitable errors. Popper does not merely assert these philosophical theories. He 'critically discusses' the context of their problem situations and their stronger alternatives.[55] Much of this effort must be considered successful, for Popper has inspired many philosophers to change or rethink their positions.[56]

Realism confers upon science and epistemology their object of study, 'reality.' It also provides us with a springboard from which our 'theories' may evolve. To the extent that 'reality' is reflected by the 'facts,' error can be eliminated from past 'theory' and the emergent 'theory' can be brought into closer harmony with 'reality.' In fact, the concept of error presupposes some notion of 'reality' or 'objective truth.'

> It is only the idea of truth which allows us to speak sensibly of mistakes and of rational criticism, and which makes rational discussion possible - that

> is to say, critical discussion in search of mistakes with the serious purpose of eliminating as many of these mistakes as we can, in order to get nearer to the truth. Thus the very idea of error - and of fallibility - involves the idea of an objective truth as a standard of which we may fall short. (It is in this sense that the idea of truth is a regulative idea.)57

These ideas of error and 'objective truth' are sufficient for the distinctions that Popper calls his three worlds. They allow a world of things themselves, a world of subjective beliefs which may be in error, and a separate world of 'objective truth.' In fact, if such a separate reality did not exist, the scientist would have to invent one. Otherwise, he would have nothing to do but to convince others and sell his beliefs. Thus, realism engenders science with its aim and its principal source of growth.

Together, fallibilism and realism specify falsificationism as the proper methodology of science. Popper's falsificationism is defined by his many methodological rules, and conjectures and refutations is his theory of the growth of scientific knowledge. It is hoped that the former sections on demarcation, Popper's science, and the growth of scientific knowledge provide a sufficient characterization of falsificationism and conjectures and refutations.

The proper choice of methodology is a pragmatic endeavor. The role of methodology is to design the general procedures and their logic for science. Specific methods can be left to the individual scientist, but the explanation and description of the general rules and the logic of science is methodology. Methodology need be concerned with ends, for how can the proper methods be chosen if we do not consider the ends to which they are employed? Thus, the choice of methodology is teleological

Pragmatism enters if we demand that our methodology give results when measured from the perspective of our chosen ends.

Popper's falsificationism may be seen as a pragmatic methodology. Realism gives science the goal of 'objective truth' or the explanation of 'reality.' Yet, fallibilism tells us that we may never reach this goal.[58] Thus, Popper calls 'truth' the aim of science.[59] It would not be useful to evaluate a methodology only by whether or not its aims are achieved. Progress towards our goals is also a valuable contribution. Realism and fallibilism identify a means of progress in science, falsificationism. Since our knowledge is fallible, we may learn from our errors. By finding inconsistencies with 'reality' (as reflected by the 'facts'), knowledge can grow. New 'conjectures' can be formulated and 'tested' until their weaknesses can be discovered and eventually explained. Thus, falsificationism leads science towards our chosen ends.

Falsificationism will work in spite of random errors or occasional mistakes, for it is the process of trial, error, and error elimination. Only if Nature is pervasively perverse will falsificationism fail to work. If our 'facts' are consistently "wrong," only then will falsificationism fail to be effective. But in such an extreme case, all other methodologies would also fail.

Moving down the metaladder to the level of science, itself, we must consider the major components of science, 'theory' and 'fact.' At the lowest rung, we have the singular existential statements that we may carefully call 'facts.' What is the character of the 'facts'? First of all, they are conventions. This conventional aspect is imposed upon the 'facts' by the inter-subjective language in which they are stated and by the methodological rules which they must obey. Language that intersubjectively communicates must be a convention. The fact that sky is blue can never be a 'fact' until different people can agree upon the meaning of "blue." Also, 'facts' have a conventional nature because of the methodological rule that they must be accepted, at least tentatively, by the scientific community. 'Theories,' on the other hand, can never be accepted. Here intersubjective agreement is unnecessary and contrary to falsificationism.

Then do 'facts' provide the "rock bottom" of scientific inquiry? The "bottom," yes; but "rock," no, replies Popper. Instead, he uses the metaphor of the swamp. This characterization is reasonable when we consider that our 'facts' are not certain. 'Observations' may be illusions or colored. All that conserves the value of 'facts' is their inter-subjective testability - another methodological demand. He who doubts a 'fact' may 'test' it until either he is satisfied or some alternative 'fact' is sufficiently 'tested.'

'Theories' always remain hypothetical and tentative. They are unprovable universal statements. 'Theories' are valuable only to the extent that they solve interesting and important problems and are consistent with known 'facts.' When a 'theory' is shown to be persistently inconsistent with the 'facts' and some "falsifying hypothesis" is not, the 'theory' is 'falsified.' Thus, we learn. "What?", you might ask. If nothing else, we learn that our previous theory is 'false.' And, hopefully, our insight and imagination will lead to a new 'theory' that solves the original problem and is consistent with the 'facts.'

'Theories' are metafactual. Since 'theories' are strictly universal statements they are infinitely more general than 'facts.' And, because 'theories' talk about 'facts' and explain them, 'theories' must be a higher level concept. One might ask whether 'theory' and 'fact' form a clear hierarchy. Can 'theory' be viewed as rising unambiguously from the swamp of 'fact'? No, the hierarchy is tangled.

'Facts' are theory-impregnated. 'Observational theories' are needed to translate basic perceptions into the type of 'facts' that have the potential of 'refuting' a 'theory.' Thus, 'facts' too are 'theoretic.' We may still consider 'scientific theory' more general and at a higher level than 'facts.' But, the hierarchy is tangled and composed of many feedback loops. The relationship between 'theory' and 'fact' does not form a simple recursive system as used in simultaneous equations. The process of 'conjectures' and 'refutations' continually builds an increasingly complex system of 'theories,' 'facts,' and 'observational theories.' 'Scientific observation' has only grown more sophisticated. Is there any 'observational' apparatus that is not built upon a rather sophisticated 'theory'?

At this point, if not sooner, a critic might yell, "infinite regress." How can anything be established when 'facts' are 'theories' and 'theories' are 'tested' by 'facts'? The critic would be right. Popper's science can never establish anything and any attempt to do so will involve an infinite regress. Not only do 'theories' and 'facts' form a tangled hierarchy but each may also be 'tested' without limit. However, this infinite regress is benign. Anything else would be inconsistent with falsificationism and fallibilism. Falsificationism does not seek to establish anything, nor prove, nor justify, nor make certain any 'theory' or 'fact.' Falsificationism merely attempts to reduce some error. Fallibilism tells us that this process of error elimination will never come to any natural end. And, any attempt to establish a 'theory' or prove that one 'theory' is better than another is misconceived, for it is inconsistent with both the methodology of falsificationism and the philosophy of fallibilism.[60]

The beauty of Popper's philosophy of science lies in its meta-consistency. Though different, each level of Popper's framework is a consistent, complementary reflection of the adjoining levels. We have just discussed how Popper's methodology is consistent with his philosophy, how 'theory' and 'facts' are consistent with his methodology and philosophy, and how 'theory' and 'facts' form their own tangled hierarchy. Yet, this consistency and the elegance of Popper's system can be better seen when we move in the opposite direction - from the specific to the general and from the empirical to the abstract.

Thus, we begin with 'facts.' No one would argue that at a minimum facts are descriptions of observable phenomena. Now we might ask what characteristics are necessary for the metafactual level. Or, what do facts demand from theory? A theory must be able to talk about facts and to explain their relationships. Such metadescriptions of facts must at least be consistent with the facts. This is precisely how Popper characterizes theory and nothing more. He does not demand, for instance, that theories be provable or derivable from the facts, as some philosophers do. There are no unnecessary assumptions. The demand for consistency with 'facts' is sufficient to guarantee the tentative nature of theory. This follows immediately when we remember the logical asymmetry of comparing universal statements to existential statements. One falsifying instance is sufficient to disprove a theory but no numbers of "verifying" instances can prove a theory. Thus, theories must be tentative and falsifiable.

Moving from theory to methodology, we would demand only that our methodology incorporates the tentative nature of theory and enhances our ability to bring theory and fact into closer agreement. Falsificationism provides science with such a methodology, and it seeks no more. Finally, the methodology of falsificationism only requires that its supporting philosophy be consistent with the assertions that our knowledge is imperfect (fallibilism) and that there is something independent of the tangled hierarchy of 'theory' and 'fact' to be discovered or 'observed' (realism). Thus, the circle is completed.

Popper's philosophy of science provides a sound epistemology for science and a secure methodology for its growth. It will be shown that no other philosophy of science so far proposed is as consistent, as defensible, or as progressive as Popper's. By maximizing the role of criticism, Popper's methodology has the greatest potential for the growth of knowledge. Though falsificationism gives no guarantee for the growth of knowledge, it both allows and encourages such growth. What more could we ask of philosophy and methodology? And if we were to ask for more, would it not be wise to consider whether we have asked too much?

Conjectures and refutations
Corroborating theories,
One may seek the truth,
But it is not to worry
Of truth not proved
Or error unfound.
For knowledge may grow
When criticisms abound.

Notes

[1] Einstein, A. [1953b], p. 261.

[2] See Kraft [1974] and Popper [1974a]. The Vienna Circle was composed of the philosophers Carnap, Feigl, Frank, Gomperz, Hahn, Hempel, Kraft, Menger, R. Von Mises, Waismann, Neurath, Reighenbach, and Schlick. Logical positivism was concerned with the proper definition of meaning which would admit only well-formed logical and 'scientific' statements and the associated problem of establishing a criterion for the justification of 'theories.' Logical positivism is the subject of section 3.3.5.

[3] Hutchison [1938]. Hutchison's influence is discussed in section 4.3

[4] Not everyone believed that induction was the basis of 'science.' Other philosophies of 'science' are discussed in the next chapter.

[5] Hume [1888].

[6] It is important to note that induction cannot be justified in any probabilistic sense. No number of 'true' 'basic statements' will confer a non-zero probability measure to the 'truth' of a universal statement, since the unknown and 'unobserved' cannot be considered finite.

[7] Much of the recent history of philosophy may be viewed as the logical consequence of Hume's disproof of induction. Irrationalism and logical positivism are, in part, attempts to either incorporate or circumvent Humes' irrationality.

[8] Popper [1972], p. 7.

[9] See Boland [1979] for a discussion of these terms and an insightful presentation of Friedman's methodology. *Modus pollens* is the usual

logical implication, while modus tollens is the converse of the contrapositive.

[10] Popper [1963], pp. 34-35.

[11] Popper [1963], p. 37.

[12] Though mathematics is not 'science' by Popper's definition, its status is not necessarily less. Popper's demarcation merely identifies the factual or empirical 'science' from other disciplines. Popper recognizes the special character of mathematics and logic. They may be considered as a special field of 'knowledge,' subject to their own rules of proof, validity, and refutation, important and necessary, but not sufficient for 'science.'

[13] Popper [1963], pp. 36-37.

[14] Popper [1972], pp. 13-17.

[15] This section is a brief characterization of Popper's science that he develops in the Logic of Scientific Discovery. In particular, see Popper [1959], pp. 59-111.

[16] Ibid.

[17] Ibid., pp. 62-64.

[18] The 'irrefutability' of existential statements is discussed in the Prologue. Also see Popper [1959], pp. 68-70.

[19] Ibid., pp. 101-102.

[20] Note that a logically inconsistent theory is consistent with all 'basic statements,' thus 'irrefutable' and 'unscientific.' See Popper [1959], pp. 91-92.

[21] Ibid., pp. 86-87.

[22] What is deemed "suitable" is a methodological choice just as the choice of the alpha level in hypothesis testing. More is said of the

role of methodological rules and conventions.

[23] The misunderstanding of the role of 'facts' and "falsifying hypotheses" may be the reason that many researchers long for a measure of the probability of different theories given the data. That such a probability measure cannot exist is discussed in the next chapter under the title "probablism."

[24] For a more extensive discussion of 'empirical content' see Popper [1962], pp. 385-388 and Popper [1959], pp. 112-135.

[25] Ibid.

[26] The term "rich" is here introduced to account for the fact that Newton's theory does not imply only one 'basic statement' but four. To be strictly comparable, the lower dimensioned theory must also imply four similar 'basic statements.'

[27] For example, Popper [1972], pp. 385-388 and Popper [1959], pp. 93-111.

[28] Popper first employed the phrase, "empirical basis," to lend ironic support to his thesis of the tentative nature of 'facts.' Popper [1972], p. 387.

[29] Popper [1959], p. 104.

[30] Take, for example, Zeno's paradox sequence, for all i belonging to the natural numbers: $x_i = (1/2)^i$ and $x_i \in (0,1)$.

[31] Ibid.

[32] It is important to realize that no generalization or universal statement can be implied by 'basic statements.' The conjunction of universal statements and basic statements implies only other singular statements.

[33] Ibid., p. 50.

34 *Ibid.*, pp. 53-54.

35 *Ibid.*, p. 102.

36 *Ibid.*, p. 86.

37 *Ibid.*, p. 106.

38 *Ibid.*, pp. 108-109.

39 *Ibid.*, p. 40.

40 Popper [1963], p. 217.

41 *Ibid.*, p. 232.

42 Popper calls this type of 'testing' the crucial experiment.

43 Popper [1959], p. 82. *The Logic of Scientific Discovery* is replete with examples of critically enriching methodological rules.

44 *Ibid.*, p. 81.

45 See Popper [1963], Addenda.

46 *Ibid.*, p. 234.

47 All of Popper's works have many such examples.

48 See Popper [1972], pp. 197-202 for the following story.

49 Popper calls his position metaphysical realism to emphasize its 'unscientific' nature. To Popper, 'metaphysics' is any position that cannot be 'falsified.' See Popper [1972], pp. 38-44, particularly p. 40 + n. Here philosophical realism or just realism is equivalent to Popper's metaphysical realism.

50 Einstein [1953c], p. 342.

51 Popper [1972], p. 106. This is part of a paper first presented at the Third Congress for Logic, Methodology, and Philosophy of Science.

52 *Ibid.*, p. 107.

53 Ibid., pp. 37-44, and Popper [1963], pp. 194-199. Most all of Popper's life work can be viewed as a 'rational argument' for realism in opposition to these alternative views.

[54] See Popper [1962], pp. 228-231, for a brief discussion of fallibilism.

[55] For example, Ibid., pp. 193-200.

[56] Recall that Popper is credited for the destruction of logical positivism. See n. 2.

[57] Popper [1962], p. 229.

[58] This statement may be considered a little too strong, for it is possible, under fallibilism, that we might reach the goal of 'objective truth.' Only, we would not know it if we did.

[59] See Popper [1972], pp. 154-205.

[60] We can call this type of inconsistency metainconsistency, since the inconsistency exists only across levels - from science to methodology and philosophy. Metaconsistency is logically distinct from simple logical consistency, for the latter only requires that a theory does not counterdict itself.

CHAPTER 3

CRITICISM IN THE PHILOSOPHY OF SCIENCE

"Philosophers of science talk about talk about talk."

- I. J. Good

Science, V. 129, p. 443.

Popper's philosophy, unlike Athena, did not spring fully developed from the Godhead. Nor is his philosophy the only acceptable or accepted view. To state the significance and the relation of Popper's philosophy *vis-a-vis* the philosophy of science, we present a 'rational discussion' of the philosophy of science, past and present. The philosophies are compared and contrasted with Popper's in the hope of reaching a broader understanding and synthesis.

First, the work of two contemporary philosophers, Kuhn and Lakatos, is sketched and discussed. Next, the discussion broadens to cover many schools of philosophical thought, identified by their central theses. Finally, a brief historical story is provided to place the 'rational discourse' into historical context and to summarize its content.

3.1 History/Sociology as Epistemology: Kuhn's Paradigm

> "I thought I recognized Kuhn's *problems*; and while I tried to account for certain *aspects* of science to which he had drawn attention, *I was* quite unable to agree with the *theory of science* which he himself proposed; and I was even *less* prepared to accept the general *ideology* which I thought formed the

> background of his thinking. This ideology, so it seemed to me, could only give comfort to the most narrow-minded and the most conceited kind of specialism. It would tend to inhibit the advancement of knowledge."
>
> - P. Feyerabend [1974], p. 197

In 1962 the thesis that history can provide the foundation for an epistemology of science was advanced by Thomas S. Kuhn's The Structure of Scientific Revolutions. Kuhn's work is considered an important contribution to the philosophy of science. Firstly, this philosophy forced the philosophers to explicitly consider the history of science rather than to concentrate solely upon the analysis of abstract scientific theories. And, Kuhn's book was the source of a rekindling of scientists' interest in the philosophy of science. If for no other reason, we must discuss Kuhn for his popularity. Even economists make frequent reference to Kuhn and his phrases - "paradigm" and "scientific revolutions." To cite but a few examples: Johnson [1971], Leijonhufvud [1974], Ward [1972], and Worland [1972]. Although Kuhn's contribution need be considered significant (if for no other reason, by the controversy and attention that it received), Kuhn adds little, if any, substance to the philosophy of science.

At first sight, Kuhn appears to invert all of Popper's philosophy. Where Popper sees the growth of knowledge, Kuhn sees crisis. Where Popper sees dogmatism, Kuhn sees development and progress. Where Kuhn describes the history of science, Popper rationally interprets the logic of science's history. Where Kuhn uses historiography or sociology, Popper employs logic or epistemology. Yet, the differences of these philosophers are not as simple nor as great as they may first seem. Much of this apparent controversy is either semantic or a matter of emphasis.[1]

3.1.1 Kuhn's Science

> "I venture to guess that the ambiguity is intended and that Kuhn wants to fully exploit its propagandistic potentialities."
>
> - P. Feyerabend [1974], p. 199

Kuhn's view is primarily historic. He views the methodology of science from the history of science and not vice versa. Science is what scientists do. In the introductory paragraph of Structure of Scientific Revolutions, Kuhn states that the aim of his essay "is a sketch of the quite different concept of science that can emerge from the historical record of the research record itself."[2]

Kuhn is quite aware of the skepticism that many philosophers hold for the inference of any epistemology from the historical record. "Undoubtedly, some readers will already have wondered whether historical study can possibly effect the sort of conceptual transformation aimed at here .... Again, many of my generalizations are about the sociology or social psychology of scientists, yet at least a few of my conclusions belong traditionally to logic or epistemology."[3] His approach is defended by an "appeal to the facts." "How could history of science fail to be a source of the phenomena to which theories about knowledge may legitimately be asked to apply?"[4]

How does Kuhn arrive at this "different concept of science"?

> I began as a historian of science examining closely the facts of scientific life. Having discovered in the process the scientific behavior, including that of the very greatest scientists, persistently violated accepted methodological canons, I had to ask why those failures to conform did not seem at all to inhibit the success of the enterprise. When I later discovered that an altered view of the nature of science transformed what had previously seemed aberrant behavior into an essential part of an explanation of the science's success, the discovery was a

> source of confidence in the new explanation. My criterion for emphasizing any particular aspect of scientific behavior is therefore not simply that it occurs, nor merely that it occurs frequently, but rather that it fits a theory of scientific knowledge.[5]

The irony of this passage lies in its consistency with Popper's theory of scientific growth - conjectures and refutations. Kuhn begins with some "theory" of scientific growth, "accepted methodological canons," and he observes potential falsifiers, "failure to conform." Later, he succeeds in developing an alternative "theory" which is consistent with the "facts." Finally, the alternative theory's "success" gives Kuhn confidence (which is something that Popper has for no 'theory') and a means of interpreting scientific behavior. Except, perhaps, for the choice of words, how is Kuhn's description of his own research methodology different from Popper's falsificationism?

Much of the controversy surrounding Kuhn can be traced to some misunderstanding of words, either by Kuhn or his readers. It is not only easy to misinterpret Kuhn but it is also virtually impossible to avoid significant ambiguity in Kuhn. The controversy that Kuhn's work created among the "Popperians" is itself sufficient to illustrate Kuhn's ambiguity. In <u>Criticism and the Growth of Knowledge</u> the interpretations expressed by virtually all the contributors are at odds with Kuhn's apparent intent.[6] Even the most sympathetic contributor to Kuhn's views, Masterman, found significant ambiguity in Kuhn's central concept, "paradigm." She finds at least twenty-one different meanings of Kuhn's characterization of his scientific unit of appraisal. For example, a "paradigm" is:

> (1) a universally recognized scientific achievement ...
> (2) a myth ...
> (3) a "philosophy," or constellation of questions ...

(4) a textbook, or classic work ...
(7) an analogy ...
(8) a successful metaphysical speculation ...
(12) a device, or type of instrumentation ...[7]

While such diversity of expression does not invalidate Kuhn's views or the potential insights they might offer, it does make the interpretation and application of his philosophy difficult and readily susceptible to error. Kuhn himself acknowledges this ambiguity and unsuccessful attempts to minimize the number of meanings for "paradigm."[8]

As further evidence of Kuhn's ambiguity, we cite only one of many potential passages.

> That my criteria for discriminating between the essential and nonessential elements of observed scientific behavior are to a significant extent theoretical provides also an answer to what Feyerabend calls the ambiguity of my presentation. Are Kuhn's remarks about scientific development, he asks, to be read as descriptions or prescriptions? The answer, of course, is that they should be read in both ways at once. If I have a theory of how and why science works, it necessarily must have implications for the way in which scientists should behave if their enterprise is to flourish. The structure of my argument is simple and, I think, unexceptionable: scientists behave in the following ways; those modes of behavior have (here theory enters) the following essential functions; in the absence of an alternative mode <u>that would serve similar functions</u>, scientists should behave essentially as they do if their concern is to improve scientific knowledge.[9]

In our view the reader may be forgiven if he does not come away from such passages with a clear understanding of Kuhn's position. After all, is not Kuhn explicitly addressing Feyerabend's charge of ambiguity? But Kuhn's answer that his words "should be read in both ways at once" hardly lessens the ambiguity.

The above passage is also insightful in its illustration of Kuhn's logic, for it is one of the few places where he explicitly discusses the logic of his argument. First, Kuhn claims to "have a theory of how and why science <u>works</u>" and that such a theory must imply prescriptions for scientific practice. But this argument depends crucially upon what Kuhn means by "work" and upon the nature of the explanation of such "success." Yet nowhere can one find an explicit description of Kuhn's theory nor what he means by "work." It appears that Kuhn is going from description to prescription and saying little more than "scientists should do what they do do." Although it is perhaps unfair to "nail" Kuhn for such a position, for he knows better, it is difficult to find any other logic to his argument.

In the last sentence of the former quotation Kuhn states the logic of his position. There we find:

(1) observations of scientific "behavior"
(2) a theory which describes the role of these "behaviors"
(3) a prescription to engage in these "behaviors"

Kuhn claims that (3) is implied by (1) and (2) in the absence of an alternative theory that could provide a similar explanation.

At first sight, it appears that Kuhn is beginning with observations or descriptions (of scientific behavior) and inferring a theory. That such an implication is impossible is expressed by the fallacy of induction. But Kuhn should not be accused of committing this fallacy, for he is completely aware that a theory is needed to interpret "observations."

Instead, closer inspection of Kuhn's argument reveals its circularity. If the behaviors that Kuhn observes are those which "fit a theory of scientific knowledge" and thus exhibit the "following essential functions," then there is a completely closed and circular connection between Kuhn's "theory" and his "observations." Kuhn even admits at least one aspect of this circularity.[10]

Such a circularity makes Kuhn's argument misleading and much of it superfluous. If Kuhn's "observations" of scientific behaviors are dictated by his theory, then there is no need to mention how the scientists behave in the first place. These "observations" carry no weight and can only confuse matters. When reference to Kuhn's observations of scientific behavior is removed, the structure becomes simpler and, I think, less objectionable: I have a theory of how scientific knowledge grows (here the theory should be explicitly stated and explained), thus, scientists should seek to conform to this mechanism "if their concern is to improve scientific knowledge." By such a reformulation, Kuhn's argument becomes the same as other philosophers of science, including Popper. Only, Kuhn is never as clear about his theory of scientific knowledge as the others.

Clearly, Kuhn's mode of expression is subject to different interpretations, depending upon how different readers fill in the "gaps" of his argument. It is quite possible that readers will come to different understandings of Kuhn's theses, even when they make an intelligent attempt to be fair. As we shall see in Chapter 4, a similar ambiguity exists in Friedman's methodological essay.

More to the point, we must ask, what is Kuhn's view of science? Generally speaking, Kuhn's historical perspective sees science as a succession of periods of "normal science" interrupted by sporadic "crises" and "revolutions." "Normal science" is the period of paradigm exploitation and "puzzle-solving" while "revolutions" and "crises" describe those times when the "paradigm" falters causing unrest in the scientific community and the eventual replacement of the old "paradigm" by a new one. "Normal science consists in the actualization of that promise (of future success of the "paradigm"), an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between the facts and the paradigm's predictions, and by further articulation of the paradigm itself."[11] [Parenthesis added.]

Kuhn shares the accepted view that science is somehow empirical. In fact, Kuhn accepts Popper's demarcation criterion of 'falsifiability.' "First is Sir Karl's demarcation criterion without which no field is potentially a science: for some range of natural phenomena concrete predictions must emerge from the practice of the field."[12] "For no theory that was not in principle testable could function or cease to function adequately when applied to scientific puzzle-solving."[13] Yet Kuhn does not transfer the criterion of testability, in principle, to testability, in practice, in a manner consistent with Popper's falsificationism. To this point we shall return.

To Kuhn, "normal science" is the process of "puzzle-solving" within the framework established by the "paradigm." But just what Kuhn means by these central terms is far from clear. In the second edition of The Structure of Scientific Revolutions, Kuhn reduces the number of meanings of "paradigm" to two, "disciplinary matrix" and "shared examples." Still ambiguity remains. For Kuhn still requires the fuzzy concept of an "entire constellation of beliefs, values, techniques and so on shared by the numbers of the scientific community" to specify his "paradigm."[14]

Although Kuhn claims to accept Popper's principle of falsification, Kuhn twists it back upon the scientist where his concept of "puzzle-solving" is the opposite of Popper's "risky predictions."[15] "Normal science does not aim at novelties of fact or theory and when successful finds none."[16] "Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles."[17] "It is no criterion of goodness in a puzzle that its outcome be intrinsically interesting or important."[18] "Though intrinsic value is no criterion for a puzzle, the assured existence of a solution is."[19]

"Puzzles," then, are anomalies between the ruling "paradigm" and nature or extensions of the "paradigm" to as yet unexplored territory. Science is primarily "normal science," and "normal science" is merely the process of "puzzle-solving." The scientist must always be a "puzzle-solver." This "puzzle-paradigm" connection is Kuhn's central thesis.

Kuhn's inverted falsificationism is best seen by his view of "testing." To Kuhn, "puzzle-solving" is always a test of the scientist and never the theory or "paradigm."

> Normal science does and must continually strive to bring theory and fact into closer agreement, and that activity can easily be seen as testing or a search for confirmation or falsification. Instead, its object is to solve a puzzle for whose existence the <u>validity of the paradigm must be assumed</u>. Failure to achieve a solution discredits only the scientist and not the theory. Here, even more than above, the proverb applies: "It is a poor carpenter who blames his tools."[20] [Emphasis added.]

Thus, the only testing in "normal science" is competence testing of the scientists and not the 'testing' of 'theory' or 'fact.'

Kuhn's position on testing is grounded upon a logical error. Is not Kuhn saying, "Since the existence of a theory must be assumed before any anomaly, 'puzzle' or factual inconsistency can be identified, any failure to resolve the 'puzzle' can only logically reflect upon the attempt and not the assumed theory"? But just because we must assume something, it does not follow that we must assume any particular theory nor that we are unable to 'test' our 'theories.' Thus, failure to solve the puzzle need not discredit the scientist; it can discredit the theory.

Clearly, from previous quotes, Kuhn sees no role for imagination or the unexpected in the progress of science. Instead, it is the dogmatic adherence to the ruling "paradigm" that connotes progress. "In its normal state, then, a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigms define. Furthermore, the results of solving those problems <u>must inevitably be progress</u>."[21] [Emphasis added.]

Though the "paradigm" of "normal science" is paramount, it cannot provide a complete description of science's history. To fill in the gaps between "paradigms," Kuhn employs "scientific revolutions." "Scientific revolutions" are the result of the persistent failure to solve some anomaly of fact.[22] This persistence will eventually lead to "crisis" and the emergency of alternative "paradigms."[23] These alternative "paradigms" will be more than just different; to Kuhn they must be incommensurable.[24] Thus, theories or "paradigms" only become problematic when they are riddled with pervasive and persistent inconsistencies to "facts." To Kuhn, minor discrepancies are merely "puzzles" to be solved; crisis occurs only when the "paradigm" has a major breakdown of its "puzzle-solving" ability.

The alternative paradigms that must follow a "crisis" are more than logically incompatible. They are incommensurable. Because of the large gulf between "paradigms" and the inability to adequately communicate across them, "the transition between competing paradigms cannot be made a step at a time, forced by logic and mutual experience. Like a gestalt switch, it must occur all at once or not at all."[25]

Are "paradigm" shifts contributions to knowledge? Do "scientific revolutions" bring in progressive ruling paradigms? Or, will the rulers only say that their administration is progressive? Kuhn seems to answer all three questions affirmatively. "Revolutions close with a total victory for one of two opposing camps. To them (the victors), at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see past history the same way."[26] [Parenthesis added.] How does the "winning paradigm" force us to judge it as progressive? Only those who accept the new paradigm will be the authors of the next textbooks and explicate both the old and new paradigms.[27]

Yet we can find a more substantive view of scientific knowledge in Kuhn. Successful new "paradigms" preserve a great deal of the most concrete parts of past achievements and they always permit additional concrete problem-solutions besides."[28] The ability to save past "puzzle-solutions" and the promise of new "puzzle-solutions" provides a clearer conception of continuous progress.

Still, Kuhn sees scientific progress as primarily "puzzle-solving." This is a progress that cannot be defined outside a given "paradigm." For the incommensurability of "paradigms" and the lack of any logical evolution forbid any more objective framework in which to view the growth of knowledge.

The distinctive contribution of Kuhn is his conception of the "paradigm" as the giver of meaning and definition to scientific activity.

Science is "puzzle-solving" within an established "paradigm." The growth of knowledge is most clearly seen as "puzzle-solving, intra-paradigm," while "inter-paradigm puzzle-solving" will only be progressive if the "winning paradigm" defines and admits the transitional "puzzles" within its framework. "Paradigms" are essential to "puzzles" without which "puzzles" can neither be seen nor solved.

3.1.2 Kuhn versus Popper: Logic and Explanation of Scientific Knowledge or History and Psychology of Research?[29]

3.1.2.1 Description and History versus Methodology and Logic of Science

What are the roles of methodology and description of science? How are each accomplished, and which perspective does each take? The role of methodology is to provide general guidelines for scientific choice. Methodology is derived from the logic of science. First, scientific knowledge is analyzed, and a theory of its growth or a logic of its recognition is developed. Methodology, then, is the application of the logic of science or epistemology to the practice of science. Methodology can be seen as the "art" of the logic of science. Methodological guidelines are not meant to uniquely define the optimal alternatives for each specific scientific choice. They only help the individual scientist to evaluate and choose. Methodology is more of a heuristic for scientific activity than a demand or description of specific scientific behavior.

History and description, on the other hand, take an inverse view of science. Descriptions of science are analogous to 'observations' in 'science.' Just as observations, descriptions are impossible without some type of theory to translate and select the raw data of scientific activity. Would a simple accounting of every action and object of the scientific enterprise be considered a description of history or science?

Or, would a complete diary of a scientist suffice? No, scientific activity must at least be categorized to qualify as description, and those categories necessarily involve a theory.[30]

Historical description seeks to report the actual activity of science as objectively as possible. If only lower level theories are used to develop these reports, then they might possibly be used to "test" more general theories or explanations of science. But one must be careful not to employ the theory under test to also interpret the raw data. If one theory is so jointly used, the "test" becomes tautological and empty, proving only that we see what we have eyes for.

Yet, descriptions have other uses. Descriptions of science can simply be entertaining stories. They may "justify" or "confirm" some theory of historical or scientific explanation. Or, they may be employed as illustrative examples of methodologies or 'scientific theories.' The role and method of description are the choice of the story-teller, and they often vary dramatically.

Kuhn's prescriptions and much of his epistemology result from historical descriptions of science. Recall that Kuhn "began as a historian of science examining closely the facts of scientific life. Having discovered in the process that scientific behavior ... persistently violated accepted methodological canons," he formulates an alternative theory of scientific knowledge.[31] Although, as mentioned earlier, Kuhn's procedure appears to fit Popper's theory of science, Kuhn misapplies it.

Methodology does not claim that its rules or canons are actually used by scientists nor that they need be used. Methodology only claims that these rules may help promote knowledge, if scientists are guided by their wisdom. The correct "test" of a methodology is a "test" of its epistemological foundation and its consistency with these foundations. We can 'rationally criticize' the ability of a methodology to reflect a logic of scientific explanation or question the adequacy of that epistemology. Or, descriptions of science can serve to "test" our theory of scientific growth (recall the Introduction and the last section of Chapter 2). But the role of these descriptions is limited, and they cannot provide "direct" evidence for or against a methodology.

A viable theory of scientific knowledge must be consistent with our "best" historical examples of growth. It need not be consistent with all or even the majority of the history of science, for it is not a 'fact' that all, or most, of the history of science demonstrates the growth of knowledge. Although Popper's theory of scientific knowledge is vulnerable to "refutation," I assert that all the better historical examples of the growth of science are consistent with Popper's conjectures and refutations.

Of course, the critically minded reader will notice that no "proof" of the assertion is presented. But such a "proof" is impractical to provide here, for it would require many volumes of documentation to establish the appropriate "facts" of scientific growth.

In any case, the ultimate "test" of a methodology, as opposed to a theory of knowledge, resides in its value of application. A methodology cannot be judged by the frequency of its use, but only by its long-run ability to provide significant help for the "tougher" scientific choices. Such a practical value can, in turn, only be judged after a long experience in applying the methodology in question. Thus, in the final analysis, only time will tell if Popper's falsificationism is the appropriate methodology of science.

The point to this apparent digression is to clarify and avoid some of the confusion that surrounds "tests" of methodology and epistemology. In particular, Kuhn's apparent rejection of some methodology because scientists' behavior does not conform to accepted methodological canons is misgiven and illogical. First, a methodology cannot be tested; only a theory of scientific knowledge is potentially testable. Secondly, any "test" of a theory of scientific growth would concern 'scientific theories' and not the behavior of scientists. Scientists may arrive at increases in knowledge by luck or by "bad" methodology. Yet, these possibilities are irrelevant to the logic or theory of scientific knowledge. What is relevant concerns only the characteristics of scientific theories that identify scientific progress.

If Kuhn could demonstrate that the behavior of scientists is, indeed, inconsistent with Popper's methodology, this "fact" alone would only make Popper's methodology <u>more</u> valuable; for Popper's theory of scientific explanation would remain uneffected and the practical value of his consistently formulated methodology could be seen as increased by the relative scarcity of its employment. Thus, Kuhn's "observations" from the history of science totally fail to "test," "reject," or influence the methodology of science.

Part of the confusion among history, description, logic, and methodology is rooted in Popper's writings. All of Popper's works contain references and descriptions of the history of science. Yet, it is incorrect to interpret these illustrations as "tests" or evidence for Popper's logic or his methodology. These historical examples are primarily illustrative, demonstrating how various technical aspects of Popper's theories work in the history of science. Popper mainly views the history of science as providing an understanding of past science's problem-situation.[32] He does employ his theory to explain some of the history of science, but he does not claim that such historic sketches "prove" or even "test" his epistemological theories. Implicit in Popper's many historical references is his belief that a theory of knowledge needs to be consistent with the "better" examples of historical growth of knowledge. But it is difficult to justify those who might interpret Popper as resting his epistemology upon the history of science.

Kuhn, however, makes a different connection between the history and methodology of science. As previously discussed, he erroneously rejects some methodology because it is inconsistent with the behavior of scientists. What methodology does Kuhn reject? It is impossible to tell, for Kuhn does not explicitly tell us. It is our opinion that this "rejected" methodology must have something to do with Popper's falsificationism. Otherwise, what is all the fuss and controversy about? While it is true that Kuhn accepts Popper's principle of 'falsifiability' (recall quotations 12 and 13), it does not appear that he accepts it wholeheartedly (see quotations 15-19).

First, Kuhn does not accept Popper's 'falsifiability' as a demarcation criterion. At one point Kuhn goes so far as to turn Popper's philosophy "on its head" by suggesting that "it is precisely the abandonment of critical discourse that marks the transition to a science."[33] But then Kuhn is speaking, as always, about the history of science and not what characterizes the logic of the enterprise. Furthermore, Kuhn interprets Popper's principle of 'falsifiability' as the ability of a theory to generate "concrete predictions."[34] Such an interpretation is not equivalent to Popper's principle and can easily be misleading (as we shall see later, Friedman similarly transforms the principle of 'falsifiability'). The statement "all men are mortal" can be used to make "concrete predictions," but it is not 'falsifaiable.' Or, more common to economics, statements such as "bodies fall according to formula $S = 1/2gt^2$ in the absence of disturbing influence" have the same problem. They can "predict," but they cannot be 'falsified.' One can always point to some "unobserved" "disturbing influences" as the "cause" of any apparent 'refutation.' In any case, it is not clear which methodology Kuhn rejects or which he accepts, but it is clear that Kuhn does not fully accept Popper's methodology in the manner which Popper intends.

Kuhn appears to develop and justify his view of epistemology and methodology by attempting to generalize from history.

> My descriptive generalizations are evidence for the theory (about the nature of science) precisely because they can also be derived from it, whereas on other views of the nature of science they constitute anomalous behavior. The circularity of that argument is not, I think, vicious. The consequences of the viewpoint being discussed are not exhausted by the observations upon which it rested at the start.[35]

What is a descriptive generalization? It must be a theory, since descriptions and facts are singular instances. But there is no direct connection from fact to theory, for there is no valid principle of induction. We do not wish to claim that Kuhn is committing the fallacy of induction. But the interpretation that the history of science somehow gives additional support to Kuhn's theory of science rests upon this fallacy. Given a theory, one can always interpret the "facts" in its light and thereby find consistency. In such a manner, Popper, too, can find consistency between his theory of the growth of scientific knowledge and the history of science. Yet, to claim that such a process can lend additional or independent support for one's theory rests upon a valid method of induction or something like it. Our only point is that Kuhn's appeal to the historical record of science does not provide a source of unique support for his theory. In this sense, Kuhn's theory of science is no more substantive or credible than is Popper's. Thus, Kuhn's evidence for his theory of the nature of science is but another theory of the nature of science. One must wonder if these two theories of science are not one and the same, or if Kuhn's general theory of science is not merely a "generalization" of his descriptive generalization.

This circularity, Kuhn tells us, is somehow harmless. His only support for this assertion is another assertion that his theory has excess content over the observation upon which it is derived. But this tells us nothing, for all theories and universal statements have content greater than a finite set of observations. Yet, Kuhn gives us no "test" of the 'empirical content' of his theory. Kuhn's theory of science then rests only upon assertion and some alleged consistency with "descriptive generalizations."

But Kuhn goes on to insist that his viewpoint is a "useful tool for exploration of scientific behavior and development. No merely circular point of view can provide such guidance."[36] Cannot religion or metaphysics provide similar guidance, even when they are circular?

Kuhn's argument is again irrelevant or erroneous. If the grounds upon which Kuhn bases his theory are circular, it would not, in itself, invalidate the theory. It would only fail to provide support or 'corroboration' for the theory. In addition, any position can provide guidance, just as myth, magic, and metaphysics can guide the scientist to "better" 'theories.' Even when a position is circular, adherents have no difficulty in using it as a searchlight to reflect the world in its own image.

What type of guidance can Kuhn's theory provide? It can guide the historian in his labeling process or it can be a cohesive plot for the story of science. Yet many other stories, including most histories, can provide similar guidance. Clearly, something more is needed.

If Kuhn would limit his theory to a framework for the making of histories of science, then there would be no quarrel. But he claims it can be used to guide methodology and epistemology.[37] Kuhn's central methodological thesis seems to be that "scientists should do what they do do." Although Kuhn rarely goes beyond this advice, he explicitly addresses the problem of theory choice. "Take a <u>group</u> of the ablest available people with the most appropriate motivation, train them in some science and in the specialities relevant to the choice at hand; imbue them with the value system and ideology, current in their discipline; and finally <u>let them make the choice</u>."[38]

Aside from Kuhn's allusion to indoctrination, this is no answer or guide to the problem of theory choice. Is it not the scientist who makes the choice? No one, including Popper, suggests that scientists <u>should not</u> make such choices. Nor has anyone claimed that scientists do not, in fact, make such choices. In this passage, Kuhn is merely prescribing what actually occurs, allusions aside. Although Kuhn claims he does not confuse prescription and description, passages like the above force the reader to be so confused.[39]

As discussed in Chapter 2, Popper's system of thought provides a sound foundation for science. He provides us with a logic of scientific growth and explanation that is consistent with the facual history of science, at least to the degree that we can elevate history to 'fact.' And, Popper's falsificationism is a healthy methodology that can be a useful guide for scientific choice and growth. In contrast, Kuhn mainly gives us a means of describing the history of science, where the logic and methodology of science is reduced to little more than Kuhn's descriptive generalizations.

To secure or to understand science, we need an explanation of its growth and a logic of its knowledge. To encourage and promote the growth of knowledge, we need a reasonable methodology consistent with a logic of science. Descriptions and histories of science have a limited role to play in the epistemology of science. They can be a source of criticism or illustration of our logic or theory of science. Although important, descriptions or histories alone cannot establish an understanding nor any foundation of science. If we take the history of science too literally, as Kuhn appears to do, nothing can be learned; for then science becomes only what scientists do.

3.1.2.2 Descriptions of Science

Although Kuhn and Popper view science from opposing vantage points, they often describe science and its history similarly. As Kuhn expresses this point, "Sir Karl and I do appeal to the same data; to an uncommon extent we are seeing the same lines on the same paper; asked about those lines and those data, we often give virtually identical responses .... Nevertheless ... our intentions are often quite different when we say the same thing. Though the lines are the same, the figures which emerge from them are not."[40] "What are these responses?", you ask.

Popper's descriptions of science are all but entirely devoted to the analysis of problem-situations and their evaluations. To Popper, every 'scientific theory' attempts to solve a problem concerning the explanation of some phenomenon, the unification of previously unrelated 'theories,' or the explanation of past anomalies. The attempt to solve a problem always involves the formulation of a tentative theory followed by some process of error-elimination. Eventually, this process will lead to a new problem or series of problems, and the process continues.[41]

Popper does not attempt to describe how things actually happen, although some detail is given to color the story. Instead, he focuses upon a mere skeleton of the 'tests' of 'theories' and their results.[42] Each description is more of an illustration of a particular point of logic, methodology, or understanding than a description of how research is actually accomplished.

Although Popper's descriptions are primarily points of methodology, he, like Kuhn, is cognizant of the routine aspects of scientific activity. Popper is not a naive falsificationist who shouts, "falsified," at the first sign of an experimental or factual inconsistency. He is aware that before a scientist 'refutes' a 'theory' he checks the mathematics, possible accidental disturbances, the instruments, and the 'observational theory' of the instruments, repeats the experiment, and develops independent 'tests,' when possible.[43] Popper broadly calls such activities 'corroboration' of the 'initial conditions' and 'tests' of 'basic statements' but spends little space describing their detail.

Popper is a metascientist who is interested in the logic of science and not the punctuation of its sentences. He simply assumes that the individual scientist has correctly performed his job. For the logic of science, this assumption is harmless, particularly by Popper's logic. Popper's science requires the replicability, 'testing,' and 'corroboration' of all its constituents. Thus, if an individual experiment or scientist is in error, this stain on our knowledge will come out in the wash. Instead of describing scientific activities, Popper explains how various 'corroborated' results relate to scientific knowledge and its growth.

Kuhn's description of "normal science" concentrates more upon these routine aspects that Popper largely ignores.[44] Kuhn calls these activities "puzzle-solving," and they involve the extension, not the 'test,' of current theory. Falsificationists would agree that the extensions articulated by Kuhn exist and are important. However, they would be disturbed by the dogmatic character that Kuhn ascribes to "normal science." "When engaged with a normal research problem, the scientist must premise current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that, given sufficient brilliance, it can be solved."[45] The only hypotheses that are tested are those concerning the scientist's ability and the adequacy of his approach.[46] Application and extension are Kuhn's principal descriptions of science.

Popper, however, sees 'testing' everywhere. Popper describes all improvements, even the most routine, as involving the critical approach or 'testing.'

> The heating engineer who faces the problem of how to install a central heating system required to work under unusual conditions may just apply his established rules of thumb, and thus fail to solve the problem: in the face of this failure he may depart

> from his routine and (after eliminating several possible solutions) arrive at a critical solution of his problem. He will have acted as an applied scientist in my sense of the word, and he will have made a minor discovery by critical thinking, by the critical rejection of erroneous solutions.[47]

Although Popper admits that Kuhn's dogmatic "normal science" exists, he disagrees with the extent of its presence and views it as a danger to science.[48]

Lakatos also sees a "danger" in Kuhn's description of science and an inaccuracy in Kuhn's emphasis:

> What he (Kuhn) calls "normal science" is nothing but a research programme that has achieved monopoly. But, as a matter of fact, research programmes have achieved complete monopoly only rarely and then only for relatively short periods, in spite of the efforts of some Cartesians, Newtonians, and Bohrians. <u>The history of science has been and should be a history of competing research programmes</u> (or, if you wish, "paradigms"), <u>but it has not been and must not become a succession of periods of normal science: the sooner competition starts, the better for progress.</u> "Theoretical pluralism" is better than "theoretical monism"; on this point Popper and Feyerabend are right and Kuhn is wrong.[49] [First parenthesis added.]

Here the difference between Popper and Kuhn does not concern the existence or lack of any aspect of science. Their disagreement is only a matter of emphasis which derives from their conflicting perspectives.

Turning to descriptions of what Kuhn calls "crisis" and "revolutions," we can see a mirror image of their descriptions of "normal science." During these times of "crisis," a major theory becomes problematic and is in the process of revision. Kuhn sees these times of critical activity as relatively infrequent, while Popper sees continual critical activity.[50] Kuhn describes these "revolutions" from a sociological and psychological vantage. He sees little logic in choosing between competing theories; they are incommensurable. To Kuhn, rationality can be defined only within a "paradigm" and not across "paradigms."

In contrast, Popper views such dramatic shifts in scientific theories as paradigms of the growth of science. And, he formally analyzes the logic of theory choice. Again, the difference is one of emphasis. For instance, Popper suggests that Kuhn's incommensurability thesis "simply exaggerates a difficulty into an impossibility."[51]

A final observation concerning the descriptions of science by these philosophers of science is the striking difference in the clarity of their prose. Kuhn's descriptions are usually quite complex, long, and confusing. Although he uses abbreviations - "paradigm," "normal science," "extra-ordinary science," etc. - each has a matrix of indefinite constituents. Kuhn continually refers to constellations of beliefs, traditions, gestalt switches, shared examples ... to describe science. Yet even this ambiguous jumble of words can be seen as necessary when one realizes that Kuhn is attempting to describe what scientists do, think, and believe and not the knowledge content of their theories.

On the other hand, Popper's descriptions are usually quite simple, direct, and easy to follow. This, too, may have an explanation, for Popper is not interested in the second-world (psychological) manifestations of 'scientific' problems but only their 'objective,' third-world content and relationships. To a significant extent, these philosophers are talking cross-purposefully. They are describing different worlds.

3.1.2.3 Psychology versus Logic of Science

Both logic and psychology impose a particular viewpoint upon their objects of inquiry. It is this imposition that explains much of the differences in the philosophies of Popper and Kuhn. The fact that each describes a different world is the result of their chosen discipline.

Popper is strictly interested in the logic of science. He continually explains the relationships among the third world of science and not the second world. Recall from Chapter 2 that the third world consists of the 'objective' content of thoughts, problem-situations, 'theories,' 'critical debates,' 'corroborations' ... while the second world concerns subjective knowledge, beliefs, ideas, confidence, judgments, understanding ....

Thirty years before Kuhn published, Popper saw the need to eliminate psychologism from the logic of science.

> The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor be susceptible to it. The question how it happens that a new idea occurs to a man - whether it is a musical theme, a dramatic conflict, or a scientific theory - may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge .... "Rational reconstruction"? ... If it is the process involved in the simulation and release of an inspiration which are to be reconstructed, then I should refuse to take it as a task of the logic of knowledge. Such processes are the concern of empirical psychology but hardly of logic. It is another matter if we want to reconstruct rationally the <u>subsequent tests</u> whereby the inspiration may be discovered to be a discovery, or become known to the knowledge. In so far as the scientist critically judges, alters, or rejects his inspiration we may, if we like, regard the methodological analysis undertaken here as a kind of "rational reconstruction" of the corresponding thought-process. But this reconstruction would not describe these processes as they actually happen: it can give only a logical skeleton of the procedure of testing.[52]

Popper's insistence upon the purely 'objective' side of problems is also seen in his discussions of the problem of induction. Section 2.1 outlines how Popper turns Hume's "disproof" of induction into a logical basis for falsificationism. Yet, Popper goes further to argue that Hume's psychological theory of induction is misconceived and that the psychological aspects of knowledge can be better seen by a more 'objective' formulation of Hume's problem.[53]

Popper's 'objective' bias is most strongly developed in "Epistemology Without a Knowing Subject" and "On the Theory of the Objective Mind."[54] Here, Popper argues that subjective knowledge is best analyzed and understood by third-world objects and relations. "The activity of understanding consists, essentially, in operating with third-world objects."[55] We can understand science only by analyzing the third-world problems, their tentative solutions, and their tests. Scientists, even the "best," often did not understand their own 'theory' or problem-situation.[56] Thus, if the "best" scientists formulate and choose theories for misconceived reasons, how can the psychology of science be a guide to understanding or explaining science. Yet, much can be learned from analyzing the 'objective' problem-situation for both science and the scientist.

Kuhn's position, however, is the reverse. He believes that science can best be analyzed from sociology or psychology. "Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. That position is intrinsically sociological and, as such, a major retreat from the canons of explanation licensed by the traditions which Lakatos labels justificationism and falsificationism . . . ."[57]

"The type of question I ask has therefore been: How will a particular constellation of beliefs, values, and inspirations affect group behavior?"[58]

> Group behavior will be affected decisively by shared commitments, but individual choice will be a function also of personality, education, and the prior pattern of professional research. To many of my critics this variability seems a weakness of my position. When considering the problems of crisis and of theory-choice I shall want, however, to argue that it is instead a strength.[59]

Is this how Kuhn wants his "theory" to be "tested"? What is clearest about Kuhn's position is that his goal is to explain the second world of scientific knowledge. His method is to employ second-world "constellations" of beliefs, values, etc. to explain other second-world phenomena, e.g., group behavior and acceptance of the "paradigm."

"The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced."[60] This attempt to explain the second world of the scientist explains why Kuhn's descriptions are more complex and forced than Popper's. Kuhn thus wishes to explain much more than Popper attempts to understand. Or, is the second world "less" than the third world though, no doubt, more complex.

To the investigations of the epistemology and methodology of science, the concepts of "acceptance," "judgments," or beliefs by the scientific community are irrelevant.[61] The fact that "conversion" takes a week or a hundred years does not affect the evaluation of the associated content of knowledge nor the logic of its substantiation. Since Popper only desires to explain and understand the third world and its growth, he needs no second-world entities. Other third-world constructs are sufficient for this task.

Kuhn's explanations require considerably more <u>explicans</u>. The explanation of the phenomenon "paradigm conversion" requires the use of a constellation of second-world constructs plus all the third-world concepts. Or, are beliefs sufficient to explain other beliefs? I think not, and Kuhn implicitly admits that third-world analysis is necessary but not sufficient for "conversion."[62] "To name persuasion as the scientists' recourse is not to suggest that there are not good reasons for choosing one theory rather than the other."[63] What can "good reasons" be if not logic or similar third-world concepts? Kuhn's tells us that these "good reasons" include the ability of a new theory to explain past anomalies and to add precision to our explanation.[64] Are not these "good reasons" at least a part of Popper's third-world notions of 'empirical content' and 'corroboration'?

Our point is that Popper and Kuhn discuss and explain different phenomena - logic vs. psychology of science. Yet each, in his own way, is attempting to address two questions: What is scientific progress? How can the growth of knowledge be encouraged? Their answers to these questions are discussed in the next section. Here only one question remains. Of what value is a theory of the psychology of science?

Clearly, this question may be answered in many ways. Such a theory might provide insight or answers for related problems in psychology, or it might be of interest in itself. It could be employed to translate the literal record of science. And, it might help us identify means that could promote the "conversion" of scientists to a new theory. But it cannot substitute for logic or theory of scientific knowledge. Nor can a psychological theory be a 'test' of some theory of knowledge.

However, the relationship between the logic and psychology of science is not symmetric. Second-world phenomena can be partially explained and understood by analyzing the third-world problem-situation. In the nexus of epistemology and psychology, the most interesting question is: How will the individual scientist react to different quantities and qualities of third-world evidence? Or what factors explain the differences in their reactions? Although the answer to these questions would not be a substitute for epistemology, it could tell us some interesting things. For example, we could then "predict" how the scientist and scientific community would respond to a new theory with a given level of 'corroboration' and which stimuli would cause "conversion." Although Kuhn seems aware of the effect that the third world has upon the psychology of science, his theory is unable to account for it. Thus, Kuhn's theory of science may be called under-identified. We need to explicitly incorporate more exogenous variables (third-world phenomena) to model the interdependent second world of science.

3.1.2.4 Reconciliation of Kuhn and Popper

Kuhn language is more equivocal than we have reflected here, and his position has evolved over time. This evolution can be seen by his 1969 Postscript that he added to The Structure of Scientific Revolution. It is ironic that Kuhn's evolution shows influence from his confrontation with the Popperians at the International Colloquium in the Philosophy of Science; for Kuhn expressed doubt about the usefulness of such discussions at the colloquium itself.[65]

The purpose of this section is to show that views of Popper and Kuhn are essentially the same when they are more loosely considered and that the remaining differences are explained by corresponding differences in emphasis and focus.[66] Also, it is hoped that any misrepresentation of Kuhn is here rectified.

First, then, let us look at the evolution of Kuhn's position. At the colloquium, John Watkins identified five distinctive theses in

Kuhn's original position. These are:[67]

(1) The Paradigm-Monopoly thesis: "A paradigm brooks no rivals." A scientist can accept only one paradigm at a time.

(2) The No-Interregnum thesis: There is no gradual transition between paradigms. A scientist quickly changes from one to another paradigm.

(3) The Incompatibility thesis: Different paradigms are necessarily incompatible.

(4) The Gestalt-Switch thesis: "The transition between competing paradigms cannot be made a step at a time .... Like the gestalt switch, it must occur all at once (though not necessarily in an instance)."[68]

(5) The Instant-Paradigm thesis: The invention of the new paradigm happens all at once.

One might believe that Watkins has exaggerated the clarity of Kuhn's position. But he does well in identifying and isolating the salient aspects of Kuhn's theory. In fact, it is only the distinctive nature of these theses, particularly the first three, that gives substance to Kuhn's theory. In any case, Watkins provides us with a good characterization of Kuhn's initial position, found in Kuhn [1962].

As a result of the 1965 colloquium, Kuhn revised his position. Since then Kuhn says:

(1) With respect to the Paradigm Monopoly Thesis:

"Consider, for example, the reiterated emphasis, above, on the absence or, as I should now say, on the relative scarcity of competing schools in the developed science."[69] "Usually individual scientists, particularly the ablest, will belong to several such groups either simultaneously or in succession .... Paradigms are something shared by the members of such groups."[70]

(2) With respect to the No-Interregnum Thesis:

Arguments statable in the vocabulary that both groups use in the same way are not, however,

> usually decisive, at least not until a very late stage in the evolution of opposing views .... As translation proceeds, furthermore, some members of each community may also begin vicariously to understand how a statement previously opaque could seem an explanation to members of the opposing group. The availability of techniques like this does not, of course, guarantee persuasion .... Nevertheless, as argument piles on argument and challenge after challenge is successfully met, only blind stubbornness can at the end account for continued resistance.[71]

(3) With respect to the Gestalt-Switch thesis:

> The conversion experience that I have likened to a gestalt switch remains ... Good reasons for choice provide motives for conversion and a climate in which it is more likely to occur. Translation may, in addition, provide points of entry for the neural reprogramming that, however inscrutable at this time, must underlie conversion. But neither good reasons nor translation constitute conversion ....[72]

Thus, Kuhn has evolved the monopoly of the paradigm into a "relative scarcity" of paradigm competition where scientists belong to a number of paradigms. Instead of paradigm conversion being an instant transition, it now seems to be a long evolution of translations, communications, arguments, reasons, and finally the "gestalt electro-shock." Yet, even the "gestalt electro-shock" can be partially explained by reason and translation. Now, incompatibility and incommensurability of paradigms merely reflect the fact that initial communication between groups will not be perfect.

Whatever might have been distinctive about Kuhn's original theory of science, it is destroyed by the 1969 Postscript. We no longer have a theory (if we ever had one), for Kuhn's story is now consistent with all possible histories of science. In an effort to achieve descriptive accuracy, Kuhn has provided us with, at best, a classification scheme in which to pigeonhole different histories of science. Thus, the only unique attribute of Kuhn's description is his choice of words - "paradigms," "revolutions," "conversion," "translations," etc. But the content of the story is not restricted by Kuhn's "theory," except that Kuhn focuses upon the psychological and sociological aspects of science.

Now we can see similarity in both Kuhn's and Popper's descriptions of science. Kuhn accepts Popper's demarcation criterion and sees "puzzles" as problems between facts and theory. Also, Kuhn gives anomalies - the persistent discrepancy between fact and theory - the key role in inducing "crisis" and "revolution." All this is quite Popperian. It would also be fair to translate Kuhn's "puzzle-solving" and "crisis-resolution" into 'corroborated empirical content.'[73] In this way, the basic description of science becomes the same for both philosophers.

Yet, there remains a substantial difference in emphasis that each would choose. Kuhn still concentrates upon the more dogmatic aspects of the extensions of theories. Here, Kuhn appears to be describing the growth of technology or applied science, where a well-defined practical result is that activity's goal, rather than the experimental "pure" sciences.

Popper, however, maintains that growth, even in the applied sciences, will occur by trial and error or by 'falsification.' Kuhn's description of "normal science" serves only to remind us that before a 'refutation' can be proclaimed the scientist need check all possible malfunctions in the equipment, and replicate the experiment. Popper, too, is aware of these necessary activities, only he chooses not to get embroiled in them.

Popper and Kuhn still insist on talking about two different worlds on the issue of theory change. Kuhn is entirely concerned with the second-world conversion of scientists. But he is willing to permit a role for reason in this process. Only reason cannot be a sufficient reason for conversion. Popper, in contrast, remains as firmly entrenched in the third world of theory choice. He logically analyzes each 'theory's' content from his methodological perspective. By the concept of 'corroborated empirical content,' Popper can then explain and "predict" the growth of knowledge. In Popper's third world, the beliefs and "conversion" of scientists are irrelevant and, by choice, suspect. For theory choice, Popper provides a clear, simple, and enlightening picture, while Kuhn can say no more than conversion depends on everything else and reason alone is not sufficient.

The basic point is that Popper and Kuhn view the history of science essentially the same. They simply prefer to start at different places and to emphasize opposing characteristics of science and epistemology.

Kuhn emphasizes the relativistic and subjective aspects of knowledge and epistemology. His subjectivism is seen by his continual focus on what scientists think and believe about their theories, and his relativism is reflected by his concentration upon the "paradigm."

> Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other. The inevitable result is ... a misunderstanding between the two competing schools ... communication across the revolutionary divide is inevitably partial .... The proponents of competing paradigms practice thier trades in <u>different worlds</u>.[74]

Where Kuhn concentrates upon the distinct nature of "paradigm" and the difficulty of communicating across "paradigms," Popper discusses the logical analysis of theories and how 'tests' can be conducted to "settle" the differences. Where Popper focuses upon the 'objective' content of "competing" theories and their relationship to empirical evidence, Kuhn speaks of the beliefs of individual scientists and how partial communication across paradigm barriers will eventually affect the "conversion" of the scientific group.

Perhaps Kuhn gives us a more "realistic" view of how science is actually accomplished. But is such realism "useful"? Not if the object of our inquiry is epistemology or methodology. At best, Kuhn's "observations" might be helpful in establishing a new methodology in science and making it "work." The difficulties in communication and conversion that Kuhn describes would need to be overcome if methodological change is to "succeed." Yet, given these difficulties in affecting change, what better reason is there for emphasizing the purely logical analysis of scientific theories? Popper's philosophy can help overcome the difficulties that Kuhn can only describe. Even for the completely practical issues of methodological progress and scientific change, Popper's view of science is more useful.

Kuhn's position can provide no guidance to science or its growth other than "scientists should do what they do do." His philosophy solves no problems nor creates any problems that might be solved. He merely describes some of the difficulties that are found in the practice of science. Thus, by Kuhn's own criterion, "puzzle-solving," he fails to provide an adequate framework.

Kuhn's theory of science is only a classification schema; thus, it tells us nothing. It is merely a dictionary of colorful words which may be used to label episodes from the history of science. Kuhn's theory of science can only be advantageously employed as an exploratory inquiry into the social psychology of scientists. The next step should then be to identify what type of behavior cannot happen as a consequence of given stimuli.

Finally, the methodology and epistemology of science is totally unaffected by Kuhn. Kuhn only confuses the issues that Popper previously clarified and simplified. To ground logic and epistemology of science on psychology or historical description is, indeed, a retrogression. The study of psychology can benefit from logic, but logic cannot be reduced to nor explained by psychology or sociology. Any philosopher or scientist who is interested in issues other than social psychology or the descriptive history of science need not consider the philosophy of Kuhn nor any compromise or synthesis between Kuhn and Popper. For any such purpose, Kuhn can be ignored not only without loss but also with a positive reduction of error.

3.2 Lakatos: The Methodology of Scientific Research Programmes

The late Imre Lakatos was a student of Karl Popper, and his philosophy is directly dependent upon his teacher.[75] Lakatos can best be understood as both a philosopher and a historian of mathematics and science. His principal contributions are his many clarifications and refinements of Popper's falsificationism and his own development of a methodology for analyzing the history of science. Lakatos may also be seen as one of the more sophisticated and harsh critics of Popper's philosophy. Lakatos' analysis is a series of quests into the problems, or perhaps "puzzles," of Popper's falsificationism in which he identifies those problems and offers their solutions.[76] Yet, we shall argue that Lakatos adds little, if any, substance to Popper's philosophy. Lakatos' criticism gives us an excellent opportunity of more fully understanding Popper's philosophy.

3.2.1 Lakatos' Internal History as Methodology

At the surface, Lakatos contributes to the philosophical inquiry of science by emphasizing that a 'scientific theory' is an organic whole. He does not view science as composed of isolated hypotheses or theoretical systems composed of conjunctions of individual statements but, instead, as a "research programme" which is a hierarchical complex of theories, heuristics, metaphysics, and ontology. The view that a system of 'theories' is the unit of scientific knowledge is explicit in Popper, although often overlooked.[77] Yet, Lakatos adds to the understanding of scientific theories by explicit emphasis of this point. Also, his conception of the "scientific research programme" has more degrees of freedom in which to explain the history of science.

"Scientific research programmes" are divided into parts - "hard core" and "protective belt of auxiliary hypotheses," and "positive" and "negative heuristics." Though we may analyze the "research programme" in the above terms, the programme itself is defined only after the separate parts are merged into a cohesive whole. The "hard core" is composed of the central theoretical propositions of the "research programme." For example, the "hard core" of the Newtonian programme is simply the conjunction of Newton's three laws of dynamics and his inverse square law.[78] The "negative heuristic" relates only to the "hard core." It merely forbids the operation of modus tollens within the "hard core."[79] The "negative heuristic" demands that the "hard core" remain "irrefutable." 'Falsification' is barred from entry into the "hard core."

Instead, a "protective belt of auxiliary hypotheses" bears the brunt of all 'testing' and 'refutations.' The bulk of scientific activity and knowledge is contained within the "protective belt." Here is contained the 'observational theories' and all the many hypotheses needed to define measurement and instrumentation. The 'positive heuristic' is a partner to the 'protective belt.' It is the "research policy" that dictates in more or less detail how to conduct research.[80] "The positive heuristic consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research programme, how to modify, sophisticate, the 'refutable' protective belt."[81]

"Research programmes" are then appraised only in a dynamic sense. The manner in which the "protective belt" is altered, "problemshifts," determines the appraisal. A "research programme" is deemed "theoretically progressive" if its series of "problemshifts" contains excess 'empirical content'; "empirically progressive" if some of the excess 'empirical content' is 'corroborated'; "progressive" if it is both "theoretically" and "empirically progressive"; and "degenerating" if it is not "progressive."[82] Thus, Lakatos' appraisals of science depend crucially upon Popper's concept of 'empirical content.' Lakatos differs only in stressing how 'empirical content' of a 'research programme' develops over time rather than static comparing of the 'corroborated empirical content' of rival 'theories.'

Lakatos illustrates his concept of a "progressive research programme" by a characteristic story of a contrived Newtonian physics.[83]

The problem with Lakatos' example is that it is more apt to confuse the reader than to clarify his concept. It appears to be a continual series of ad hoc adjustments in the "protective belt" and not scientific progress.[84] The best way to understand Lakatos' conception of "progressive" is to merely visualize a series of "theories" and their adjustment in which each adjustment results in greater content and some of that content is 'corroborated.' Popper's description of scientific growth is Lakatos.' Only Lakatos adds a few refinements.

Together, these parts of "scientific research programmes" and this method of appraisal define Lakatos' methodology of scientific research programmes (MSRP). MSRP is an elaboration of Popper's falsificationism, but not a trivial one. A significant difference is that an anti-Popperian brand of conventionalism is placed at the heart of MSRP, the "hard core."[85] Lakatos holds that the "hard core" is 'irrefutable,' thus criticism is not permitted in the center of his "research programmes." This dogmatic attitude towards theories and the associated defensive tactics of adding ad hoc auxiliary hypotheses to "save" a theory are associated with conventionalism (recall the "conventionalist strategems" of Chapter 2). Lakatos sees this dogmatism of the 'irrefutability' of the "hard core" as a virtue which supports the continuity of scientific growth.[86] Popper, in contrast, believes that any such 'irrefutable' element needs to be minimized for growth. Popper is aware that even the best scientific theories contain statements that are, by themselves, 'irrefutable'; for example, mathematical tautologies, theoretical terms, or implicit definitions. Yet, whenever we succeed in reducing the tautological nature of these statements, the 'empirical content' and the explanatory power of a 'theory' is increased.

To Lakatos, the "hard core" may be changed only at the expense of the entire replacement of the "research programme." The change of "research programmes" is not an abrupt "revolution," but a gradual supersession of a "progressive research programme" over a "degenerating" one. Yet, there is a great deal of similarity between Lakatos and Kuhn in their descriptions of the changes in "research programmes" or "paradigms." Here, Lakatos is most unique by his assertion that a "scientific research programme" cannot be 'falsified,' thus denying Popper's demarcation criterion, something which Kuhn does not deny.[87]

Another unique aspect of MSRP is its "positive heuristic." The positive heuristic" is what identifies appropriate problems, guides their solutions, and generally directs the research. This "positive heuristic" is quite similar to a list of Kuhn's "puzzles" and some parts of the "paradigms." Here, less rational elements are allowed to affect the definition and development of the "research programme." Lakatos provides us with no explicit limits for potential "positive heuristics," but it is clear that ideology, world-view and ontology are permitted.[88] The advantage of Lakatos' "positive heuristic" lies in its ability to "explain" the history of science. With such flexible means of description available to us, how could we fail to explain more history than Popper's logic? Yet, we might ask Lakatos just how one "observes" a "heuristic," or if his "heuristic" can "predict" some unique and "falsifiable" state of affairs?

Lakatos' MSRP strikes a fairly equal compromise, not a synthesis, between the theories of Popper and Kuhn. MSRP can be used to explain the tenacity of 'scientific theories,' the progress of science with "oceans of anomalies," and the gradual replacement of one "research programme" by another more "progressive" one, as can Kuhn but not Popper. Also, MSRP can rationally explain how one "research programme" supersedes another and how each is internally developed, as can Popper but not Kuhn.

Lakatos is best seen as offering an amended Popperian methodology to rationally explain Kuhn's histories. Or, as Lakatos puts it, "Indeed, as I had already mentioned, <u>my concept</u> of a 'research programme' may be construed as an objective, 'third world' reconstruction of Kuhn's socio-psychological concept of 'paradigm': thus Kuhnian "Gestalt-switch" can be performed without removing one's Popperian spectacles."[89]

Lakatos also differs from Popper in his obsession with "rational reconstructions" of the history of science. To Lakatos, a "rational reconstruction" is the application of some normative methodology to the explanation of the actual growth of science. It is "rational" in the sense that it provides a "characteristic pattern of the rational growth of scientific knowledge" from the perspective of some given methodology.[90] Each separate methodology could write a distinct "rational reconstruction" of the history of science.

How might we compare or evaluate these different "rational reconstructions"? Simple - the "best" methodology will provide the most sweeping "rational reconstruction" of the actual history of science, where the deviations between actual historical events and their "reconstructions" are minimized.[91] A "rational reconstruction" is like an econometric model where the parameters, or the methodology, are chosen for their ability to minimize the sum of the absolute deviations of the model from the data.

Lakatos goes on to define "internal history" as the "rationally reconstructed" history and "external history" as the residual, non-rational events in science.

> The history of science is always richer than its rational reconstruction. But rational reconstruction or internal history is primary, external history only secondary, since most of the important problems of external history are defined by internal history. External history either provides non-rational explanations of the speed, locality, selectiveness, etc. of historic events as interpreted in terms of internal history; or, when history differs from its rational reconstruction, it provides an empirical explanation of why it differs. But, the rational aspect of scientific growth is fully accounted for by one's logic of scientific discovery.[92]

Lakatos recognizes the dependence on our "external history" or how we view the logic of scientific knowledge. Yet, he feels that our logic may be judged by its ability to make "sense" out of the predominance of the scientific record.

It must be admitted that MSRP is, in this sense, "better" at "rational reconstruction" than falsificationism. This follows from the simple fact that the increased degrees of freedom made available in Lakatos' "positive heuristic" can only help to explain, or to "rationalize," the history of science. It would also be fair to assert that Lakatos' MSRP is better than any other explicit framework in "internalizing" the history of science. But, this should not be too surprising when one considers that MSRP was designed for just this purpose. In any case, Lakatos has no difficulty in finding historical examples that show the superiority of his framework over all its rivals.[93]

Yet, we might ask, "Do we wish our methodology or theory of scientific growth to explain all or most of the history of science?" If our purpose of a methodology is to point out the underlying logic of scientific knowledge and to provide guidance in using this logic to promote the growth of knowledge, why should we expect a methodology to also give us a descriptive account of the history of science? And, could we not fail in our purpose if our theory of knowledge explains too much? To this last question Popper answers, undeniably, yes. "A crucial experiment which any theory of knowledge must satisfy is that it must not explain too much. Any non-historical theory explaining why a certain discovery had to be made must fail because it could not explain why it was not made something earlier."[94]

As an intuitive justification of this trade-off between the descriptive accuracy and the identification of the logic of science, we propose a parody of one of Lakatos' "rational reconstructions" of the history of science.[95] Also, it is hoped that the following is an insightful metaphor of the value of Popper's falsificationism.

Popper: A Methodology of Science Progressing Science
From an Ocean of Anomalous History of Scientific Growth

Popper proposed a logic of science and claimed that the growth of scientific knowledge could be seen as the 'rational' operation of falsificationism. Popper is just like Prout who "claimed that the atomic weights of all pure chemical elements were whole numbers." "(Each) knew very well that anomalies abounded."[96] Prout explained these anomalies by asserting that chemical substances as usually found or separated at the time were impure, while Popper believed that the historical growth of ideas always contains "an irrational element."[97] Prout's programme led chemists through a century of successes and defeats before the final victory, just as Popper's methodology led philosophers and scientists through a half-century of successes and defeats, but the final "victory" is still over the horizon.[98]

"Our sketch shows how a research programme can challenge a considerable bulk of accepted scientific knowledge."[99] While my sketch might demonstrate how a methodology of science can challenge a considerable bulk of accepted scientific history. To see this, the analogy must be extended.

Popper's logic of scientific growth, like the periodic table, can provide the idealized framework which explains the logical skeleton of scientific growth and promotes an understanding of the growth and development of science. As the periodic table is an idealized framework which explains only the elemental skeleton of chemical substances and promotes an understanding of how chemical substances are formed. Neither the periodic table nor falsificationism can explain all, or even most, of the richness of the process of chemical combination or scientific progress, and neither should. For, to do so would be to throw away a general, yet simple, framework that explains a complexity of important characteristics. In either application, the framework which can describe all the complexity of these processes would fail to highlight the important characteristics (it would get lost in its own details) and thus could not serve as a guide for further inquiry. Any such complex framework that can describe almost everything could explain little.

To some, this metaphor may seem a bit "stretched." But, if the goals of a methodology are to provide a logic, or understanding, of the general characteristics of the growth of knowledge and to serve as a guide for further growth, then this analogy is quite natural. The periodic table provides just this type of general framework and guidance for the understanding and explanation of chemical compounds, while at the same time the periodic table explains precious little of how actual chemical compounds are formed or interact.

A quite similar relation between methodology and "rational reconstruction" was seen by Popper as he first proposed falsificationism. These words can bear repeating. Rational reconstructionism?

> If it is the processes involved in the simulation and release of an inspiration which are to be reconstructed, then I should refuse to take it as a task of the logic of knowledge. Such processes are the concern of empirical psychology but hardly of logic. It is another matter if we want to reconstruct rationally the subsequent tests whereby the inspiration may be discovered to be a discovery, or become known to be knowledge. In so far as the scientist critically judges, alters, or rejects his inspirations we may, if we like, regard the methodological analysis undertaken here as a kind of "rational reconstruction" of the corresponding thought-process. But this reconstruction would not describe these processes as they actually happen, it can give only a logical skeleton of the procedure of testing.[100]

Can the relationships between the logic or the methodology of science and the actuality of science be better stated?

Again, we assert that Lakatos' MSRP has the decided edge over Popper's conjectures and refutations (CR) as a framework for creating histories of science. To the extent that such histories are useful so is MSRP. But, the utility of MSRP ends here. As a methodology or epistemology of science, MSRP can only be considered inferior to its origins, falsificationism.

To justify this assertion, we must focus upon the differences in these methodological views. The central difference lies in the 'irrefutability' of Lakatos' "hard core." Along with the "positive heuristic," this "hard core" defines Lakatos' research programme. Lakatos believes not only that scientists have been dogmatic about their "hard core" but also that this dogmatism is a positive advantage. Perhaps it is the actual influence that conventionalism has had upon science that causes MSRP to better explain the history of science. In any case, our position is that the explicit inclusion of dogmatism in a methodology is sufficient for its rejection.

In moderation, dogmatism and tenacity are to be admired and can promote scientific progress. "The dogmatic attitude of sticking to a theory as long as possible is of considerable significance. Without it we could never find out what is in a theory."[101] But any greater dogmatism than that required to patiently explore a theory's potential can only slow the wheels of scientific progress.

Lakatos is right to remind some naive Popperians to suspend their cries of "refuted," "inconsistent," or "abandon the ship" over an emerging theory. New theories deserve a period of grace in which we may learn their full meaning or come to understand their implications. Yet, 'rational criticism' also has a valuable role to play in new theory development. Only by identifying inconsistencies and potential falsifiers can a rough new theory be improved and fortified.

Lakatos goes too far. No theory deserves the indefinite refuge that Lakatos' "hard core" provides. The best way to improve, even our central theses, is by 'rational criticism' and confrontations with the 'facts.' How else can we learn? In an effort to incorporate the historic continuity of science, Lakatos gives dogmatism an essential role in his methodology. For this reason, we must reject MSRP as a methodology or a guide for science. Here there is no loss, for Popper's falsificationism has all positive aspects of MSRP without the latter's excess dogmatism.

3.2.2 Lakatos: Criticism and Inconsistency

Lakatos is not content with the elaboration of Popper's ideas. His writing contains some of the harshest criticisms of his teacher. In fact, Lakatos' criticisms of Popper are so numerous that time and space does not permit their adequate treatement. Thus, we shall focus on only Lakatos' major criticism of Popper's position.

It appears that the preponderance of Lakatos' criticism is derived from his trouble with Popper's demarcation criterion. The cleavage of science and non-science was Popper's first and sharpest philosophical stroke.[102] In _The Logic of Scientific Discovery_, Popper proposed 'falsifiability' as the criterion of demarcation, contrary to Wittgenstein and the logical positivists' criterion of meaning. Simply stated, "_it must be possible for an empirical scientific system to be refuted by experience_."[103]

Lakatos is not satisfied with Popper's simple solution. Instead Lakatos provides a strong criticism of Popper's position and an alternative solution.[104] Lakatos characteristically begins his criticism of the demarcation criterion by presenting a parody of Popper's "game" of science. Then Lakatos makes his opening move, the reformulation of Popper's demarcation criterion.

> He (Popper) is not content with tests which are designed to test large systems: he calls on the scientists to specify, beforehand, those experiments which will, if their outcome is negative, lead to falsification of the _very heart_ of the system. He demands of the scientist that he specify in advance under what experimental conditions he will give up his _most basic_ assumptions. This, indeed, is the gist of Popper's "demarcation criterion" or, to use a better term, of his definition of science.[105] [Parenthesis and first emphasis added.]

Lakatos is not content with his false assertion that the above somehow reflects Popper's view on demarcation, but he further asserts that this is Popper's basic rule for the "game" of science.

Lakatos gives the following as the only evidence for these assertions.

> Popper's basic rule is that the scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions: "criteria of refutation have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst not merely a particular clinical diagnosis but psychoanalysis itself? And have such criteria ever been discussed or agreed upon by the analysts."[106]

In the above, Lakatos is quoting from Popper [1963], p. 38, n. 3; and this is the sole reference that Lakatos uses to define and criticize Popper's demarcation criterion.

What is disturbing about Lakatos' selected reference is that it has nothing to do with Popper's demarcation criterion. Lakatos chooses a tangential remark that Popper makes about 'corroboration' to misrepresent Popper's position. Such a misrepresentation is reprehensible when one realizes that: i) Lakatos has read all of Popper's work; ii) Lakatos was both a former student and colleague of Popper at the London School of Economics; and iii) Popper provides many unambiguous and explicit statements of his demarcation criterion, including some in the work which Lakatos references.

To make our case of misrepresentation, we must quote the portion of Popper's text to which Popper's footnote refers.

> The two psycho-analytic theories were in a different class (from Marxist theory of history). They were simply non-testable, irrefutable. There was no conceivable human behavior which could contradict them .... But it does mean that those "clinical

> observations" which analysts naively believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice.[107] [Parenthesis added.]

It is to this last sentence that Popper adds the footnote from which Lakatos quotes. This footnote is too long to give full citation, but we must present the first part to understand the portion that Lakatos uses.

> "Clinical observations," like all other observations, are interpretations in the light of theories, and for this reason alone they are apt to seem to support these theories in the light of which they are interpreted. But real support can be obtained only from observations undertaken as tests (by "attempted refutations")' and for this purpose criteria of refutation have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted.[108]

Notice that Popper qualifies the section that Lakatos quotes with "for this purpose." "For this purpose" is the purpose of claiming that some observation counts as 'corroboration.' Thus, Popper is not here addressing the issue of the demarcation of a theory, as Lakatos asserts, but only discussing why "clinical observations" of psychoanalysts do not count as 'corroboration' of the theories of Freud and Adler.

Popper nowhere states that the scientist need add conditions that will cause him to reject his theory before his theory can be considered scientific nor any other such condition. The issues of 'corroboration' and demarcation are separate. Again, all that demarcation requires is the possibility of 'falsification.' To demonstrate the extent of Lakatos' misrepresentation, we need return to the text from which Lakatos quotes.

There, Popper clearly asserts that these psycho-analytic theories (Freud and Adler) are not 'testable' or 'falsifiable.' This is Popper's premise and is equivalent to the assertion of non-scientific status.

But, Popper considers the naive assertions made by analysts that all those "clinical observations" confirm their psycho-analytic theories. Thus, Popper's footnote is merely an answer to this naive assertion.

Popper's logic is as follows:

(1) Assertion: The psycho-analytic theories of Freud and Adler are not 'falsifiable,' thus not scientific.

(2) Assertion: "Clinical observations" of analysts do not count as confirmation.

(3) Requirement for "not" (2): To count as confirmation, the observation must result from attempted 'refutations.'

(4) Specification of (3): To count as attempted refutation, the experimentor (not the theorist as Lakatos implies) needs to have some idea of what constitutes a 'falsification,' i.e., "criteria of refutation."

(5) Potential falsifiers of (2):

> "But what kind of clinical response would refute to the satisfaction of the analyst not merely a particular analytic diagnosis ('observational theory') but psycho-analysis itself (the theory in question)? And have such criteria ever been discussed or agreed upon by analysts?"[109] [Parenthesis added.]

These last two questions may be seen, in context, as Popper's suggestion of potential falsifiers to his own assertion that these theories have never been 'tested.'

While it is true that Popper is talking about demarcation in the article from which Lakatos quotes, the quote itself concerns only 'corroboration' or confirmation. Popper gives us these psycho-analytic theories as an illustration of how his demarcation criterion works. He begins by asserting that these theories are 'irrefutable,' thus not scientific, criticizes the claim of confirmation by the psycho-analysts, and ends with questions that could refute his own claims.

Lakatos, however, interprets the last two steps of Popper's chain of reasoning as Popper's demarcation criterion itself. No bigger mistake could be made. For Popper even sums up the discussion of these psycho-analytic theories by restating his demarcation criterion: "The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations."[110] What could be clearer?

But Lakatos' abuse does not stop here, for he uses only the last two questions (see quotations 109 and 106) when applying Popper's alleged demarcation criterion.[111] Yet, these questions can only be seen, in Popper's context, as potential falsifiers to Popper's assertion that psychoanalytic theories have never been tested. The point is that Lakatos uses the most irrelevant and tangential statements, with respect to demarcation, in Popper's entire paper to represent a position that is clearly stated in the same paper.

Lakatos even further misrepresents Popper's position. In quotation 106, Lakatos states that these "criteria of refutations" apply to a theory's "most basic assumptions." This term is absurd, and one which Popper has never used. What are "most basic assumptions"? Are they axioms, definitions, or logical and mathematical theories? If so, then no one, including Popper, would suggest that they need be 'falsifiable' at all, let alone in advance of experimentation. Such entities are, by their nature, tautological and 'irrefutable.' Yet, they can help us translate the different parts of a theory. All that Popper requires is that the theoretical system, as a whole, be potentially 'falsifiable.'[112] One can only believe that Lakatos is attempting to give Popper's position its worst possible light in order to refute it. In any case, it is clear that Lakatos completely misrepresents Popper's demarcation criterion.[113]

By such a misconstruence, the 'rational criticism' of Popper's criterion should be easy. But Lakatos requires two additional steps to "falsify" Popper's alleged demarcation criterion. First, he defines a meta-criterion in order to judge Popper's demarcation criterion. "<u>If a demarcation criterion is inconsistent with the basic appraisals of the scientific elite, it should be given up</u>."[114] Secondly, Lakatos consults the history of science to find the <u>appropriate</u> <u>appraisals</u> of the scientific "elite." What are these appraisals? And, who shall be allowed to make them? These are the necessary questions for which Lakatos provides no answers. Instead, the entire force of Lakatos' "refutation" of Popper's demarcation criterion is carried by the following:

> But what if we put Popper's question to the Newtonian scientist: "What kind of observations would refute to the satisfaction of the Newtonian not merely the particular Newtonian explanation but Newtonian dynamics and gravitational theory itself? And have such criteria ever been discussed or agreed upon by Newtonians?" The Newtonian will, alas, scarcely be able to give a positive answer. But then, if psychoanalysts are to be condemned as dishonest by Popper's standards, must not Newtonians be similarly condemned?[115]

This is how Lakatos suggests that the "appraisals of the scientific elite" would refute Popper's demarcation criterion.

Next, Lakatos offers a new version of Popper's demarcation criterion and asserts from a pseudo-historic perspective that this too would lead to a falsification of this criterion. If Lakatos did not realize his misrepresentation of Popper's demarcation criterion, why does he offer us this new version? According to Lakatos, "Popper may certainly withdraw his celebrated challenge and demand falsifiability - and rejection on falsification - only for systems of theories, including initial conditions and all sorts of auxiliary and observational theories."[116] In fact, the "demand" for falsifiability of the entire theoretical system is precisely Popper's demarcation criterion. But, Popper has never demanded rejection upon falsification. The rejection of a scientific theory is always a risky choice, to Popper, and such rejection does not enter directly into any of his methodological rules. Thus, again Lakatos misrepresents Popper's position.

Still, the entire weight of Lakatos' argument is carried by his Newtonian story.[117] This story concerns the ad hoc defensive tactics that a Newtonian might possibly use. It is not a story of what did happen, but one which is only crudely similar to the discovery of the outer planets. In this story we learn nothing new. Its message is merely that it is always possible to rescue any potential 'falsification' by ad hoc hypotheses. But, Popper explicitly recognized this before he ever proposed his demarcation criterion.[118]

Popper himself responds to Lakatos' argument, which Popper terms the "alleged equal status of Newton's theory and Freud's theory."[119] In defense of the scientific status of Newton's theory, Popper has no difficulty in giving examples of 'observations' that could 'falsify' Newton's entire system. For example, "assume that the velocities of some planets were to decrease rather than increase when approaching their perihelion ... or assume the orbit of some planet were to be approximately rectangular."[120] In such cases, would not the inverse-square law in conjunction with Newton's dynamics be 'falsified,' even by Newtonian?

Popper further argues that Lakatos' story is not generalizable and turns "a series of brilliant scientific successes into a series of trivialities."[121] Popper completely dismisses Lakatos' "falsification" of the demarcation criterion, but gives Lakatos an acceptable potential falsifier. "I shall give up my theory if Professor Lakatos succeeds in showing that Newton's theory is no more falsifiable by 'observable states of affairs' than is Freud's."[122]

As it turns out, Lakatos' entire discussion of the "falsification" of Popper's demarcation criterion is completely irrelevant. For Lakatos admits that he does not hold the proposed meta-criterion which is employed in the alleged "falsification," and Popper maintains that his demarcation criterion is irrefutable.[123] Popper holds the view that philosophical theories are not 'falsifiable,' and both falsificationism and the associated demarcation criterion are philosophical positions. To Popper, such theories are neither unimportant nor meaningless. They are simply not empirical, but 'metaphysical' (recall the discussion of philosophical theories in Chapter 2). One might see Popper's position as a defensive stratagem, but it is an unavoidable consequence of philosophical inquiry.

In Chapter 2 we presented a discussion of possible tests for a theory of scientific knowledge. But it is necessary to distinguish this metatheory (a theory about theories) from its associated methodology or philosophy. A metatheory may be "tested," but only after the necessary qualifications are made and sufficient care is taken. And, most importantly, such a theory is "testable" only if it is interpreted as an explanation of how science actually grows and not as a methodology.

It is unclear how Popper regards his own theory of knowledge. At times he seems willing to consider possible tests. In other places, his theory is methodology, thus irrefutable. But in all cases, Popper is aware of the unique nature of such a metatheory. Popper's ambiguity is best illustrated in a reply to Medawar.

> The question is: "How should we proceed if we wish to contribute to the growth of scientific knowledge?" And the answer is: "You cannot do better than proceed by the critical method of trial (conjecture) and the elimination of error, by trying to test, or refute, your conjectures ...." I do not think that the theory of knowledge, or of scientific knowledge, is in its turn an empirical science, and testable or falsifiable in the sense in which I hold empirical theories are testable. Yet I can conceive of empirical circumstances which would lead me to revise my theory of science.[124]

Thus, following Popper, it can be reasonable to "test" a theory of knowledge, but not in the same manner in which empirical science 'tests' its 'theories.' Then, only if we are careful account for the special nature of these metatheories.

Lakatos, however, "tests" theories of knowledge in no such delicate manner. He asserts that the opinions of the scientific elite may be used as the relevant "facts."[125] How are we to identify the elite? How do we deal with inconsistent appraisals? What prior principle asserts that such appraisals constitute the growth of knowledge? And, which appraisals should we use to "test" Popper's theory? To all the questions Lakatos is mute. The reason for this silence is that Lakatos is not concerned with these issues. He is merely building towards a more sophisticated argument.[126]

The next gambit in Lakatos' discussion is to "amend" Popper's demarcation criterion. Instead of Popper's requirement of 'falsifiability,' Lakatos substitutes his entire methodology of scientific research programmes (MSRP) as the demarcation of science.[127] But MSRP does not address the same question, nor does it provide an answer to Popper's problem: "What distinguishes scientific knowledge from other types of knowledge claims?" The function of MSRP is to appraise a historical series of scientific theories and conclude "progressive" or "degenerating."[128] Yet, MSRP gives no answer to the only question that Popper's demarcation criterion addresses: What special characteristic does scientific theory have? Lakatos is replacing a demarcation criterion with a sophisticated theory of 'corroboration.' Lakatos' MSRP may well be of interest in itself, but it is unrelated to the problems associated with demarcation criteria.[129]

At this point in the discussion, it becomes clear that there has been a "problem-shift."[130] Lakatos is no longer talking about demarcation at all, but is attempting to demonstrate the superiority of his MSRP over falsificationism.

Lakatos quickly admits that MSRP would be "falsified" by the same procedure previously used to "falsify" Popper's demarcation criterion.[131] But instead of dismissing his previous argument, Lakatos chooses to amend the meta-criterion. "If we abandon naive falsificationism in <u>method</u>, why stick to it in <u>meta-method</u>? We can easily have a second-order methodology of scientific research programmes."[132] Thus, the criterion we are to use to "judge" MSRP is meta-MSRP.

A few comments about Lakatos' logic are required. In the above quote, Lakatos implies that we have some reason to "abandon naive falsification in method." Yet, as mentioned in the previous paragraph, if we have "reason" to abandon "naive falsificationism," then we have an equivalent reason to abandon MSRP. Thus, "why stick to MSRP in meta-method?" Is Lakatos merely demonstrating to us how easy it is to assume a scientific methodology and a criterion to judge it? In any case, the chain of reasoning used to get to the assumptions of MSRP and meta-MSRP is, at best, circular and does not serve as justifications, in any sense, for these "assumptions."

The relation of Lakatos' method and his meta-method also provides evidence for the superficial nature of his discussion and the circularity of his logic. Here, we must appraise Lakatos' method of appraising.[133] First, we must ask whether it is wise to judge a position from its own meta-perspective. For this is precisely how Lakatos judges his MSRP. The brief answer is that there is nothing lost by such a procedure if we are careful not to get lost in some implied paradox (recall the discussion of paradoxical self-reference in the Prologue). Furthermore, if both the original position and its meta-level reflection are compatible, then we might say that our position has some desirable consistency property. Yet, such a property could in no way serve as independent justification or as a 'rational demonstration' of the original position. The raising of some position to its own meta-level can, at best, provide tautological support or be circular reasoning.

To see this, we need only ask where such a self-referential meta-analysis leads.

Let,

P_0 represent our original position, and

P_1, the meta-level translation of P_0, and

P_2, the $(meta)^2$-level translation of P_1, and ...

If our goal is to "establish" P_0, we might be tempted to begin with P_1.

Given P_1, it may be possible to derive P_0 by using some extralogical means (logic alone does not have "transmeta" implications). Thus, if we are lucky, we may obtain something similar to the theorem, $P_1 \rightarrow P_0$.

Naturally, there would arise a question: "Is our premise, P_1, any more firmly established than our conclusion, P_0?" Unless we can provide some rational ground for the affirmative answer, our theorem is for naught. The only legitimate reason for a preference towards P_1 must concern something outside the system defined by P_0 and P_1 alone.

Yet, the defender of P_0 need not stop here. He may assume P_2 and search for a "proof" of the theorem, $P_2 \rightarrow P_1$, etc. Such a process leads to an infinite regress and establishes nothing. The increased complexity of one's argument does not necessarily lead to the increased substantiation of one's position. Support can be gained only by "independent assumptions," that is, by something other than "meta-copies."

Lakatos' method of appraising methodologies contains this problem of "self-referential meta-analysis." He is judging his MSRP from the perspective of meta-MSRP, and he uses this procedure to imply that MSRP is better than falsificationism. Such a procedure can provide no 'rational preference' for one position over another. His procedure necessarily leads to an infinite regress into "metaland." Lakatos is even aware of this problem, but casually dismisses it.[134]

Yet there is no justification for Lakatos' method of debate, even if we use Lakatos' own criterion, for then he would need to show that the assumption of meta-MSRP is a "progressive problem-shift" itself. Not only does he not argue for the progressive nature of meta-MSRP, he refuses to consider alternatives to his meta-MSRP.[135]

Certainly, Lakatos knows that his method of discussing methodologies cannot establish anything, and that any attempt to do so would lead to an infinite regress. Being a student of Popper's philosophy, he should know that the same problem resides within falsificationism. If one attempts to establish the 'truth' of a 'theory,' the non-zero probability of a 'theory,' or the "concreteness" of the 'facts,' he must get caught in an infinite loop (recall the characterization of Popper's science in Chapter 2). This, however, poses no problem to Popper, for he wishes not to establish anything in his science. Thus, it would be reasonable to assume that Lakatos is aware of this problem, since Popper has made it so explicit.

But the unwary reader is likely to miss this point. For Lakatos appears to be rationally arguing for his MSRP, and he <u>sounds</u> like he is attempting to establish its superiority. "I hope my modification of Popper's logic of discovery will be seen, in turn - on the criterion I specified - as a step forward."[136] Does this not sound like a claim for the conditional (by his criterion) superiority of Lakatos' methodology? Or, is it merely a plea for the "scientific elite" to appraise MSRP better than falsificationism?

In any case, we must now inquire as to whether Lakatos' provides any rational grounds for his claims of the progressive nature of MSRP, assuming that his method of appraising methodologies, meta-MSRP is unproblematic. Or, if we accept Lakatos' perspective, will we prefer MSRP over falsificationism? How do we judge? According to Lakatos' meta-method, "We reject a rationality theory only for a better one, for one which, in this quasi-empirical sense, represents a <u>progressive</u> <u>shift</u> .... A good rationality theory must anticipate further basic value judgments unexpected in the light of their predecessors or even lead to the revision of previously held basic value judgments."[137] What we ask our rationality theory (or theory of scientific knowledge) to do is to explain the "basic value judgments" of scientists.

Lakatos gives "basic value judgments" the same role as Popper gives to 'basic statements' in science. "Basic value judgments" are the "facts" that our theory of science need explain if it is to be adequate. But, any such "basic value judgments" are inherently more problematic than 'basic statements.' 'Basic statements' concern physical positions or readings of scientific instruments. If anyone disagrees with some alleged 'basic statement,' he has recourse to replicate or devise his own experiment. This <u>potential</u> for unlimited 'testing' is what gives 'basic statements' and 'facts' their relative substance. However, following Popper, that substance is more like a swamp than anything concrete.

What, specifically, are these "basic value judgments"? They are generally agreed valuations of scientists concerning the quality of single scientific achievements. "While there has been little agreement concerning a <u>universal</u> criterion of scientific character of theories, there has been considerable agreement over the last two centuries concerning <u>single</u> achievements. While there has been no <u>general</u> agreement concerning a theory of scientific rationality, there has been considerable agreement concerning whether a particular single step in the game was scientific or crankish, or whether a particular gambit was played correctly or not."[138]

Certainly the wisdom of hindsight allows scientists to see a few of the larger steps in science as progressive and with all but universal agreement. But is it fair to consider judgments of "crankish" equivalent to the readings of scientific instruments?

I can see at least three problems associated with "basic value judgments." They are: the problem of testing, the problem of unambiguous agreement, and the problem of myopia. What if scientists disagree? What recourse is available? There is no way to replicate a judgment; thus, there is no test for "basic value judgments." I do not wish to imply, as some do, that the only means available to resolve a conflict of values is to fight it out.[139] 'Rational discourse' can go a long way toward reaching agreement on questions of value, but it is naive to believe that such general agreement is common or easy.

The question then becomes: How do we "test" "basic value judgments" when there is not "considerable" agreement? The closest thing to such a "test" has been scientific conventions, colloquia, and societies. These devices have been responsible for greater uniformity of scientific values, and they have "tested" a number of individual value judgments. But, can we say that "considerable" agreement has resulted? And, is not the history of science (even in the last two centuries) full of cases where some single step was declared "crankish" by the "scientific community" only to be recanted? Thus, the problems of unambiguous agreement and myopia of scientists will always remain.

Thus, the best "test" of "basic value judgments" is the test of time. Only after science has progressed long past the "particular gambit" is the scientific community in a position to judge it. There are many reasons why time improves our ability to judge, not the least of which, as some suggest, is that the most ardent and personally involved critics and defenders may no longer be around.

However, as we all have experienced, time is not without its costs. Along with the increase of wisdom that a longer hindsight engenders comes a loss in our ability to see the smaller details of scientific development. After a lifetime, we may have "considerable" agreement about the progressive nature of Einstein's special theory of relativity. But, for example, would we find such agreement concerning Maxwell's mechanical ether theories? Thus, it seems that judgment will always be associated with some myopia.

It is for these reasons that in our proposed "test" (recall Chapter 2) of Popper's theory of scientific knowledge, CR, care is taken before any judgment is elevated to "fact." Though we may reasonably determine a few "basic value judgments," their insulation from testing obliges us to increase our prudence and to reduce our scope of "observation." The historian will be disappointed by the paucity of data that may be used in debating theories of scientific knowledge. Yet one whose goal is sound methodology might retort, "Are we not more likely to error if our theory of knowledge explains too much of science rather than too little?"

Lakatos, on the contrary, makes no differentiation between "basic value judgments" and 'basic statements.' Referring to Popper's biblical metaphor, Lakatos asserts: "But even if there were agreement about 'basic' statements, if there were no agreement about how to appraise scientific achievement relative to this 'empirical basis,' would not the soaring edifices of science equally soon lie in ruins? No doubt it would."[140] To unravel Lakatos' twist of Popper's metaphor, we twist another. If the bold structure of scientific theories rises, as it were, above the swamp of 'basic statements,' then the bold structure of rationality theories hangs, as it were, below the clouds of "basic value judgments."[141] Friedman, as we shall see, also gives "judgment" an essential role in the methodology of positive economics.

Since Lakatos appraises "basic value judgments" as equivalent to 'basic statements,' he has no difficulty in appraising his MSRP as a "progressive problem-shift" over Popper's falsificationism. The only evidential support presented by Lakatos concerns two stories from the history of physics. "For instance, on Popper's theory, it becomes _irrational_ to retain and further elaborate Newton's gravitational theory after the discovery of Mercury's anomalous perihelion; or it becomes _irrational_ to develop Bohr's old quantum theory based on inconsistent foundations. From my point of view, these were perfectly _rational_ developments."[142] From this evidence, we are led to infer that Lakatos' MSRP is "better" than Popper's falsificationism, at least from the perspective of meta-MSRP. According to the logic of meta-MSRP, since MSRP can explain these implicit "basic value judgments" (Lakatos does not tell us what they are in these cases from the history of science) and falsificationism cannot, we must conclude that MSRP represents a "progressive problem-shift." This, perhaps, is Lakatos' opinion. Yet, even granted the dubious assumption of meta-MSRP, Lakatos' conclusion is wrong, and his use of history is misguided. As we have discussed, falsificationism is a methodology and, as such, indifferent to the history of science. In addition, Popper's position when interpreted of a theory of actual scientific growth is as consistent with historical physics as is MSRP, though perhaps less colorful. The only reason that Lakatos argues otherwise is due to his conflation of the 'falsifications' of a theory with its rejection from the scientific community. Popper does not combine these separate methodological issues. In fact, Popper's own research programme may be seen as a progressive explanation of the distinction of these concepts.[143]

Take, for example, Lakatos' Newtonian reference. In this case, neither Popper's methodology nor his theory of science would suggest that the further elaboration of Newtonian physics is "irrational." Here, the problem was that of a persistent anomaly. The observed orbit of Mercury did not exactly correspond to what Newton's theory in conjunction with a variety of relevant hypotheses concerning the positions and masses of bodies in the solar system would "predict." Thus, somewhere there is an inconsistency. As a resolution, we could "blame" our 'observations' of the 'basic statements' and/or the 'initial conditions,' or we might "blame" the conjunction of Newton's theory plus the necessary auxiliary hypotheses.[144] If our 'observations' are 'well-corroborated,' as with Mercury, there still remains an entire system of theories, any one of which could be responsible for the inconsistency. Falsificationism rarely tells us which hypothesis should be changed or replaced. It only says that something must be changed if we wish to eliminate error. Popper's falsificationism never tells us that we must reject or abandon some theory or theoretical system. It only says that we must abandon our belief that the entire system in question is 'true.' Thus, falsificationism in no way suggests that the anomalous perihelion of Mercury causes us to rationally reject any of Newton's laws. In fact, falsificationism allows many potential elaborations of Newtonian physics that could both explain this phenomenon of Mercury and provide increases to knowledge.

To further illustrate this nature of falsificationism, we might recall a similar example from the history of science - the discovery of

Neptune. The initial problem-situation for this historical "observation" of science is the same as for Mercury. There was an anomalous orbit for Uranus. Here again, we <u>might</u> reject Newton's theory, for something was wrong. Yet falsificationism does not require us to do so. Instead, Adams and Leverrir postulated another planet, Neptune, which in conjunction with the Newtonian system could explain the orbit of Uranus. If this was the end to the story, falsificationism would correctly proclaim that the Neptune hypothesis was <u>ad</u> <u>hoc</u>. For a factual inconsistency would only be rationalized away by reference to some unknown. But the Neptune hypothesis has independently (from the 'observations' of Uranus' orbit) 'testable' consequences (i.e., reflection of electro-magnetic radiation), and these consequences were subsequently 'corroborated' when Neptune was actually observed. Thus, a potential falsification of some theory may be turned into a dramatic 'corroboration.' And, without one you cannot have the other. Falsificationism only emphasizes the value of factual inconsistencies and perhaps hints at a resolution to such problems.

Popper's theory of science, CR, simply asserts that we will learn from such potential 'falsifications' by an adjustment of our theoretical system which renews consistency. This adjustment will also result in a theoretical system with greater 'empirical content,' and eventually some of the excess 'empirical content' will be 'corroborated.' CR correctly "predicts" how both cases of anomalous orbital behavior, Mercury and Uranus, were to be resolved. With the addition of Neptune into the Newtonian system, the "anomalous behavior" is resolved, new phenomena are "predicted" and these have been repeatedly 'corroborated' (for example, Pluto). The same is the case with Mercury. General relativity theory explains Mercury's anomalous perihelion and has considerable excess 'empirical content,' some of which has been 'corroborated.'

Ironically, Lakatos chooses this Mercuric case, for the historical resolution is perfectly consistent with falsificationism, while MSRP does not fare as well. Mercury's anomalous behavior was not explained by that same research programme that discovered it. "Rational" or not, it was the distinct research programme of Einstein that eventually explained this anomaly of Newton's theory. Thus, MSRP is less well illustrated by the case of Mercury than is Popper's theory of scientific growth.

In any case, the implication that falsificationism would proclaim it "irrational" to retain Newton's theory after establishing Mercury's strange behavior is simply "wrong." CR permits many possible progressive elaborations of a theory in such cases. Beside the examples just provided, consider a theory that uses the interrelation of Mercury's gravitational field and the sun's magnetic field to resolve our orbital problem. If such a resolution within the Newtonian system were to be independently and experimentally 'corroborated,' it might produce an advancement over Einstein's general theory of relativity. For just such a unified field theory is what Einstein considered a generalization of his general relativity.

The point is that Lakatos incorrectly applies Popper's theory to history in order to demonstrate the advantage of MSRP. If a fair "test" is conducted, along the lines outlined in Chapter 2, it would result in a tie. And this conclusion would hold even if Lakatos' meta-MSRP is granted.

The reason is simple. Both theories of scientific knowledge are consistent with the relevant "facts" of the historical growth of scientific knowledge. Sufficient examples of the historical consistency of CR have been provided, and only an assertion can "justify" the nonexistence of counter-examples. Thus, we rest our case. Since only the more unproblematic "facts" of the growth of knowledge can be used to "test" such theories of science, the identification of counter-examples should not be difficult, if they exist. Yet neither Lakatos nor Kuhn succeeds in providing any reasonable, historical examples of how Popper's CR fails. Given that both are historians, this fact alone should "establish" Popper's theory of science as well as any rival. We conclude that Lakatos has not succeeded in demonstrating any <u>reason</u> to prefer MSRP over either Popper's methodology or his theory of science. Unless, of course, one prefers a more dogmatic methodology or a more colorful vocabulary.

Returning to the issue which generates the current discussion, the criticism of the demarcation criterion, we can now make even stronger claims. Lakatos' entire discussion is replete with logically erroneous demonstrations and misrepresentations of Popper's position. In fact, Lakatos fails to provide any reason for each of the necessary steps in his criticism of Popper's demarcation criterion.

We deny Lakatos' claims. He has not "falsified" Popper's demarcation criterion, nor has he succeeded in providing any good reason for considering it deficient. In fact, Lakatos' attempt to "shoot down" Popper's demarcation can only "backfire." His strong argument that any theory (in particular, Newton's) be rendered 'irrefutable' only underscores the need for Popper's demarcation criterion. This is a need of which Popper has always been aware and is the issue that is responsible for Popper's proposal of the demarcation criterion as a methodological rule or convention. Thus, Lakatos succeeds only in sharpening our wits on the finer points of falsificationism and by reminding us not to be naive falsificationists.

We might end our discussion of Lakatos here, but if we did we would fail to understand the rationale of Lakatos' philosophy and would also miss a few important lessons. In order to broaden our understanding we must ask: Where does Lakatos wish to lead us? Why does he attack Popper's demarcation criterion so vigorously?

Lakatos summarizes his criticism of "Popper on Demarcation" by two points:

> _First_, I advocate a primarily quasi-empirical approach instead of law-giving to science. I do not lay down general rules of the game _a priori_, so that, if history or science turns out to violate the rules, I would have to call the business of science to start anew .... Methodological progress lags behind instinctive scientific verdicts in the sense that the main problem is to find, if possible, a theory of scientific rationality which would explain actual scientific rationality rather than to bring legislative interference by the philosophy of science to the most advanced sciences. _Secondly_, I hold that philosophy is more a guide to the historian of science than the scientist .... I find it difficult fully to share Popper's optimism that a better philosophy will be of _considerable_ help to the scientist; although no doubt, it may - and Popper's philosophy has helped ....[145]

Aside from Lakatos' colorful prose and his misguiding implications, he is merely saying that:

(1) He prefers to apply the philosophy of science to actual scientific judgments rather than to methodology.

(2) He prefers to view the philosophy of science as a framework for the history of science rather than as a methodology.

Lakatos is certainly free to hold such a preference. Just as we are free to invert his rankings. And, we can additionally assert that Lakatos fails to provide us with any reason for his choice.

It appears we have full circle in this chapter. We began with Kuhn's sociological and historical view of epistemology, and Lakatos brings us back to a judgmental and historical perspective of epistemology. To the extent that we have provided reasons for not retrogressing to Kuhn's perspective, we have likewise 'rationally demonstrated' the shortness of Lakatos' view.

Before we can continue our discussion, we must again correct Lakatos' poor scholarship. Firstly, it is absurd to imply that Popper would "call the business of science to start anew," "if history of science turns out to violate (his) rules." That Lakatos means to imply this cannot be denied by context of this quotation; yet that it is "wrong" is equally undeniable. Popper has always recognized that progress, even in his sense of the term, may occur as the consequence of many things, including myth, metaphysics, luck, or "bad" methodology.[146] Knowledge is knowledge; its source is irrelevant. No one else has ever made this point so clear.[147] Popper's logic merely tells us how to identify knowledge, and his methodology only provides us with helpful advice for the growth of knowledge. He has never "legislated" "universal laws" to the scientific community. And why would Popper demand that the actual activity of science obey his philosophy when he frankly admits that his philosophy is not obliged to conform to this actual activity?

Secondly, we find another erroneous citation and misrepresentation of Popper's statements. "Popper's optimism that a better philosophy of science will be of considerable help to the scientist" is greatly exaggerated. Here, Lakatos references The Logic of Scientific Discovery, p. 19, where we find that the most optimistic statement is: "I am inclined to say more ... the theory of knowledge was inspired by the hope that it would enable us not to know more about knowledge, but also to contribute to the advance of knowledge - of scientific knowledge, that is."[148] Or, in other words, Popper claims that philosophy has developed from the hope that it might contribute to scientific knowledge. This hardly constitutes "considerable help to the scientists." In fact, the implication of "help" is just the reverse of Lakatos' interpretation. That is, to paraphrase Popper, the hope of contributing to science has been of "considerable help" to the philosophers. The irony of Lakatos' misrepresentation is that Lakatos further asserts what Popper does not claim. Namely, that "Popper's philosophy has helped." Thus, Popper's hope has been fulfilled.

What does Lakatos contribute to Popper's philosophy? Lakatos claims, "Finally, let me say that although I do think that my criticism of Popper's solution of the problem of demarcation is a genuine further development in the very tradition he himself set for the 'logic of scientific discovery' I do not think that my 'criticism' of Popper's 'solution' of the problem of induction is more than an attempt to make explicit the full implication of his own theory of verisimilitude for the problem of induction ...."[149] Thus, Lakatos believes that his "amended" demarcation criterion, MSRP, is a "genuine development" to the philosophy of science. With this belief we cannot concur. In our previous discussion we showed that at each step the logic of Lakatos' criticism represents a misquotation, a misrepresentation, or a logical fallacy, and that Lakatos' MSRP cannot even address the same problem that Popper's demarcation criterion solves. Thus, how can Lakatos' criticism be considered a "genuine development"?

Yet, the fact that Lakatos believes that he has improved the philosophical issue of demarcation leads us to wonder why Lakatos is so opposed to Popper's demarcation criterion. Does it pose problems for Lakatos' own methodology - MSRP? Does Lakatos believe that it has negative sociological implications? It appears that the answer to both questions is "yes."

In Lakatos' introduction to Methodology of Scientific Research Programmes is a "broadcast" of Lakatos' concern for any such demarcation.

> The problem of demarcation between science and pseudo-science has grave political implications also for the institutionalization of criticism .... The Central Committees of the Soviet Communist Party in 1949 declared Mendelian genetics pseudoscientific and had its advocates, like Academician Vavilov, killed in concentration camps .... The new liberal Establishment of the West also exercises the right to deny freedom of speech to what it regards as pseudo-science, as we have seen in the case of the debate concerning race and intelligence. All these judgments were inevitably based on some sort of demarcation criterion. This is why the problem of demarcation between science and pesudoscience is not a pseudo-problem of armchair philosophers: it has grave ethical and political implications.[150]

We may well join in Lakatos' lament for these persecuted "pseudo-scientists" without, at the same time, "blaming" any demarcation criterion, and particularly not Popper's. The methodological demarcation of science from science has nothing to do with political persecution. In fact, the historical cases that Lakatos cites did not employ any such methodological demarcation criterion. The methodology of science has no political or ethical implication. Unless, of course, you choose to so implicate some methodology by adding, explicitly or implicitly, ethical assertions. Or, if "the powers that be" wish to abuse methodology. In this latter case, "these powers" could as well abuse the absence of any such a criterion or, instead, some scientific theory. Nothing is immune from abuse.

It is erroneous to associate Popper with these cases of political persecution, irrespective of how tangential. Popper not only contributes to the philosophy and methodology of science, probability theory, and quantum mechanics, but also to political philosophy. In his works The Open Society and Its Enemies and The Poverty of Historicism are brilliant criticisms of authoritarian philosophies and totalitarian politics. Popper leads the way in the philosophical refutation of Marxism, whether conceived as philosophy, "science" or government (recall from Chapter 2 that it was Popper's recognition of the "metaphysical" nature of Marxism that inspired his demarcation criterion). In The Open Society we are shown how Popper's philosophy of science can help induce an open and free society and how the difficult issue of social values may be addressed by Popper's critical methods. Popper's critical philosophy is the antithesis of the persecution of scientists or any prohibition of dissent.

In addition, Popper is always clear in the separation of values from the issue of demarcation. He admits that he is a "metaphysician," or "pseudoscientist" as Lakatos prefers, by his own criterion. Contrary to the linguistic philosophers, he states that philosophy and other nonsciences can contribute to knowledge and can guide science.[151] One can only wish that Lakatos would be as careful in distinguishing his methodology from implicit and explicit value judgments. Need we say "basic value judgments"? Thus, we are forced to pronounce Popper's demarcation criterion innocent of any "grave political implications."

Why does Lakatos make such an inappropriate connection? Perhaps the answer lies in the difficulty that Popper's demarcation criterion imposes upon MSRP and its application. Paradoxically, Lakatos believes that Popper's demarcation criterion, DC, is a threat to his methodology, MSRP. This belief is expressed in two ways. First, Lakatos treats DC and MSRP interchangeably. Lakatos uses MSRP as an "amended" demarcation criterion (recall the former discussion). Secondly, Lakatos applies his "methodology of _scientific_ research programmes" to science and non-science alike, at least by Popper's demarcation criterion. "The methodology of research programmes may thus be applied to normative knowledge, including even ethics and aesthetics ..."[152] For such applications only the name changes (for example, "scientific" can be deleted or replaced by "histiographical"). The methodology remains. Also, Lakatos' ease in raising his MSRP to its own metalevel, the "amended metacriterion," clearly demonstrates that Lakatos wishes to appraise more than just science, by anyone's definition. Thus, Lakatos' methodology is indifferent to Popper's DC or, if you will, violates DC. Yet, this is paradoxical, or at least inconsistent, for MSRP requires Popper's DC.

While it is true that Lakatos need not agree with Popper on the definition of science, it turns out that MSRP is defined only relative to DC. Recall our discussion of Lakatos' terms "empirically progressive," "progressive" and "degenerating." The whole point to MSRP is to appraise research programmes and their associated "problem-shifts" in these terms. Yet, each of these terms is defined only if the objects of appraisals, research programmes, are 'falsifiable.' For 'irrefutable' theories these terms are meaningless. To see this requires only one intermediate step.

Lakatos defines all these lables in reference to Popper's notions of 'excess empirical content' and 'corroboration.'[153] And, he accepts Popper's definition of 'empirical content' and 'corroboration'; he only believes that Popper's terms are not easy to apply.[154] What is 'empirical content'? It is the set of "potential falsifiers" to a 'theory,' statement, or system of theories. How can a theory have "potential falsifiers" if it is not 'falsifiable'? It cannot. As we discussed in Chapter 2, the notions of 'empirical content' and 'corroboration' are totally dependent upon 'falsifiability' and thus the satisfaction of Popper's DC. Any combination of 'irrefutable' statements has zero 'empirical content.' Thus, how could we talk about 'corroboration' or growth of zero 'empirical content'?

Lakatos cannot appraise statements, or systems of theories, or research programmes as "progressive" or "degenerating" unless DC is satisfied. DC is a binding constraint to Lakatos' appraisals. Yet, Lakatos insists upon applications of MSRP outside the field of 'falsifiable theories.' He applies his methodology outside its range of application, or Lakatos demands that his methodology of science be unfeasible by his assertion that research programmes are 'irrefutable.'

Lakatos can only appraise series of problem-shifts as "progressive" or "degenerating" if they are 'falsifiable.' Otherwise, Lakatos is only asserting his opinion. Perhaps this is why Lakatos equates "basic value judgments" with 'basic statements'?

In any case, Lakatos is committing a logical fallacy. His position is inconsistent. For clarity, let us review. First, Lakatos' methodology requires the satisfaction of Popper's demarcation criterion. Or,

$$MSRP \longrightarrow DC$$

Yet, Lakatos asserts that scientific theories are not 'falsifiable.'[155] Or,

$$\sim DC$$
$$\therefore \quad \sim MSRP$$

Or, the above assertion carries the logical implication that MSRP is invalid or "false." Yet, Lakatos wishes to maintain both his assertion and his methodology. Thus, Lakatos is inconsistent.

And this is only the simplest expression of Lakatos' "hopelessly knotted hierarchy" of philosophical positions. Imagine how one must represent Lakatos' replacement of DC with MSRP or his raising of MSRP to its own metalevel. Yet, only a simple logical exercise is sufficient to 'rationally refute' Lakatos' methodology.

The inconsistency of MSRP with the denial of 'falsifiability' and Lakatos' inappropriate applications of MSRP to 'irrefutable' programmes is sufficient for us to conclude that Lakatos' methodology is unsound. We might attempt to "patch up" Lakatos' logic by refusing to apply it to 'irrefutable' systems and by adding back Popper's demarcation criterion. This would also involve the rejection of Lakatos' "hard core" and his associated conventionalism. Yet, Popper's falsificationism and Lakatos' "positive heuristics" would remain. The resulting consistent methodology would have all the advantages of Popper's (for it is just that), and it could create more colorful histories by the appropriate specifications of history's "positive heuristics." Such an "amended" MSRP would be fine. It would give us sound methodology and, at the same time, be more satisfying to the historian of science.

Thus, we have no quarrel with Lakatos' methodology of scientific research programmes as long as Popper's demarcation criterion is added and consistency maintained. Yet, Lakatos would not favor such an "amendment," for we have shown how strongly he argues against demarcation. Still, we are left wondering why.

Perhaps Lakatos realizes that Popper's DC is damning to his MSRP and its goal of providing a framework for the history of science. Yet, it is difficult to imagine that the logician, Lakatos, could advocate an inconsistent philosophical position.[156] But, on the other hand, Lakatos may have made the common philosophical mistake of becoming locked in his own framework. Through Lakatos' "meta-spectacles" inconsistency may not be so tainted. Lakatos would then be just another Bohr who was able to obtain "progressiveness" from a research programme based upon inconsistent foundations.[157]

Before summarizing this difficult criticism of Lakatos' criticism, we should allow Popper to respond to the issue of demarcation. "I am disturbed to find the argument which appears to be crucial for his (Lakatos') criticism of my views on demarcation must, in my opinion, be rejected as totally unsound, that among his criticisms he raises points which I would not have expected from one who is well acquainted with my work; and that his examination of my views seems to have left him - and, unfortunately, large numbers of people who have read his papers - with an interpretation of my theory of falsifiability that makes nonsense of all my views."[158] [Parenthesis added.] Although Popper does not quite specify which crucial step in Lakatos' criticism is unsound, we believe that Popper is also aware of the inconsistency between Lakatos' position on methodology and demarcation. For it is this issue that makes nonsense of all Popper's and Lakatos' views.

To summarize, let us first consider Lakatos' alleged "falsification" of Popper's demarcation criterion and his associated appraisal of falsificationism.

(1) Methodologies and their rules are not 'falsifiable.' They are philosophical positions and as such are not 'scientific' - by Popper's demarcation criterion.

(2) Thus, the attempt to "falsify" a demarcation criterion assumes its own conclusion. You have to assume that the demarcation criterion does not hold, at least in some sense, in order to "falsify" it.

(3) Lakatos misquotes and misrepresents Popper's demarcation criterion.

(4) Given Lakatos' "test," he fails to "falsify" Popper's alleged demarcation criterion. His argument rests upon the "fact" that Newton's theory is not 'falsifiable,' but it is.

(5) Given Lakatos' "logic," his own theory, MSRP, is "falsified," as admitted by Lakatos.

(6) Lakatos erroneously implies that there is some reason for using his MSRP as a meta-method to appraise methodologies.

(7) Lakatos' meta-method is, at best, circular and is biased against all other methodologies. "The deck is stacked."

(8) Lakatos' evidence for the "progressive" nature of his MSRP is wrong. Lakatos misapplies and misinterprets Popper's theory of scientific knowledge. In particular, Lakatos must incorrectly equate 'falsification' with rejection in order to establish a preference for MSRP.

(9) When fairly analyzed, both theories of scientific knowledge are "corroborated." Both can explain the major developments in science.

(10) Lakatos is merely appraising methodologies in the light of his own method appraisal. This leads nowhere. At best, Lakatos can only establish that, "given his perspective, his perspective is given."

(11) Lakatos' position is inconsistent. He <u>cannot</u> both deny Popper's demarcation criterion and at the same time maintain his MSRP.

Clearly, from the force of this criticism, we cannot accept Lakatos' criticism. Yet, Lakatos offers many other criticisms and "clarifications" of Popper's philosophy. Time demands that we simply assert that the same can be said for the rest of Lakatos' "contributions" to falsificationism. His exposition is overly complex, technical, confusing, and misleading. For example, he defines three different Poppers, $Popper_0$, $Popper_1$, $Popper_2$, four types of falsificationism, and triplets of ad hocness and acceptability. Such excessive terminology only creates confusion and misleads the reader concerning the "real Popper." Or as Popper expresses it, "Professor Lakatos has, in the course of his discussion of my work, introduced a large number of complications, distinctions, and epicycles; technical terms abound, and everything Professor Lakatos touches seems to sprout numbered subdivisions. All this regretably, for it will make it difficult for people to comprehend and to criticize what were originally some simple ideas of mine."[159]

Popper denies and criticizes many of Lakatos' major points. Symmetry requires us to present Popper's general impression of Lakatos' elaborations and interpretations of Popper's philosophy.

> Professor Lakatos has, nevertheless, misunderstood my theory of science; and that the series of long papers in which, in recent years, he has tried to act as a guide to my writings and history of my ideas is, I am sorry to say, unreliable and misleading .... In reading his contribution I was again and again surprised by the views which he attributes to me and by the nature of the quotation and references which he uses to back up his interpretations. Passages, and extracts from passages, are frequently taken out of context; minor remarks, sometimes of the nature of asides, and mere allusions to some view, given when I am discussing something else, are taken as if they were representing my position on the question at hand; and my major discussions are frequently ignored. Moreover, his quotations and references are not always correct and are often highly

> misleading .... In view of all this, I suppose that only somebody who knows my work well, or who takes the trouble to check Professor Lakatos' references carefully, will be able to appreciate fully my objections to his contribution.[160]

As one who has carefully checked Professor Lakatos' references, we can appreciate Popper's objections. And we would go further than Popper's guarded criticism. If one has yet to read Lakatos, he should not. For the utility of a few minor clarifications of falsificationism, one must pay the price of many misinterpretations of falsificationism, complex and belabored pseudo-arguments, and, in general, high leveled intellectual chaos. The only way to come away from Lakatos ahead is to read and re-read Lakatos, checking <u>all</u> his references and the full works cited. Otherwise, one is misguided. Yet, almost equal wisdom is gained by just reading all of Popper, and this is a much easier task.

Thus, we may profitably ignore all of Lakatos' discussions of Popper. Generalizing to Lakatos' methodology, we find little contribution. If, and only if, we add Popper's demarcation criterion to MSRP will we find a tenable methodological position. Then, we are left with Popper's falsificationism and Lakatos' "positive heuristics." The latter provides increased ability to describe research programmes and to 'internalize' history. Yet, "positive heuristics" can add little, if anything, to the philosophy or logic of science. Nonetheless, if wisely chosen, these heuristics can provide pragmatic advice to the practicing scientist. Only in this manner can MSRP contribute to the history and methodology of science.

Still, we must keep in mind that this amended position is not Lakatos'. In order to achieve a tenable methodological position, we rejected several of Lakatos' main points. First, we <u>cannot</u> deny Popper's demarcation criterion, as Lakatos does, or inconsistency results. This adjustment forces us to reject Lakatos' conception of an 'irrefutable' "hard core" of scientific theories. Along with the rejection of the "hard core," we obtain the positive external benefit of reducing the dogmatism, or the anticritical attitude, that this "hard core" engenders. Finally, it is important not to follow Lakatos' examples in applying our "amended" MSRP. For he has no hesitation in applying it to all types of 'irrefutable' ideas. By adding Popper's DC, the limited applicability of MSRP to only 'falsifiable' theories is made explicit. Of course, one might still apply MSRP outside its field of application by using personal judgment as appraisals. Yet, if any application of MSRP wishes to claim any epistemological support, it must be limited to the appraisal of 'falsifiable' theories (or series of systems of theories, if you prefer).

3.3 Some History of the Philosophy of Science

Contemporary philosophy of science is not independent of its tradition. Its current position is largely a result of its history. To understand ideas, knowledge of their problem-situation and their tentative solutions is necessary. Such information may be gleaned from the "history" of their development. The three philosophers of science discussed here developed their positions as the result of the philosophical tradition, at least in part. Thus, we need to broaden our perspective if we are fully to understand current philosophy of science.

Since the philosophical tradition is so voluminous, we need some selection mechanism and some framework in which to describe its characteristics. Lakatos provides us with just such a framework. Our previous criticism did not concern the inadequacy of Lakatos' method of writing history. Only we could not accept his claims that his method is a sound foundation for the methodology of science. We use his framework only to briefly outline the history of these ideas. No claim is made that such an outline can provide the rationality of the chosen ideas, only that some framework is necessary to organize such a complexity. Lakatos develops a simple and insightful framework for the history of ideas. However, when individual philosophical positions are analyzed, Popper's critical approach is used. But as an outline of these ideas, Lakatos' descriptive framework for the history of mathematics can be employed.

From the history of mathematics Lakatos finds three central research programmes.[161] Each is characterized by how a given deductive system is viewed. Thus, our goal is to describe the philosophy of science by the manner in which each school of thought approaches the logic of science.

First, there is the Euclidean programme. Here, the deductive system is seen as a set of "obviously true" premises, or axioms, which logically derive the remainder of knowledge. "Truth" is inserted at the top and deduction transmits it down to propositions of lower generality. Lakatos terms this activity the Programme of Trivialization of Knowledge, since it seeks to derive all knowledge from a finite set of trivially true axioms.[162] All knowledge claims are then deductive proofs from unquestionable premises. This Euclidean conception of knowledge is, indeed, consistent, yet it cannot explain how knowledge grows. Is it only by deduction that we may learn? The Euclidean programme began in ancient Greece where it found its first explicit expression in Euclid's geometry. Euclidean geometry has served as the paradigm of "pure knowledge" until comparatively recent times. Yet there has always been some dissension concerning the Euclidean conception of knowledge.

The first major criticism comes from the Sceptics. They asserted that any effort to establish a claim to knowledge must involve an infinite regress. Sure, one might deduce "truth" from assumed "true" axioms, but how can one establish the "truth" of these axioms? Perhaps we might derive them from some other set of even "more primitive" axioms. Yet, any such rescue operation will, itself, require being rescued. An infinite regress is inevitable; nothing can be established by deduction alone.

Sextus Empiricus, perhaps, best made this Sceptical argument when discussing a criterion of truth.

> Besides, in order to decide the dispute which has arisen about the criterion (of truth), we must possess an accepted criterion by which to judge the dispute; and in order to possess an accepted criterion, the dispute about the criterion must first be decided. And when the argument thus reduces itself to a form of circular reasoning the discovery of the criterion becomes impracticable, since we do not allow them to adopt a criterion by assumption, while if they offer to judge the criterion by a criterion we force them to a regress *ad infinitum*.[163]

This argument holds equally well if "a set of axioms" replaces "a criterion of truth." In the Euclidean system the only criterion of truth is deduction from given sets of axioms. This Euclidean conception of knowledge is particularly vulnerable to such skeptical criticism, since it is so preoccupied in the establishment of "truth" by proof.

Thus, other conceptions of knowledge were needed to counter such criticism. Our deductive system must anchor "truth" in some other manner. The chosen salvation of knowledge was "experience." Thus was born the Empiricist programme. Now knowledge is grounded upon some type of experience. The Empiricist programme has an upside-down view of a deductive system. "Truth" is injected into the bottom by singular statements of "facts." Yet, because of the asymmetry of universal statements and singular ones (as discussed in the Prologue), only "falsity" can be transmitted up the deductive system by modus tollens. Thus, in the Empirical programme a general proposition is either false or conjectural. Experience can only "disprove" our previous notions. Even if we have infallible truth from experience, our knowledge cannot be established.

To many, such an incomplete state of affairs cannot be accepted. Again, knowledge needs firmer foundation. So a method of transmitting "truth" upwards, from specific propositions to general statements and laws, was sought. Induction is just this type of required method. Thus the Inductive programme attempts to ground all knowledge upon the foundation of infallibly "true" singular instances.

The history of the philosophy of science can be described as the interplay of these three research programmes. The resulting continuity of such a description may be more of an illustration than a true synthesis of history's content. Yet, the simplicity of this description can give some insight without the quantity of detail that a full understanding of history would require.

The natural starting point for the philosophy of science is Descartes. Little science developed before Descartes, and pre-Cartesian philosophy spoke little, if at all, about what would now be considered science, however defined. The Cartesian method is Euclidean. Intuition is deemed the source of knowledge from which springs our beginning premises or axioms. To Descartes, only intellectual intuition is sufficiently clear and distinct to stand as the foundation of knowledge.

Assumptions can be "true" only if they are self-evident.

At the same time, a different conception of science was developing across the Channel. Classical empiricism turned Cartesian intellectual intuition on its head, or at least so it appeared. The philosophies of Bacon, Locke, Berkeley, and Hume began the difficult task of grounding scientific knowledge upon "experience" or "observation."[164] Yet, it was not classical empiricism that overcame Cartesian intuition.

The definitive blow against Cartesian philosophy of science, and this one variant of the Euclidean programme, was struck by science itself - Newton's physics. Although the Newtonian victory was a long time in the making, the eventual success of his physics proved conclusive for Newton's anti-Cartesian method. What is Newton's method? Interpretations differ and Newton was himself ambiguous about the derivation of his theories. Newton's stated methods were more justifications of his theory against Cartesian criticism than descriptions of how he accomplished his research.

Paradoxically, Newton claimed to have established the truth of his theory, as evidenced by his famous statement, "hypotheses no fingo - I make no hypotheses." To fortify his theory against metaphysical criterion, Newton asserted that his theory was derived from observations by induction.[165] Newton's inductive methodology is stated as Rule IV of the second edition of Principia.

> In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypothesis that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions. This rule we must follow, that the argument of induction may not be evaded by hypothesis.[166]

Newton went further to prohibit Cartesian metaphysical criticism by insisting that the only valid criticisms are calling into question the "phenomena" upon which the induction is based or experimentally contradicting a theory.[167] Thus, Newton established an inductive methodology for science and the Inductivist programme became firmly established by the success of Newtonian physics. Also, one might view Newton's method as a new foundation of empiricism.

The irony of this chapter from the philosophy of science is that Newton did not discover his laws by induction and he did employ hypotheses. Although it may appear that Newton's laws were derived from Kepler's laws, Kepler's laws are themselves hypotheses and are inconsistent with Newton's.[168] What was from the beginning an error of method used only to insulate Newton's theory from criticism became the "most accepted" model for scientists and philosophers until recently. In fact, many still seek some type of method of induction.[169]

With Newton, we have all three philosophy of science's research programmes. Since Newton, the development of the philosophy of science can be seen as syntheses, compromises, criticisms, and circumlocutions of the problems associated with these programmes.

Hume and Kant might be considered as the next contributors to the philosophy of science. Hume, an empiricist, is best remembered for his views on causation and induction. As we discussed in Chapter 2, his definitive criticism of induction still holds. There is no infallible method of transferring truth from singular instances to universal statements. As a result of Hume's criticism, some philosophers fell into scepticism, irrationalism, or dogmatic subjectivism.[170] Yet, the Inductivist programme continued to be developed (for example, Mill's Logic) and scientists continued to believe that they were using induction.

Kant, on the other hand, reconciled Newtonian physics into the Euclidean programme. Kant, too, saw the absurdity of the claim that Newton's theory could be derived from observations; yet he believed in the truth of Newtonian physics.[171] Kant asserted that synthetic *a priori* truth is the basis of our knowledge. Kant's philosophy is much more than a statement of the basic principles of the Euclidean programme: the existence of primitive propositions or axioms and the validity of deductive inference. His philosophy is build upon the premise that these *a priori* truths are necessary to interpret nature. Or as Kant tells us, "our intellect does not draw its laws from nature, but imposes its laws upon nature."[172]

Kantian philosophy is a synthesis of the Euclidean and Empiricist programmes. These *a priori* truths do not come from intellectual intuition, as Descartes thought, but from some pure form of sensual intuition without which our experience could make no sense.[173] "According to Kant, sense-intuition presupposes pure intuition: our senses cannot do their work without ordering their perceptions into a framework of space and time. Thus space and time are prior to all sense-intuition; and the theories of space and time (including Newton's theory) - geometry and arithmetic (and physics) - are *a priori* valid."[174] [Parenthesis added.] With Kant, an aprioristic philosophy of science and knowledge is strongly stated. Kantian and Cartesian philosophies give the Euclidean programme its intellectual creditability.

From these philosophical perspectives spring the diversity of philosophies and methodologies of science. The next task is to analyze and criticize the main schools of philosophical thought about science. The first step in such an analysis is to identify the representative schools of thought. Next, each system needs to be reduced to its central tenets. Then these positions can be 'rationally criticized' in the light of their problem-situations and in reference to the central questions of scientific epistemology. As any analysis, some things will be left out and some philosophers will not quite fit into the chosen categories. Yet, it is hoped that the salient features from the philosophy of science are adequately identified and discussed. No claim is made that this analysis is completely comprehensive nor that all the adherents of their positions held precisely those tenets chosen. But such is the limitation of any type of analysis. Its value lies only in sharpening our focus and thereby providing a clearer view.

Briefly stated, the schools of the philosophy of science may be described as follows:

(1) _Dogmatic subjectivism_ holds that knowledge is based upon subjective sense impressions. Or, "the world is my dream." Dogmatic subjectivism is an extreme version of the Empiricist programme.

(2) _Apriorism_ holds that some knowledge is given to us as valid, _a priori_, and that these "truths" are somehow known through intuition or introspection. The rest of the body of knowledge is composed by deduction from these _a priori_ "truths." Apriorism is best considered as one form of the Euclidean programme.

(3) _Conventionalism_ holds that knowledge can be "true" only by convention. Theories are only convenient means of organizing the "facts" and are chosen by their simplicity. Conventionalism is most easily seen as part of the Euclidean programme.

(4) _Instrumentalism_ holds that theories are instruments of "prediction" and no more. They are not "true" or "false" but only inference licenses. Instrumentalism is the extreme form of conventionalism which results when a conventionalist also holds a pragmatic view of truth.

(5) Logical positivism holds that all meaningful statements are reducible to elementary statements of experience. All else is senseless and thus not knowledge. Logical positivism is ultra-empiricism or another extreme version of the Empiricist programme.

(6) Probablism holds that our knowledge and theories can be appraised by their probability relative to the "facts." This is a catchall for all philosophies of science that solve the problem of theory choice through some algorithmic device. Other labels subsumed by probablism include justificationism, verificationism, Bayesianism, and modern inductivism. Probablism is the modern version of the Inductivist programme.

To each we now turn.

3.3.1 Dogmatic Subjectivism: Man Is His Own Prisoner

Dogmatic subjectivism, or as some say idealism, is the extreme form of empiricism which believes that knowledge, like perception, must be subjective. The adjective, "dogmatic," identifies the philosophical position that asserts that only subjective knowledge exists and denies 'objective knowledge' altogether. Or, knowledge is confined to the second world. Knowledge of the first world exists only as perceived by the second world. And, third-world knowledge can be only a second-world illusion.

It is our intent to differentiate dogmatic subjectivism from subjectivism in general. Broader forms of subjectivism can be harmless. No one would deny that our personal knowledge is subjective and based upon our frameworks or biases. To assert that the second world exists is both harmless and trite. Such a position does not come into conflict with any of the other philosophical positions here addressed and, as such, is empty. Subjectivism is unique and problematic only when it additionally asserts the non-existence of 'objective knowledge,' in any sense. Thus, we criticize only this extreme form of subjectivism, dogmatic subjectivism.

This subjectivism is a natural development of the Empiricist programme. If our knowledge need be grounded upon "observation" or "empirical facts," it is only natural that we might inquire into the formation of these "observations." At the limit, such an inquiry would lead to the notion that "reality" is merely our subjective perception of it. Or, in its crudest formulation: "The world is my dream."

The subjectivistic philosophy may be attributed to Berkeley and Hume. In their day, this philosophy could well have been considered an improvement over Cartesian intuitionism. Yet, weaknesses remain in their positions. Popper succinctly identifies and criticizes their problem-situation.

> We find that Berkeley and Hume believed that our knowledge was reducible to <u>sense-impressions</u> and to associations between <u>memory-images</u>. This assumption led these two philosophers to adopt idealism (or subjectivism); and in the case of Hume, in particular, very unwillingly. Hume was an idealist (subjectivist) only because he failed in his attempt to reduce realism to <u>sense-impressions</u>. It is, therefore, perfectly <u>reasonable</u> to criticize Hume's idealism by pointing out that his sensualistic theory of knowledge and of learning was in any case inadequate, and that there are less inadequate theories of learning which have no unwanted idealistic (subjective) consequences.[175] [Parenthesis added.]

Hume's subjectivism resulted from his criticism of induction. Yet, induction is not the only way one might learn about 'reality.'

The mistaken reduction of knowledge to sense-impressions is the source of dogmatic subjectivism. It can be 'rationally demonstrated' that our second-world knowledge cannot be reduced to sense-impressions only. There must be some type of 'theory' (or expectation or disposition to act) before an organism can perceive, and 'scientific observations' are 'theories' themselves. Sense-impressions in and of themselves are meaningless. Some decoding mechanism - a theory - is required for sense-impressions to be more than random noise. Support of these assertions comes from all those psychological studies that find the selectivity of perceptions in animals and small children. That is to say that organisms do not attend to all stimuli at once, or at random. Instead, what the organism chooses to perceive is highly selective. The only explanation of such phenomena is to assert that the organism begin with some type of "theory." This "theory" can change over the development of the organism by "conditioning," say the behaviorists, or by trial and error clashes with 'reality,' say the realists.

In any case, there is a great deal of "scientific knowledge" of the second world, psychology, that is inconsistent with the subjectivists' total reduction of knowledge to sense-impressions. If such is the conclusion of our best scientific knowledge, it would be a philosophical mistake to insist upon this reduction. If any subjectivist were to admit that perceptions are themselves theories, then his problem-situation would be substantially altered. This revision would force the consistent subjectivist to abandon any notion that there exists some type of "pure experience." Even our "most basic perceptions" would then be hypothetical and subject to change and interpretation. Thus, reduction to sense data would, at best, be ambiguous.

Also, the subjectivist who admits the theoretical nature of perception would need to explain why initial human perception is so common and how it changes. Otherwise, the subjectivist cannot even explain second-world knowledge. Yet, we can see no way that a dogmatic subjectivist could explain how human perceptual theories or abilities change. If any "external cause" is hypothesized, would that not be an implicit rejection of dogmatic subjectivism? The subjectivist's only recourse is to assert that our perceptions change because we wish them to or because that is our dream. But, this is no explanation. It is only an empty assertion that leads nowhere. Thus, the subjectivists cannot even explain second-world knowledge. Yet the dogmatic subjectivist asserts that third-world knowledge does not exist. At least third-world knowledge can explain second-world knowledge and thus lead outside itself. From a pragmatic point of view, we may conclude that subjectivism can be ignored since it has no value. Still other 'rational arguments' against subjectivism are available.

The subjectivists' claim that all knowledge is derived from the senses is further contradicted by the growth of scientific knowledge. For example, what physicists have learned about atomic 'reality' cannot be directly experienced, at least not by the senses. Instruments, themselves 'theories,' need be employed to 'observe' these processes. No one has directly perceived atomic phenomena. The subjectivist might retort that the scientists experience their instrument readings. But this too misses the point. For all the 'observations' could have been made automatically, fed into a computer to be compared, and statistically analyzed against the 'theory' without the intervention of the second world. Such a procedure could, equally, have produced our current 'knowledge' of quantum mechanics. While it is true that it is we who 'conjecture' and 'refute,' it is not true that we must "perceive" to 'observe.' All that 'observation' requires is that some conventional and replicable procedure serve as 'facts.' Yet, subjectivistic epistemology cannot explain and is inconsistent with this type of 'knowledge.'

Still, we have not "disproved" subjectivism, for the dogmatic subjectivist could assert that I or we have merely dreamed atomic physics and the whole thought experiment - instruments, phenomena, computers, and all. Two important criticisms are left. Since from a subjectivist point of view all is a dream, my dream of an external real world is just as valid as the subjectivist's dream of a dream. I would further assert that my "dream" of realism is more 'useful,' since it prevents the erroneous belief that "gravity" does not exist or that bullets are imaginary. The final criticism is that the subjectivist needs to dream something in contradiction to realism. Otherwise, subjectivism is merely a superficial interpretation of realism that may be realistically and 'rationally' dismissed, surgically removed by Ockham's razor. If any subjectivist asserts that some fundamental distinction exists, we simply ask that his assertion be 'refutable' or 'testable.' Many tests are possible, one only needs to be reminded of the many variants of the "lightening in the forest" theme. Of course, all of these arguments could be wrong. But, would that not presuppose something that was 'objectively' right?

The last point presents a considerable philosophical problem to subjectivism. The idea that we can error or make mistakes, fallibilism, presupposes some notion of 'objective reality' or 'truth.' Without some standard, there is no fallibility. Thus, the dogmatic subjectivist must be infallible. To forbid any notion of the 'objective' and thus inter-subjective 'reality' is to deny any standard from which we may error. Yet the infallibility of the dogmatic subjectivists has an inherent contradiction. Two subjectivists may assert, or dream, different "realities" that are mutually inconsistent. But each is infallible. Is this not a contradiction? Is it not absurd to allow both inconsistent and infallible "truths"? But the dogmatic subjectivist has no choice. Unless, of course, he wishes to assert that either his dream is the only infallible "truth" or that inconsistency is "truth." Is it necessary to comment upon the absurdity of any such defensive tactics?

In summary, subjectivism fails to be a viable philosophy of science because:

(1) It is based upon an erroneous reduction of knowledge to "raw" sense-impressions.

(2) It cannot explain subjective knowledge or its development. It can only assert that it is and that it has changed.

(3) It denies the existence of 'objective knowledge.' Thus, (2) implies that subjectivism cannot explain scientific knowledge or its growth.

(4) It requires infallible knowledge and allows inconsistent "truth."

(5) It has no practical value. It does not lead beyond itself. It solves no significant problems.

This last point may deserve elaboration. A necessary condition for the adequacy of a philosophy or methodology of science should be that it leads beyond itself and, as a consequence, has some practical advantage. That is, an adequate philosophy of science need be more than a system of consistent, yet tautological, assertions. Subjectivism is no more than the assertion that all knowledge or truth is a dream or belief. It can provide, at best, a terse answer to the question: What is knowledge? It solves no other problems; it cannot lead beyond itself. It does not imply or prescribe any means for knowledge to grow.

Fallibilism is quite different. It too answers the simple question: What is knowledge? But it says much more. The notion that our possession of knowledge may be mistaken implies both 'objective truth' and subjective "truth." Also, fallibilism implies a means for inducing the growth of knowledge, criticism or 'testing.' For, if we can find our error, we will learn. With only a little common sense, fallibilism results in a sound methodology, falsificationism. And, we obtain all of this practical value by such a simple and clear assumption.

Dogmatic subjectivism might be rescued by adding other assertions which permit us to go beyond our own framework; for example, by adding some notion of fallibilism or 'objective truth.' But either defensive tactic results in the denial of dogmatic subjectivism. You cannot have 'objective truth' and assert that 'objective truth' does not exist at the same time. Thus, it is possible to dogmatically maintain subjectivism but only at the price of being locked in one's own framework. And, cannot all of us imagine where that leads? ø

Dogmatic subjectivism can only inhibit the growth of knowledge. It can only foster a dogmatic attitude towards science and knowledge. For dogmatic subjectivism can be defended from criticism by the assertion, "Well, that may be your belief, but it is not mine." If dogmatic subjectivism is substantively amended, then what remains of subjectivism would be merely a superficial trapping upon fallibilism or some other tenable philosophy.

With subjectivism, 'rational criticism' and empirical testing are completely superfluous. They could only be used to convince yourself and others. If there were no 'objective reality,' scientists would be forced to invent one. Otherwise, all they would have to do is to convince others and sell their beliefs. The common adage, "If wishes were horses, beggars would ride," is indeed trite. However, at another level it provides an accurate prediction of the consequences of a subjectivistic epistemological foundation of science.

3.3.2 Apriorism: If You Already Know How Can You Learn

Apriorism is the philosophical position that some propositions are given to us as valid, *a priori*. It is a school of thought firmly within the Euclidean tradition. *A priori* "truths" are the axioms from which all other knowledge may be deduced.

The classical economic methodology is aprioristic. The British economic tradition was dominated by this philosophical view until recent times (these topics provide the central discussion of the first two sections of the next chapter). A more Kantian apriorism has also had a significant influence upon economics through the Austrian school (for example, L. von Mises). Thus an understanding of apriorism is essential for a thorough understanding of economic theory and its philosophical basis.

Descartes was perhaps the first to realize the impact that the "new astronomy" had upon traditional epistemology. He saw that this scientific knowledge contradicted both the religious and naive empirical foundation of the philosophy of the Middle Ages.[176] Thus Descartes sought a new foundation for knowledge and what he discovered was intellectual intuition. To Descartes intuition was "the conception which an unclouded and attentive mind gives us so readily and distinctly that we are usually freed from doubt about that which we understand."[177] Once these concepts were intuited, the mathematician, Descartes, had no difficulty in deducing the rest. Some suggest that Descartes' true method was implicit theorizing.[178] Or, he assumed just what his mathematics required.[179]

The nuances of Cartesian philosophy are not relevant for the present inquiry. What is important is the fact that Descartes established *a* priori intuitions as the foundation of knowledge. Also Descartes provided a paradigm for such intuition. "Cogito ergo sum." Thus starting from doubt, Descartes' famous assertion "proves" itself. For if I doubt my own existence, then there must be a doubting subject, "proving" my existence. This type of casual introspection became the accepted method for generating knowledge. Yet, such casual introspection is rarely sufficient for knowledge. In fact, the Cartesian formula does not logically follow. The existence of a doubting subject, assuming that this has been "proved," does not imply my or Descartes' existence. The doubting subject could be God, or Thomas, or anything, not necessarily me. In any case, such a loose form of reasoning became a paradigm of the Cartesian methodology of science. Yet, Descartes was not overly concerned, for he believed that God is not a deceiving God; thus, our intuitions must be true.[180]

Kant expanded and strengthened the apriorism of Descartes. To Kant experience contains both a subjective and an objective component. The subjective component contains judgments of perceptions given only by sensuous intuition, while the objective part concerns the necessary and general union of perception in the consciousness.[181]

> Hence the pure concepts of the understanding are those under which all perceptions must be subsumed where they can serve for judgments of experience, in which the synthetical unity of the perceptions is represented as necessary and universally valid .... The principles of possible experience are then at the same time universal laws of nature, which can be known a priori. And thus the problem of our second question, "How is the pure science of nature possible?" is solved.[182]

Kantian philosophy sees the objective aspects of experience as those concepts that are necessary before perception is possible. Such necessary principles then express the synthetic unity of our experience and are valid, a priori. Thus is solved Kant's principal problem: "How is the pure science of nature possible?" or "How is it that Newton's theory is true?"[183] True science is only possible because our experience requires certain a priori "truths." These a priori "truths" and deduction generate all knowledge, including scientific knowledge.

Again, our point is to simply identify Kantian philosophy as aprioristic. Thus, both Cartesian and Kantian philosophies establish apriorism as a philosophy of science well within the Euclidean programme. The central difference between these outstanding thinkers concerns their choice of a mechanism to generate a priori "truths." Descartes required only some type of unbiased and free intellectual intuition, while Kant seemed to demand some demonstration of their "logical" necessity and experiential priority.

To 'rationally criticize' apriorism, two main arguments are available. We can show the inadequacy of apriorism in solving the problems for which it was designed. Or, we can show the inadequacy of apriorism in providing a sound foundation for scientific knowledge and its growth. Popper gives us the first.[184]

Kant's solution to the problem of the existence of true natural science was to elevate Newton's laws, his concepts of space and time, and Euclidean geometry to a priori "truths." Thus, of course, a "true" Newtonian science follows. Kant was correct to believe that something precedes and is necessary for perception (for example, 'observational theories'). But he is wrong to assert that some specific set of concepts must be "true," a priori. In fact, scientific knowledge refutes Kant's proof of the existence of "true" scientific knowledge. All of the principles that Kant considered a priori "true" - Newton's laws, the concept of absolute space and time, and Euclidean geometry - have been "falsified" by the advance of scientific knowledge. Einstein's theories demonstrate that not only are these concepts not necessary to perceive the world but also that our 'observations' are inconsistent with these Kantian a priori principles. What better refutation of a philosophy is possible? And the same could be said of Cartesian apriorism.

Our argument against apriorism revolves around two central weaknesses.

(1) Apriorism provides no secure epistemological foundation for science. Scientific knowledge, to the apriorist, is but a deductive system based upon intuitively obvious a priori "truths." These truths cannot be considered a sufficient basis for scientific knowledge, for the claim of their "truth" cannot be 'rationally demonstrated' or criticized.

(2) Apriorism cannot explain the growth of scientific knowledge. Even if one disagrees on the constituents of growth, the fact that many of the fundamental scientific propositions have changed refutes apriorism. The apriorist could only explain these fundamental changes by asserting that the a priori "truth" has also changed. But, would that not be a contradiction?

With respect to the first point above, apriorism needs to establish, in some sense, the validity of its a priori "truths." In turn, this validity can be established either by some demonstration of the obvious nature of the propositions or by the demonstration of the necessity of these propositions for further reasoning. This first line of demonstration simply does not work and is often misleading when attempted. What is obvious? How do we recognize obviousness? What is obvious to you may not be so to me, or vice-versa. "Obvious" is a subjective or second-world concept, and it is unlikely that it can be reasonably defined otherwise. In any case, what was considered obvious to Descartes and

Kant has been "falsified" by science. Cartesian vortices, Euclidean geometry, and Newtonian physics have all been demonstrated to be "false," at least by our best scientific knowledge. If these obvious "truths" turned out to be "wrong," would it not be wise to reject this technique of demonstration?

The other track available to the apriorist is to show that a priori "truths" are necessary for some type of reasoning. Yet this too is impossible. While it is true that some framework or set of axioms is necessary for experience and reasoning, it is not true that any particular set is required. Logically speaking, the axioms we choose to prove some given result are never unique. If one set of axioms "works" so will another. Logical necessity runs the opposite direction, from axioms to consequences. Aside from the question of uniqueness remains the question of the necessity of a particular result. Fallibilism demands that no proposition is necessarily true. In any case, the necessity of any specific proposition cannot be demonstrated.

To some our argument may seem a bit biased. When discussing fallibilism and falsificationism we did not demand that it establish the "truth." Yet, it is part of fallibilism's creed that possession and "proof" or "truth" is impossible. Thus, such a demand would be unreasonable. Apriorism, however, does claim to possess the "truth," and a priori at that. Since this is a central tenet to apriorism, it is open to criticism.

One major difficulty with the apriorists' possession of "truth" is that they can give no reasonable grounds for such an assertion. And more importantly from our perspective, how can claims to a priori "truth" be criticized in order to find their error? Do you offer alternative statements that are more "obvious" or more "necessary"? If this is legitimate criticism, then we assert that there is always a multiplicity of "acceptable" a priori "truths." Yet for such a state of affairs, what have we learned?

History provides harsher criticism of apriorism. For the a priori notions of Descartes and Kant have been "disproved" by modern science. If what the greatest thinkers consider necessary for rational thought is not only unnecessary but does not hold in the physical world, should we not refuse to give a priori or "necessity" the position of arbitrator for our knowledge?

A philosophy or epistemology of science must provide some criterion of identifying knowledge or its growth. Otherwise, what can it tell us? Apriorism does not provide us with any such criterion. How can we "discover" a priori "truths"? Or, how can we choose among candidates for the status of a priori "truths"? The notions of intuition, obviousness, or necessity simply cannot serve this function.

To illustrate the last assertion, let us consider the notion of simultaneity. No notion is more intuitive than to consider two events occurring at the same time. It is intuitively obvious to believe that if these events are simultaneous then they occurred at the same time and that is that. Also, simultaneity seems, as well, a necessary concept. Without it we could not say thinks like, "The news came on at 5:00." This requires the simultaneity of the event, news, and another event, the clock shows 5:00. And, what is more obvious than: "If events A and B are simultaneous and if C occurs 5 seconds after A, then C occurs 5 seconds after B." Yet, all these descriptions of absolute simultaneity are not consistent with the 'facts.' Einstein's relativity gives us a relative notion of simultaneity. In relativity theory, it makes no sense to say that two events happen at the same time. Simultaneity depends on our position and inertial frame of reference.[185] What is simultaneous at one point in space may not be so in another. Thus, our most obvious and intuitive notion is not only unnecessary but it also gives us an inaccurate description of physical processes. These notions of absolute space, time, and simultaneity are not used in our best scientific theories, yet they are the best examples of _a priori_ "truths." Thus, science refutes the adequacy of apriorism.

The dogmatic apriorist might attempt to defend his position by admitting that specific propositions cannot be regarded as _a priori_ true and by asserting that _some_ propositions, nonetheless, must be taken as primitive "truths." Yet, any such defense renders apriorism empty, a worthless philosophy. If apriorism does not tell the set of axioms to base our knowledge upon, it tells us nothing. The notion that at any point in time our knowledge may be regarded as a deductive system in which some statements can be regarded as axioms and others as deductions is trite and trivial. All philosophies of science are consistent with this conception of knowledge. To assert that current knowledge can always be made into a deductive system where some statements become "true" axioms is simple logic but not a philosophy of science. Thus, apriorism must be able to identify the particular set of axioms to base our knowledge upon, or it says nothing. Or at least it needs to provide us with a mechanism to identify these propositions.

More importantly, apriorism cannot explain the growth of knowledge. Apriorism is essentially a static theory of knowledge. Once we "discover" the set of _a priori_ "truths" all knowledge is given. Certainly, we need to deduce other knowledge claims from our "true" axioms. Yet deduction does not produce knowledge. Deduction is a reductive process. By deduction, all knowledge is contained in the premises; it is only diluted or translated to the consequences.

If it is rationally accepted that scientific knowledge has grown from Kepler to Newton to Einstein, then apriorism is 'rationally refuted.' For the only way that apriorism could explain this growth of knowledge and its associated change in fundamental "assumptions" is to assert that a priori truth has also changed. But such an assertion must certainly be seen as a contradiction.

Apriori is not a viable philosophy of science. It cannot provide an adequate foundation of knowledge, nor can it identify or discriminate knowledge from other claims. And, apriorism as a philosophy or methodology of science is refuted by science itself, since it is inconsistent with the growth of scientific knowledge. Any attempt to render apriorism consistent with the growth of knowledge would destroy apriorism. For any such successful effort would also render apriorism fallible. And, would this not likewise be a contradiction?

3.3.3 Conventionalism: Knowledge As An Empty File Cabinet

> The logical basis of the theory of relativity is the discovery that many statements, which were regarded as capable of demonstrable truth or falsity, are mere definitions ... The word "relativity" should be interpreted as meaning "relative to a certain definitional system."
>
> - Reichenbach [1953], pp. 198 & 200

Conventionalism is the philosophy of science that regards knowledge and scientific theories as organizing systems. Theories are only file cabinets in which facts are sorted. Knowledge is neither 'true' nor 'false'; it is, at best, "true by convention." Scientific theories are only convenient frameworks which are useful in the organization of our facts. Conventionalists may be seen as holding a coherence theory of truth, for conventional truth's only quality is to provide a consistent structure for knowledge.[186] Or, as Lakatos puts it:

> Conventionalism allows for the building of any system of pigeonholes which organizes the facts into some coherent whole. The conventionalist decides to keep the centre of such a pigeonhole system intact as long as possible: when difficulties arise through an invasion of anomalies, he only changes and complicates the peripheral arrangements. But the conventionalist does not regard any pigeonhole system as provenly true, but only as "true by convention" (or possibly even as neither true nor false). In revolutionary brands of conventionalism one does not have to adhere forever to a given pigeonhole system: one may abandon it if it becomes unbearably clumsy and if a simpler one is offered to replace it .... Genuine progress of science is cumulative and takes place on the ground level of "proven" facts, the changes on the theoretical level are merely instrumental. Theoretical "progress" is only in convenience ("simplicity"), and not in truth-content.[187]

Here, one may notice some similarities between Lakatos' characterization of conventionalism and his MSRP. This is why we characterize MSRP as a compromise between falsificationism and conventionalism.

Of the philosophical alternatives discussed in the current section, conventionalism is the soundest. Yet, conventionalism has a number of weaknesses, among which include:

(1) Conventionalism is based upon a misunderstanding of science. Scientific theories are not merely conventions.

(2) Conventionalism's regulative principle, "simplicity," cannot be reasonably defined or evaluated. Thus, it cannot provide a basis for theory choice nor for the identification of knowledge.

(3) There is only one escape from the above, (2) if, following Popper, one defines "simplicity" as 'empirical content,' then "simplicity" can give some basis for theory

choice. Yet, if the conventionalist incorporates Popper's definition, he will no longer be a conventionalist, as distinctly defined, but an incomplete falsificationist.

(4) Conventionalism cannot explain the growth of scientific knowledge. This follows from (2) above.

(5) Conventionalism contains unfortunate heuristics. It encourages the dogmatic defense of a particular file cabinet and the senseless collection of "fact."

The historical development of conventionalism is more difficult to quickly portray. This difficulty arises from the fact that shades of conventionalism have existed throughout history. The ambiguity of the historical origins of conventionalism is illustrated by Blaug's assertion of the conventionalism of Adam Smith and David Hume who lived in times when conventionalism had yet to be a specified philosophical position.[188] Yet it seems clear that conventionalism developed from apriorism and, perhaps, is the last defense of the Euclidean programme. As the inadequacy of Cartesian and/or Kantian epistemology became clear, it was necessary to deny the "truth" of our deductive systems. If for a priori "truth" we substitute "truth by convention," much of the criticism against apriorism vanishes. Thus, one might see the invention of "truth by convention" as a "conventionalist stratagem" designed to preserve the philosophical integrity of deductive systems of knowledge.

Conventionalism is further influenced by apriorism in its Kantian recognition that we need some framework in which to interpret sense-impressions or "facts." Thus, conventionalists emphasize the requirement that knowledge must provide an organization for the "facts." Also, conventionalism blends some elements of Schopenhauer's voluntarism. The conventionalistic file cabinets or theories are the willful creations of man. Human use, not 'truth,' chooses our knowledge. And if conventionalism is not already too eclectic, it shows the influence of the Empirical programme. In conventionalism, the whole point is to design a "simple" system that can organize the "facts" or empirical knowledge. Thus, in some sense, "facts" provide the substance from which theories are "convened." Still, it is best to view conventionalism as a school within the Euclidean programme, since the conventionalist focuses upon knowledge as the logical-deductive structure of the file cabinet itself. To the extent that conventionalistic philosophy incorporates factual knowledge, it is a Euclidean-Empirical hybrid. But, its Euclidean connections remain strong. Conventionalism was first proposed to maintain Euclidean geometry in physics, and Euclidean geometry has always been the centerpiece of the Euclidean programme.

Poincaré is the founder of conventionalism as a distinct philosophy of science.[189] Poincaré based this philosophy on his observation that Euclidean geometry is merely a convention of our physical measuring process. It is the simplest axiomized geometry and can always be maintained by continually adjusting our natural laws as experience dictates. Though correct, this view misses the point. Our geometry may always contain an element of convention (like 'observations'), but we need not constrain ourselves to one geometry. And the simplest system of axioms (if such an entity exists) does not necessarily lead to the simplest or most reasonable system of physical measurements.

This is dramatically demonstrated by Einstein who considered the following reflection essential to the theory of relativity. "In a system of reference rotating relatively to an inert system, the laws of disposition of rigid bodies do not correspond to the rules of Euclidean geometry on account of Lorentz' contraction; thus if we admit non-inert systems we must abandon Euclidean geometry."[190] Thus, the theory of relativity demonstrates that real gains in knowledge can result from changes in our conventions. Although knowledge may contain some conventions, it is not only conventions.

Yet Poincaré's philosophy soon found followers and was applied to science in general. This is evidenced by Pareto's statement of Poincaré's interpretation of Newtonian physics.

> The same facts may be expressed by an infinity of theories, equally true, because they all reproduce the facts to be explained. It is in this sense that Poincaré could say that from the very fact that a phenomenon allows one mechanical explanation it allows an infinity of them .... The theory of universal gravitation does not have a real absolute content to oppose "the error" of the theory which assigns to each heavenly body an angel who regulates its movements. Moreover, this second theory may be made as true as the first one by adding that the angels, for reasons unknown to us, make the heavenly bodies move <u>as if</u> they were attracted to each other in direct proportion to the masses and inversely to the square of the distances.[191]

Thus, it is conventionalism that gives us the qualifier, "as if," to convey the notion of "true by convention." Economists were quick to incorporate some of Poincaré's conventionalism. In the above quote we find a strong expression of the misunderstanding of technical terms. "Gravity" is a technical term. It is only implicitly defined by the Newtonian system; it has no "meaning" or ontology in itself. "Gravity" has two functions in physics. It stands as a label for Newton's inverse square law and poses the question: "How can the inverse square law be further explained?" If you will, "gravity" has no content.

In this last passage, both Poincaré and Pareto believe that if the word "gravity" is replaced by this story of "heavenly attraction" then a new theory results. This is simply a misunderstanding. Their story and

Newton's theory are the same theory. Unless, of course, someone cares to add the conjecture that "heavenly attraction" is related to some other phenomena which has 'empirical content.' In this context, there is not an infinity of equivalently "true" theories, but only one. Thus, the argument given by both Poincaré and Pareto is mistaken; for the example is not a different theory. That this misunderstanding is common in economic methodology is shown in the next chapter.

We have now made our first point, conventionalism is based upon a misunderstanding of science. Although the argument just outlined is correct, there is some empirical evidence which suggests that it is also sufficiently elusive to warrant a more complete exposition. Any thorough discussion of this "conventionalist mistake" must begin in formal science, particularly geometry.

"Pure" geometry is only an abstract and formal system of definitions. It can make no claims to 'knowledge' in any sense that is discussed here. A formal system is a set of "axioms" or "primitive propositions" (words do not matter but only how we interpret them) and their logical implications. The system is neither 'true' nor 'false' (whether single quoted, double quoted, or not quoted at all) but only consistent. The consistency of our implications is guaranteed as long as our logic is simple (recall the Prologue) and we make no mistakes. Thus Euclidean geometry is consistent but not true.

Within every formal system appears a peculiar set of terms called implicit terms, implicit definitions, or undefined terms. In Euclidean geometry the implicit terms are "point," "line," "plane," etc. These terms have no explicit definition; they correspond explicitly to nothing. Their only "meaning" is realized by their use in the "axioms" and logical derivations. In some sense, they have only pragmatic meaning, for their meaning is exhausted by how they are handled within the formal system.

In the present context, it should be clear that these implicit terms are only conventions.[192] They are only words agreed upon and chosen for their convenience. Poincaré's position is faultless, if restricted to "pure" geometry or formal systems.

As we move away from "pure" geometry the situation becomes quite different. In "physical" geometry, implicit terms are correlated to physical entities or to actual measuring techniques by the way of rules - sometimes called "rules of correspondence." Such rules explicitly engender our "meaningless" terms with "meaning." For example, Einstein relates the geometric concept of "line" to the propagation of light in empty space.[193] After we give such explicit interpretation to implicit terms, we can have "good reasons" and empirical support for our preference of one "physical" geometry over another. This is the position advanced by Einstein (recall quotation 190). Einstein goes even further by proposing empirical tests of the postulates of "physical" geometry, including the postulate that "if two tracts are found to be equal once and anywhere, they are equal always and everywhere" and the question of the spatial finitude of the universe.[194]

Poincaré's mistake is the result of the failure to see a distinct "physical" geometry. To Poincaré, "physical" geometry is "pure" geometry. This is apparent in Poincaré's equating of the choice between Euclidean geometry and non-Euclidean and the choice between meters and feet.[195] Yet the latter is a completely arbitrary choice of what to call our units of displacement and nothing more. Poincaré goes even further. He asserts that no empirical results could cause us to adopt a non-Euclidean geometry. For no matter how perverse our experimental results, we could "modify" the laws of optics and "suppose that light does not travel rigorously in a straight line."[196] And, Poincaré believes that such an adjustment must be seen as advantageous, since Euclidean geometry must remain the most convenient and the simplest.[197]

To be sufficiently clear, we must divide Poincaré's argument into two steps. The first concerns the assertion that we may always retain Euclidean geometry by adjusting our physics, regardless of our experimental experiences. With this step we immediately agree; Poincaré's logic is impeccable. Such an adjustment of our theoretical system is precisely what Popper dubs a "conventionalist stratagem."[198] Falsification is designed, at least in part, to identify these "conventionalist stratagems" and to rule out such <u>ad hoc</u> theory construction. It is always possible to rescue a theory from potential 'falsification' by adding hypotheses, <u>ad hoc</u>, or by reinterpreting the theory. But such tactics can only lower the value of a theory, unless 'empirical content' is correspondingly increased. To a falsificationist such <u>ad hoc</u> adjustments are merely dogmatic defenses which if perpetuated reduce a potential scientific explanation to sophistry and 'metaphysics.' Yet, even when we ignore falsificationism, Poincaré's argument is mistaken.

This mistake, again, is the result of Poincaré's inability to see "physical" geometry. Following his argument, suppose we observe some potentially non-Euclidean property and well corroborate its existence. Furthermore, we <u>choose</u> to reformulate our optical theory while maintaining Euclidean geometry. What then is our theory? The new theory is now the conjunction of Euclidean geometry and our new optics, and the Euclidean component is only a definitional system, a "pure" geometry.

Thus, our "physical" geometry becomes the conjunction of Euclidean geometry and "non-Euclidean optics." The resulting theory of "physical" geometry might be less ambiguously called non-Euclidean. If Poincaré wishes to argue that our "physical" geometry can always be retained as Euclidean, then he is wrong, as Einstein's general relativity demonstrates.

If, on the other hand, Poincaré wishes to assert that a Euclidean "pure" geometry can always be maintained, regardless of our empirical evidence, he tells us nothing, for such an assertion merely reflects the definition of "pure" geometry or formal science in general. We might assert with equal validity that we can forever maintain the "pure" geometry of Lobachevski or Riemann. Yet, Einstein gives us "good reasons" to prefer Labachevski's geometry, and his reasons are 'falsifiable' at that.

All that Poincaré succeeds in demonstrating is that our "physical" geometry may be linguistically divided into two components, one <u>called</u> "Euclidean geometry", the other <u>called</u> optical theory. Yet, our scientific theory of optics or "physical" geometry remains more than arbitrary convention. Although we may have total freedom in choosing <u>words</u> with which to describe any theory, we do not have total freedom in choosing the overall representation or interpretation of scientific theories. Thus following Einstein, we just reject Poincaré's philosophy of physical geometry.

Still the second phase of Poincare's argument remains. Here Poincaré seems to say that since Euclidean geometry is the "simplest" it must be the "best."[199] Thus, as the argument proceeds, we must then hold onto Euclidean geometry. Yet the concept of "simplicity" is quite complex and elusive.

Before Poincaré can use such a support for Euclidean geometry, he must give a reasonable definition of the criterion for "simplicity." This requirement becomes even more crucial when this philosophy of geometry is applied to science in general. It is fair to say that neither Poincaré nor any other conventionalist has succeeded in specifying "simplicity." In fact Poincaré assumes Euclidean geometry to show that Euclidean geometry is "simplest."

What is Poincaré's definition of "simplicity"? He defines "simplicity" only by example, "just as a polynomial of the first degree is simpler than one of the second, the formulas of spherical trigonometry are more complicated than those of plane trigonometry, and they would still appear so to the analyst ignorant of their geometric signification."[200] And Euclidean geometry is the "simplest" in the same sense.[201] Yet this concept of "simplicity" is totally relative. The "simplicity" of polynomials or spherical trigonometry depends upon what coordinate system and geometry we have chosen. For example, the formulas of spherical trigonometry become "simpler," in Poincaré's sense, than plane trigonometry if we use polar coordinates. Similarly, second degree polynomials are "simpler" than first orders under a parabolic geometry. The complexity of our mathematical expressions depends upon the implicit terms and axioms of our formal system, and our choice of formal systems is totally arbitrary. Thus, Poincaré assumes Euclidean geometry and Cartesian coordinates as the standard in order to show that Euclid's is the "simplest" geometry. We should not ask whether such a "circular" logic is "simpler" than our usual "linear" logic.

In summary, Poincaré fails to demonstrate that Euclidean "physical" geometry may be consistently maintained, and he fails to show that

Euclidean geometry is the "simplest." Shortly after Poincaré made his argument, the history of science "falsified" his assertions. Einstein's theories not only illustrate that Euclidean "physical" geometry can be rejected but also that non-Euclidean geometry is "simpler," at least in some more realistic sense. Since Poincaré's conventionalism is derived from his allegation of these characteristics of Euclidean geometry, his conventionalism is 'rationally refuted.' One might view Poincaré's assertions about Euclidean geometry as the axioms of his conventionalism. Thus, if the axioms do not hold, how can their implications for science hold?

Poincaré's mistake is to confuse symbols (words) with their content (meaning). This mistake can be generalized to conventionalism. The conventionalist's notion of "simplicity" seems always to concern only the relative "complexity" of the symbolic, mathematical form used to express a theory and not the content of theory or its explanation. Even the careful Russell seems to have this naive notion of "simplicity" in mind when he argues for free will.

> What science does, in fact, is to select the <u>simplest</u> formula that will fit the facts. But this, quite obviously, is merely a methodological precept, not a law of nature. If the simplest formula ceases, after a time, to be applicable, the simplest formula that remains applicable is selected, and science has no sense that an axiom has been falsified.[202]

What can the "simplest" formula be, other than its form? Clearly, Russell is no falsificationist nor a dogmatic conventionalist. His revolutionary conventionalism is unmistakable. We dub this confusion of the formula for its content the "conventionalist mistake."

Schlick, too, sees this "conventionalist mistake." He claims that conventionalists mistake the mathematical expression (which is, of course, a tautology) of a physical theory as the sum total of its content, and he argues that the associated philosophy of conventionalism is likewise mistaken.[203]

> Thus we see that all genuine propositions, as for instance natural laws, are something objective, something invariate with respect to the manner of representation, and not dependent in any way upon convention. What is conventional and, hence, arbitrary is only the form of expression, the symbols, the sentences, thus ... immaterial to the scientist .... The insight that conventions play an important role in formation of knowledge claims must therefore not be misunderstood to mean that these claims are therefore lacking in objective validity, as if truth were somehow subjective, or natural laws merely a product of our preferences. Wherever conventionalism asserts anything of this sort, it is guilty of confusing sentences with propositions. It mistakes the garment for the essence.[204]

Loosely speaking, the "conventionalist mistake" is to take the "medium for its message." Yet, not all conventionalists view "simplicity" as merely the "economy of expression." Some see "simplicity" as aesthetics, or a subjective preference for elegance. The subjectivity of this conventional concept is all but impossible to avoid. Popper is the only philosopher to define "simplicity" without any subjectivity. But due to the subjectivity of conventionalists, few, if any, accept Popper's definition. This is well illustrated by Blaug's criticism of Popper's solution to the problem of "simplicity."

> It is doubtful that this (Popper's solution) is a convincing argument, since the very notion of simplicity of a theory is itself highly conditioned by the historical perspective of scientists. More than one historian of science has noted that the elegant simplicity of Newton's theory of gravitation which so impressed nineteenth-century thinkers, did not particularly strike seventeenth-century contemporaries, and if modern quantum mechanics and relativity theory are true, it must be conceded that they are not simple theories. Attempts to define precisely what is meant by a simpler theory have so far failed

> and Oscar Wilde may have been right when he quipped that the truth is rarely pure and never simple.[205] [Parenthesis added.]

Thus Blaug refuses to accept Popper's notion of "simplicity," simply because it does not carry all of the subjective connotations that scientists have placed upon it. Still it must be admitted that relativity is simpler than Newtonian physics (even if both are <u>false</u>) when one <u>understands</u> relativity. Perhaps twenty-first-century social scientists will learn to see the elegance of relativity and quantum mechanics. In any case, Blaug gives no effective criticism of Popper's solution; he only illustrates the inherent subjectivity of the word "simplicity." For Popper anticipated this "criticism" before he ever offered a solution.

> It is therefore possible to reject any attempt (such as mine) to make this concept precise by saying that the concept of simplicity in which epistemologists are interested is really quite a different one. To such objections, I could answer that I do not attach the slightest importance to the <u>word</u> "simplicity." The term was not introduced by me, and I am aware of its disadvantages. All I do assert is that the concept of simplicity which I am going to clarify helps to answer those very questions which, as my quotations show, have so often been raised by philosophers of science in connection with their "problem of simplicity."[206]

Blaug, then, takes us back to where this philosophical issue stood before Popper without addressing the issue that Popper raises. Popper unambiguously dismisses the aesthetic and subjective associations of the word "simplicity" before he offers a solution to the "problem of simplicity."[207] Yet Blaug and conventionalists, in general, force us back to these connotations. The only valid criticisms of Popper's solution would concern the adequacy of Popper's specification of the "problem of simplicity" or the adequacy of his proposed solution to <u>this</u> problem.

Apparently it is too much to ask a conventionalist to argue about <u>problems</u>, for they can only see the <u>words</u>.

Like Popper, we dismiss some pseudo-criticisms of the solution to the "problem of simplicity" before expressing the solution itself. Thus, you must be wondering, "what is Popper's solution?" Simply stated, "simplicity" can serve as an epistemological regulative concept only if it is equated to 'testability.' A 'theory' is "simpler" if it has more 'empirical content,' if it is more 'falsifiable,' or if it can be more rigorously 'tested.'[208] As we discussed in Chapter 2, Popper's concept of 'testability' is related to the dimensionality of a theory. Briefly, the "simplicity" of a theory may be measured by the number of measurements (or the number of different 'facts') that are required to potentially 'falsify' the 'theory.'

This conceptualization of "simplicity" may or may not conform to the usual intuitive notion of the "simplicity" of a formula. For example, we might compare the "simplicity" of circular and elliptical planetary motions. By looking only at the formulae, one quickly concludes that the circle is "simpler." If we fix no parameters in our 'theory,' then Popper's "simplicity" agrees with our basic intuition. This follows since to 'falsify' an elliptical motion we need more 'observations' or measurements in order to specify the additional parameters. Yet in other formulations elliptical motion may be "simpler" than a circle. If, for example, we specify the values of three parameters in our "elliptical theory," then its 'falsification' would be easier thus "simpler" to Popper. Or if the "circular theory" allows for any combination of epicycles and "elliptical theory" does not, the "elliptical theory" would be "simpler." In fact, if we do not limit the number of epicycles that can be placed upon each other, the circular theory is 'irrefutable.' Thus, although Popper's own definition of simplicity may agree with our intuition, it need not.[209]

Popper's "simplicity" captures the rational aspects of the common notion of simplicity while, at the same time, it goes beyond our simple intuitions. "Simplicity" is 'empirical content,' and 'empirical content' involves not only the form in which a theory is presented but also how some of its theoretical terms are correlated to 'observable' states of affairs. Popper's solution, then, satisfies all our epistemological problems, as discussed in Chapter 2. 'Empirical content' allows us to identify knowledge, to compare alternative theories, and to explain the growth of knowledge. Popper's view of "simplicity" is the only critically sound position on the subject. And this notion of "simplicity" is the only one that can explain how Einstein's theories and quantum mechanics are "simpler" than their predecessors. Yet, are they not of a more complicated form?

At first, one might try to use Popper's "simplicity" to rehabilitate conventionalism. Yet to do so is to map conventionalism into falsificationism. Popper's "simplicity" is empirical content which is defined only relatively to 'falsifiable theories.' With the notion of 'falsifiable,' we have some notion of 'objective truth' which is incompatible with the conventionalistic notion of "truth" as merely convention. When 'empirical content' is our epistemological regulative concept, some form of falsificationism must result. Yet, this resulting form of falsificationism would be incomplete, unless the 'corroboration' of 'empirical content' is, as well, added. In any case, the attempt to incorporate 'empirical content' into conventionalism is completely inadmissible. 'Empirical content' and 'falsifiability' directly lead one to the methodology of falsificationism where it is required to prohibit "conventionalist stratagems." In fact, the conventionalistic strategy of adding peripheral arrangements to a theory when it is inconsistent with the 'facts' makes 'falsifiability' impossible and 'empirical content' zero, in the limit. Most of Popper's methodological rules are designed to prohibit conventionalism. To employ Popper's "simplicity" in a conventionalistic philosophy would be a travesty of methodology. Thus, "simplicity," when reasonably defined, refutes conventionalism and implies falsificationism. And our first three points of criticism have now been 'rationally demonstrated.'

Now we arrive at our fourth point: Conventionalism cannot explain the growth of scientific knowledge. Here we must go back to the conventionalist's notion of "simplicity," or we would have nothing left to discuss. To a conventionalist, knowledge grows in one of two ways, either by sorting more "facts" into our file cabinet or by designing a "simpler" file cabinet.

The first of these cannot be seen as a growth of knowledge, since our organizing theory has not changed. Or, is the collection of "facts" themselves a growth of knowledge? To this question conventionalism provides no suitable response. Unless, of course, we choose only those "facts" that fit into our file cabinet. It seems that conventionalists are confused by the 'facts.'[210] For, as Popper shows, 'facts' are also 'theories.' How do we choose our 'facts'? Are they merely arbitrary conventions as well? The conventionalists are silent.

Again, it is Popper who solves a conventional riddle (recall the discussion of 'facts' in Chapter 2). Oddly enough, Popper sees 'facts' as conventions. Thus, to Popper, science is conventional in the accepting of only singular statements of fact but not in accepting theories or explanations. We simply must agree upon our methods of 'testing' 'basic statements.' Otherwise, we would not know what our 'theories' are talking about.

Thus, Popper's falsificationism is a form of conventionalism but only in the smallest possible degree. Falsificationism is conventional only in its treatment of singular instances and not in its view of universal statements or theories as is the philosophy of conventionalism. Yet this conventional nature of facts is unavoidable for any philosophy of science. How can our current methods of 'observation' or 'testing' be considered 'true'? The answer is: only by convention.

In order to 'test' our 'theory,' the 'facts' must be taken as given. Both cannot be 'tested' simultaneously. Yet, by the temporary acceptance of 'observations' science has learned to improve its methods of observation and 'testing.' It is Popper who recognizes the proper role of conventions in science, not the conventionalists. They do not understand the conventional role of 'facts' nor the "unconventional" role of scientific explanation.

Central to the conventionalistic philosophy is that knowledge grows when the "simplicity" of our file cabinet is increased. Progress occurs when a simple organizing system replaces a clumsy, unbalanced one. However, this conception of progress is not well illustrated by the "better" historical examples of scientific growth. Einstein's general relativity employs more complex formulae than Newton's, and it is more difficult to understand or less intuitive. Without Popper's notion of "simplicity," how can the relativity of time, space, and motion be seen as more "simple"? Quantum mechanics also has grown by increasing the complexity of its mathematical representations and by increasing the number and variety of the hypotheses that are employed to explain sub-atomic phenomena. Another example is given by Lakatos. He shows that the Copernican system cannot be regarded as "simpler" than the Ptolemaic system even though conventionalists have used this story as their best example.[211] Conventionalism and its notion of "simplicity" cannot explain these historical examples of scientific growth. Only Popper's definition of "simplicity" could salvage such a conventionalist "reconstruction" of history. Thus, conventionalism, when distinctly defined, cannot explain the growth of knowledge.

Finally, we turn to the most important reason to reject conventionalism. Conventionalism engenders a dogmatic attitude towards knowledge, and it fortifies all defenses from criticism. Conventionalism virtually eliminates the most effective instrument for the growth of knowledge - criticism. It is this characteristic of conventionalism that induced Popper to call _ad hoc_ defenses of a particular theory "conventionalist stratagems."[212]

> Whenever the "classical" system of the day is threatened by the results of new experiments, which might be interpreted as falsifications according to my point of view, the system will appear unshaken to the conventionalist. He will explain away the inconsistencies which have arisen; perhaps by blaming our inadequate mastery of the system. Or he will eliminate them by suggesting _ad hoc_ the adoption of certain auxiliary hypotheses, or perhaps of certain corrections to our measuring instruments.[213]

Whatever our criticism, the conventionalist will be unmoved, unless we can convince him that we have a smaller or "simpler" file cabinet. To regard theory as a mere convention cannot but severely restrict the domain of 'rational criticism.' Criticism of theory could only be viewed as arguing about words, since they are only conventions, and not as a rational analysis of genuine problems and their solutions. Thus, there is no role for 'rational criticism' in conventionalism. Only a convincing "proof" of "simplicity" or "convenience" is of interest to a conventionalist.

Conventionalism views knowledge like an accountant views a balance sheet. Knowledge is then a practical system of labeling items "debits" or "credits" and tabulating their balance. Questions concerning whether some item may be better seen as a credit rather than as a debit or whether an accelerated depreciation might better reflect the economic obsolescence of our equipment are irrelevant. The accountant will only change, and then with resistance, his bookkeeping procedure if we provide him with another that makes his "accounting" easier.

For the promotion of progress, the dogmatism of conventionalism must be considered excessive. It is one thing to hold onto a 'theory' until its implications are understood or until valid criticisms accumulate. But it is quite another to hold a theory immune to all genuine criticism. Kuhn describes the result of the conventionalism of "normal science." A "crisis" and an alternative "paradigm" are required before the conventionalistic "normal scientist" will consider criticism or potential falsifications of the traditional theory. And Popper shows us that such a "normal science" is unnecessary, and he describes its dangers (recall the first section of this chapter). We can always <u>choose</u> to be open to criticism and to learn from our mistakes.

Conventionalism contains the "myth of the framework" - the file cabinet. Yet it is less dogmatic about a particular framework than subjectivism. For we can, at will, change our file cabinet.

We can now see how Lakatos' MSRP represents a definitive move towards conventionalism from falsificationism. His "hard core" is the conventionalist's file cabinet that is 'irrefutable.' MSRP is a step backwards from falsificationism, but a step forward from conventionalism if we correct Lakatos' inconsistent treatment of 'falsifiability.' Lakatos gives conventionalism some reasonable method to choose theories, Popper's 'empirical content,' while retaining the conventional character of the research programme's "hard core." This is only a compromise and no synthesis of these two philosophies of science. Lakatos provides us with no methodological advantage over falsificationism, yet MSRP might be easier for conventionalists to accept.

From a broader perspective, we can find a pervasive difficulty with conventionalism. It solves no genuine problems, cannot lead beyond itself, and destroys itself. The only problem solved by conventionalism concerns the truth-status of theory. This problem is solved only by asserting it away: truth is not a problem but only a convention. Some may see this as an improvement over the older views that saw theory as absolute "truth." Yet a better description results when the conventionalistic view of truth is seen as a defense from criticism of particular theories and the Euclidean programme in general. Instead of forthrightly admitting that a scientific theory might be 'false' or just mistaken, conventionalists circumvent the question by asserting that the issue is irrelevant. Theories are only conventions neither true nor false. Thus, conventionalism does not solve the problem, for the question of "truth" still remains.

The self-destructive nature of conventionalism is stated by Popper and Schlick. Popper summarizes the conventionalistic position with a second look at "simplicity."

> The conventionalist concept of simplicity turns out to be indeed partly aesthetic and partly practical. Thus the following comment by Schlick applies to the conventionalistic concept of simplicity but not to mine: "it is certain that one can only define the concept of simplicity by a convention which must always be arbitrary." It is curious that conventionalists themselves have overlooked the conventional character of their own fundamental concept - that of simplicity. That they must have overlooked it is clear, for otherwise they would have noticed that their appeal to simplicity could never save them from arbitrariness, once they have chosen the way of arbitrary convention.[214]

Thus, the philosophy of science that sees theory as arbitrary convention is itself merely an arbitrary convention. Conventionalism can defend itself or a particular theory only by reference to convention. Therefore, conventionalism cannot lead anywhere, and it collapses into nothing upon the arbitrary weight of its own convention.

Though conventionalism, like all philosophical positions, is "irrefutable," there are many good reasons for not embracing conventionalism. We have attempted to show that:

(1) Poincaré correctly describes the ontology of a formal system. Yet his original problem-situation is erroneously generalized to 'science' and is dramatically contradicted by the later theories of Einstein. This philosophy of science is founded upon a mistaken understanding of science.

(2) The "conventionalist mistake" permeates the entire philosophy. Theories are nothing more than conventions. They are words and symbols without content.

(3) Conventionalism's only regulative concept, "simplicity," is insufficient. Popper's definition of "simplicity" is adequate but inconsistent with conventionalism.

(4) Conventionalism cannot explain the growth of scientific knowledge. Significant examples from the history of science do not reflect a conventionalistic construction.

(5) Dogmatism is the related heuristic of conventionalism. As such, it can only inhibit the growth of knowledge.

(6) Conventionalism solves no genuine problems and does not lead beyond itself. Thus, it has no philosophical or practical value.

(7) Conventionalism is grounded upon convention. The philosophical structure of conventionalism collapses upon itself into nothing.

However, Poincaré's conventionalism is not entirely without value. It clearly describes the character of formal theoretical systems and the issue of implicit definitions which were greatly misunderstood before Poincaré. By criticizing the conventionalistic notion of science, we learn more about the nature of science. Popper himself sees value in conventionalism. "The philosophy of conventionalism deserves great credit for the way it has helped to clarify the relations between theory and experiment. It recognized the importance, so little noticed by inductivists, of the part played by our actions and operations, planned in accordance with convention and deductive reasoning, in conducting and interpreting our scientific experiments."[215] Yet, he adds:

> We, and those who share our attitude will hope to make new discoveries and we shall hope to be helped in this by a newly erected scientific system. Thus we shall take the greatest interest in the falsifying experiment. We shall hail it as a success, for it has opened up new vistas into a world of new experiences. And we shall hail it even if these new experiences should furnish us with new arguments against our own most recent theories. But the newly rising structures, the boldness of which we admire, is seen by the conventionalist as monument to the "total collapse of science" as Dingel puts it. In the eyes of the conventionalist one principle only can help us to select a system as the chosen one from among all other possible systems: it is the principle of selecting the simplest system - the simplest system

> of implicit definitions; which of course means in practice the "classical" system of the day.[216]

3.3.4 Instrumentalism: Knowledge as Financial Instruments

> It is important to clarify the relation between conventionalism and instrumentalism. Conventionalism rests on the recognition that false assumptions may have true consequences; therefore false theories may have great predictive power. Conventionalists had to face the problem of comparing rival false theories. Most of them conflated truth with its signs and found themselves holding some version of the pragmatic theory of truth. It was Popper's theory of truth-content, verisimilitude and corroboration which finally laid down the basis of a philosophically flawless version of conventionalism. On the other hand some conventionalists did not have sufficient logical education to realize that some propositions may be true whilst being unproven; and others false and approximately true. These people opted for "instrumentalism": they came to regard theories as neither true nor false but merely as "instruments" for prediction. Conventionalism, as here defined, is a philosophically sound position; instrumentalism is a degenerate version of it, based on a philosophical muddle caused by a lack of elementary logical competence.
>
> \- Lakatos [1978a], p. 106

Instrumentalism is a radical form of conventionalism. Conventions become instruments for prediction, "truth by convention" - pragmatic truth, and "simplicity" - "predictive success." All our former criticism of conventionalism holds if "predictive success" is found as inadequate as "simplicity." Nonetheless, since instrumentalism currently receives considerable support (for example, Friedman's "positive economics"), it need be discussed on its own "terms."

Instrumentalism takes the position that scientific theories are only instruments for prediction. Instrumentalism claims that theories do not explain anything, and they have no relation to 'reality.' Instrumentalism is grounded in pragmatism, where 'truth' is superseded by 'usefulness.' Theories can be compared only in terms of their 'usefulness.' Yet, 'usefulness' is not an 'objectively' well-defined term, as 'truth' is. It can be defined only relative to a known purpose, teleologically. In disciplines known to engage in normative disputes often disguised as 'scientific,' it would be highly dangerous to choose scientific theories solely upon a teleological criterion that has practically unavoidable normative connotations. To an instrumentalist, "facts" have cash value and "theories" are like financial instruments creating rights to cash under stipulated conditions.

In discussing Darwinian epistemology before Popper's falsificationism, Baldwin spoke of the danger of turning a trial and error epistemology (which he termed instrumentalism, yet is quite similar to falsificationism) into instrumentalism (which he termed pragmatism).

> Pragmatism (or instrumentalism) turns instrumentalism (or falsificationism) into a system of metaphysics. It claims that apart from its tentative instrumental value, its value as a guide to life, its value as measured by utility, seen in consequences of its following out, truth has no further meaning. Not only is all truth selected for its utility, but apart from its utility <u>it is not truth</u>. There is no reality then to which truth is still true, whether humanly discovered or not; on the contrary, reality is only the content of the system of beliefs found useful as a guide to life.
>
> I wish to point out that, in such a conclusion, not only is the experimental conception left behind, but the advantages of the Darwinian principle of adjustment to actual situations, physical and social, is lost; and if so interpreted instrumentalism defeats itself.[217] [Parenthesis added.]

No one disagrees with the contention that theories can be used as instruments or that theories can be applied 'usefully' to given ends. But most scientists and philosophers would disagree with the assertion that <u>theories</u> are <u>only</u> instruments. Instrumentalism cannot be modified from this extreme view. "It makes no good sense to speak of modified instrumentalism, since the bold idea that science is merely instrumental for technology with the slightest modification becomes rather trite."[218] Any such modification would bring instrumentalism squarely back to conventionalism, falsificationism, or whatever and destroy the uniqueness of this philosophy of science.

Perhaps the strongest argument for instrumentalism was given by Berkeley.[219] He based his analysis on a theory of language which maintained that any dispositional word (for example, gravity, breakable or soluble) is meaningless, for it connotes nothing that can be observed. Dispositioned words do not describe anything which can be directly measured, but only give us the ability to derive other statements which might describe 'observable facts.' We cannot 'observe' the "forces of gravity," but we can 'observe' the results of those "forces" - the falling (or motion) of objects.

Thus, by this thesis scientific theories are meaningless, since all use some dispositional words. The instrumentalist might then argue that the only meaning of the dispositional words and, consequentially, the theories resides in the predictions about observables which these words allow; and further that these predictions exhaust the meaning of the dispositional words and hence the theory. Though this argument seems conclusive, it is irrelevant because it rests upon the pseudo-problem of meaning.[220] The view that a theory is only the sum of its <u>words</u> is wrong. The meaning of a theory (if such a thing can be meaningfully discussed) goes beyond its words. Words are arbitrary conventions or instruments, but not 'theories.' The analysis of words is always a pseudo-problem without content, thus irrelevant and often misleading.

This argument for instrumentalism assumes that theories have no content which, of course, implies that 'use' is the only value of a theory. But since 'theories' have content, this argument collapses.

Any such argument for instrumentalism involves the "conventionalist mistake" of confusing a theory with its symbolic representation. Scientific theories express "meanings" that go beyond its symbols and implicit definitions. Implicitly and explicitly some theoretical terms are correlated to 'observable' entities. Yet such correlations rarely exhaust the "meaning" of a scientific theory. They can carry excess "meaning" that suggests new ways to 'observe' or 'test' 'reality.' About "meaning" Popper says,

> In science we take care that the statements we make should never depend upon the meaning of our terms. Even when the terms are defined, we never try to derive any information from the definition, or base argument upon it. This is why our terms make so little trouble. We do not overburden them. We try to attach to them as little weight as possible. We do not take their "meanings" too seriously. We are always conscious that our terms are a little vague (since we have learned to use them only in practical application) and we reach precision not by reducing their penumbra of vagueness, but rather by keeping well within it, by carefully phrasing our sentences in such a way that the possible shades of meaning of our terms do not matter. This is how we avoid quarrelling about words.[221]

Popper has argued convincingly throughout his career against instrumentalism.[222] His refutation of instrumentalism may be interpreted as the following. Instrumentalism is an incorrect philosophy of science since it fails to account for:

(1) the difference between "pure" science and applied science (e.g., physics vs. mechanical engineering);

(2) the growth of scientific knowledge;

(3) the excess content of richer theories.

Any philosophy of science which cannot be used to explain all of these points is inadequate.

Instrumentalism holds that there is no difference between the pure and applied sciences. Thus pure scientific theories are nothing but computational rules. Yet, there is a difference, and any difference is a refutation of the applicability of instrumentalism to the pure sciences.

Computational rules are selected for their "simplicity" and applicability, while 'theories' are rationally preferred for their 'truth' or 'corroboration' and 'empirical content.' Like verification, applicability is not analogous to 'corroboration,' for applicability seeks 'irrefutability.' Nor are computational simplicity and 'empirical content' comparable. Thus, there is a difference between the pure and applied 'sciences.' This difference is conceptually equivalent to the differences between Popper's "simplicity" and the more intuitive notion. All applied science is teleological, while the "pure" sciences can be (and, at least, the best theories are) epistemological, that is, knowledge producing.

For example, before sensitive machinery (e.g., jets, turbine blades, atomic reactors) are used, scientists test the theory of the equipment's operation. The magnitude of the stresses employed in these tests is sufficient to destroy the equipment and in some sense the operational 'theory.' Such "testing to destruction" is necessary for many 'useful' applications and underscores the importance of 'testing' for a 'theory's truth.'

To an instrumentalist there are no attempted 'refutations,' only 'useful' (or costly) "predictions." This lack of severe 'testing' and the ambivalence toward 'truth' would cause a proliferation of 'theories,' since the most accurate or 'useful' predictor of any given situation is usually unique to that situation. If 'scientific theories' were not tested to destruction, there would be no way of determining their applicability. These deficiencies represent a serious reduction of the "utility" of theories. Without severe 'testing,' for 'refutation,' instrumental applications would be equally likely to be as costly as 'useful,' or as risky as safe. Thus, even on its own terms an instrumental science defeats itself. This type of severe 'testing' is precisely what characterized falsificationism, without which our instruments might be very dangerous to apply. Is not the potential cost of misapplied theories sufficient to demand caution and harsh 'testing'? Must one be reminded of the consequences of the poorly 'tested theories' of the biological impacts of radiation, or of the safety of various insecticides? Instrumentalism can make no differentiation of the issues relating to applied and "pure" sciences, thus it collapses.

Neither can instrumentalism explain the major scientific advancements. The theories of Newton and Einstein did not immediately predict more accurately (nor more 'usefully') than their predecessors. In fact, both theories forced applied scientists to develop better instruments before the newly predicted phenomena could be seen. For much of the observable phenomena, these theories showed no increase in accuracy until the 'observational theories' were revised.

Each example of scientific progress mentioned in this paper did not merely predict more accurately, but all predicted phenomena not yet 'observed' and taught us how to discover new 'facts.' This excess content cannot be explained by instrumentalism.

> It took a Popper to hammer it into the minds of the philosophers of science (and into the mode of speech of scientists who knew this all along) that theories are instrumental (without quotes) in the growth of observational knowledge while increasing our understanding. Just because theories have "surplus" content, partly through the use of strong logics and partly through the use of theoretical terms with "surplus" meaning that goes beyond their "operational definitions," they have growth-inducing power, by encouraging new types of observations and experiments.[223]

How can the unknown be 'useful' or practical? Only by measuring "usefulness" by 'truth' can we provide any value to making the unknown known. Instrumentalism is inconsistent with the growth-inducing, excess content of theories, and any modification of instrumentalism to account for scientific growth would transform instrumentalism into falsificationism.

Instrumentalism's regulative concept "predictive success" is as problematic as the conventionalist's "simplicity." How do we identify and compare "predictive success"? Without some notion of "truth," "predictive success" makes no sense. Instrumentalists implicitly assume the "truth" of their observations. For how else could they tell how well their theories "predict" or whether their "predictions" are realized? Thus, the "instrumental mistake" is to believe that there is "use" without "truth." One can only evaluate "prediction" given some unproblematic basis of "facts." Since instrumentalists do not speak of the 'testing' of our 'facts' or of the conventional nature of these "facts," they are assuming them to be true.

We might attempt to solve this problem of "prediction" by using Popper's 'corroboration' as "predictive success." Such a solution would satisfy our general intuitions about "prediction" and remove all the ambiguity that revolves around the instrumentalist's notion of "prediction." But again, to do so would be to destroy instrumentalism. For then theories could be 'true' or 'false,' and the distinct character of instrumentalism would vanish.

Thus, to be consistent with instrumentalism we must find some other method to identify "predictive success." But there is no reasonable method of determining a theory's "predictive success." It is absurd to define some metric of the difference between what a given theory "predicts" and the "facts" as the only measure of the theory's quality.

Such measures of "predictive success" would entail a number of unfortunate characteristics. First of all, there is no guarantee that the theory which currently "predicts" "best" would continue to do so. This problem is equivalent to Hume's criticism of causation and induction. Or, in other words, a 'false' theory might provide us with the "best predictions." Falsificationism, too, has a similar problem, but it at least minimizes this error by critically 'testing' both 'theory' and 'fact.'

To use such a measure of "predictive success," then, rests upon an assumption of some type of induction. Since instrumentalism asserts that theories can be chosen for their "predictive success," without 'falsification,' some type of induction from the "facts" must be employed. "Predictive success" commits the "fallacy of induction." Yet the facts can only 'falsify' a theory and not "justify" it. Instrumentalism erroneously assumes the existence of some method of induction in order to "apply" its regulative concept, "predictive success."

Instrumentalism assumes both a notion of "truth" and a method of induction. If an instrumentalist attempts to reject these assumptions, "predictive success" will be hopelessly ambiguous and unable to identify knowledge or its growth. Therefore, instrumentalism is an inadequate philosophy of science. Have you not wondered why the more sophisticated models which minimize "past predictive performance" (or least squares) often predict the future less satisfactorily than their naive alternatives? This capricious nature of "predictive success" might provide a clue to such a phenomenon.

History provides us with at least two excellent examples of instrumentalism in action. Astrology, the oldest form of systemized knowledge, most closely resembles an instrumental science. For millennia the stars were used as instruments of predictions. Everything from wars, pestilence, famine, and the weather to the birth of the King's son was predicted by these instruments. Astrology is not an unfair example, but the <u>only</u> example of a purely predictive science. Astrologers were often the wisest and most knowledgable men of their day and were seriously concerned with accurate predictions (Kepler was an astrologer). However, in their zest for 'useful' predictions, they neglected to <u>explain</u> the interconnections of the phenomena under study. It is this lack of explanation, and not the predictive inaccuracy, which explains why astrology did not become a 'science.' There is nothing which preempts the use of the stars and planets as an instrument for the prediction of human and natural events (the sun spot theories of drought cycles and their effects on stock market prices may be said to support the astrological approach). In fact, astrology could still be used to predict all these events, since the vast number of degrees of freedom available in the movement of the stars guarantees the existence of astrological transformations which are highly correlated to any other chosen phenomena, particularly if we are allowed a trial and error search for the most 'useful' transformation and/or if we predict the past.

Another historical example of instrumentalism deals with Galileo's clash with the Church. Cardinal Bellarmino had no objection to Galileo's teaching of the Copernican theory as long as Galileo would stress in his lectures that it is only a mathematical trick, an instrument, which eased the calculations. The Cardinal chose to adopt the instrumental view of Andress Osiander who said of the Copernican theory: "There is no need for these hypotheses to be true, or even to be at all like the truth; rather, one thing is sufficient for them - that they should yield calculations which agree with the observations."[224] Galileo agreed that the Copernican system had instrumental value, but he also believed that it was a true description of the universe. Yet, for the mere belief that the earth actually moves, Galileo opposed the power of the Church and spent the remainder of his life under arrest.

But the instrumentalists would have us believe that Galileo was a fool and the movement of the earth a meaningless fiction, valuable, if at all, only in the "utility" of the resulting predictions. However, Galileo knew that the earth moved. For what else could explain his 'corroborating observations' of the phases of Venus or of the mini-Copernican system of Jupiter? Science adopted Galileo's methods, and most would agree that there was no 'science' before Galileo. Science has advanced far beyond the theories of its founder. Newton's theory has replaced those of Galileo and Copernicus and Einstein's theory has replaced Newton's. Yet, science has vindicated not only Galileo's methods but also his beliefs. For even after the last four hundred years of accumulated scientific and instrumental knowledge, Galileo could still accurately say of the earth, "and yet, it moves." It can only be hoped that four hundred years hence someone may say the same of our knowledge.

Now we can see that instrumentalism is subject to the same criticisms as conventionalism. Instrumentalism's regulative concept is as ambiguous and inadequate as conventionalism's "simplicity." Perhaps "predictive success" is even more subjective than "simplicity." "Predictive success" carries with it some idea of "utility" or "usefulness." The latter can only be defined relative to a given "problem at hand," and this may change with every "application" of a theory. Unless "predictive success" and "usefulness" are grounded upon some other concepts, they tell us nothing beyond: "A theory may be usefully applied everywhere that it is a "predictive success." But instrumentalism is silent about how.

Like conventionalism, instrumentalism does not lead beyond itself, solves no problems, and swallows its tail. Similarly, instrumentalism only asserts a terse answer to the question of "truth": theories are only "useful" or instruments of "predictions," neither true nor false. Instrumentalism's vacuous nature is even more apparent. To instrumentalists, theories are only practical instruments; the purpose of science is practical value; and "truth" is only practical value. To defend the practical value of a theory or a philosophy by assuming a "practical truth" is, at best, empty and fails to provide any rational support. Thus, instrumentalism swallows its own tail before we can "cash in" our "financial instruments."

To give practical value to our scientific theories, we require something beyond the assertion that they are "useful." As a by-product, falsificationism provides this practical value that instrumentalists desire. Scientific theories will have practical value only to the extent that they are 'true' and that we obey their limits of application.

How else may we discover these limits other than by severely 'testing' in the attempt to 'falsify'? Only if we subject our theories to the harshest criticism can we expect the by-product of 'usefulness.' It would be naive to believe that the reverse is true: that truth might be a by-product of "useful" applications. Schopenhauer also saw this asymmetry. "Philosophy is misused, from the side of the state as a tool, from the other side as a means of gain. Who can really believe that truth also will thereby come to light, just as a by-product."[224]

Instrumentalists are quick to point out that 'falsified theories,' such as Galileo's law, are useful in application. Yet they are mute regarding the extent of their "utility" or in specifying in which cases the application is sufficiently "useful." Only by 'falsifying' these past theories and replacing them with others of greater 'corroborated' content do we know how 'useful' these past theories might be in application and how great our error of "prediction" in each application.

Instrumentalism provides a sound methodology for engineering or other applied sciences, if we assume that there exists a corresponding "pure" science. But it provides no foundation for "pure" science itself. For those scientists who are interested only in the application of science, instrumentalism may be adequate. But for those who seek 'truth' or a growth of scientific knowledge, instrumentalism is not 'useful.'

3.3.5 Logical Positivism: Only the Facts Have Meaning[226]

Logical positivism is probably the most difficult philosophy of science to adequately characterize. Its development was intricate, its influence insidious. Although logical positivism is entirely a twentieth-century story, its roots run much deeper. Because of the importance of its historical context, we concentrate more upon this history than in previous discussions. Yet, what follows is only the briefest of historical sketches.

Logical positivism is important because it has a significant influence upon contemporary philosophical thought. Also, aspects of its empiricism have entered the economic methodological debate (for example, Samuelson's operationalism). However, the current methodology of "positive economics" is unrelated to logical positivism. Here, logical positivism is discussed primarily to clarify and contrast Popper's philosophy, and secondly to distinguish it from "positive economics" (the latter is discussed with Friedman's methodology).

Though the currents of logical positivism may be seen to rise and recede between the high tide of Wittgenstein's Tractatus and the low tide of Popper's falsificationism, this story should begin with Russell and Whitehead's Principia Mathematica. They attempted to reduce mathematics, philosophy, and science to logic. Russell and Whitehead's theory of types was a language of languages in which all must speak to be clear.[227]

Philosophy began to be seen as a branch of logic. A student of Russell's, Ludwig Wittgenstein, advocated the position of logical atomism. "The world is the totality of all facts and these facts have a peculiar relation, or non-relation, to one another; 'Any one fact can either be the case, or not be the case and everything else remains the same.'"[228] Wittgenstein's position is to reduce all knowledge to the logic of language of Principia and to reduce its content to the "facts."[229] Thus, the Tractatus may be seen as a modern version of the Empirical programme that uses the most advanced version on the Euclidean programme - Principia Mathematica. Yet the resulting philosophical position is no more secure than former attempts to ground knowledge in empirics. Like all other forms of empiricism (excluding falsificationism), logical atomism involves the fallacy of induction. What carried Wittgenstein's vision was its grand scope and the belief that the Principia finally provided an unambiguous framework for logic and reason.

The fate of Principia is now a well-known story. In the 1930's, Kurt Gödel discovered paradox and ambiguity in the heart of the Principia.[230] Ironically, the Principia was designed to remove all such paradox and ambiguity from logic; yet there it was. Philosophers were not quick to incorporate Gödel's undecidability problem into their logic, for at this time logical positivism was at full swing. Tractatus became the inspiration for a group of Viennese intellectuals, the Vienna Circle, and the seminal statement of logical positivism.

Logical positivism began in the 1920's in post-war Vienna where Popper was a college student. Although the influence of the Vienna Circle was wide and their membership long, the more notable members were Carnap, Feigl, Frank, Kraft, Menger, R. von Mises, Waismann, Nuerath, Reichanbach, and Schlick. Each member advanced, somewhat, his own philosophy, yet the work of Carnap is most characteristic of the group's shared position. Thus, our analysis will more reflect Carnap's work than the many refinements of others.

Of only historical interest is Popper's connection with the Vienna Circle. Popper was friends with a number of these logical positivists, and his own philosophy developed largely as a criticism of logical positivism.[321] But Popper was never a member of the Vienna Circle nor shared their common views. The problem with making too much of Popper's connection is that it has been a source of the misunderstanding of Popper. The Popper connection is dubbed the "Popper legend" and is stated as:

1. Popper is a logical positivist and a member of the Vienna Circle.

2. Popper, in agreement with other logical positivists, sought a criterion of meaning in order to proclaim metaphysics as meaningless.

3. Popper rescued the old verifiability criterion of meaning by replacing it with the falsifiability criterion of meaning.[232]

Regardless of the historical beliefs of philosophers, the legend is wrong on all points.[233] This fact is clear to anyone who has read Popper. The correct view, and the only view consistent with Popper's philosophy, is that Popper was influenced by logical positivism only by criticizing it. At almost every step in the development of logical positivism, Popper took an opposing view. And much of Popper's criticism became incorporated in subsequent statements by the logical positivists. It was logical positivism that was influenced and changed by Popper, not the other way around.[234]

At any rate, logical positivism began in the 1920's with some grandiose goals. In particular, logical positivists wish to solve:

(P.1) The problem of demarcation: A criterion is necessary to separate meaningless sentences from meaningful ones. The solution is then used to separate science from metaphysics.

(P.2) The problem of derivation: A method to derive "valid" statements from "valid" "atomic sentences" is also required. The logical system of these "valid" statements is then the totality of knowledge.

(P.3) The problem of language: The proper language of science is required to speak about knowledge. In this language only scientific statements would make sense, and metaphysics would be only senseless utterances.

Associated with these problems are the central tenets of logical positivism:

(T.1) The verifiability criterion of meaning: A statement must be verifiable by the "facts" to be meaningful. This view is associated with a physicalist notion of the "facts." They are called "atomic sentences" or "protocol sentences" and are used to verify "molecular sentences."

(T.2) Ultra-empiricism: All knowledge can be logically constructed from elementary perceptions or "protocol sentences."

(T.3) Anti-metaphysics: Philosophy is only the analysis of language and meaning. There are no genuine philosophical problems, and all "metaphysics" is meaningless.

If we describe the above philosophical positions as the "hard core" of logical positivism, then we would require a great number of pages to describe the "protective belt of auxiliary hypotheses" that were used to specify and defend logical positivism. In such a framework, we could see "conventionalist stratagems" of logical positivists in changing their minor hypotheses in response to Popper's criticisms.

The most important thesis of logical positivism is the verifiability criterion of meaning. It asserts, "a statement is meaningful if it can be verified; statements of which no verification is possible are meaningless 'pseudopropositions.'"[235] The logical positivists first required derivation from true "protocol sentences" to serve as verification.

Popper was quick to point out that such a criterion is "'too narrow and too wide' ... for it excluded unintentionally the theories of science and included as meaningful, just as unintentionally, some typical existential statements of metaphysics."[236] The asymmetry of universal and existential statements (as discussed in the Prologue) makes it impossible to go from 'true' singular statements of fact, "protocol sentences," to universal statements, scientific theories - the fallacy of induction. Thus, the logical positivists define scientific statements as meaningless. As Popper notes, this implication is quite the reverse of what the logical positivists intended. Even more ironic is that this criterion of meaning implies that any criterion of meaning must itself be meaningless, for no criterion of meaning could possibly be derived from the "facts." The discovery of this absurdity leads Popper to assert that the entire question of meaning is a pseudo-problem that we should simply ignore.[237]

The fact that Popper recognizes the self-destructive nature of any criteria of meaning makes absurd the notion that Popper wishes falsifiability to replace verifiability as the criterion of meaning. For then falsifiability would be meaningless. As discussed earlier, falsifiability cannot be "falsified" in the same sense as scientific theories. Thus, the assertion that falsifiability determines meaning would assert its own meaninglessness. This self-denying character of the problem of meaning was also recognized by Wittgenstein. "My propositions are elucidatory in this way: he who understands me finally recognizes them as senseless ...."[238] Although logical positivism changed its specific criterion of meaning in response to Popper, it was maintained as a central thesis. One can only wonder how the logical positivists persisted in their self-proclaimed, senseless pedantry.

Popper changes the problem from one of meaning to one of the demarcation of science by its methods, not its words. While 'falsifiability' demarcates science from non-science, it does not imply that non-science is meaningless nor that science is somehow "more meaningful." The problem of method is a genuine problem; the pseudo-problem of meaning is empty and self-denying. The logical positivists generally accepted Popper's criticism and his notion of 'testability.' This is strongly evidenced by the "classic" statement of logical positivism, Carnap's Testability and Meaning. Here Carnap still asserts a criterion of meaning and longs for some way to "justify" theories by "protocol sentences." Yet Carnap only requires some form of "confirmation" or "testability" rather than complete "verification." Carnap never gave up his search for a language of science nor the criterion of meaning, but his position became successively weaker.[239]

Popper's criticisms of this issue of meaning must now be seen as a conclusive refutation of logical positivism. Logical positivism is dead. It would be hasty to proclaim that Popper was its slayer; for logical positivists never succeeded in identifying their "meaningful" language of knowledge.[240]

All their efforts were doomed to fail. Meaning cannot be "nailed down" by abstract language construction, nor will such an effort remove all ambiguity or paradox. This can be seen by the four levels of discourse that are introduced in the Prologue. This vague language system is not complete nor free of paradox. The fourth level of philosophical discourse is meant to be vague and infinitely elastic. Otherwise, we would be forced to add a fifth level to talk about the fourth, a sixth level to talk about the fifth, and so on. We cannot even establish the meaning of any word in all its shades or levels without getting lost in an infinite regress. But then, why should we? Establishing meaning is like establishing truth, both are impossible and unnecessary. All we need to do is to state our positions as clearly as we can and further clarify as it becomes necessary. Language is no "philosopher's stone." Only how words are related to other words, concepts, and processes matters. If we reject the problem of "meaning" as genuine, then logical positivism must fall into this emptiness of "meaning."

There is much more to the story of logical positivism than here stated. To document each new version of logical positivism, Popper's criticism, and the positivist's defense would itself occupy no slim volume.[241] The logical positivists eventually found themselves holding some other position, generally some type of verificationism or inductionism. These are discussed in the next section under the title probablism. Yet, we should here mention some general criticisms of T.2 and T.3.

The attempt to reduce all knowledge to some type of true "protocol sentence" has two irreconcilable criticisms. The first is the fallacy of induction. It is simply impossible to derive general statements from a finite set of "facts," nor can scientific knowledge be reduced to such "facts." Secondly, the "facts" themselves are not "true" or "valid" or even capable of being established. Thus, there is no way to give an unproblematic basis for knowledge, regardless of our preferences.

With respect to logical positivism's position on philosophy, little need be said. In their asserted role as analysts of language and meaning, the logical positivists were miserable failures. They never succeeded in their goals of identifying meaningful statements nor in developing the appropriate language of knowledge. And, to the assertion by a philosopher that philosophy can pose no genuine problems, we need say nothing. There is no reason for words. The best answer to a self-denying statement is a simple nod of the head, made without interrupting our effort to pose genuine problems and their tentative solutions.

3.3.6 Probablism: Although We Cannot Know, Our Ignorance Is Measurable

Modern philosophy almost universally rejects the notion that we can know with certainty. Yet many philosophers still maintain the pretense that theories can somehow be verified. And few have realized that fallibilism is inconsistent with verification, in any form. Modern verificationists have chosen a number of labels and a multiplicity of methods for justifying theories. Nonetheless, each holds one view in common: alternative theories may be judged by their evidential support, and a measure of their truth, probability, or confirmation may be determined conditionally upon this evidence. Usually the word "probability" is ascribed to this measure, hence our term "probablism."

Before discussing probablism, our bias against verificationism should be restated. Following Popper, serious attempts to 'falsify' our knowledge are all that is required. Verification, in any sense, is unnecessary and also misleading. To establish the verification or the probability of a theory can only foster dogmatism. Yet, theories remain fallible regardless of the assertion that their verification or probability has been established.

Popper was the first to convincingly argue that confirming evidence does not count at all, unless it is the consequence of a failed 'refutation.'[242] Confirmations are easy when we look for them. The most ridiculous and the most "false" theories can be confirmed without limit, if we only look in the "right" places or in the "right" manner. Observations and evidence, in general, require an interpretive theory. Thus, if our "observations" are made in the light of the theory in question, confirmation can be seen everywhere. Since it is all but unavoidable to abuse such confirmations, verificationism, in all its forms, should be rejected as a methodology of science.

J. M. Keynes is chosen as representative of probablism. This choice is justified by the frequency of Popper's references to Keynes in The Logic of Scientific Discovery. Keynes provides one of the earliest axiomatic formulations of probability theory - subjective probability at that.[243] In Treatise On Probability, Keynes attempts to establish a theory of knowledge based upon subjective probability. "In metaphysics, in science, and in conduct, most of the arguments upon which we habitually base our rational beliefs are admitted to be inconclusive in a greater or lesser degree. Thus for a philosophical treatment of these branches of knowledge, the study of probability is required."[244] In other words, Keynes asserts that fallibilism implies probability.

Although Keynes's contribution was one of the most comprehensive, it is more confusing than clarifying. Keynes cannot see probability in any objective sense, whether by the frequency or propensity of interpretation. Probability only measures our beliefs about events and not the events themselves. It reflects only our ignorance and not any quality of the phenomenon itself.

Keynes clearly states his subjectivism in the beginning and maintains this view throughout Treatise On Probability. "The terms certain and probable describe the various degrees of rational belief about a proposition which different amounts of knowledge authorize us to entertain. All propositions are true or false, but the knowledge we have of them depends on our circumstances."[245] Yet, Keynes believes that probability is objective to the degree that it enables us to make logical inference from given information.[246] Both the origin and interpretation of probability are subjective, only the intermediate manipulation of axioms is objective. "What particular propositions we select as premises of our argument naturally depends on subjective factors peculiar to ourselves; but the relations in which other propositions stand to these, and which enable us to probable beliefs, are objective and logical."[340]

To the degree that Keynes's theory is subjective, it is susceptible to much of our previous criticism of subjectivism. It seems that Keynes's subjectivism is the result of Laplace's demon, or the extreme determinism of the nineteenth century. "Now a careful examination of all the cases in which various writers claim to detect the presence of 'objective chance,' confirms the view that 'subjective chance,' which is concerned with knowledge and ignorance, is fundamental, and that so-called 'objective chance,' however important it may turn out to be from the practical or scientific point of view, is really a special kind of 'subjective chance' and a derivative type of the latter. For none of those adherents of 'objective chance' wish to question the deterministic character of natural order."[248] Thus Keynes, like Einstein, believed that a probablistic description of 'reality' somehow refutes determinism.[249]

That this view is mistaken should be obvious. If natural law is capable of a prediction or explanation of the probability of events, is that not determined? Only the extreme form of omniscient determinism is in conflict with a probabilistic explanation. But is such a position necessary for adequate explanation or for phenomena to be thus determined? As Einstein states, quantum mechanics probably rules out the possibility of a complete and exact description of atomic phenomena.[250] Yet are not theories which limit or forbid possible behavior, and all 'scientific theories' do, deterministic? At least what cannot happen -

potential 'falsifiers' - is determined, and we require no more of knowledge.

A comprehensive discussion of "free-will" vs. determinism is not necessary, for we only wish to point out that the extreme determinism that characterized nineteenth-century science is unnecessary for scientific knowledge. Theories that use probabilities can be just as 'objective' as those that do not. Thus, Keynes's argument fails to support the notion of subjective probability. His argument follows only if we assume that there exist explanations for all phenomena that contain no probabilities or errors and that the beliefs of those "various writers" in this complete determinism is the 'truth.' Only then would it follow that probability could measure only our ignorance. Yet, the study of quantum mechanics and social phenomena suggests that "nature does play with dice."

We must reject Keynes's subjective interpretation of probability as a degree of "rational belief." As Tom Settle cogently recognizes,

> "What is rationality," a question that can easily get left behind by inquirers after "What is belief"?, unless an equation is made of the type: "rationality = the way people's minds work" = "the way they choose beliefs." I cannot see good reasons for making an equation of that sort; but I can see good reasons for not doing so .... Belief notoriously conceals and protects errors (as every believer knows, at least with respect to his opponent's beliefs).[251]

We can remove this unfortunate aspect of Keynes's theory not only without adversely affecting its epistemological significance but also with the positive benefit of avoiding some of the absurdities that result from Keynes's subjectivism. One such absurdity involves Keynes's confusion concerning the relationships among probability estimates, the accuracy of such estimates, conditional probability and certainty.

> According to this view, an increase in the amount of evidence strengthens the probability of the probability, or as De Morgan would say, the presumption of the probability. A little reflection will show that such a theory is untenable. For the probability of X on hypothesis h is independent of whether as a matter of fact X is or is not true, and if we find out subsequently that X is true, this does not make it false to say that on hypothesis h the probability of X is 3/4. Similarly the fact that X/h, h_2, is 2/3 does not impugn the conclusion that X/h is 3/4.[252]

Keynes is quite correct to suggest that it is possible for a conditional probability to decrease as evidence accumulates; thus the probability of X given h_1, "X/h_1," may equal 3/4 with "X/h_1h_2" = 2/3. But he is wrong to assert that the probability of X given h_1 is independent of whether X is true. For all probability systems hold that if X is true, P(X) = 1, then the probability of X given any other event is still 1 (assuming, of course, that this other event is a possible, non-zero probability). Such a result directly follows from the use of the addition and multiplication rules. Yet, because Keynes sees probability as subjective belief, our beliefs must influence probabilities, regardless of the 'truth' of the event.

The subjective notion of probability also leads to absurd locutions, such as "the probability of probability" which to Keynes means the degree of "rational belief" of the degree of "rational belief." Words aside, such meta-notions of probability or beliefs only point to the infinite regress that lies at the heart of this formulation. Keynes cannot distinguish between probability and the accuracy of our estimate of it, as can those who hold an objective notion of probability. For if probability is merely "rational belief," either our "rational beliefs" are infallible or we must measure their accuracy by the "rational belief" about a "rational belief." We have no recourse to measure accuracy by the objective notion of actual frequency of occurrence. This absurdity is also mentioned by Popper while referring to a different passage from Keynes.[253] It seems clear that a consistent subjective view of probability must inevitably lead to such absurdities. Thus, to continue our discussion of probablism in general, we must interpret Keynes's theory as grounded upon an objective notion of probability.

Keynes's purpose is to use probabilities as justifications for our beliefs in particular propositions and for theory choice. Keynes sees this as a probabilistic form of induction which he terms "inductive correlation."[254] Yet induction is impossible, including any form of probabilistic induction. All the forms of probablism or verificationism involve this same weakness, and it is this deficiency that destroys probablism as an epistemology or methodology of science.

To prove the "fallacy of probabilistic induction," all we need to do is to consider Bayes's theorem. Bayes's theorem is a result of formal science and as such is not susceptible to empirical or philosophical criticism. It follows directly from our definitions of probability. For this application, we may write Bayes's theorem as:

$$P(T_0/E) = \frac{P(T_0)\ P(E/T_0)}{P(E)}$$

$$= \frac{P(T_0)\ P(E/T_0)}{\Sigma\, P(T_i)\ P(E/T_i)}$$

where:

T_0 is some theory

E is our empirical evidence or "facts."

$P(T_0/E)$ is the probability that T_0 is 'true' given the empirical evidence. To Keynes, a knowledge of E justifies a rational belief in T_0 of the degree $P(T\ /E)$.[255]

$P(T_0)$ is the marginal probability that T_0 is 'true.' To Keynes, $P(T_0)$ is the a priori probability.[256]

$P(E/T_0)$ is the conditional probability that E is "fact" when T_0 is true.

$P(E)$ is the marginal probability of the evidence, and it need be non-zero.

This simple theorem can be used to demonstrate that any attempt to establish $P(T_0/E)$, or justify any measure of it, will lead to apriorism or an infinite regress. We simply cannot satisfactorily answer the question: How can we assign a value to $P(T_0/E)$? Certainly, following the Bayesians, we might update our assessment of the probability of some theory given new empirical evidence. But what is our prior probability, and how was it assigned? Alternatively, we might try to reduce T_0 to its constituent elements and attempt to use knowledge about their probabilities to infer the probability of T_0. Yet here again we encounter the same problem: How can we assign probabilities to these constituent elements? There is no escape from this problem. Either we assert some values of probabilities, a priori, or we continue to infinite regress.

To demonstrate these assertions, we refer to Bayes's theorem. In order to determine the conditional probability of the theory in question, T_0, we must know $P(T_0)$. Assume that assigning probabilities to $P(E/T_0)$ and $P(E)$ is unproblematic and gives us non-zero measures - this is the most favorable assumption for probablism. The whole updating or assignment process depends upon the value of $P(T_0)$, and here is the problem:

Assertion:

A_1: For all scientific theories, T_0, $P(T_0) = 0$ or is undefined.

Clearly if A_1 is true, the hopes of all forms of probablism are dashed. For then all theories would have zero probability given our empirical evidence, $P(T_0/E) = 0$. Thus, there would be no way to use our evidence to choose or rank different theories, destroying all epistemological significance of probability.

Proof:

Obviously we cannot "prove" A_1 in the same manner as a mathematical proof. Yet a philosophical proof of 'rational demonstration' is available. One tack which we could try is to interpret A_1 as a philosophical axiom. In fact, such an axiom can be seen to follow from the philosophical position of fallibilism. But such a "proof" would be far from "convincing."

The other method of demonstration is to argue that $P(T_0)$ would be zero under general circumstances. Although we do not accept that $P(T_0)$ is defined at all, we shall assume that it is in order to give probablism all the benefits of doubt. Furthermore, to give probablism the advantage we confine the range of alternative theories to some class that depends upon only one parameter. All theories depend upon at least one parameter (for example, Newton's theory depends upon the gravitational constant, G, and Einstein's theory - the speed of light, c). The values of this parameter will change how our evidence supports the corresponding theory. Would not the orbital behavior of the planets be evidence against a negative value of G?

Even under these most favorable conditions for probablism, $P(T_0) = 0$. There is a continuum of alternative theories in this simple class, thus causing the probability of any single theory to be zero. The defender of probablism must assert that the number of alternative theories is finite to obtain a non-zero value for $P(T_0)$. Yet would that not be more presumptuous?

Probablists have a procedure to circumvent this problem of infinite alternatives. They can define a probability density upon this class of theories (usually the uniform distribution is initially assumed) and find the probability for some range of theories. That this can be done is unquestionable, but does it make sense? No, the choice of any density brings us back to apriorism, for it is necessary to assume that we have knowledge of the relative likelihoods of all alternative theories <u>before</u> any evidence is given.

The only escape from A_1 is to assert that you know that there is only a finite number of alternative theories that are possible or that you know exactly how the probabilities of these alternative theories relate. And this knowledge must be given before we have any empirical evidence. Such assertions of knowledge are consistent only with some type of apriorism. Thus, the only defense from A_1 is to withdraw into apriorism.

Support for A_1 is greatly enhanced when we allow the class of theories to vary. The above argument is based on the assumption we know the class of theory, or mathematical equation, in which 'truth' lies. All that we do not know is the value of some parameter in our model, but we do know its form. Yet this is an unreasonable assumption, and we have assumed part of the probablists' desired conclusion of assigning probabilities; in particular, $P(\acute{T}) = 0$, for all $\acute{T}$ not a member of our limited class. The entire epistemological problem of theory choice is to determine the relative merits of classes of theories or particular ones. We cannot eliminate by "assumption" the bulk of the competition. Thus, other classes of theories must be considered.

When we generalize our problem across classes, it is more obvious that $P(T_0)$ must be zero. The number of different classes of theories must certainly be infinite and uncountably infinite at that. For each type of mathematical specification we have a different class, and for each set of variables that we correlate to the observations we have a different class. The number of possible combinations of theoretical classes is limitless. Again, the only way to avoid this result is to limit the potential set of theoretical classes by asserting an even higher order of apriorism. Thus, we have now 'rationally demonstrated' A_1 is true or we must embrace apriorism, $A\omega$

$$\therefore \quad A_1 \quad \vee A\omega$$

QED

Our argument may be summarized. All forms of probablism or attempts to determine the probability of some theory based upon the evidence are doomed to failure. Any such attempt must lead to an infinite regress or to apriorism. And, without apriorism, the probability of any specific theory remains zero.

If anyone chooses theories based upon their probability, they have implicitly assumed some a priori knowledge which is not justifiable by any amount of empirical evidence. And it is usually quite difficult, if not impossible, to see how their "assumptions" bias their results. Nonetheless, it is clear that by choosing the appropriate a priori probabilities any theory may be made the "most probable." Probablism is the perfect methodology for a dogmatist. He can implicitly assume his conclusion while appearing to "prove" it and while putting the critic in the weak position of "proving" his bias. Since probabilities cannot be used to choose theories empirically, probablism can only be abused.

Keynes's attempt to establish some form of "probabilistic induction" suffers the same criticisms, since his notion of "probabilistic induction" is exactly this concept of the probability of a theory given the empirical evidence. Thus Keynes's attempt to use probability as an epistemological regulative principle fails. This is an ironic situation, since Keynes should have known of this inadequacy of probability. This follows from the fact that Keynes is quite aware of Hume's criticism of induction, and it turns out that Hume's criticism applies to "probabilistic induction" as well.

Keynes begins his chapter on induction with a quote that states Hume's skeptical position on induction. But Keynes believes there is a weakness in Hume's criticism.

> Hume's account, however, is incomplete .... There is no process of reasoning, which from one instance draws a conclusion different from that which it infers from a hundred instances, if the latter are known to be in <u>no</u> way different from the former. Hume has unconsciously misrepresented the typical inductive argument.[257]

Nonetheless, Keynes does recognize much of the force and generality of Hume's argument.

> Hume showed not that inductive methods were false, but that their validity had never been established and that possible lines of proof seemed equally unpromising. The <u>full</u> force of Hume's attack and the nature of the difficulties which it brought to light were never appreciated by Mill, and he makes no attempt to deal with them. Hume's statement of the case against induction has never been improved upon; and the successive attempts of philosophers, led by

> Kant, to discover a transcendental solution prevented them from meeting the hostile arguments on their own ground and from finding a solution along lines which might, conceivably, have satisfied Hume himself.[258]

Only Keynes believes that he found a way around Hume's "hostile arguments."

> Much of the cogency of Hume's criticism arises out of the assumption of methods of certainty on the part of those systems against which it is directed .... When probable knowledge is, nevertheless, real, a new method of argument can be introduced into metaphysical discussions. The demonstrative method can be laid on the side, and we may attempt to advance the argument by taking account of circumstances which seem to give some reason for preferring one alternative to another.[259]

We can agree with Keynes that argument can advance without methods of certainty. But this advance can come only through tentative knowledge, for "probable knowledge" is as elusive and as nondemonstrable as "certain knowledge."

In any case, it is clear that Keynes is in the strange position of having a better understanding of Hume's criticism of induction than all but a few and yet believing that Hume's criticism could be circumvented. The irony of such a position is unavoidable when we realize that Hume anticipated Keynes's circumvention and that Keynes knew it. It is Popper who brings to light the full force of Hume's criticism.

> For Hume argues "<u>even after the observation of the frequent constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience</u>." If anybody should suggest that our experience entitles us to draw inference from observed to unobserved objects, then Hume says, "I would renew my question, <u>why from this experience we form any conclusion beyond those past instances, of which we have had experience</u>." In other words, Hume points out that we get involved in infinite regress if we appeal to experience in order to justify any conclusion concerning unobserved instances - <u>even mere probable</u>

> conclusions, as he adds in his Abstract. For there we read: "It is evident that Adam, with all his science, would never have been able to demonstrate that the course of nature must continue uniformly the same .... Nay I will go farther, and assert he could not so much as prove by any probable arguments that the future must be conformable to the past. All probable arguments are built on the supposition that there is conformity betwixt the future and the past, and therefore can never prove it."[260]

Thus, long ago, Hume realized that "probabilistic induction" is just as unjustified as induction. For we have to assume what we cannot justify - the future is like the past - in order to use probable arguments. If anyone should have been aware of the depth of Hume's position, it should have been Keynes. Hume's Abstract was in Keynes's personal collection and became public only after Keynes released it. Yet, Keynes does not succeed in "meeting these hostile arguments on their own ground." He merely asserts his own "probable ground."

Before we completely dismiss Keynes's probablism, we should look a little closer at his theory. Keynes divides the issue of induction into two types: "universal induction" and "inductive correlation." The former refers to an assertion of universal law which holds "absolutely," while the latter refers only to probable connections.[261] Keynes succeeds in deriving the properties of one class of "inductive correlation" - "pure induction." Pure induction is the case for which all instances are exactly the same. For such a happenstance, Keynes shows that the probability of our inductive generalization will increase as the instances multiply and that this probability approaches one in the limit.[262] Yet, even these seemingly obvious results depend crucially upon the assumption of a non-zero a priori, probability of the inductive generalization, the necessity of which Keynes himself recognizes.[263] Yet by our former argument we can conclude that the probability of any theory or inductive generalization is zero. Keynes's "probabilistic induction" cannot demonstrate or justify the probability of any theory nor can it be practically applied. The only way to assign probabilities to theories or even to rank theories as more or less probable is to assume your conclusion. For such probabilities depend on the *a priori* probabilities, and we have no method to calculate *a priori* probability of a theory or assertion. Thus, "probabilistic induction" fails.

Another important criticism of probablism remains. All methods of induction or verification must assume the truth of our single instances of "facts." The probability of theories is discussed only given these "facts," and this means assuming that the "facts" are true. Yet, the "facts" are not infallible. They are theoretic and not concrete. How are we to establish the "facts"? Should we try to establish their probability? Any such procedure would be turned into an infinite regress, for we would have to consider some theory as given to evaluate the probability of our "facts." Here too the probablist will be forced into an infinite regress or apriorism. Everywhere we look the attempt to establish or justify theories leads to apriorism or an infinite regress.

The weakness of Keynes's theory of "probabilistic induction" is candidly admitted in Braithwaite's foreword to the *Treatise*.

> This theory, however, does not seem able to provide the justification for inductive inference which Keynes hoped to achieve. Keynes was one of the first to realize clearly that any justification for assigning a positive probability to an empirical hypothesis on the basis of knowledge of some (but not all) instances of it must presuppose that a probability was assigned to the hypothesis prior to any knowledge

> of instances of it .... But even if we are not making the inference to a general hypothesis but to a new instance of the hypothesis, a belief (full or partial) in some proposition about the universe appears to be essential in order to give prior probabilities which would justify inductive inference.[264]

Clearly, Braithwaite recognizes that inductive inference must ultimately rest upon apriorism - "belief in some proposition about the universe." Braithwaite also correctly generalizes this "fallacy of induction," inherent in Keynes's theory, to all statistical methods of theory choice.

> Those who are now called "Bayesian" would compare statistical hypotheses by comparing their Keynesian probabilities, given the data. Indeed almost any plausible principle for preferring one statistical hypothesis to another is equivalent to a Bayesian procedure with suitable prior probabilities ascribed to the hypotheses. For example, if these are taken to be equal, we have R. A. Fisher's maximum likelihood principle.[265]

Not only Fisher's maximum likelihood, but all current statistical methods of hypothesis testing are methodologically and epistemologically equivalent to Keynes's probablism. They only differ in their chosen technique. All our criticisms of probablism apply to these techniques as well. We simply cannot say which theory is more "probable" in any sense of the word, given the empirical evidence. The "fallacy of induction" is involved in all statistical "tests" of hypotheses, if we use them to justify or verify our theories.

In econometrics this "fallacy of induction" appears in the so-called "specification problem." Here the problem is: "What is the proper form of our statistical model of reality and how can we 'test' some asserted form?" While it is true that we can test specific statistical hypotheses, all these "tests" presuppose some particular conditions that our model of reality must satisfy. The "specification problem" then becomes:

"How do we "test" our presupposed conditions upon which our former specification "tests" are based?" In some cases, econometricians have been able to discover more general "tests" of specification. Yet each, in turn, assumes some "general specification" that the model must satisfy. Thus, "tests" of specification" lead to an infinite regress from which there is no escape.

Yet there is a solution to the "specification problem." There exists no method of statistically "testing" the specification of a model against all alternatives. Furthermore, there exists no method to assign a probability to a model's specification. Of course, our solution assumes that one is not allowed to employ a method that assumes its own conclusion. All we can do is hypothesize a complete model and consider it 'falsified' whenever sufficient evidence accumulates against any of its assumptions or implications.

As one might expect by now, Popper has discussed these issues of probablism and made all of our previous criticisms in his Logic of Scientific Discovery.[266] Furthermore, Popper finds a fundamental misconception in all these attempts to justify theories. Briefly stated, they get their probabilities backwards.

The desirable characteristic of a theory is its 'empirical content' = 'testability' = 'falsifiability.' *Ceteris paribus*, we should rationally prefer theories with the greater 'empirical content.' But, Popper sees an inverse relation between 'empirical content' and probability. Highly informative theories are highly improbable. How can Popper arrive at such a contrary result?

To begin, Popper does not require the prior probability of a theory as does probablism. Instead, he uses the probability of empirical evidence conditional upon our background knowledge - what we currently hold as knowledge. Popper calls this conditional probability the "severity of tests" or "explanatory power."[267] By focusing on these probabilities, Popper avoids the problem of the zero probability of theories.

Then Popper combines the notion of "severity of tests" with 'empirical content' to obtain his view of a theory's probability. Recall that 'empirical content' is the set of "potential falsifiers" of a theory. The complement of 'empirical content' is the set of 'observations' or empirical evidence that is consistent with the theory. Now we can define some subclass relation for sets of empirical evidence that conform to probability calculus.[268] When we combine the notions of the probability of empirical evidence given our background knowledge and a measure of the size of the set of empirical evidence consistent with our theory, we have a measure of a theory's probability. Essentially, the probability of a theory is equal to the probability of finding consistent, or "non-falsifying," evidence.

But this conception of probability gives us a reverse ordering compared to the probablists' notion of probability. To see this, suppose we have two theories, t_1 and t_2, where the 'empirical content' of t_2 is a subset of t_1's. For example, t_2 might be the theory that says there is always an inverse relationship between the price and quantity of some good, while t_1 makes the above assertion and adds the assertion that price elasticity is greater than one. To Popper, t_1 is more informative, has more explanatory power, and is the "better" theory - assuming that we haven't succeeded in 'falsifying' it. Yet, t_1 is more improbable. The set of empirical evidence consistent with

t_1 is _smaller_ than for t_2, since this set is the complement of 'empirical content.' Or, the theory with the larger 'empirical content' is more likely to be 'falsified,' _ceteris paribus_. Thus, falsificationism seeks improbable theories that take large risks. Science grows by developing highly improbable explanations and by failing to 'falsify' them, not by justifying highly probable theories.

"Where does the probablist's notion of a theory's probability lead?", one might ask. "To the trivialization of knowledge," responds common sense. Although we have argued that probablism cannot define the probability of a theory given the empirical evidence, let us shelve this criticism for the moment and only consider the underlying, intuitive notion of a theory's probability. If we also assume that empirical evidence is fixed and known, we can then see what type of theories will be the more probable. Now theories with more 'empirical content,' like t_1, cannot have probability greater than theories, like t_2, with less 'empirical content.' For all evidence that is consistent with t_1 is also consistent with t_2. Thus, the most trivial theories that say the least about some phenomenon must be the most probable given the evidence. By adding some informative statement to a trivial theory, we can only decrease the resulting probability. For this conjunction can be consistent with no more evidence than could the trivial theory.

Thus, even the simple intuitive notion of the probability of a theory given the evidence leads to absurd epistemological implications. In the limit, we would choose only tautological theories, since their conditional probabilities are one. Or more generally, the theory closer to a tautology will be more probable. Clearly, such trivial theories _cannot_ be considered knowledge and especially not our "best" knowledge.

At all levels, the probablist's notion of probability fails to be an adequate epistemological principle.

It would be easy to give a broader and more detailed criticism of all forms of probablism and Keynes's epistemology in particular. But our criticisms of the major tenets of probablism are sufficient for its 'rational refutation.' Readers wishing a more interesting and detailed criticism of Keynes's theory and probablism are referred to Popper's Logic of Scientific Discovery.

Some might think that probablism could be salvaged by introducing Popper's probability. To an extent this is true, but again it would reduce probablism to falsificationism. Furthermore, Popper's notion of probability (or his more elaborate concept of 'corroboration') cannot be used for theory choice. Although these concepts can be meaningfully defined, they cannot be unambiguously applied to actual theories nor be used to establish one theory over another. Popper's subclass relation can only unambiguously compare two theories when one's 'empirical content' is the subset of the other's. Besides this consideration, Popper's falsificationism cannot in any way establish a theory. Theory choice is always problematic, and Popper does not claim to have a criterion for it.[269] And such is not required for falsificationism or fallibilism, but it is required for those who claim that they have a replacement for demonstrative methods of argument.

Probablism in all its forms is an unsound methodology or epistemology of science. It leads to a trivialization of knowledge. To search for highly probable knowledge can only inhibit the growth of knowledge by reducing the value of posing interesting and informative conjectures. Probablism fails to provide a comparison of theories given our empirical evidence, since these probabilities will remain zero. Yet this comparison is necessary for the probablist's claim of probable knowledge. Thus, probablism fails in its own terms. Finally, any attempt to assign a probability measure (in theory or in practice) to a theory must result in either apriorism or an infinite regress.

Unlike many philosophies of science, probablism can lead beyond itself, when objective probability is employed. Probablism seeks to criticize theories by independent empirical sources, and its goal is 'truth' or "highly probable" knowledge. Probablism does not lock itself into some preconceived framework, it can learn from experience.

Probablism and fallibilism are related. They both begin with the realization that knowledge, scientific or otherwise, is not certain. From the absence of certainty, probablists conclude that knowledge must have probability, while the fallibilists need not make this leap. To many, the step from the lack of certainty to probability may seem very direct. But it is no trivial shift of position. In order to define or assign probability to a theory, certain knowledge must be assumed. In order to apply the classical definition of probability, we need to know exactly how many alternatives there are and that each alternative is equally likely. If this knowledge is only probable, our probability metric remains undefined.

More generally, probability must always be defined relative to some domain. This domain is then taken as given or certain in order to specify the probability metric. Yet, at the level of epistemology, we can never know with certainty the proper choice of the domain of theories. Thus, probablists must assume certain knowledge to employ the concept of probability. However, the assumption is in direct conflict with the initial position that knowledge is not certain. For epistemology, probability gets us nowhere.

Again, a probablist might attempt to defend his position by assuming some probability-like metric over the domain of theories. Yet, here too, we would point out that this metametric must assume some domain (of the domains) as given. Thus, we force the probablist into an infinite regress, or he must eventually admit that probability is defined only when something is taken as certain.

This curious result might be troublesome to some. Yet viewed from the epistemological level, it as as it should be. Statements of probability are statements of knowledge; they have content. Their content is determined by what they say cannot happen and by what they say is "unlikely." To the extent that they forbid some occurrences, they are equivalent to non-probablistic knowledge. To the extent that probability assertions can clash with 'observations,' they are 'falsifiable.' Probability statements are not qualitatively different than the usual forms of "deterministic" statements. Is it any surprise that probability statements can only be <u>derived</u> from statements containing certain knowledge? The common economic adage is correct. "You can't get something from nothing," or "There is no such thing as a free lunch." To derive knowledge, you must begin with knowledge. And fallibilism holds that at every step knowledge is fallible and tentative.

Nonetheless probablists, and J.M. Keynes in particular, have advanced our scientific knowledge. They have advanced the formal science of probability theory which, in turn, can be used to express scientific theories. There is a great potential for scientific advance by better using probablistic explanations of natural and social phenomena - witness quantum mechanics. At the level of scientific explanation, a probability statement need only be asserted, not derived, and 'observations' can then 'falsify' our assertion.[270] While, at the level of epistemology, probability cannot be derived, nor can it be used to establish or justify one theory over another.

From the view of science, probability can only provide a medium in which to express our conjectures, and it is among the richest media at our disposal. To the extent that probablists have developed this medium, their efforts are laudable. Yet, this rich potential has and will continue to go largely untapped, until Popper's methodology for the testing of probablistic hypotheses is embraced.

3.3.7 Eclecticism: A Defense from Criticism

Most philosophers or scientists do not hold only one of these philosophical positions. Instead, most entertain some combination of positions. There is nothing wrong, in itself, with selectively choosing the best parts of philosophical positions from a set of alternatives. Yet, the principal tenets of philosophies are usually inconsistent amongst one another. Unless one is very careful, an eclectic philosophical position is likely to be inconsistent.

For example, subjectivism and/or relativism is inconsistent with fallibilism, for the latter assumes some sort of 'objective truth.' Inductivism, probablism, and fallibilism are inconsistent with apriorism. And conventionalism or instrumentalism does not mix well with falsificationism.

Lakatos' MSRP is a good example of eclecticism. His "methodology of scientific research programmes," MSRP, is an attempt to combine the best of Popper's falsificationism with the best of conventionalism. Yet, as previously discussed, the compromise has inconsistent consequences. In cases of inconsistent philosophical positions, anything can be justified or maintained. Such philosophies can provide no guidance, for they can be used to support most anything. The only remedy in such a circumstance is to remove the inconsistent elements from the philosophical position or to reject the position altogether. In the case of MSRP, we recommend that 'falsifiability' be explicitly added and thus eliminate the conventionalism of Lakatos' methodology.

Another problem with combinations of philosophical positions is that the resulting position inherits all the criticisms and deficiencies of its parent philosophies. Eclectic philosophies are merely convex combinations of other philosophies, and are therefore no more than their "principal components." Thus, there is little to gain and much to lose in seeking a compromise philosophy of science.

Furthermore, there are dangers in permitting eclecticism in the philosophical debate. Eclecticism can too easily be used as a "conventionalist stratagem" on the philosophical level. Any intelligent dogmatist can frustrate 'rational discourse' by continually attaching some auxiliary tenets to his position in the face of 'rational criticism.' 'Rational criticism' has the same weakness as methodological falsificationism. They can work and help us eliminate our errors only if we allow them to.

What better method of avoiding criticism exists than to add _ad hoc_ philosophical appendages? Yet, such a metaconventionalism is just as dogmatic and as inhibiting to methodological progress as conventionalism itself. Unless the eclectic position can demonstrate some increased ability to solve problems and to pose new ones, it should be ignored. And whenever some philosophy is held above criticism, it should be recognized as merely a "conventionalist stratagem" designed only to dogmatically maintain some philosophical or scientific position. In such cases 'rational discourse' ends.

In contrast, our objective is to discuss each philosophical theory as clearly and distinctly as possible. The hope is to reach a full understanding of each philosophical position and to discover any error, inconsistency, or inadequacy of individual positions. While such a philosophical analysis may be the best way to obtain progress in ideas, we must remember that it will not accurately portray the history of ideas nor fully characterize individual contributions. Still no one has ever offered a viable alternative to this type of 'rational discourse.'

3.4 Philosophy of Science in Retrospect: Historical Fantasy as Understanding

In an effort to summarize our philosophical analysis, we return to the historical framework employed in the beginning of the last section. But we have no pretense that any such description will provide an adequate explanation in a manner similar to that of scientific explanation. In an effort to explain philosophy, there is no recourse to experimental evidence nor to inter-subjective and testable observations. Nor can the words or beliefs of philosophers serve as our facts. We have no desire to explain the second world of philosophical beliefs, but instead hope to characterize the development of the third world of philosophical problems and their tentative solutions. For this activity interpretation is required. Thus, our story will be, in some sense, fantasy. As

Kuhn remarks about a similar attempt by Lakatos to explain the history of science: "(it) is not history at all but philosophy fabricating examples."[271] But Lakatos responds, "histories of science are always philosophies fabricating examples."[272] Agreeing with both, some fantasy must reside in our efforts to explain the cognitive development of ideas. Yet to the extent that such fantasy furthers our understanding of those ideas, they are valuable. With these qualifications in mind, we turn to our story of the philosophy of science.

In the beginning, as in all beginnings, there was myth. A grand deductive system in which all knowledge could be expressed and derived was imagined. The myth and its first glimpses were developed by the classical Greeks. With the myth was born the Euclidean programme and its paradigm of knowledge - Euclidean geometry. Probably no other single vision has had as much influence upon Western thought as has Euclid's geometry. The influence of this programme remained strong until the current day. Throughout the Middle Ages, it was maintained by the clergy who were incessantly "proving" God's existence by "deduction."

Descartes freed the Euclidean programme from the authoritarian and religious associations that resulted from the clergy's stewardship. Instead, Descartes grounded the Euclidean programme in intellectual intuition. Bacon, too, wished to liberate science. His desire for some empirical foundation of science was later turned into the two rival programmes, the Empiricist and the Inductivist.

The philosophical basis of the Empiricist's programme was developed by Berkeley and Hume. They sought to base knowledge upon the second world of perceptions and beliefs. This conception of the subjective as the "real world" was a reaction to the metaphysical conception of reality that results from Cartesian intuition. Berkeley's philosophy begot dogmatic subjectivism which believes that the second world of beliefs and human perceptions is the only world. Dogmatic subjectivism gives us only a terse answer to the question, "How do we know?" Otherwise it tells us nothing. This subjectivism cannot explain the growth in either subjective or scientific knowledge and it permits inconsistent knowledge claims. Thus, it is an inadequate philosophy of knowledge.

Hume, however, realized the inadequacy of Berkeleianism. Hume's criticism of causation is also a criticism of subjectivism. As Kant, Keynes, and Popper correctly saw, Hume's criticism of causation is more importantly a criticism of induction. Yet, as Hume discovered, the impossibility of a valid method of induction also made subjective knowledge incomplete. For how could one rationally believe laws or regularities when such generalities could not be derived from individual perceptions?

Understanding Hume's problem caused Kant to seek a more solid basis for scientific knowledge. Kant raised some scientific and perceptual regularities to a synthetic level which is *a priori* valid. Given these "truths," Kant could explain how we perceive, the regularity of our perceptions, and the possibility of scientific knowledge. The Kantian system accomplished a synthesis of the formerly disconnected Euclidean and Empiricist programmes. Essential empirical principles became axioms and the rest of knowledge was deductively derived. Kant placed the Euclidean programme on its strongest philosophical footing.

Yet at the same time as Hume and Kant were shaping the Euclidean and Empiricist programmes, the Inductivist programme was gaining support. From the rough outline of Baconian science, Newton established induction as the correct method of science. Newtonian induction was merely a defense against Cartesian metaphysical criticism. Newton himself did not use induction nor did he believe that inductive methods necessarily lead to correct inferences. Yet, the conviction of the connection between theory and observation necessitated the belief in some type of inductive method. The success of Newtonian physics perpetuated and strengthened the myth of induction and the Inductivist programme. Neither the criticism of Hume nor denial of induction's strict validity by Newton, Mill, or Keynes could overcome the steam-rolling success of Newtonian physics in promoting the myth of induction. This myth remained, and perhaps still is, the accepted view of science for both "the man in the street" and scientists. Ironically, no influential thinkers ever believed in the validity of inductive inference, yet the myth continued and no one seemed to care. It took Popper to convince philosophy, and perhaps science, that the obvious invalidity of induction had important epistemological implications.

Throughout the nineteenth century increased emphasis was given to the philosophy and methodology of science, and philosophical positions proliferated. During this period, apriorism flourished though its roots go back to Descartes. Apriorism is the view that some propositions are given to us, valid and _a priori_; from these _a priori_ propositions all other knowledge is derived. Clearly, apriorism fits into the Euclidean programme, but it is not clear whether its method is principally Cartesian or Kantian. Introspection was generally held to be a legitimate source for discovering these _a priori_ "truths." But apriorists disagreed whether the _a priori_ "truths" were also necessary for experience - or not all apriorists were Kantian. In any case, apriorism cannot be considered a reasonable theory of epistemology. It cannot explain, even ex post, the growth of scientific knowledge. And science itself has repetitively rejected one obvious a priori principle after another.

Still the Euclidean programme remained and found new fortification with Poincaré's conventionalism. Poincaré defined a new version of the old Euclidean programme that could never be shown "false." For Poincaré dismissed such notions as "truth" and "falsity." With conventionalism comes the recognition that scientific theories are not "true." Instead, they are only convenient file cabinets in which to sort scientific "facts." Theories are not chosen for "truth," "falsity," nor how well they explain the "facts"; they are picked by their "simplicity."

Anyone wishing to enthrone his favorite theory for all time can adopt a conventionalistic methodology and dogmatically defend a given theory indefinitely. To maintain a given system, one only needs to add ad hoc hypotheses for each new threat to the system. To a conventionalist, there can never be a falsification. In fact, falsification does not even make sense in conventionalism; for would that not mean that something was "false"? When forced, conventionalists can always reduce their theories to a set of implicit definitions and continue to hold them.

Like its theories, conventionalism is irrefutable. Yet, there are excellent reasons for rejecting it as a methodology or epistemology of science.

(1) Conventionalism is based upon a misconception of science. Science is not merely a set of implicit definitions. To the extent that science makes genuine assertions about the interrelation of phenomena, conventionalism is false.

(2) Conventionalism is totally arbitrary. It uses a conventionally defined concept, "Simplicity," to choose

theories. A system of conventions based upon a convention and which sorts other conventions - scientific "facts" - can only be empty - by its own convention.

(3) Conventionalism cannot explain the growth of science and the history of science is dramatically different than what the conventionalists would have us believe. For example, Einstein's theories directly oppose those theories which Poincaré asserted could never be replaced - Euclidean geometry and Newtonian physics.

(4) Conventionalism does not promote the growth of knowledge but elevates dogmatism to a virtue. Conventionalism not only provides the means to dogmatically defend the Euclidean programme, but also the methodology to defend any theory against 'rational criticism.'

For these reasons conventionalism should be forgotten. Yet, in spite of the criticisms of Popper and others, conventionalism is still with us, and it is seen in the sociology of science that Kuhn describes.

Along with conventionalism, we also have its extreme form - instrumentalism. Instrumentalism holds a pragmatic view of truth, and "predictive success" replaces "simplicity." Scientific theories are only useful instruments. Their value and "meaning" is completely exhausted by their use. Instrumentalism inherits all the criticisms applicable to conventionalism. In addition, instrumentalism has its own weaknesses.

(1) It cannot distinguish between "applied" and "pure" sciences. To the extent that a "pure" science exists which attempts to pose genuine problems and explanations of phenomena, instrumentalism is mistaken.

(2) It is inconsistent with the notion that scientific theories have excess content.

(3) It provides no means for making theory applications "useful." It must rely upon a "pure" science to develop the theories, yet it denies the existence of "pure" science.

Instrumentalism is the philosophy of technology, engineering, or any application of science. It provides no epistemological answers, other than "knowledge is power and that is all you need to know." While this may be true in many applications, it is not all that we need to know.

Early in this century, the Euclidean programme became solidified by the acceptance of conventionalism and instrumentalism and the success of modern mathematics. One might "predict" that conventionalism and instrumentalism will be around for quite some time to come, since these philosophies are so 'useful' for those who have professional motives other than the search for 'truth.' Yet, most of the twentieth-century philosophy of science is not concerned with either conventionalism or instrumentalism.

Probably the most influential twentieth-century philosophy is logical positivism. Logical positivists were enchanted with the pseudo-problems of meaning and the language of science. They taught that science could be analyzed only by its language. Like conventionalists, logical positivists "mistake the garment for the essence." Unlike the conventionalists, logical positivists thought that scientific theories are more than implicit definitions; their "meaning" is the set of "facts" (or "protocol sentences") from which they are derived.

Thus science has content only to the extent that it says something directly about the "facts." Any statement not reducible to the "facts" is senseless metaphysics. Popper's criticisms and the logical positivists' inability to achieve their goals destroyed this philosophy. Although it is currently considered a dead philosophy, one can still hear its echoes in almost every field.

Lastly, there was and still is induction. Since Newton, induction has remained a popular scientific and philosophical myth. This century has seen almost countless new methods of induction. Modern attempts to justify, verify, confirm, or "probabilify" theories may be called "probablism." All such methods try to establish a preference for some theory given the empirical evidence, and they all are consequently inductive. J. M. Keynes was the founder of modern probablism and his lead is currently being followed by the Bayesians. Yet, all these efforts to justify theories fail. Probablism is not an adequate philosophy or methodology of science because,

(1) Any attempt to establish a probability measure for a theory, _a priori_ or conditional upon the evidence, leads to an infinite regress or apriorism.

(2) The probability of all theories given the evidence is zero.

(3) The search for high probability leads us to trivial knowledge, when we assert the above problems away. In trivial knowledge we have no interest and need no justification.

From the logical point of view, the solution to induction has been obvious since Hume: "There exists no valid method of induction." And yet, the myth lives on. Because of the abundance of technical "puzzles" that surrounds modern mathematical methods of induction, the probability is high that the myth of induction will continue.

In the midst of conventionalism, probablism and logical positivism came Popper. He effectively showed the emptiness of these philosophies of science. Though critical, Popper has contributed more by his positive solutions to philosophical problems. He founded the philosophy of critical fallibilism and the scientific methodology of falsificationism. These provide the answers to all relevant epistemological questions that other philosophies of science unsuccessfully attempted.

What is unique about Popper's approach is how he inverted the traditional epistemological questions. While everyone else asked, "How can we justify our knowledge?", Popper asked, "How can we improve our knowledge?" and "How can we recognize a potential growth in our knowledge?" And he answered, "By guessing and criticizing our guesses, error can be eliminated and our knowledge can grow."

Popper's philosophy of science is the only one that can meet all 'rational criticism' squarely and fully survive the challenge. Though respected, Popper's philosophy is not generally accepted and his views are continually criticized. But this criticism is almost entirely irrelevant. It is, in almost all cases, based upon misunderstanding and misinterpretation of Popper's position. Most critics have focused upon Popper's theory of 'corroboration' or verisimilitude and found it lacking.[273] These critics mistakenly believe that Popper, like they, is trying to establish something, either his theory or scientific theories. What they overlook is that the establishment of 'truth,' necessity, or probability of a theory is in direct opposition to Popper's central theme - critical fallibilism. Since knowledge remains fallible, any attempt to establish, justify, or verify it must lead to an infinite regress or apriorism. That Popper has repeatedly emphasized this point has somehow been missed or failed to be incorporated by the majority of philosophers and scientists.

The impossibility of establishing knowledge is damning to traditional philosophies of science but not to Popper's. Any infinite regress of guessing is no less substantial than a single guess, and a guess is all that Popper claims for any theory.

Popper's philosophy defines its own programme, for it does not fit comfortably in any other. It is best seen as a synthetic hybrid of the other three programmes of the philosophy of science. He finally answers all the problems that the other programmes only muddled.

With Popper, induction is purely a myth, yet he maintains the strong connection between fact and theory that is the heart of the Inductivist programme. His notion of 'corroboration' is all but completely opposite to what the inductivists and probablists seek. 'Corroboration' is a means (but no guarantee) to eliminate counter-factual theories; it does not justify theories as "probable," or in any other sense.

Popper's empiricism is completely critical and theoretical. Unlike the empiricists, he does not take perceptions, observations, or facts as givens or unquestioned "truths." "Facts" are important sources of criticism, yet they too need be criticized. Empirical evidence is itself a theory and its substance questionable. While the replicable observation is the most important source of scientific criticism, to Popper, his philosophy emphasizes and incorporates the interdependency of theory and fact. Thus, falsificationism does not fit well within the Empiricist programme.

Popper is probably more Euclidean than anything else. He has the highest regard for the deduction system. Yet, unlike the Euclideans, he views knowledge in its dynamic aspect. The goal of the Euclidean programme is to establish the deductive system in which all knowledge is expressed. It is a static view of knowledge. Popper is more concerned with the changes in our deductive systems.

> The stress I am laying upon change in scientific knowledge, upon its growth, or its progressiveness, may to some extent be contrasted with the current

> idea of science as an axiomatized deductive system. This ideal has been dominant in European epistemology from Euclid's Platonizing cosmology (for this is, I believe, what Euclid's Elements were really intended to be) to that of Newton, and further to the systems of Boscovie, Maxwell, Einstein, Bohr, Schrödinger, and Dirac. It is an epistemology that sees the final task and end of scientific activity in the construction of an axiomatized deductive system. As opposed to this, I now believe that these most admirable deductive systems should be regarded as stepping stones rather than as ends, as important stages on our way to richer and better testable, scientific knowledge.[274]

Thus Popper's philosophy is a new vision and a new research programme for the philosophy of science. Unlike many other philosophies, critical fallibilism and the methodology of falsification has "genuine utility" in its applications to science. It is towards these economic applications that we now turn.

Notes

[1] At times, Kuhn is concerned only with Popper's locutions and not the content of his philosophy. Kuhn [1970a], p. 3, and each section's introductory discussion thereafter.

[2] Kuhn [1962], p. 1.

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

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

[5] Kuhn [1970b], pp. 236-237.

[6] See Kuhn [1970b].

[7] Masterman [1970], pp. 61-63.

[8] Kuhn [1970b], p. 234, and Kuhn [1970c], p. 173.

[9] Kuhn [1970b], p. 237.

[10] *Ibid*.

[11] Kuhn [1962], p. 24.

[12] Kuhn [1970b], p. 245.

[13] *Ibid*., p. 248.

[14] Kuhn [1970c], p. 173.

[15] Recall Popper's view of science described in Chapter 2.

[16] Kuhn [1962], p. 52.

[17] *Ibid*., p. 36.

[18] *Ibid*.

[19] *Ibid*., p. 37.

[20] *Ibid*., p. 80.

[21] *Ibid*., p. 165. In the same paragraph, Kuhn goes on to mention revolutions as another source of progress. We shall return to Kuhn's conception of scientific advance.

22 See Kuhn [1962], pp. 52-65.

23 *Ibid.*, pp. 66-76.

24 *Ibid.*, pp. 147-149.

25 *Ibid.*, p. 149.

26 *Ibid.*, p. 165.

27 *Ibid.*, pp. 135-142.

28 *Ibid.*, p. 168.

29 This title is taken from titles chosen by Popper and Kuhn and an interpretation of their major theses. Popper's original work is entitled *The Logic of Scientific Discovery*. This title has been the source of misunderstanding of Popper's intent. For Popper is not concerned with the process of discovery itself, but with the logic of explanation and the growth of scientific knowledge. Kuhn, on the other hand, admits both his historical and psychological perspective. See Kuhn [1970a] and notice its title.

30 Kuhn is also aware of the theoretical foundation of description; see quotation 5.

31 Kuhn [1970b], p. 236.

32 Popper [1972], pp. 170-180.

33 Kuhn [1970a], p. 6.

34 Kuhn [1970c], pp. 245-246.

35 Kuhn [1970c], p. 208.

36 *Ibid.*

37 This issue is discussed in detail in part 3.1.2.4 of this section.

38 Kuhn [1970b], pp. 237-238.

39 See for example Kuhn [1970c], pp. 207-208.

[40] Kuhn [1970a], p. 3. Notice also the sound of induction in Kuhn's passage.

[41] See Popper [1972], pp. 119-122 and 287-289.

[42] Popper [1959], pp. 33-34.

[43] See Wisdom [1974], p. 825, and Popper's reply - Popper [1974b], p. 1150.

[44] For descriptions of "normal science," see Kuhn [1962], pp. 24-28. Also, Wisdom [1974], pp. 822-825, provides a brief but insightful comparison of Kuhn's and Popper's descriptions.

[45] Kuhn [1970a], pp. 4-5.

[46] Ibid.

[47] Popper [1974b], p. 1147.

[48] Ibid., pp. 1145-1146.

[49] Lakatos [1978], p. 69.

[50] Popper sometimes calls 'science' "revolutions in permanence" to emphasize the continuous role of criticism and 'testing' of the fundamentals of science. See Popper [1974b], p. 1147.

[51] Popper [1970], pp. 56-57.

[52] Popper [1959], pp. 31-32.

[53] Popper [1963], pp. 42-46, and Popper [1972], pp. 21-29.

[54] Popper [1972], pp. 106-190.

[55] Ibid., p. 164.

[56] A list of such "misunderstandings" could be made quite long. Popper cites examples of Schrodinger, Einstein, and Kepler. See Popper [1972], p. 179n.

[57] Kuhn [1970b], p. 238.

[58] Ibid., p. 240.

[59] Ibid., p. 241.

[60] Kuhn [1962], p. 150.

[61] Unless, of course, we are using a subjectivistic epistemology. Subjectivism is addressed in depth later in this chapter. Here, it should be noted that Kuhn's concentration upon the second world and his insistence that this second world has epistemological consequences identified him, at least implicitly, as a subjectivist.

[62] Kuhn addresses the insufficiency of logic in Kuhn [1970b], pp. 260-261.

[63] Ibid., p. 261.

[64] See Kuhn [1962], p. 152.

[65] See Kuhn [1970a], p. 1, and Kuhn [1970b], pp. 231-237.

[66] Just this type of reconciliation is intelligently presented in Wisdom [1974].

[67] The following points are taken from Watkins [1970], pp. 34-35.

[68] Kuhn [1962], p. 149.

[69] Kuhn [1970c], p. 209.

[70] Ibid., p. 178.

[71] Ibid., pp. 203-204.

[72] Ibid., p. 204.

[73] It must be noted that Kuhn, unlike Popper, does not limit or explain what may be considered an adequate "puzzle-solution." To the extent that Kuhn permits ad hoc conventionalist strategems, "puzzle-solving" is not the Popperian growth of knowledge.

[74] Kuhn [1962], pp. 148-149.

[75] See, for example, Lakatos [1978a], p. 139.

[76] See Lakatos [1978a], pp. 139-168.

[77] See Lakatos [1959], p. 13.

[78] See Lakatos [1978a], pp. 47-52, for a description of his terms and p. 48 for the definition of the Newtonian "hard core."

[79] *Ibid*., p. 48.

[80] *Ibid*., p. 50.

[81] *Ibid*.

[82] *Ibid*., pp. 33-34.

[83] *Ibid*., pp. 48, 16-17, and n. 5.

[84] This is also how Popper characterizes Lakatos' story. See Popper [1974b], pp. 1006-1009.

[85] Conventionalism is not discussed in detail until the next section of this chapter. Here we wish to merely identify this element in Lakatos' methodology and to briefly mention its implication. A full discussion on conventionalism need be given in its own context. Thus, patience is asked.

[86] Lakatos [1978a], pp. 148-149.

[87] See Lakatos [1978a], pp. 148-149. This issue of the demarcation criterion is soon discussed in great detail.

[88] For example, see *Ibid*., p. 180, where the perfection of heavenly bodies is a "positive heuristic."

[89] *Ibid*., p. 91, n. 2. It should be mentioned that Lakatos' conception of MSRP first emerged at the 1965 International Colloquium in the Philosophy of Science, where Popper and Kuhn debated.

[90] *Ibid*., p. 118.

[91] See *Ibid*., pp. 118-138, for Lakatos' application of his method of appraising methodologies.

[92] *Ibid*., p. 118.

93 For examples, see Lakatos [1978a], pp. 47-138. We do not argue that MSRP fails to give richer stories for the history of science. Yet, some of Lakatos' examples, above, are erroneous comparisons of falsificationism and MSRP. This point will soon be made in the text.

94 Popper [1963], p. 95, n. 63.

95 The following is a parody of Lakatos' history of Prout's programme; see Lakatos [1978a], pp. 53-55. To understand this story one must, of course, read Lakatos. Yet, this parody is much deeper than the obvious similarities one will find on pp. 53-55. It is at least a meta-parody, for it also parodies Lakatos' method of argumentation: the use of his opponents own method to criticize that same method. How Lakatos employs this method of argumentation against Popper is discussed later in this section.

96 Ibid., p. 55.

97 Ibid., and Popper [1959], p. 32.

98 At a minimum, the fall of logical positivism and the advent of Nobel Prize-winning Popperian scientists (e.g., Eccles, J. C. and Medawar, p. 13; see Lakatos [1978a], p. 93, n. 2.) must be deemed successes to the Popperian programme, while the popularity of Kuhn could be judged a failure.

99 Ibid., p. 55.

100 Popper [1959], pp. 31-32.

101 Popper [1963], p. 312, n. 8.

102 Recall the former discussion of demarcation in Chapter 2.

103 Popper [1959], p. 41. Not only is Popper clear and unambiguous in this simple formulation of the demarcation criterion, but the entire work, Popper [1959], is a logical and philosophical explication of this principle.

104 Lakatos' discussion is given in "Popper on Demarcation and Induction," Chapter 3 of Lakatos [1978a] or equivalently Lakatos [1974]. Lakatos writes the paper for The Philosophy of Karl Popper. Fortunately, Popper responds to the criticism of his student; see Popper [1974b], pp. 999-1013.

105 Lakatos [1978a], p. 141.

106 *Ibid.*, p. 146.

107 Popper [1963], pp. 37-38.

108 *Ibid.*, p. 38, n. 3.

109 *Ibid.*

110 *Ibid.*, p. 39.

111 See Lakatos [1978a], pp. 146-147.

112 Recall how Popper states it in quotation 110.

113 Popper himself makes the point. See Popper [1974b], pp. 999-1013, particularly p. 1010.

114 Lakatos [1978a], pp. 145-146.

115 *Ibid.*, p. 147. It must be mentioned that Lakatos gives no evidence that Popper would "condemn psychoanalysts as dishonest." This use of intellectual "dishonesty" is Lakatos' not Popper's.

116 *Ibid.*, p. 147.

117 See *Ibid.*, pp. 16-17, for the entire telling of this tale.

118 See Popper [1959], p. 50.

119 See Popper [1974b], pp. 1004-1007.

120 *Ibid.*, p. 1007.

121 *Ibid.*, p. 1009.

122 *Ibid.*, p. 1010.

123 *Ibid.*, and Lakatos [1978a], p. 148.

[124] Popper [1974b], p. 1036.

[125] Recall quotation 114.

[126] See Lakatos [1978a], p. 148, for his reflection upon the nature of his argument.

[127] Ibid., pp. 148-151.

[128] Ibid., p. 148.

[129] Actually, as we shall argue, there is a relation between MSRP and Popper's demarcation criterion. But, not only is MSRP not a substitute for Popper's criterion, but also MSRP must assume that Popper's criterion is satisfied before it is meaningful.

[130] It is not clear that Lakatos ever intended to discuss Popper's demarcation criterion but merely wished to compare MSRP with Popper's entire methodology. Yet, since Lakatos entitles this discussion "Popper on Demarcation," it is appropriate to discuss it "as if" it concerned the demarcation of science. For evidence that Lakatos is comparing Popper's entire methodology, not just the demarcation criterion, to his MSRP, see Lakatos [1978a], pp. 148-151, particularly p. 150. Popper's "game of science" and his patterns of trial and error concern much more than the demarcation of science.

[131] Ibid., p. 151. Although Lakatos' admission is not explicit, it is implied by his lack of answers to his "falsifying" questions.

[132] Ibid.

[133] I must apologize to the reader for the complex nature of this discussion. Lakatos is continually jumping meta-levels, and we must follow if we are to understand. Meta, like "knowledge" or "truth," is a valuable concept and, like these, it can be used productively or abused by overuse. To jump to "metaland" in order to avoid or obfuscate issues

is inexcusable. Although it is our desire to be clear, we must now enter this "metaland." For Lakatos' central criticism of Popper is grounded in the nexus of, at least, two meta-levels.

[134] Ibid., pp. 152-153.

[135] Ibid.

[136] Ibid., p. 152.

[137] Ibid., p. 151.

[138] Ibid., p. 124.

[139] This issue of resolving value disputes by fighting it out is discussed with Friedman's methodology.

[140] Lakatos [1978a], p. 124.

[141] Adopted from Popper [1959], p. 111.

[142] Lakatos [1978a], p. 152.

[143] To understand Popper's position on falsification vs. rejection see Popper [1974b], pp. 1009-1010 and the references cited there.

[144] See Putman [1974], pp. 225-227, for a specification of some of these necessary auxiliary hypotheses in testing the orbit of Mercury.

[145] Lakatos [1978a], pp. 153-154.

[146] For example, see Popper [1959], pp. 31-32 or Popper [1963], p. 216.

[147] See the first paper in Popper [1963].

[148] Popper [1959], p. 19.

[149] Lakatos [1978a], p. 166.

[150] Ibid., p. 7.

[151] For example, recall quotation 148. This reference to the linguistic philosopher is elaborated in the forthcoming discussion of logical positivism.

[152] Lakatos [1978a], p. 152, n. 5.

[153] See Lakatos [1978a], pp. 33-34.

[154] Ibid., p. 27, n. 6. Here Lakatos also misinterprets his reference to Popper [1959], p. 401, n. 7.

[155] Recall Lakatos' concept of "hard core" and its 'irrefutability.' This assertion does generalize to entire theories; for example, see Ibid., p. 16.

[156] Lakatos has written considerably upon the subjects of logic and mathematics. See, for example, Lakatos [1978b].

[157] This is Lakatos' Bohrian appraisal. See Lakatos [1978a], pp. 55-69.

[158] Popper [1974b], p. 1000.

[159] Ibid.

[160] Ibid., pp. 999-1000.

[161] See Lakatos [1978b], pp. 4-23, for his presentation of this framework. What follows is an applied interpretation of this history of mathematics.

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

[163] Empiricus, S. [1965], II, 20 quoted in Rescher [1973], pp. 12-13.

[164] See Popper [1963], "On the Sources of Knowledge and of Ignorance," for some support of these assertions.

[165] For references concerning this Newtonian story see Popper [1963], pp. 184-192, and Lakatos [1978a], pp. 201-222.

[166] Quoted in Lakatos [1978a], pp. 204-205, n. 4.

[167] Ibid., p. 205.

[168] We argued this case in Chapter 2. Or, see Popper [1963], pp. 184-192, for a more extensive demonstration.

[169] Is not the modern attempt to choose theories by a Bayesian decision procedure only a more mathematically sophisticated version of induction?

[170] Dogmatic subjectivism is a label we shall use to represent the position that "the world is my dream and that is all there is to know."

[171] See Popper [1963], p. 185.

[172] From Kant's Prolegomena quoted in Popper [1963], p. 180.

[173] See Popper [1972], p. 129, n. 5.

[174] *Ibid*., pp. 130-131. The additions to Popper's text follow from Popper [1963], pp. 94-95.

[175] *Ibid*., p. 199. Although I agree with Popper that there is no point in arguing about words, I use "subjectivism" in place of his term "idealism." This choice is made only to avoid confusion with what others have called idealism.

[176] See Mini [1974], pp. 15-40, for a discussion of Descartes.

[177] Descartes, *Rules for the Direction of the Mind*, Rule III, quoted in Mini [1974], p. 17.

[178] *Ibid*., p. 19.

[179] Burrt [1932], p. 110.

[180] Mini [1974], p. 20.

[181] Kant [1783], reprinted in Weinberg and Yandell [1971], pp. 16-17.

[182] *Ibid*., p. 17.

[183] For this interpretation of Kant's problem, see Popper [1963], p. 184.

[184] _Ibid_., pp. 184-200, and Popper [1972], pp. 85-93.

[185] Originally, I wrote a simple description of Einstein's concept of relative simultaneity. But I found that such an exposition seemed belabored and required the intuitive notion of absolute simultaneity to be understandable. Only a long description would avoid using absolute simultaneity to explain relative simultaneity. Since simultaneity is so "_obvious_," this difficulty of exposition must "_necessarily_" follow.

[186] The coherence theory gives truth only coherence. That is, truth is the quality of statements being well-formed and consistent. See Rescher [1973] for a description and formulation of the coherence theory of truth.

[187] Lakatos [1978a], pp. 105-106.

[188] Blaug [1980], p. 57.

[189] See Lakatos [1978a], p. 21.

[190] Einstein [1953a], p. 191.

[191] Pareto [1909], quoted in Latsis [1974], p. 11.

[192] For similar characterizations of the role of implicit definitions and their conventional nature see Einstein [1953a], pp. 190-191; Schlick [1953], pp. 185-188; or Popper [1959], pp. 72-75.

[193] See Einstein [1953a], p. 192.

[194] _Ibid_., pp. 192-194.

[195] See Poincaré [1921], pp. 81-82, and Nagel [1961], pp. 260-265.

[196] Ibid., p. 65 and p. 262, respectively.

[197] _Ibid_.

[198] See Popper [1959], pp. 78-84, and recall the discussion in Chapter 2.

[199] Refer to the reference cited in 195.

[200] Poincaré [1953], p. 176.

[201] Ibid.

[202] Russell [1953], p. 401.

[203] Schlick [1953], pp. 184-186.

[204] Ibid., pp. 187-188.

[205] Blaug [1980], p. 25.

[206] Popper [1959], p. 140.

[207] Ibid., p. 137.

[208] Ibid., pp. 140-142.

[209] Popper shows how Euclidean geometry is the "simplest"; Ibid., pp. 143-144. These pages and the ones that follow provide illustrations of Popper's "simplicity."

[210] See Lakatos [1978a], p. 106, n. 1 and 2.

[211] Ibid., pp. 173-175.

[212] See Popper [1959], pp. 78-84.

[213] Ibid., p. 80.

[214] Ibid., pp. 144-145.

[215] Ibid., p. 80.

[216] Ibid., pp. 80-81.

[217] Baldwin [1909], pp. 68-73, quoted in Campbel [1974].

[218] Agassi [1974], p. 693.

[219] See Popper [1963], pp. 109-114.

[220] The problem of meaning is a major concern of the logical positivists. Thus we postpone its complete discussion to the next analysis. Popper convincingly argues that the question of meaning is a pseudo-problem. For the best synopsis of this issue see Popper [1974a], pp. 12-23.

[221] Popper [1962], p. 19.

[222] See Popper [1963], pp. 97-119, for the following argument.

[223] Bar-Hilel [1974], pp. 345-346.

[224] Popper [1963], p. 98. Truth is never necessary for a theory but it is sufficient.

[225] Quoted in Popper [1962], p. 46.

[226] Logical positivism should not be confused with the earlier positivism of Comte. His philosophy asserted our ability to separate "normative" and "positive" statements permitting induction from "positive" beginnings. Apparently, Comte's positivism has had significant influence upon economics as witnessed by the frequency of the debate about "normative" vs. "positive" economics.

[227] See Barrett [1979], pp. 3-29, for a more detailed description of this story.

[228] Wittgenstein [1922], p. 21, quoted in Barrett [1979], p. 36.

[229] Ibid., pp. 36-40.

[230] See Gödel [1965] and Rosser [1939].

[231] See Popper [1974a], pp. 63-71 and n. 106.

[232] Popper [1974b], p. 964.

[233] See Popper [1974b], pp. 963-984, for his statement of his relationship with the Vienna Circle and logical positivism.

[234] Ibid., and see Kraft [1974], particularly p. 200.

[235] Kraft [1974], p. 191.

[236] Popper [1974b], p. 963.

[237] For example, see Popper [1959], p. 51, and Popper [1974b], p. 964.

[238] Wittgenstein [1922], quoted in Popper [1959], p. 51, n. 2.

239 See Carnap [1953], pp. 47-49 and 72-74.

240 See Kraft [1974], pp. 196-197 and 201; Popper [1974a], pp. 67-71; and Popper [1974b], pp. 968-974.

241 See Kraft [1974] for a survey of the major shifts in the Popper-logical positivism debate.

242 For example see Popper [1963], pp. 36-37.

243 For example see Settle [1974], p. 733.

244 Keynes, J. M. [1973], p. 3.

245 _Ibid_.

246 _Ibid_., p. 4.

247 _Ibid_.

248 _Ibid_., p. 317.

249 For example see Einstein [1954], pp. 334-335.

250 _Ibid_.

251 Settle [1974], p. 707.

252 Keynes, J. M. [1973], p. 80.

253 Popper [1959], p. 181 and n. 1.

254 Keynes, J. M. [1973], p. 244.

255 _Ibid_., p. 4.

256 _Ibid_., p. 78.

257 Keynes, J. M. [1973], p. 243.

258 _Ibid_., p. 303.

259 _Ibid_., p. 266.

260 Popper [1959], p. 369. Popper is quoting from Hume's _Treatise of Human Nature_ [1739-40], sections XII and VI, and _An Abstract of a Book Lately published entitled A Treatise of Human Nature_ [1938], edited by J. M. Keynes and P. Sraffa. The emphasis is Hume's.

[261] Keynes, J. M. [1973], p. 244.

[262] *Ibid.*, pp. 260-264.

[263] *Ibid.*, pp. 262-264.

[264] *Ibid.*, p. XXII.

[265] *Ibid.*, p. XIX.

[266] See particularly Popper [1959], pp. 20 and 262-273.

[267] See Popper [1963], pp. 388-391, and Popper [1959], pp. 146-214 and 251-387, for his development of the probability of theories.

[268] See Popper [1959], pp. 212-214 and 269-273, for development and use of such a subclass relation.

[269] See, for example, Popper [1963], p. 218, n. 3.

[270] Much of Popper's *Logic of Scientific Discovery* is devoted to the methodology of 'testing' theories that contain probability statements. As Popper first proposed his falsificationism, he saw the potential for statements of probability of being 'irrefutable' and thus a threat to his philosophy. Yet, like all statements, probability can be 'falsifiable' or 'irrefutable,' depending only upon how we hold them. Popper developed the necessary methodological rules which allow probability statements to be 'falsified.'

[271] Lakatos [1978a], p. 192.

[272] *Ibid.*

[273] See the two volume set of *The Philosophy of Karl Popper* for many such examples and Popper's replies.

[274] Popper [1963], p. 221.

CHAPTER 4

METHODOLOGY OF ECONOMICS: AN IRREFUTABLE STORY

There are many reasons why one might wish to investigate the methodology of economics. Methodology may be of interest in itself. Or, an understanding of methodology may help promote an understanding of economic knowledge. For what is knowledge other than its production process? And, methodological understanding could spur the growth of economic knowledge by rectifying past methodological errors and inadequacies.

Many, if not most, studies of economic methodology are undertaken to criticize a particular economic theory or school of economic thought - for example, Friedman vs. the antimarginalists, Hollis and Nell [1975], and to some extent Hutchison as well as most others. Defenders and critics alike point to methodological issues as support for thier positions on economic theory. Yet, for scientific knowledge, there is no direct connection between theory and methodology. "Good" theory may be derived from "bad" methodology, or "good" methodology may support some "bad" theory. There is no "deterministic" relationship between scientific methodology and the 'truth' of its theories. Rather, sound methodology may increase the rate of knowledge accumulation, *ceteris paribus*, or, at least, adequate methodology will not slow the growth of knowledge. Thus, it is incorrect to use methodology as support or criticism of a specific economic theory.

This chapter seeks only to review the development of economic methodology from the perspective of the philosophy of science. We wish merely to identify and categorize economic methodologies and to reflect upon them the philosophical analysis of previous chapters. Our discussion should not be considered as an attack upon some traditional economic theory, but only as a criticism of the methodology of economics. The 'truth' of economic theory is an empirical question which cannot be addressed here.

Our methodological position is developed in the previous chapters, and, in this sense, our analysis of economic methodology is a foregone conclusion. If a particular economic methodology is not falsificationism, it is not adequate in making claims of economic knowledge nor in promoting the growth of economic knowledge. To the extent that economics has employed falsificationism, we will have no quarrel. To identify the methodologies of economics, we now turn to several authors of economic methodologies.

4.1 Keynes as Classical Methodology

Most economists describe Adam Smith's _Wealth of Nations_ as the beginning of economics as a distinct discipline. That economics is older than this is not a relevant question for the current discussion, for the methodology of economics was not clearly discussed until the nineteenth century.

Throughout the nineteenth century, economists were essentially apriorists with some inductive overtones. The criterion of falsifiability was not only unemployed, it was never discussed. Blaug succinctly characterizes this period of economic methodology.

> A subtle but significant difference separates the methodological writings of the nineteenth-century economists from those of twentieth-century ones, or rather from those of modern economists in the last forty years or so. The great British nineteenth-century economic methodologists focused attention on the premises of economic theory and continually warned readers that the verification of economic predictions was at best a hazardous enterprise. The premises were said to be derived from introspection or the casual observation of one's neighbors and in that sense constituted _a priori_ truths, known, so to speak, in advance of experience; a purely deductive process led from premises to implications, but implications were true _a posteriori_ only in the absence of disturbing causes. Hence, the purpose of verifying implications was to determine the applicability of economic reasoning and not really its validity. The ingenuity of these nineteenth-century writers knew no bounds when it came to giving reasons for ignoring apparent refutations of an economic prediction, but no grounds, empirical or otherwise, were ever stated in terms of which one might reject a particular economic theory. In short, the great British nineteenth-century methodologists of economics were _verificationists_ not falsificationists, and they preached a _defensive_ methodology designed to make the young science secure against any and all attacks.[1]

To turn an ironic phrase one might say, "Nineteenth-century British economists promoted the doctrine of methodological protectionism to insure the development of their own productive capacity against the 'unfair competition' of other economic and philosophical ideas while espousing a 'free trade' doctrine to insure the wealth of the British nation." In any case, nineteenth-century methodology was not open or critical.

The best description and analysis of nineteenth-century methodology was given by J. N. Keynes at the end of this period. _The Scope and Method of Political Economy_ (originally published in 1891) provides a general and accurate characterization of this economic methodology. Where Cairnes, Mill, Senior, and the German historists were obscure or ambiguous, Keynes clarifies. Here, we employ Keynes as our interpreter and guide through classical economic methodology.

Keynes's essay begins with a discussion of definitions. "In the terms economy and economic there is an ambiguity that underlies much of the current confusion as to the nature of political economy."[2] Keynes's use of definitions for discussion is illustrative of the preoccupation that economists throughout history have had for definitions. It is as if the defining of a concept is a genuine contribution to knowledge and a substitute for substantive criticism. The definition and redefinition of economics is the usual vehicle for methodological discourse, and it is clearly reflected in Keynes.

After pointing out some ambiguity in the word "economics," Keynes asserts that "economics is a body of doctrine relating to economic phenomena in the above sense ..." (i.e., relating to wealth) and that the purpose of his contribution is to "discuss the character and scope of this doctrine, and the logical method appropriate to its development."[3] It is clear that the purpose of his discussion is to investigate the methodology or economics or, if you prefer, the logic of economic discovery.

What are the constituents of this "body of doctrine"? Keynes answers, "A positive science may be defined as a body of systematized knowledge concerning what is, a normative or regulative science as a body of systematized knowledge relating to criteria of what ought to be, and concerned therefore with the ideal as distinguished from the actual; an art as a system of rules for the attainment of a given end."[4] Each type of economic inquiry is defined relative to its goal and not by the methodology that it employs. It should be mentioned that Keynes adds a number of footnotes to clarify his word choice. Of particular interest is Keynes's dislike of the term "positive." Keynes remarks that Cairnes uses this term as the antithesis to "hypothetical." Keynes wishes to express the "abstract-theoretical" nature of "positive science," yet he does not care for the ethereal connotations of these other words.[5] Keynes is expressing the same notion of science as we have discussed, while some of his predecessors wished to imply its concreteness - as opposed to "hypothetical."

Classical methodology clearly separates the issues of science, "what is," from those of a more ethical nature, "what ought to be." Senior was the first to make this distinction, yet he is not as clear as Keynes.[6] Senior separates science as "facts" and art as "use." "Science is a statement of existing facts, and an art is a statement of the means by which future facts may be caused or influenced, or, in other words, future events brought about."[7] Senior does not state that art must necessarily be "normative" in its methods, only that some goal of application must be presupposed. Art requires scientific knowledge in order to adequately prescribe means to assumed ends, but science does not require art. Science is concerned only with the acquisition of knowledge.[8] Of tangential interest is that Senior, like Cairnes, uses the term "positive" in opposition to "hypothetical."[9] Our only point in discussing the term "positive" is one of historical interest and to clarify the original context of the term. The current use of "positive" economics can be traced back to the nineteenth-century economists through Friedman. Its original context has nothing to do with logical positivism nor was it an antonym of "normative." It was first used for contrast with a pure abstract and "hypothetical" inquiry.

Keynes is clear and explicit in his subdivision of the doctrine of political economy. His three-way classification describes how the issues of value, knowledge, use and fact interrelate. "Positive science" is generally what we have called science, yet Keynes further asserts that it is ethically neutral and separate from "normative science."[10]

Is "positive science" completely independent of normative issues? No, replies Keynes. The goal of "positive science" is application which must have its "normative" aspects. "It is universally agreed that in economics the positive investigation of the facts is not an end in itself, but is to be used as the basis of a practical inquiry, in which ethical considerations are allowed their due weight."[11] The universality of agreement has at least one exception in the author of this distinction, for Senior says, "They (history and science) do not presuppose any purpose beyond the acquisition of knowledge."[12] [Parenthesis added.] But all agree that application, art, presupposes "normative" ends.

Keynes admits another role for the "normative" within "positive science." Economic science may need to incorporate the "normative" beliefs of its subject. "Men's activities are determined partly by moral considerations; it may be necessary in positive economic science to take account of the operation of moral motives. It is not, however, the function of the science to pass ethical judgments; and political economy, regarded as a positive science, may, therefore, be said to be independent of ethics."[13] Can an activity which uses ethics as raw materials and has ethics as a goal (practical inquiry with its ethical considerations) be considered <u>independent</u> of ethics? We do not mean to imply that Keynes's "positive economics" is "normative science" in disguise, but only that a field of inquiry that can have "normative" inputs and outputs is not completely independent of ethics.

It appears that Keynes exaggerates the ethical neutrality of "positive economic science." In any case, all that is required is for "positive science" to be something more than ethics - not just the opinions and the judgments of economists. The only reason that classical methodologists assert that "positive science" is somehow ethically neutral is to give the impression that economic science is "pure" or in some sense capable of making "objective" statements.

To falsificationists, complete ethical neutrality is unnecessary. If there are unnecessary "normative" qualities to our theories, criticism will eliminate or minimize them. But, to inductivists and apriorists, the ethical nature of their inputs poses a difficulty to their claims to unambiguous "truth" or "fact." Once ethical bias finds its way into a deductive theory, apriorists have no methodological means for removing it. Deduction itself would only pass such biases along. Yet the interpretation which is required to "establish" the "facts" and thus the assumptions of a deductive theory is fertile ground for the inadvertent insertion of such "normative" considerations.

Keynes claims that "positive science" is subject to a normative asymmetry. Normative statements may be used as "assumptions" or as desired ends, but "positive science" can have no normative implications. We must apply "positive science" to obtain value implications.

The art of economics is even more directly dependent upon normative issues. The art of economics is the practical application of "positive economic science" in pursuit of some given ideal. Still, economic application will be incomplete. "If, therefore, the art confines itself to the practical application of the science, it must of necessity be to a large extent non-economic in its character and its scope becomes vague and ill-defined."[14] Keynes is straightforward in admitting that applying economics requires more than just economic knowledge and a knowledge of the desired objective. We need to know something about sociology, politics, etc. Perhaps obvious, but it is a frequently overlooked fact.

Among the additional qualities that need be considered is ethics. "It is clear, accordingly, that practical discussions of an economic character cannot be isolated from ethics, except in so far as the aim is merely to point out the practical bearing of economics facts .... It may be added that although in the past there may have been a tendency with a certain school of economists to attempt the solution of practical economic questions without adequate recognition of their ethical aspects, there is, at the present time, no such tendency discernible amongst economists who have any claim to speak with authority."[15]

Thus, in order to apply economics, one must consider other consequences of his "policy recommendations" including, perhaps, ethical questions of distribution, justice, and moral development. It may be added that although in the past economists did not overlook this need, there is a discernible tendency, at the present time, for a certain school of economists to be short-sighted in applying economics. Applied economics, an "art," must incorporate knowledge of economic science, knowledge from other sciences, as the application demands, relevant normative consequences, and ethical precepts. Thus, the inputs to application are normative and scientific and the output is practical value - 'usefulness.'

The third area of political economy is "normative science." "Normative science" is a "branch of applied ethics that seeks to determine standards, whereby judgments may be passed on ... economic activities."[16]

Oddly, this field of economic thought is the most independent of the other two. There are no constraints upon "what ought to be." One need not consider "what is" in order to determine "what ought to be." The only constraint is that the "normative science" of political economy is somehow related to economics; otherwise, we would be dealing with pure ethics not "applied ethics."

Keynes's distinctions are important and insightful. Although these definitions have no genuine content, they could be used to avoid the pointless arguments that often surround these issues. Or as Keynes puts it,

> The main point to notice is that the endeavour to merge questions of what ought to be with questions of what is tends to confuse not only the economic discussions themselves, but also discussions about economic method .... It is because differences of this kind are often overlooked that divergences of view on questions of method become exaggerated. In the controversies that ensue, one set of disputants is thinking mainly of theoretical problems, while the other set is thinking mainly of practical problems; and hence each in turn is liable to commit the fallacy of _ignoratio elenchi_.[17]

We can agree with Keynes's motivation in making these distinctions, and we can further agree that Keynes gives us a reasonable articulation of the scope of economics through these definitions. Yet, we may not fully agree with the force of Keynes's rhetoric. "Positive science" is not totally independent of "normative science," according to Keynes's definitions. All that need be asserted is that economic science is not "just normative" and that its claims to knowledge are not merely opinions. What is important to remember is that "positive science" may have normative inputs and goals, but its output need not be normative. Thus, there can be inferences of "positive science" without the injection of personal values or ethics.

Most of Keynes's book is a critical and well-balanced discussion of two general methodologies of economics, "One of which describes political economy as positive, abstract, and deductive, while the other describes it as ethical, realistic, and inductive."[18] The first is the classical British school (particularly Cairnes, Mill, and Senior) and the second is the German historical school with their few British adherents. Although Keynes advocates methodological pluralism - "The method of political economy cannot be adequately described by a single phase" (p. 30) - he clearly favors the "deductive" methodology.[19]

The deductive methodology of economics rests upon several doctrines.

(1) The possibility of a sharp distinction between economic theory and its practical applications, or between "art" and "positive science."[20]

(2) The possibility of separating the study (and thus the phenomena) of economics from social philosophy, in general.[21]

(3) The validity of the deductive method. "The right method of procedure is ... deductive, or ... a priori."[22]

(4) The fact that "positive economics" is an abstract science similar to mathematics and physics.[23] Thus, the concept of "economic man" is a convenient abstraction not to be taken literally.[24]

Of these four doctrines the last finds the greatest divergence of opinion among the "deductivists."

Keynes strikes a fairly even compromise among the deductivists on this issue. He sees that realism demands that man is more than just abstract "economic man," but he feels that such abstraction can approximate actual facts.[25] "That other motives besides the desire for wealth do operate on various occasions in determining men's economic activities is recognized. They are, however, to be neglected - at any rate in the first instance - since their influence is irregular, uncertain, and capricious."[26]

Senior, in contrast, completely disdains the "hypothetical" character of economic theory. He seems to believe that economic reasoning can somehow be based upon the "truth."[27] Senior's focus on truth and certainty is also reflected in an introductory lecture. "I hope in the course of these lectures to prove the truth of my statement, that the theoretic branch of the science, that which treats of the nature, production and distribution of wealth, is capable of all the certainty that can belong to any science, not founded exclusively on definition; and I hope, also, to show that many conclusions and those of the highest importance, in the practical branch, rest so immediately on the conclusions of the theoretic branch as to possess equal certainty and universality."[28] Although Senior carefully qualifies his assertion of certainty, it seems clear that he regards economic theory as such. This follows from Senior's disavowal of the "hypothetical" nature of economic theory and by the popular association, at the time, of science - mathematics and Newtonian physics - with truth and certainty. Recall that this is Kant's position.

At the other extreme on abstraction is J. S. Mill. Mill is given credit for the creation of our current notion of "economic man."[29] In so doing, Mill speaks of the hypothetical nature of "economic man."

> Political Economy is concerned with man solely as a being who desires to possess wealth, and who is capable of judging the comparative efficacy of means for obtaining that end .... It makes entire abstraction of every other human passion or motive, except those which may be regarded as perpetually antagonizing principles to the desire of wealth; namely, aversion to labour, and desire of present enjoyment of costly indulgences .... Not that any

> political economist was ever so absurd as to suppose that mankind are really thus constituted, but because this is the mode in which science must necessarily proceed .... The conclusions of Political Economy, consequently, like those of geometry, are only true, as the common phrase is, in the abstract; that is, they are true under certain suppositions, in which none but general causes are taken into account.[30]

There can be no doubt that Mill sees economic theory as abstract and hypothetical, like geometry, for which the implications remain conditional and qualified.

Cairnes's position is similar to Senior's, yet stronger. Cairnes asserts that economic assumptions are not only <u>not</u> hypothetical, they are based upon unquestionable "facts."[31] Clearly, both Senior and Cairnes are apriorists, and, as we shall show, so are Keynes and Mill. The latter two are only more cautious and qualifying in their descriptions of economics. There was a great deal of controversy concerning the nature of economic theory among the classical economists, yet all used some type of "deductive" method. And, this question of how to regard the "assumptions" is paramount to the classical methodology. For, when one's method is solely deductive, what else is questionable?

Regarding the other doctrines of the "deductive" method - previous points (1), (2), and (3) - more need be said. The first two which separate economic science from art and other sciences may easily be taken for granted. Why not? What is interesting about these doctrines is what their use tells us about classical methodologists. Since economics is distinct from physical and other social sciences, economists may use the results of these fields as economic premises. And, these classical economists regard these results as "fact." The confusion of fact and theory is a continual theme in economic methodology, and we dub it the "factual fallacy."

This fallacy makes its strongest appearance with nineteenth-century economists, and all committed this mistake. The "factual fallacy" is to use universal statements instead of singular instances as "fact." In addition, these "facts" are often incapable of being, even potentially, observed. To cite one example, "The bare facts that other things being equal men prefer a greater to a smaller gain are psychological facts of great economic importance."[32] The first difficulty with this statement is that it refers only to universals. There are no qualifiers for time, place, or the individual; clearly Keynes means to say that all men always prefer more to less. In addition, these "psychological facts" are theories; they are highly abstract and conditional; and they cannot be observed. Preference cannot be observed, only the resulting behavior is observable. Preference is a theory. Only if we state our preference theory in a manner that is 'falsifiable' (and current preference theory is not) could we say that observed behavior, "fact," has any relation to our theory of preference. The central point is that general statements cannot be observed and they cannot be "facts." The classical economists merely base one theory upon another and claim that this process justifies or establishes the "truth" of at least one of these theories.

This "factual fallacy" is even more strongly stated by Keynes as he discusses the role of physical theory in economic theory. "The political economist does not attempt to explain the physical laws on which qualities of the soil depend; and no more does he undertake to analyze the nature of these feelings of self-interest in the minds of the landlord and the tenant which regulate the terms of the bargain. He regards them both as facts, not to be analyzed and explained, but to be ascertained and taken account of; not as the subject matter, but as the basis of his reasoning."[33] Thus, physical laws or theories are also "facts," and these "facts" may be taken as proven "assumptions" upon which to deduce further "facts." It is this "factual fallacy" that is the source of much methodological error and confusion about economic epistemology. For example, Robbins commits such an error in his view of preference theory (see the next section).

Still questions remain. "What is the deductive method"? Or more importantly, "How does it regard and relate 'assumptions,' 'facts,' and 'theories'?" Here too, there is some disagreement among classical methodologists.

At one extreme is Cairnes. To him, "assumptions" are "facts" and they can be easily "proved."[34] "The economist starts with a knowledge of ultimate causes. He is already at the outset of his enterprise, in the position which the physicist obtains only after ages of laborious research."[35] With such a belief the deductive method would obviously be the "correct" one. If we begin with truth, our deductive inference must also be true. Yet there is a catch. To Cairnes subsequent economic "facts" need not correspond to our deductive implications. When they are inconsistent with our theory, it is due to a failure of the <u>ceteris paribus</u> clause. That is, any discrepancy between theory and fact is the result of the intervention of some disturbing influence. For example, when discussing an apparent empirical contradiction of the quantity theory of money, Cairnes asserts that:

> It is not to be supposed that the discrepancy alluded to goes the length of invalidating the elementary law that, <u>ceteris paribus</u>, the value of money is inversely related to its quantity. This still rests upon the same basis of mental and physical facts as every other doctrine of political economy, and must always constitute the fundamental principle in the

> theory of money. It merely showed that in the practical case the condition _ceteris paribus_ was not fulfilled. The fact in question is no more inconsistent with the economic law than the non-correspondance of a complex mechanical phenomenon with what a knowledge of the elementary law of mechanics might lead a tyro to expect is inconsistent with these elementary laws. A guinea dropped through the air from a height falls to the ground more quickly than a feather; yet no one would on this account deny the doctrine that the accelerating power of gravity is the same for all bodies.[36]

Therefore, empirical evidence cannot have any effect upon our theory. Such evidence cannot reject, invalidate, nor, dare we "say," falsify a theory.

Nonetheless, Cairnes sees a role for verification. Although verification is an imperfect affair, it can give us a high degree of confidence in our deductive conclusions.[37] One must wonder about the asymmetry of such a position. How can imperfect empirics provide confidence in a theory when it cannot provide evidence against a theory?

Clearly, Cairnes is an apriorist and his methodology is defensive in the extreme. To point to the _ceteris paribus_ clause for each empirical threat to one's theory is the classical example of an _ad hoc_ "conventionalist strategem." No more dogmatic can a defensive methodology be. Yet, Cairnes's dogmatism is unavoidable, once his apriorism is accepted. If our theories use only proven "fact" and deduction, any resulting discrepancy with subsequent events must be due to "other things." These "other things" may make our predictions incorrect but they cannot invalidate the theory. Thus, theories are true or proven "facts"; only our application of theory is "hypothetical" and dependent upon "disturbing causes."

In contrast to this extreme apriorism of Cairnes is the more moderate apriorism of Keynes and Mill. Keynes's book seeks a compromise between the "deductive" and "inductive" schools of economic methodology. Consequently, Keynes "sounds" less extreme, and he states the "deductive" methodology in its most flattering light. For this reason, we use Keynes's methodological position as representative of the classical methodology.

Keynes employs much rhetoric in stating the empirical connections of the "deductive" method. The deductive method, says Keynes, "must both begin with observation and end with observation."[38] "In the first place observation guides the economist in his original choice of premises."[39] And he adds, "The observation requisite for the selection of premises sometimes involves little more than reflective contemplation of certain of the most familiar of everyday facts."[40] We do not question the fact that classical methodologists believed that observation is important, but we do question whether this type of observation has any similarity with the modern sense of the word or with the characterization of 'observations' and 'facts' presented in previous chapters.

What we need to recognize is that the empiricism of the classical methodologists, including Keynes, is some type of "casual introspection." Some inner realization allows us to "intuit" the underlying "facts." Mini goes as far as to explain all of the history of classical economics as the application of the Cartesian ego and philosophy.[41] To Mini, the Cartesian duality of subject and object becomes theory vs. practice or, as Keynes would say, positive science vs. art.[42] Perhaps Mini exaggerates the Cartesian influence upon economics, yet it is clear that the classical notion of "observations" has a distinctive Cartesian ring. What is "casual introspection" of universal statement other than some type of Cartesian intuition?

Some economists, Cairnes and L. von Mises, as we shall see in the next section, go even further to believe that these "facts" are necessary for experience; thus, they follow Kant. Words aside, the "deductive" method does not begin with observations, but with Cartesian intellectual intuition or Kantian a priori, synthetic "truth." How else can we explain how the classical economists "observed" the law of diminishing returns or the "fact" that one must "prefer" more to less?

The second step of the deductive method is straightforward and unproblematic. It is simply deduction from our factual premises. However, the third step is not so simple. The last step somehow involves the verification or modification of our theory. At this stage, "observation enables the economist to determine how nearly his assumptions approximate to the actual facts under given economic conditions. He thus learns how far his premises require to be modified; or to what extent, where no actual modification of premises is necessary or feasible, allowance must be made for the effects of so-called 'disturbing causes.'"[43] Keynes verbally recognizes the value of "negative verifications." They can modify how we apply our theory or interpret our applications. But, empirical evidence can, in no sense, falsify our theory. Instead, empirics are used in the deductive method only to "verify" the a priori theory or to adjust the theory's range of application. "Without the aid of an extensive knowledge of facts, there is danger of ascribing to economic doctrine a much wider range of application than really belongs to them.[44] At worst, negative empirical evidence will be deflected by a theory's "application clause." Such a modification to the art of economics could be useful, but how many times were such adjustments incorporated into later economic "predictions"?

Though Keynes is fond of claiming the significant role of "observation," he does not wish to rest his case upon empirical evidence. He has confidence in economic theory, regardless of the "empirical support."

> For we may have independent grounds for believing that our premises correspond with the facts, and that the process of deduction is correct; and we may accordingly have confidence in our conclusions in spite of the fact that there is difficulty in obtaining explicit verification. There must not be a manifest discrepancy between our theoretical conclusions and the actual facts. But we should not hastily draw negative conclusions or suppose theories overthrown, because instances of their operation are not patent to observation. For the complexity of the actual economic world, which in the first place makes it necessary to have recourse to the deductive method, may also render it difficult to determine whether or not the actual effects of any given agency really correspond with the results of our deductive calculations.[45]

Keynes's apriorism is even more strongly seen when one realizes that the above passage is Keynes's "qualification" to Mill's more moderate position. Mill goes so far as to say that 'the ground of confidence in any concrete deductive science is not the _a priori_ reasoning itself, but the accordance between its results and those of observations _a posteriori_.

So what is the independent evidence? It can only be the faith one has in the economic assumptions and the faith in deduction itself; though, perhaps, intuitively satisfying these considerations cannot provide any "independent grounds" for holding a theory. "Independent grounds" come only from the "facts." Yet, Keynes is defensive and inconsistent when considering empirical evidence. If the evidence supports our theory, then we have reason to suppose our theory is correct.

But, due to the difficulty in determining whether theory "really corresponds" to the facts, we cannot be too hasty to reject a theory that is counterfactual. What epistemological principle would count "verifying" evidence while ignoring "falsifying" evidence? While Popper, too, finds difficulty in the determination of the 'facts' (recall quotations 34 and 40 of Chapter 2), he does not use their fallibility as a justification for the concentration upon "verification," as does Keynes. In fact, the fallibility of the 'facts' should only redouble our efforts to 'falsify' our theories and to 'test' our 'facts.'

This empirical bias is quite strong and unambiguous in the deductive method. This is further evidenced by the footnote that Keynes adds to our last quotation which concerns the "verification" of the quantity theory of money. "In some cases confirmation may be very clear and decisive, but sometimes there may be the greatest difficulty in allowing properly for the effects of an increase or diminution in the general volume of trade ... and so forth ...."[46] Or, if the "facts" agree with our deductive theory, we have "clear and decisive confirmation"; otherwise, the discrepancy can be accounted for by the _ceteris paribus_ clause. What stronger dogmatic defense is possible? This type of methodology simply refuses to learn. Anything that conforms to what we already believe is accepted, anything that does not is rejected. Is it any wonder why the same economic theories have lasted so long?[47]

Keynes summarizes his view on the deductive method by stating that,

> Deductive political economy is rightly described as hypothetical, if by this nothing more is meant than that, in the first place, its laws are statements of _tendencies_ only, and are therefore usually subject to the qualifying condition that _other things are constant_, and that, in the second place, many of its conclusions depend upon the realization of certain

> positive conditions, which are not as a matter of fact always realized. Given the conditions however, the laws may be stated categorically. The conditions are, moreover, not arbitrarily assumed, but are chosen so as to correspond broadly with the actual facts in the different forms in which economics phenomena manifest themselves. In saying, therefore, that political economy, in so far as it has recourse to the deductive method, is a hypothetical science, it is necessary to guard against the idea that this implies unreality or want of correspondence with the actual order of economic phenomena.[48]

We can all agree that the "hypothetical" nature of economic theory does not directly imply its "unreality," but neither does it imply its reality. When are deductive economic theories "realistic"? Whenever certain "positive" conditions are given - or in other words whenever the "assumptions" hold - the derived "laws" are categorically "true," responds Keynes.

Nonetheless, this view of "deductive" theories says nothing about the empirical nature of these theories, nor does it concern science, as we have discussed it. All economic theories have the logical form if __________, then __________. Obviously, if the "givens" are given, then the "thens" follow. The deductive methodology says no more than this elementary logical truth. It is only in this sense that Keynes believes that economic theory is "hypothetical."

Although it would be hasty to claim that such "hypothetical" theories are unrealistic, it is a fact that such theories are completely independent of reality. As such, they are members only of formal science. Hofstadler aptly terms if-then statements "fanacy rules."[49] Not only are such statements independent of the "facts," they can be "true" and empty at the same time. For, if the assumed conditions are impossible, even within formal science, then the if-then statements will be "true" but never fulfilled.

It is only in this logical sense that the deductive methodology can claim validity for economic theories, for, in actuality, the necessary "positive" conditions of a theory may not hold. And, the deductive methodologists are quick to assert that we cannot be sure whether there are discrepancies between the "facts" and our assumptions. Thus, deductive political economy is only an abstract, formal science in the same sense as Euclidian geometry (recall our previous discussion of conventionalism). Keynes, too, is an apriorist.

Some might perceive differences between the classical deductive method and apriorism. Yet these differences are mostly verbal. For instance, the classical methodologists often speak of induction, but their induction is no more than the first and third steps of the deductive method. Neither "casual introspection" nor "verification" by favorable events can be reasonably considered induction. Where and how do they derive universal statements from singular observations? No explicit method is given.

Where the deductive method might differ from apriorism is the process of "verification." If classical economists actually modify the range of a theory's application as a consequence of the "facts" and incorporate the limitation in subsequent applications, they improve apriorism, at least in its "art." What is never quite said explicitly is that every time that a theory is used to address some actual economic phenomenon, it is considered an "application," or as the "art" of economics. Yet, others have recognized this classical view of empiricism (for example, recall quotation 1). Thus, the classical methodology holds that whenever we consider a "verification" of a theory, we are in the "art" of economics not its "positive" science.

Although this is never stated in these terms, it is clear in the context of their methodology and it is the only explanation for apparent inconsistencies in the deductive method. This identification of empirical economics with application still exists. Currently, applied microeconomics is anything that deals with the empirical behavior of firms or the individual. To the classical economists, application is "art."

The "art" of empirical economics may also be seen when we ask what role does "verification" have in the deductive method. Is it used to reject or falsify erroneous theories? Is it needed to increase our confidence in our deductive theory? Is it used to identify the "positive" conditions or to check the <u>ceteris</u> <u>paribus</u> clause? Or is it employed to adjust the deductive theory's range of application? The answer to the first question is unambiguously negative. Nor does it seem that "verification" is useful in obtaining a high level of confidence in the deductive theory. Since the deductive theory uses only "facts" as "assumptions," its implications are known to be valid and factual. In addition, the deductivists have other reasons for high confidence in their theories without bothering with deceptive empirical questions (recall quotation 45). With respect to the third question, what purpose is there to identify the "positive" conditions or to check the <u>ceteris</u> <u>paribus</u> clause if not to determine the applicability of the deductive theory? Therefore, the only reasonable role for "verification" is to check and/or adjust the theory's applicability.

This interpretation of verification as part of the "art" of economics also clears up some of the apparent contradictions of the deductive method. Now it can make sense to ignore factual inconsistencies and, at the same time, to count "positive verification" as support for a theory. If the process of verification is the art of economics, then factual descrepancies only point to the inadequacy of our "art" not our "science." While, on the other hand, if such application ever turns out as the theory predicts, then this would be evidence of the usefulness of our theory and thus its "truth." Or, at least, this is what a deductivist could assert about the results of "verifications."

This interpretation also explains how the deductivists could say that "facts" are important and yet forbid them the ability to reject or 'falsify' a theory. Such a methodological position is reasonable only if "facts" and theory are considered as parts of different processes. If empirical economics is part of the art of economics, it might be seen as reasonable to forbid "bad art" from influencing "good science." And still the importance of "facts" can be maintained within the art of economics which is, after all, the goal of "positive" science. Thus, negative empirical testing only reflects upon our art, and "verification" is confirmation that our theory is practical. In any case, this interpretation of empiricism as "art" presents the deductive method in its best light by explaining its apparent contradictions and its excessive dogmatism.

The interpretation that nineteenth-century methodologists equate empirical economics with the art of economics only makes stronger the apriorism of these classical methodologists. For then "positive" economic science which uses only the deductive method is not empirical at all. Empiricism, as we know it, is reserved for the art of economics. Again, we may conclude that these classical methodologists, including Keynes, are apriorists. One might see some more eclectic methodology for the art of economics, but science and its methodology is confined to the "positive" side by these economists. And, one could ask, just how empirical were the nineteenth-century economists?

The methodology of Senior, Mill, Cairnes, and Keynes is apriorism, an apriorism that is best described as Cartesian (recall our discussion of apriorism, Chapter 3, section 3.3.2). "Casual introspection" fulfills the role of the more general concept of Cartesian intellectual intuition. The very fact that economists recognized the possibility of some discrepancy between theory and fact suggests a non-Kantian apriorism.

It is somewhat ironic that classical economics is Cartesian apriorism. The usual metaphor of economic science, then and now, is Newtonian physics; for example, the concept of "equilibrium." Yet, Newtonian physics and the Newtonian methodology are all but the reverse of Descartes.

In summation, the classical nineteenth-century methodology is the deductive method. The deductive method is aprioristic and holds that:

(1) A theory's "assumptions" may be obtained from "casual introspection" and may be regarded as "facts."

(2) Deduction can then derive all the remainder of our knowledge from these given "assumptions."

(3) The "positive" science of political economy is abstract, in the same sense as "pure" geometry. Deductivists deny only the negative connotations of the words "abstract" and "hypothetical."

(4) The process of "verification" can support a theory but never "disconfirm" it.

(5) Empirical investigations are applications of theories, thus art not science.

Classical methodology is, as a result, extremely defensive towards economic theory.

Since classical methodology is a form of defensive apriorism, it is subject to the full force of our previous 'rational criticism' of apriorism (recall section 3.3.2). These criticisms may be briefly restated as:

(1) Classical methodology makes erroneous and inadequate claims of knowledge. "Casual introspection" cannot be considered "fact" or "truth," nor can it be used to justify, establish or provide a foundation for scientific knowledge.

(2) Classical methodology cannot explain the growth of knowledge nor does it encourage its growth. The deductive method is inconsistent with the growth of knowledge. It is inconsistent with the growth of knowledge since it does not permit the replacement of one theory by another or the basis of empirical evidence.

(3) Classical methodology not only permits dogmatism, it forges the dogmatist's tools. Any discrepancy between theory and fact is due to "disturbing causes." Reference to "disturbing causes" is merely the "conventionists' strategem" of "blaming" the _ceteris paribus_ clause or the data.

The outstanding characteristic of nineteenth-century methodology is its dogmatism and its resistance to change. The dogmatism of the classical methodologists is well illustrated by how they practiced their science. They simply never allowed the "facts" to modify their theories, regardless of what they said about the role of such modifications.[50]

It is this feature of economic methodology's tradition that explains the tenacity of particular economic theories. With such an anticritical, protective methodology, is it any wonder that most of the classical economic theories are still contained in current economic theory?

4.2 Apriorism Regained: The Twentieth-Century Story

Keynes was unsuccessful in eliminating economic methodological competition. With the advent of the twentieth century, methodological alternatives blossomed, including a new arrival, American institutionalism. Instead of harmony of method, there was a greater dispersion of techniques for the production of knowledge. Thus, orthodox economics required another statement of classical methodology to achieve methodological unity. Just such a restatement of classical methodology was given by Lionel Robbins in An Essay on the Nature and Significance of Economic Science [1932].

In general, the content of Robbins' essay is merely another assertion of the deductive method of Senior, Mill, Cairnes, and Keynes. Yet, Robbins manages a few new twists upon this old theme. In particular, Robbins states a definition of economics that has remained the most accepted definition of the field.

Robbins begins his essay with a discussion of definitions. Again, there is a presumption that disagreement is due to differences of definitions and that the only problems of methodology concern the associated definitions. In fact, Robbins' entire essay is little more than a discussion of definitions.[51]

Robbins gives definition a preeminent methodological role. He even seems to believe that definitions are subject to "test." "But the final test of the validity of any such definition is ... its capacity to describe exactly the ultimate subject-matter of the main generalizations of the science."[52] But, is not the validity of a definition given, by definition? Yet, Robbins is attempting to establish his beliefs about the scope of economics and its ultimate nature, at that, by definitions.[53] It is certainly easier to argue about words than to rationally analyze the content of problems and their tentative solutions. And, who is to say what the ultimate subject-matters are? The quest for precise meaning, like the quest for certainty, is misguided. Each is as impossible - both involve an infinite regress - as they are unnecessary.[54] Nothing can be proved or somehow justified by definitions, and the question of meaning is but a pseudo-problem which can all too easily cause confusion.

The remainder of Robbins' essay is a restatement of apriorism.

> The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality .... These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realized. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious.[55]

Just as it is "obvious" that Robbins is using Cartesian intellectual intuition to determine his "facts." Robbins is not content to merely assert that economic assumptions are "facts" of such "obvious validity" that controlled experiments are not required to "establish their validity." He insists that economic "facts" are "essential constituents of our conception of conduct with an economic aspect."[56] Not only is it unnecessary to question economic "facts," but we have no alternative to conceiving them in economic behavior. Thus, to Robbins, "assumptions" are "facts," and "facts" are obvious and sometimes Kantian, synthetic, _a priori_ truths.

Robbins believes that we cannot test our theory but only its application. Robbins labels all empirical considerations "realistic investigations," and he recognizes three types. But only one of these comes even close to being a test.

> As we have seen already, the validity of a particular theory is a matter of its logical derivation from the general assumptions which it makes. But its applicability to a given situation depends upon the extent to which its concepts actually reflect the forces operating in that situation. Now the concrete manifestations of scarcity are various and changing; and, unless there is a continuous check on the words which are used to describe them, there is always a danger that the area of application of a particular principle may be misconceived .... If then, while retaining the original term, we proceed to interpret a new situation in terms of the original content, we may be led into serious misapprehension. We may even conclude that the theory is fallacious .... Only by continuous sifting and scrutiny of the changing body of facts can such misapprehensions be avoided.[57]

To Robbins the "truth" of a theory depends only upon its logical derivation. "Realistic investigation" is only a check of the applicability of our words. This, by Robbins' words (see quotation 55), makes economic science the study of words which we use to describe scarcity. Again, we see the circularity of Robbins' methodology.

Robbins illustrates his empiricism by discussing the quantity theory of money. The theory is both true and a "fact." The only empirical question is what should we call money.[58] Clearly, there is no possibility of testing, in any sense, the theory. At best, we are testing how we regard the "facts." Robbins, like the classical economists, views empirical study as an application of economics, or art. We can only test our art, never our theory. If Keynes is ambiguous on this point, Robbins is not.

Nonetheless, Robbins recognizes two additional roles for empirical study. "Realistic investigations" may suggest auxiliary hypotheses or areas for extending our theory. Robbins summarizes,

> Realistic studies may suggest the problem to be solved. They may test the range of applicability

> of the answer when it is forthcoming. They may suggest assumptions for further theoretical elaboration.[59]

But, this adds only the possibility of suggestion. Empirical studies cannot change or criticize our theories, they can give us only suggestions. This tells us nothing. Anything from any source is equally valid in suggesting possible theoretical changes. Suggestions and sources are irrelevant. Only severe empirical "testing" and 'corroboration' give weight for or against a scientific theory.

Here Robbins gives us nothing new. His position is merely a dogmatic assertion of apriorism. With other apriorists, Robbins has a problem with the growth of knowledge. When our knowledge depends only upon the validity of our derivation and our "assumptions" are "obvious facts," theory changes imply that either our derivations were wrong or our "obvious facts" are not fact.

Robbins gives us an example of the growth of economic knowledge. "But the great discovery, the Mengerian revolution, which initiated the period of progress, was the discovery of the premises themselves."[60] Yet, these premises are the same ones that Robbins regards as "facts" and *a priori* truths.[61] Is this not a contradiction? If the existence of a preference ordering is so obvious and true, how can you explain why it took so long to be discovered? Is it because economists before 1870 were so blind that they could not see the obvious? Robbins' problem with the progress of economic theory is only a reflection of the inconsistency of apriorism with the growth of knowledge.

Robbins is merely repeating the nineteenth-century methodological position, except perhaps that he is more dogmatic and parochial than his predecessors. His parochialism is illustrated in his argument against methodological monism (i.e., the position that holds a single method for all sciences).

> It may be admitted that our knowledge of the facts which are the basis of economic deductions is different in important respects from our knowledge of the facts which are the basis of the deductions of the natural sciences. It may be admitted, too, that for this reason the methods of science - although not the tests of its logic consistency - are often different from the methods of the natural sciences. But it does not follow in the least that its generalizations have a "merely formed" status - that they are "scholastic" deductions from arbitrarily established definitions. Indeed, it may be urged that, on the contrary, there is less reason to doubt their real bearing than that of the generalizations of the natural sciences. In Economics, as we have seen, the ultimate constituents of our fundamental generalizations are known to us by immediate acquaintance. In the natural sciences they are known only inferentially. There is much less reason to doubt the counterpart in reality of the assumption of individual preferences than that of the assumption of the electron. It is true that we deduce much from definitions. But it is not true that the definitions are arbitrary.[62]

If so, why is it that physicists know much more about what "electrons" will not do than economists know about what people will not do? Robbins is completely confused about facts, logic, and definitions. Every point in the above argument is mistaken. This argument is, at best, an empty and dogmatic defense of economics from the critics. He admits that the methods of economics are different from the natural sciences and asserts that these differences only make economic "assumptions," and consequently the associated theories, more substantial. Lord Robbins gives us no rational argument for his position, he merely asserts his preference for economic theory. Can we doubt the existence of Robbins' preferences?

What are his mistakes of reason? First, Lord Robbins commits the "factual fallacy" so common in economics. Robbins believes the unobservable universal statements are "facts." His favorite example of an economic "fact" is the existence of individual preference orderings. As discussed in the previous section of this chapter, preferences are not observable, and this "assumption" concerns all individuals at all times and places - thus universal. Furthermore, this "fact of preference" (or should one say convenience) is empty. It says nothing and has no content. The existence of individual preference orderings is consistent with all possible behaviors, particularly with respect to prices and quantities which are the only observable economic entities. Perhaps Robbins is right after all. There is no reason to doubt the "assumption" of individual preferences, for it is an empty assertion.

Many economists may still not see the vacuous nature of preference theory. Yet, I would loosely assert that there is no "fact" more "obvious" than the emptiness of preferences (which also says something about the notion of scientific discovery under apriorism). Furthermore, I will withdraw my assertion with requisite apologies if anyone can give an example of <u>potential</u> observations, concerning prices and quantities, that would 'falsify' the existence of a preference ordering. Still, some might think if the condition of transitivity is added, then preference theory has content. But when we add the assumption of transitivity, there is still no content unless we further add an assumption concerning the stability, over time, of individual preferences. Such a stable preference theory has content; for if an individual's choices are not consistent over some reasonable time period, the theory is 'falsified.' Such 'falsifying' instances of "transitivity" have been observed. Thus, preference is either empty or 'false.'

Robbins is not unaware of these problems with preference theory, but they do not seem to diminish his confidence in economic theory. Robbins admits that our theory may lead to indeterminate solutions or to upward sloping demands. But he dismisses these possibilities as improbable on no other basis than his authority. He then rests his case for preferences on the possibility of arranging our preferences.[63] Thus, to Robbins, "facts" not only can be unobservable, universal statements, but their mere possibility is also sufficient for their "factness." Apparently, Robbins' methodology permits the use of probablism in defending one's theories.

Robbins also commits the "conventionalist mistake" (recall the previous chapter's discussion of conventionalism). In the above quotation, Robbins believes that the assumption of individual preference is somehow more substantial than the assumption of an "electron." But, "electrons" have no substance nor correspond to anything in reality. The word "electron" is a technical term that is only implicitly defined, such as "point" and "line" in "pure" geometry or "gravity" in Newtonian physics. "Electron" is merely short-hand notation for a whole set of theoretical propositions concerning sub-atomic phenomena. And, no statement is derived from the "assumption" of an "electron."

The questions of whether an "electron" really exists or has a counterpart in reality are misconceived. All that matters is that the theories that contain the word "electron" are 'falsifiable' and that severe 'testing' has failed to 'falsify' them. Can anyone truly doubt whether the physical theories that employ the term "electron" better explain physical phenomena than preference theory explains individual behavior or economic phenomena? Or, to satisfy the pragmatists, has not the harnessing of the "electron" given society more practical usefulness or "money in the pocket" than all of the utility theories combined? And, all this knowledge and utility is derived without knowing or caring whether or not an "electron" really exists.

Robbins' "conventionalist mistake" is only one more example of this common misunderstanding of science among economic methodologists.[64] Economists continually take the implicit definitions of the physical sciences out of context - which is their only "meaning" - and argue that they have little or no substance. Yes, it is true that implicit definitions or primitive terms have no content, but they are not supposed to nor are they required to have any external "meaning." Physicists are well aware of the proper role of implicit terms and say stay inside them. So what is the point to the economists' assertions? It is only to create the false impression that economic theory is on an equal or better foundation than physical theories. If this be so, why do the 'facts' clearly 'corroborate' physical theories while economic theory is usually 'irrefutable'?

Clearly, Lord Robbins is more concerned with the "words" of science than its content or knowledge. This is unmistakably the "conventionalist mistake" of confusing the "garment for the essence." Robbins is more concerned with the words we use to describe our "facts" than the "facts" themselves (recall quotation 57), which brings us to Robbins' third major mistake in his dogmatic defense of economics - contained in quotation 62.

Lord Robbins does not even understand the roles of definition and deduction. "It is true that we deduce much from definitions. But it is not true that the definitions are arbitrary."[65] To the extent that definitions are not arbitrary, they are not definitions but genuine statements-theories, 'conjectures,' 'facts,' or whatever. Definitions are only arbitrary conventions, at least in their epistemological as opposed to their historical and linguistic aspects. Furthermore, nothing can be deduced from a definition but only translated by the definition. We can deduce only from an assertion or set of assertions. Something must be asserted as true or false before logic can do anything. Definitions can only translate the results of our deductions into other words. Everywhere one can look, Robbins is concerned only with <u>words</u> and not content, whether logical, scientific, or factual. Thus, there is no content to Robbins' methodological position.

Beyond Robbins' obsession with words, there is more than a little essentialism in his philosophy. Essentialism is the view that there is some type of ultimate reality or essence which science seeks to discover. One needs to read Robbins to understand the role of his essentialism, but evidence of this assertion is unnecessary since Robbins confesses his essentialism. "(T)he chapter on the nature of economic generalizations smacked too much of what nowadays is called essentialism ...."[66] Yet essentialism is a mistaken philosophy, at least to Popper.[67]

Essentialism holds that, "<u>The best, the truly scientific theories, describes the 'essences' or the 'essential nature' of things - the realities which lie behind the appearances</u>. Such theories are neither in need nor susceptible of further explanations: they are <u>ultimate explanations</u>, and to find them is the ultimate aim of the scientists."[68] Popper argues that the existence of essences is irrelevant to science, yet the belief in them can only induce obscurantism and restrict the growth of science.[69] Essentialism is related to the mistaken equivalence of the concepts "truth" and "meaning."[70]

Essentialism is also associated with the confusion of the context of a theory and its apparent ontology. It is always possible to interpret a theory as a statement about the "being" or the "nature" of the entities employed in theory. However, such an interpretation is not of scientific interest and is the source of misunderstandings. If one is to look for the "essential nature" of things, it is easy to get lost in considerations of the "nature" or "reality" of "electrons," "gravity," or other implicit terms. Perhaps such an essentialism explains the trouble that economists have with these technical terms of the physical sciences. The important lesson to learn is that questions of "essence" or ontology lead only to metaphysical speculation. Scientific progress will result only when one explains these metaphysical entities - "electrons," "gravity" - by reference to other potentially observable phenomena.

Robbins' methodology is completely misconceived. It is apriorism and is subject to all of our criticisms of apriorism. His essentialism can only confuse economic methodology. His understanding of science is totally confused and mistaken. And, his heuristic is defensive verbalism as method.

Nonetheless, Lord Robbins also provides us with a number of sound methodological positions, some of which ironically drew the greatest criticism and controversy. For example, Robbins is responsible for recognizing the conventional nature of any interpersonal utility comparison. Like the classical methodologists, Robbins argues for the separation of normative or ethical issues from science. He asserts that "is" is entirely different from "ought." They do not even exist on the same plane of discourse.[71] From such a distinction, Robbins correctly infers that any interpersonal comparison of utility or construction of social utility must be normative and thus not strictly scientific.

Any such utility analysis must be part of the "normative science of the political economy," as Keynes would say, for these comparisons all require some presupposition of an ethical principle. If one insists upon making some type of social utility comparison, regardless of its measure, he cannot draw any policy implication, for that would be suggesting what "should" be done.[72] Measures of social utility can only be conventions, arbitrary constructs that do not carry substantive policy implications. Unless, of course, one adds the normative assertion that his measure should be followed.

Here Robbins' position is faultless. He aptly criticizes the countless attempts to justify normative policies by propositions of "positive" science. How many times has progressive taxation or some other distributional outcome been justified by the "law of diminishing marginal utility"? However, it was this position for which Robbins received the harshest criticism.[73] Apparently, Robbins' critics did not wish to "diminish their marginal returns" in the influence of policy. In any case, the criticism which Robbins received over this issue is largely misconceived and beside the point.[74] The complete separation of "is" from "ought" is a tenable philosophical position, and it directly follows that "is" (or science) cannot imply "ought" (or policy).

One of Robbins' more interesting contributions is a generalization of his position on interpersonal utility comparisons. He claims that many economic measurements are merely conventions.[75] Robbins asserts that the measurement of both prices and income can have only conventional meaning beyond their ordinal and relative significance. It appears that his position is based upon our inability to place a distinct value measurement upon these statistics.[76]

> But, if this is so, it follows that the addition of prices or individual incomes to form social aggregates is an operation with a very limited meaning. As quantities of money expended, particular prices and particular incomes are capable of addition, and the total arrived at has a definite monetary significance. But as expressing of an order of preference, a relative scale, they are incapable of addition. Their aggregate has no meaning. They are only significant in relation to each other. Estimates of the social income may have a quite definite meaning for monetary theory. But beyond this they have only conventional significance.[77]

Lord Robbins is correct to place a conventional significance upon economic measurements. However, his position is ambiguous and based upon something less than the best reasons. His ambiguity can be seen in the above by his special attention to monetary theory. Why does income only have "definite meaning" in monetary theory? Do our theories change the nature of our facts? Or, is it the tautological nature of the quantity theory of money that gives "meaning" to simple sums? It seems that Robbins is not entirely consistent in his proclamation of the conventional nature of economic statistics. Yet, it is clear that his view concerning the conventional nature of economic aggregates derives from the conventional nature of interpersonal utility comparisons.

But Robbins' position is not the best way to see the conventional status of economic measurements. As discussed in Chapter 2, Popper's falsificationism causes all scientific facts to be conventions. Rules or procedures for the observation and measurement of phenomena must be given before science has any facts. Yet, such rules need not be completely arbitrary, since they become part of science's methodology.

Theories, then, explain these 'facts' which are partially methodological convention. Are economic "facts" or statistics the same as scientific 'facts'? No, and this answer is sufficiently important to demand an explanation.

The observation of individual prices and quantities for specific times and places is the same as scientific observations. Such 'observations' are inter-subjectively 'testable.' But what economic theory talks about specific observations of price and quantities? None, for all economic theories, including microeconomic theory, concern prices or quantities of some market, and a market concerns the aggregation of measurements over time, space, firms, and/or agents. When economic "facts" are some type of aggregate, they are "more conventional" than simple scientific 'facts.' An additional methodological convention for the aggregation procedure is required before these economic statistics become 'facts.' The aggregation procedure must be part of the economic methodology; otherwise such economic measurements would not be inter-subjectively 'testable.' If there is no given role for aggregation, then different economists could easily obtain different "factual" results for the same economic phenomenon or event. And there would be no way to 'test' these different "facts" or to 'corroborate' them, unless the aggregation procedure is a methodological given.

If there is no agreement or methodological rules for the observation and aggregation of economic phenomena, there are no economic 'facts.' It is probably true that there are no economic 'facts,' in the same sense as physical science. The periodic controversy surrounding the "aggregation problem" is testimony to the scarcity of economic 'facts.' The economics profession has never decided which particular method of aggregation is to be used; thus, there are no unique inter-subjectively 'testable' economic observations and no economic 'facts.'

Most economists probably believe that there are such economic facts. Are there not particular economic aggregates that are currently used to test economic theories? Yes, but will the same economic aggregate be used, if a generally accepted economic theory is in threat of being falsified? Is there not considerable freedom for the individual researcher to choose which aggregate he wishes to support his theory and which to ignore or to rationalize away if support is not forthcoming? As long as it is possible for two economists to come to different assessments of the "facts" of some given economic actuality, there are no 'facts' of this economic phenomenon. In such cases economic theory becomes 'irrefutable,' 'untestable' and non-scientific - at least according to falsificationism.

But there is a simple solution to this problem and to the so-called "aggregation problem." All that is required is to elevate some particular aggregation procedure to a methodological rule. Then the associated economic statistics are economic 'fact,' for everyone could observe an individual economic event and come to the same measurement. "Which indices should be used?" is irrelevant, as long as the chosen index is not self-contradictory. For then economic theory is the explanation of only the chosen economic aggregates. Any other interpretation of economic theory is metaphysical speculation. In this manner, the "aggregation problem" is solved along with the question of the scientific status of economics.

Over the years, Robbins has changed his methodological position, and this change is largely due to the influence of Popper's philosophy.

In his autobiography, Robbins expresses some regret for not properly emphasizing the role of testing the "assumptions." It appears that Robbins would now rewrite his Nature and Significance of Economic Science to be consistent with falsificationism. Robbins writes, "Moreover it was written before the star of Karl Popper had risen above our horizon. If I had known then of his path-breaking exhibition of scientific method as the attempt to test for falsity to reality the models of the imagination, this part of my book would have been phrased very differently."[78] Yet one is left wondering whether Robbins would change his methodological position correspondingly, or just his words.

An even stronger version of apriorism was asserted by a contemporary to Lord Robbins, Ludwig von Mises. Von Mises, too, attempts to establish apriorism as the modern economic methodology. There is no doubt about von Mises' apriorism and its Kantian influence. He believes that theories necessarily precede experience, that theories are like logic and mathematics, and that experience can never refute theory.

> The science of human action that strives for universally valid knowledge is the theoretical system whose hitherto best elaborated branch is economics. In all its branches this science is a priori, not empirical. Like logic and mathematics, it is not derived from experience; it is prior to experience. It is, as it were, the logic of action and deed.[79]

At least von Mises is unequivocal.

Yet von Mises sees some role for experience. It has heuristic value and provides theories with their practical significance. "But this in no way alters the aprioristic character of our science."[80]

Although empirical considerations may guide our choice of problems and determine which a priori theories have practical value, they are not valid criticism of any theory.

> New experience can force us to discard or modify inferences we have drawn from previous experience. But no kind of experience can ever force us to discard or modify *a priori* theorems. They are logically prior to it and cannot be either proved by corroborative experience or disproved by experience to the contrary. We can comprehend action, only by means of *a priori* theorems .... Consequently, a proposition of an aprioristic theory can never be refuted by experience.[81]

Von Mises' position is logically faultless and philosophically tenable. Unlike other apriorists, von Mises is not inconsistent. Furthermore, von Mises gives rather unique and perceptive criticism of some other methodological precepts. For example, von Mises correctly points out that "facts" require a theory, which is the Kantian position. Also induction is invalid, since inductive generalizations are from past instances and not the future instances which a "universal theorem" would require.[82] He also gives a succinct statement of the conventionalistic view of refutation - there exists no conclusive "disproof" of a theory when it is inconsistent with the "facts." "If a contradiction appears between a theory and experience, we always have to assume that a condition presupposed by the theory was not present, or else that there is some error in our observation. Since the essential prerequisite of action - dissatisfaction and the possibility of removing it partly or entirely - is always present, only the second possibility - an error of observation - remains open."[83]

Nonetheless, von Mises recognizes, at least verbally, some role for empirical evidence. The conflict between a theory and the "facts" forces the theorist to check his reasoning. But when there is no doubt of a theory's logic, there is no doubt about the theory's truth.[84] As a warning to theorists, von Mises asserts that a theory can always be wrong, if for no other reason than human error. To illustrate this point, von Mises cites the famous blunder of J. S. Mill. Mill proclaimed the perfection of the theory of value and asserted that nothing more need be explained, "precisely on the eve of a radical change in the theory of value and price."[85]

Like other apriorists, von Mises realizes that before an _a priori_ theory can be applied its "assumptions" must be checked and fulfilled. Yet he seems to believe that these conditions are virtually always satisfied. To von Mises, economic theory more generally holds, and the _ceteris paribus_ qualifications are fewer.[86] This explains why von Mises is completely unconcerned about a theory's range of applicability.

> All the propositions established by the universally valid theory hold to the extent that the conditions that they presuppose and precisely delimit are given. Where these conditions are present, the propositions hold without exception. This means that these propositions concern action as such; that is, that they presuppose only the existence of a state of dissatisfaction, on the one hand, and the recognized possibility, on the other, of relieving this dissatisfaction by conscious behavior, and that, therefore, the elementary laws of value are valid without exception for all human action. When an isolated person acts, his action occurs in accordance with the laws of value. Where, in addition, goods of higher order are introduced into action, all the laws of the theory of imputation are valid. Where indirect exchange takes place, all the laws of monetary theory are valid. Where fiduciary media are created, all the laws of the theory of fiduciary media (the theory of credit) are valid ....
>
> Every price must be either a monopoly price or a competitive price and that there can be no third kind of price. In so far as prices on the hampered market are monopoly prices they are determined in accordance with the laws of monopoly price. Limited and hampered competition that does not lead to the formation of monopoly prices presents no special problem for the theory. The formation of competitive prices is fundamentally independent of the extent of competition. Whether the competition in a given case

> is greater or smaller is a datum that the theory does not have to take into account since it deals with categorical, and not concrete, conditions. The extent of the competition in a particular case influences the height of the price, but not the manner in which price is determined.[87]

Clearly, von Mises believes that the "assumptions" of economic theories are generally satisfied in the actual economy. But, does not his assertion of the impossibility of a "third kind of price," "precisely on the eve of a radical change in the theory" of competition (monopolistic competition), "stand as a warning to all theorists"? Or, are all modern versions of imperfect competition (for example, game theory, monopolistic competition, search theory, and oligopolistic behavior) "categorically" competition or monopoly, differing only in the "height of the price but not the manner in which price is determined"?

Von Mises gives us a logically sound and explicit statement of apriorism. As a methodology of science, sufficient criticisms of apriorism are provided in our previous discussions.

(1) Apriorism cannot provide an adequate basis for scientific knowledge nor can it adequately discriminate among knowledge claims. The use of "casual introspection," "intuition," or the "obviousness" of the "facts" is not sufficient for such purposes.

(2) It cannot explain the growth of knowledge and is inconsistent with its growth.

(3) It can only encourage dogmatism, for any discrepancy between theory and fact may be conveniently blamed on some "disturbing causes."

As a methodology of formal science - logic, mathematics, "pure" geometry, or mathematical economics - von Mises' apriorism is a sound and an adequate methodology. I can see no relevant criticism of von Mises' methodology if it is applied to formal science or, in particular, to mathematical economics. Only when apriorism enters the realm of science does it fail.

Although von Mises' methodological perspective causes him to erroneously forecast the development of price theory (quotation 87), it gives him a good understanding of the history of economic thought and the ability to prophesy developments in mathematical economics. He does not assert that economics should adopt apriorism, "but that it is so already."[88] To von Mises, the history of economic thought has an aprioristic plot.[89] Von Mises' extreme apriorism not only correctly views past economic theory but also correctly predicts economic theory. His view allows one to see pure abstractions in all their glory without the unsightly constraints that realism or empiricism impose.

As an example of this vision, "it would be possible to formulate the theory of the appraisement and pricing of the factors of production in the broadest generality so that, for one thing, we would work only with an unqualified concept, viz., means of production. We could then elaborate the theory in such a way that the three factors of production that are enumerated in the customary presentation would appear as special cases."[90] What von Mises' view permits us to see as a possible formulation of the theory of the firm has become its customary presentation. Mathematical economics still follows von Mises' methodology and vision. Yet even von Mises sees an advantage to treating the three factors of production separately. This classical presentation "is altogether warranted by the object of our investigation, of which we must never lose sight."[91]

4.3 Falsificationism Found: Hutchison and the Language of Falsificationism

The decade of the thirties was a time of economic turmoil, methodological dogmatism, and progress. The Great Depression seemed to cause as much controversy over economic theory as dislocation of markets. Much of the controversy was resolved, eventually, by J. M. Keynes's General Theory, but methodological controversy continued. The most interesting and enlightening methodological contribution of the period was put forward by T. W. Hutchison in The Significance and Basic Postulates of Economic Theory [1938]. Hutchison provided a unique and insightful perspective of economic theory at a time when the dogmatism of Robbins and von Mises was typical.

Hutchison's contribution is basically two-fold. He presents a long overdue criticism of apriorism and aprioristic economic theory. And he introduces the language of falsificationism and the seeds of its methods. To learn of Popper's falsificationism and to apply his demarcation criterion to economics so quickly represents a great intellectual accomplishment.

Nonetheless, Hutchison's essay is not without its difficulties and inadequacies. If our goal is to understand how Hutchison's methodological views fit into the broader methodological and philosophical perspective attempted here, we must first describe Hutchison's central thesis and its weaknesses. In the course of this discussion, we shall show that:

(1) Hutchison does not present a complete methodology or methodological analysis. His essay is an application of a single methodological principle, Popper's demarcation criterion, to economic theory.

(2) Hutchison's philosophical view is logical positivism. Only some of the language of falsificationism is introduced and not its philosophy.

(3) The application of Popper's demarcation criterion is overly simple and naive, negating most of the benefits that Popper's falsificationism might have for economics.

In spite of these limitations, Hutchison's analysis is a step in the right direction and a seminal thesis for economic methodology. In fact, the modern cries that economic theory must be testable or falsifiable are echoes of Hutchison's essay.

Our appraisal of Hutchison's contribution is ambiguous - that is, one can reasonably find both "positive" and "negative" aspects of this methodological analysis. Hutchison must be given credit for introducing the ideas of logical positivism and Popper's falsifiability to economics. This alone represents a significant intellectual achievement. In addition, Hutchison's empiricism is an important alternative to the popular apriorism of the 1930's. On the other hand, Hutchison's analysis is not sufficiently deep to reap the benefits of Popper's philosophy. Hutchison does not give us Popper's methodology but only his demarcation criterion. The demarcation criterion is used only to linguistically appraise single economic statements, finding nearly all to be irrefutable and non-scientific. Is it any wonder that economists were not quick to accept Hutchison's methodological analysis or adopt Popper's falsificationism? Hutchison's essay is an application of logical positivism and not the falsificationism that it superficially appears. Whether the benefits that derive from Hutchison's introduction of logical positivism and falsifiability outweigh the costs associated with confounding these separate philosophies and presenting an overly simplistic analysis of economic theory is difficult to say and for the reader to judge. We will merely point out both aspects of Hutchison's essay as we discuss it.

Hutchison begins with the customary apologies and rationalizations for studying methodology. His first important point is the statement of the demarcation criterion. "If propositions of science, as against the accessory purely logical or mathematical propositions used in sciences, including Economics, are to have any empirical content, as the finished propositions of all sciences except Logic and Mathematics obviously must have, then these propositions must conceivably be capable of empirical testing or be reducible to such propositions by logical or mathematical deduction."[92] This is Popper's demarcation criterion (recall Chapters 2 and 3). One may notice the above quote does not explicitly mention falsifiability, but it is clear in Hutchison's subsequent applications that he equates empirical testing with falsifiability.[93]

More important is the fact that Hutchison does not credit this demarcation criterion to Popper in his original text, although he has many times since remedied the oversight.[94] Instead, the Vienna Circle receives Hutchison's first reference related to the criterion of testability.[95] It appears that Hutchison's empiricism is founded upon the views of logical positivism.

Shortly after the statement of science's demarcation, we find the stated purposes of Hutchison's essay.

> In the main this essay is addressed to economists who already broadly accept the criterion we proposed, though not necessarily the precise wording of this sketchy formulation of it, and who are prepared to see it applied rigidly and unwaveringly to the particular concepts and postulates of theoretical Economics, not simply out of an aesthetic pleasure in rigor for its own sake, but for the highly practical and important reason that only through this principle have we at once a method of reaching agreement and a barrier against the pseudo-scientist. We therefore decline debate with those who do not hold with criterion just as we should refuse to play chess with someone with whom we could not agree as to the rules ....[97]

Hutchison later summarizes, "We wish to emphasize once again that this book is concerned to seek solutions of certain basic problems of economic science in accordance with the criterion we have here outlined."[98]

In other words, Hutchison merely thrusts the demarcation criterion upon the reader to accept or reject without any discussion of its merits or rationale. His essay is then only the application of this criterion to economic theory. Thus, Hutchison "assumes" a methodological axiom and applies it to actual economic theories. Does this not sound like apriorism at the methodological level, where theoretical "assumptions" become methodological criteria and empirical evidences are economic theory?

We do not wish to be critical of Hutchison's perspective but only wish to point out a few implications of his presentation. First of all, Hutchison does not give us a methodology or a philosophical analysis of alternative methodologies; he merely asserts one methodological rule and applies it. There is no alternative methodology that might replace the traditional economic methodologies, in particular apriorism. Instead, the essay is concerned with parts of two radically different methodologies - falsificationism and logical positivism (recall that the logical positivists incorporated Popper's demarcation criterion). This is our first point: Hutchison's essay is an application of one methodological rule and not a methodology.

Secondly, we lament Hutchison's omission of a rational discussion of the demarcation criterion. He simply refuses to discuss it with anyone who has not already accepted the criterion (see the previous quotation). Obviously, such an attitude is no way to convince others or, more importantly, to further the 'rational debate.' What is particularly unfortunate about Hutchison's presentation is that he effectively eliminates his entire audience; for probably no economist and certainly no economic methodologist then embraced the principle of falsifiability. Is it any wonder that economists react only defensively to Hutchison's analysis of economic theory? Hutchison missed a great opportunity to influence subsequent economic methodology by refusing to discuss methodological principles. Is it not ironic that Hutchison views the demarcation criterion as a practical means of reaching agreement about economic theory when there is still practically no agreement about the criterion itself? One can only wonder what our current methodological practice would have been had Hutchison provided his readers with some reasons for adopting the principle of falsifiability.

The major difficulty in understanding Hutchison's position is his confounding of falsificationism with logical positivism. It is best to view this essay as an application of logical positivism in some of the language of falsificationism. Both of these aspects of Hutchison's position are illustrated by his dichotomous classification of all theoretical propositions.

> According to our definitions of the terms ... either a proposition which has sense is conceivably falsifiable by empirical observations or it is not. If it is not thus falsifiable it does not, if true, <u>forbid</u> any conceivable occurrence, but only a contradiction in terms. Propositions obtain their empirical content simply in so far as, if true, they <u>exclude</u>, <u>restrict</u>, or <u>forbid</u> something. Therefore a proposition with empirical content or an empirical proposition must, by definition, be conceivably falsifiable, that is, <u>exclude</u> some conceivable possibility .... According to our proposed definition all propositions with scientific sense, then, are either conceivably falsifiable by empirical observation or not and none can be both.[98]

Hutchison calls such falsifiable statements "empirical statements" and those which are not falsifiable are "tautologies." It is this definition of these two types of statements that Hutchison uses to analyze the "basic postulates of economic science." And, it is this dichotomous classification that is Hutchison's central thesis.

Yet, such a classification of statements is impossible - a fact that Popper points out as he proposes the demarcation criterion.

> I admit that my criterion of falsifiability does not lead to an unambiguous classification. Indeed, it is impossible to decide by analyzing its logical form, whether a system of statements is a conventional system of irrefutable implicit definitions, or whether it is a system which is empirical in my sense; that is, a refutable system. This however only goes to show that my criterion of demarcation cannot be applied immediately to a system of statements. The question whether a given system should be regarded as a conventionalist or an empirical one is therefore misconceived. Only with reference to the method applied to a theoretical system is it at all possible to ask whether we are dealing with a conventionalist or an empirical theory. The only way to avoid conventionalism is by taking a decision: the decision not to apply its methods.[99]

Notice that Popper emphasizes that his criterion of falsifiability refers to systems of statements and cannot be used to classify such statements. Since a conventionalist can always insulate any theoretical system from refutation, Popper proposes that the demarcation criterion be regarded as a methodological rule or convention and not as a classification of statements or systems of statements. That is, scientists should choose to interpret their theories as falsifiable.

Clearly Hutchison sounds like he is employing Popper's falsificationism, and to some degree he is. Yet, Hutchison's principal methodological position is to do precisely what Popper says cannot be done. Hutchison applies the demarcation criterion to classify the statements of theories and then to isolate hypotheses and particular "assumptions" within a theory. Such a purely linguistic exercise cannot be further from falsificationism nor closer to logical positivism. The fact Hutchison analyzes only individual propositions greatly reduces the value of his essay and makes it a bit naive and simplistic.

As you may recall from Chapter 3, logical positivism is the philosophy of science that attempted to reduce all scientific statements to specific statements of facts. If a theory could not be thus reduced, it is meaningless, at least to the logical positivists. Logical positivism is most clearly characterized by its focus upon language. A language of science was sought in which only "meaningful" statements could be made. Although they never succeeded in finding such a language, a number of criteria were proposed. First was the verifiability criterion of meaning which was eventually replaced by Popper's falsifiability. The logical positivists employed Popper's falsifiability as a way of separating "meaningful" statements from senseless ones, which is an application that Popper never accepted. Nonetheless, the logical positivists believed that such a purely linguistic analysis of the statements of science was the proper method to appraise scientific knowledge. Since Hutchison only uses Popper's demarcation criterion to label economic statements as scientific or non-scientific, his essay is an application of logical positivism.

Nonetheless, there are aspects of Hutchison's position, other than his words, that are falsificationistic. For one, he emphasizes the 'falsifiability' of scientific theories and not their verifiability. Also he correctly interprets the criterion as demarcating science from non-science and not "sense" from "nonsense," as do the logical positivists.[100] Only in this above sense is Hutchison employing falsificationism. Otherwise, Hutchison is providing economics with an application of logical positivism. In the final analysis, his position is closer to logical positivism than falsificationism.

The influence of logical positivism upon Hutchison is strongly demonstrated by his references. Carnap, Schlick, Feigl, and the Vienna Circle, in general, are credited for the demarcation criterion and its requirement of falsifiability.[101] Popper is not given credit for his idea of falsifiability, although he is later referenced. From these references it is clear that Hutchison's position is derived from the logical positivists.

Hutchison's logical positivism is most dramatically betrayed by his mode of analysis. He merely uses his dichotomous classification to label individual statements within theoretical systems. To linguistically analyze such sentences is the paradigm of logical positivism and inconsistent with Popper's falsificationism. Such a procedure is no substitute to 'rational criticism' nor to the analysis of genuine problems and their tentative solutions. Hutchison simply looks at economic statements, including "basic postulates," and pronounces them "empirical statements" or "tautologies." Such a linguistic analysis is quite similar to Hutchison's aprioristic predecessors who look at the "words" of physical sciences and find them "meaningless" or without substance (for example, "gravity" or "electron"). One cannot look at the "words" of theories and reach any conclusion or understanding, for it is entirely how the words are used that <u>defines</u> their content and not their explicit or implicit definitions.

To apply falsificationism to economic theory, one first needs to consider the entire theoretical system including auxiliary hypotheses and attempt to find all possible empirical refutations. If such an exercise were to find an entire system empty of content, the analyst should then recommend how the system might be rendered falsifiable. One may always find interpretations of theoretical terms that allow the theory to be tested, just as one can always employ "conventionalist strategems" that deflect all criticism. The purpose of a falsificationistic analysis of economic theory would be to open the theories to increased criticism and to find ways that they might "learn from experience."

Hutchison, instead, seems only to proclaim that most of the "assumptions" of economics are "tautologies." At the time of the publication of Hutchison's essay, his criticisms were perhaps necessary and certainly provocative. If only his analysis were a bit broader in scope and perspective and his methodological suggestions more "positive," it would have greatly improved the methodological debate.

Nonetheless, Hutchison does contribute to the economic methodological debate if for no other reason than his introduction of the ideas of Popper and the logical positivists to economics. His essay is filled with references and quotations from these works. To apply these philosophical positions, however incomplete, represents a substantial intellectual accomplishment and a positive contribution to the economic methodology of the 1930's. The failure to do so caused at least one economic methodologist, Lord Robbins, some later regret.

An additional theme to our present discussion is to contrast the methodological views of Hutchison with the apriorists, as represented by Robbins. As we shall discuss, Hutchison correctly criticizes the apriorists for choosing to interpret their theories tautologically and the obvious "factness" of their central propositions.

That Hutchison confuses Popper with the logical positivists is quite understandable. Such an interpretation was common among philosophers (recall the "Popper Legend"; section 3.3.6). Hutchison wrote his essay only a couple of years after Popper's first publication, Popper [1959], when some of the logical positivists had yet to realize the differences in Popper's ideas.[102] Thus, Hutchison may be forgiven for his oversight.

Hutchison first applies his methodological analysis to "pure theory." Here, he finds "that 'propositions of pure theory' is a name for those propositions not conceivably falsifiable empirically, and which do not exclude or 'forbid' any conceivable occurrence, and which are therefore devoid of empirical content, being concerned with language."[103] Hutchison arrives at this conclusion quite naturally and by definition. He simply defines "propositions of pure theory" as those statements which concern only logical relations and definitions.[104] Then by his former definitions of "empirical statements" and "tautologies" - recall quotation 98 - these propositions are "tautologies" concerned only with language and devoid of content. Viewed in this manner, "pure theory" is of course "tautological," but it need not be viewed as such. It is usually possible to correlate some of the terms in the "purest" of theories to empirical magnitudes in a manner which permits their falsification. It is a different matter to assert that economists have refused to interpret theories accordingly.

There is sufficient ambiguity and confusion surrounding the concept of tautology to warrant further explanation. Thus, we offer a threefold classification of tautologies. We, however, make no pretense that by extending definitions any substance is added to the debate, but only that by discussing different types of tautologies can we better understand Hutchison's analysis of economic theory.

First, we define a "zero-order tautology" as any statement that follows from definitions alone. For example, A = A follows definitionally from the concept, "=", where A is in some sense "meaningfully" defined. All zero-order tautologies are equivalent to A = A. For another example, we might define "pure competition" as a situation where all producers can sell any or all of their production at a fixed price. From this definition, it is possible to conclude that "pure competitors" face "perfectly elastic demand" or that "marginal revenue" equals price. At first blush, one might believe that something is revealed in these consequences of "pure competition," but, in fact, nothing can be gained from such manipulations. We have merely exchanged one zero-order tautology for an equivalent one, for the above "consequences" follow strictly from the definitions of "marginal revenue" and "perfectly elastic." Notice that deduction is not required when translating zero-order tautologies. Although many economists have given the impression that something is gained by such zero-order tautological exercises, they cannot possibly have any consequences, logically or empirically.

Moving away from pure definitions, we may define a "first-order tautology" as any combination of definition and deduction. A first-order tautology then results whenever the "truth" of some assertion is reduced to some "lesser truth" via deduction. All deductive systems are first-order tautologies. Hutchison's definition of "tautology" is equivalent to the union of our zero- and first-order tautologies. We must reiterate that all theories are first-order tautologies when they are interpreted only as logical interrelations and make no error of logic. By merely introducing correspondences between the words and symbols of a first-order tautology and 'observable' states of affairs, the tautology evaporates into a genuine statement of content.

Finally, one can conceive of a third type of tautology, a "second-order tautology." A second-order tautology is a statement that talks about empirical quantities but due to the manner in which it is stated or held is not falsifiable. Existential statements are second-order tautologies. For example, "There exists a black swan." Assuming that we have appropriate empirical means to establish "black" and "swan," this statement still remains irrefutable. For no number of observations of white swans or of absences of black swans precludes the possibility that a black swan exists and would be observed if only we look a little harder. In economics, there is no scarcity of second-order tautologies. Both the "laws" of demand and diminishing marginal returns are second-order tautologies as they are usually stated. The "law" of diminishing returns always carries a qualifier, either explicitly or implicitly, that marginal returns need decrease only after some point. "What point?" you might ask. "Why, the point of diminishing marginal returns, of course," economics answers. The necessity of this qualifier is obvious to any economist, for the marginal returns of a variable factor may quite naturally increase, at first, when applied to fixed levels of another resource. Nonetheless, this qualifier, "after some point," renders the "law" of diminishing marginal returns irrefutable, impervious to empirical reality. For if any study were to find that actual marginal returns increase, one may simply point out that production levels have not yet reached "the point of diminishing returns" and if we just wait until production sufficiently increases we will observe a diminution of the relevant marginal products.[105]

Similarly, the "law" of demand is a second-order tautology. In the first place, "demand" is not observable nor is the "quantity demanded." The law of demand is "empirical" only within a broader theoretical system that includes a "law" of supply and a dynamic adjustment mechanism which is usually said to be "towards equilibrium." Only this complete theoretical system has consequences for prices and quantities that are exchanged and thus might be observed. Yet, it is insightful to view the "law" of demand in isolation.

Even if the law of demand is interpreted in a manner that talks about the facts, it would only become a second-order tautology. For example, if we correlate "price" and "quantity demanded" to the observed prices and quantities, the "law" of demand might then be falsifiable. Or would it? Still the "law" of demand would be irrefutable as long as it carries an unspecified _ceteris paribus_ clause or one containing "unobservables." It is not always clear just which "other things" are required to be held constant, but they are usually thought to include all other prices, incomes, tastes and preferences, and expectations. Before the law of demand is falsifiable, one must be able to 'observe' these "other things" and 'corroborate' their constancy; otherwise, all potential refutations become merely violations in the _ceteris paribus_ clause. The same can be said of the role of technological progress in the law of diminishing marginal returns.

Yet such things as "tastes," "preferences" or "expectations" are not observable. They can be inferred only, _ex post_, from the behavior of economic agents. In practice, economists "infer" that these unobservables have changed whenever it is thought that demand has shifted. Thus, if we were to have 'well-corroborated' evidence that both the quantity demanded and price have increased, it would only be seen as an increase in the future price "expectations," or, perhaps, "tastes" and "preferences" have changed.

Theories containing unspecified or unobservable ceteris paribus clauses are second-order tautologies and cannot be falsified. This too is Hutchison's position. Unless the "other things" are explicitly specified, the "law of demand" is not falsifiable and therefore not scientific.[106] In fact, this nonspecific ceteris paribus clause causes Hutchison to change his dichotomy of statements into a trichotomy - "tautologies," "empirical statements," and "hopelessly ambiguous statements."[107] Hutchison concludes that "'ceteris paribus' propositions are frequently hopelessly ambiguous and that the ceteris paribus assumption should be used less often and more cautiously.[108]

Part of Hutchison's essay is a criticism of the earlier aprioristic views of economics. The "deductive method" of Robbins and the classical economists is called the "hypothetical" or the "isolating" method by Hutchison.[109] He characterizes it as the investigation of "simplified cases and examples which, it is claimed, 'throw light on the real problems.'"[110] However, this hypothetical exercise in deduction cannot "throw light on the real problems" but only upon the words which we use to talk about real problems. Or, as Hutchison states:

> All these analyses of hypothetical simplified communities are concerned with language. They are concerned in no way with some mystical "real" connection between facts which we discover by deductive thinking. Rather they have no direct connection with facts but flow from the way in which we talk about the facts.[111]

This is a correct interpretation of the "deductive method," one which Lord Robbins betrays in his own words (recall quotation 57). The "deductive method" is no more than a linguistic analysis of economics.

Hutchison also sees confusion in the deductivist's use of "introspection" and "a priori facts."

> Our conclusion, for the reason given above, is that we are dealing here with a confusion between the a priori and introspection - two concepts which themselves are certainly obscure enough to permit of such a confusion - and further with a lack of clarity as to the precise content of the "Fundamental Assumption" (maximization). This is supported by "the all too human love of certainty," and an urge to exalt the certainty and inexorable necessity of the propositions of Economics above those of the natural sciences.[112] [Parentheses added.]

Perhaps Hutchison is correct to imply that economists' choice of apriorism is derived from their "all too human love of certainty," but it would be difficult to subject such an assertion to an empirical test. Be this as it may, Hutchison seems to find some ambiguity in "urging that these propositions 'logically precede all experience and are a condition and assumption of all experience,' and by giving them the name of 'a priori facts.'"[113]

It appears that Hutchison sees inconsistency in holding both a Cartesian and a Kantian view of knowledge. Will our intuitive introspections lead us only to necessary propositions? Again the "factual fallacy" appears. The deductivists do not understand the "facts," and neither their methodology nor their theories have any room for the 'facts.' Hutchison simply points out that "introspection" is something completely different from empirical testing. While the former can in no way substitute for the latter, it can be a source of hypothesis, just as can anything else, we must add.[114] Also we might suggest that the reason for this difference between "introspection" and empirical testing is that "introspection" is not intersubjectively testable as the 'facts' of falsificationism demand.

Hutchison's criticism of the "deductive method" is apt. To linguistically analyze the propositions of the deductivists is fair play, since it is they who linguistically analyze economic phenomena. To point out the ambiguity of their statements is also appropriate for the same reasons. And to demand "realism of the assumptions" of deductive theories is again just. For if a deductive theory is to have any genuine content or if it is to explain actual phenomena, only in the "assumptions" can such content be contained.

However, Hutchison himself is aware that he is only providing a linguistic analysis of the propositions of economic theory.[115] To merely proclaim that propositions of economic theory are tautologies and empty of empirical content causes at least one dogmatic defender of economic theory to reject Hutchison's analysis as "ultra-empiricism."[116] But Hutchison's contribution cannot be completely dismissed in such a superficial manner. There is more to his position than such a simplistic label can capture.

The depth of Hutchison's position may be seen in his recognition that the character of economic theory depends upon how it is used and not merely what it says. "At the bottom, <u>given</u> the scientific criterion, propositions and concepts fulfill this criterion if we <u>choose</u> to define them as doing so, and do <u>not</u> fulfill it if we do <u>not</u> choose."[117] He even goes so far as to suggest that the concepts of "expectations," "utility," and "social utility" could become scientific.[118] Hutchison does not dogmatically assert that economics is not or cannot be a science. On the contrary, he has an optimistic view of the potential of economics.[119] Hutchison's criticisms follow from his belief that economists, particularly the apriorists, choose to define and employ their theories tautologically. If such a belief is justified, then so are his criticisms of economic theory. One might even see this assertion about the actual choices of economists as being "confirmed" by their own words - for example, the descriptions of economic theories by von Mises and Lord Robbins given in the previous section.

Though Hutchison also argues by the use of definitions, he is unique among economists in seeing the insignificance of any definition of economics. Hutchison's view of definitions is similar to Popper's and the view we have attempted to express throughout this essay.

> Though the discussions leading up to it may well be of interest, the actual assignment of a definition to the word "Economics" does not appear to solve, or even help the solution of any useful scientific problem whatsoever. The divisions between the individual sciences - the division, that is, of the scientific labour as a whole - have arisen more or less as the result of historical accident and considerations of convenience, and though scientists are rapidly becoming more self-conscious in their procedure and though a "science of science" is growing up and it is interesting and suggestive to attempt to foresee what the most convenient division of labour is going to be, the laying-down of rigid frontier lines between the particular sciences seems an unprofitable undertaking leading to even more interminable disputes than those over national frontiers in Eastern Europe; with the difference that whereas the ardent nationalists desire to include as much as possible, the definers of the subject matter of Economics seem often more concerned to turn out and exclude as much as possible.[120]
>
> We wish simply in this section to point out how certain authoritative definitions (for example, Robbins') of its subject matter limit propositions of economic science to the type we called in the previous chapter "propositions of pure theory" .... Of course this definition, the object of which apparently is to

> guide economic studies in a particular direction, has not been arrived at out of the blue, but in fact, as its authors claim, simply makes precise the practice and the overt teaching of many of the classical writers of the science, brilliantly summed up in Richardo's contrasting of "questions of science" with "questions of facts."[121] [Parentheses added.]

Do not the many definitions of economics that economists have <u>chosen</u> testify only to their desire for tautological knowledge? If, as Hutchison suggests, definitions of economics only serve to limit or eliminate the empirical nature of economic inquiry, then the criticism of these definitions and their associated methodology is entirely warranted.

Still, the most dramatic illustration of economists <u>choosing</u> to hold their theories tautologically comes from the analysis of the "basic postulates" of these economic theories. This analysis is Hutchison's most interesting and potentially enlightening contribution to economic methodology. Here we shall provide a discussion of only the "maximization assumption" while contrasting the views of Hutchison and the apriorists. It is hoped that the following will clarify these alternative methodological positions and provide some insight on the current position of economics.

There are perhaps as many different formulations of the "Fundamental Assumption" as there are labels of it - "maximization assumption," "maximum principle," "rationality principle," "quasi-saddle point programming" .... Hutchison begins his discussion of this "Fundamental Assumption" by citing a number of such definitions and observes that they require the additional postulate of "perfect expectations."[122] While it would be too easy to make this relationship between "maximization" and "perfect expectations" elusive, it can be made quite general and clear.

It is obvious that the agents of economic models must have some type of firm expectation of the possible outcomes of their actions; otherwise "maximization," in any sense, would be impossible or, at least, "irrational." All of the traditional theories of value are constructed on the basis that all economic agents know the "actual" alternatives available and choose the "best." All economic models must use such "perfect expectations" with "maximization," or they must hypothesize some type of "expectation generation mechanism." One either assumes that expectations are given and correct, or he postulates how they are formed and how they err. Without some type of expectation, maximization would be absurd. Thus, in the first instance, Hutchison's observation is that most economic theory has chosen the easy way out by assuming that expectations are somehow "perfect."

The logic of maximization demands something quite close to the assumption of "perfect expectation." Recognition of this inherent logic of maximization is what causes Friedman to explicitly incorporate this relationship into his formulation. "Individual firms behave _as if_ they were seeking rationally to maximize their expected returns and _had full knowledge of the data needed to succeed in this attempt_."[123] [Second emphasis added.]

However, Hutchison sees some methodological problems associated with the joint assumptions of maximizations and "perfect expectations."

> So long as one is concerned with a world where the choice is always an automatic one between a return which is _certainly_ maximum and others which are _certainly_ smaller, the assumption that people _expect_ to maximize their returns and the assumption that they _actually do_ maximize them come to the same thing .... Where uncertainty is present, as in principle the case with any piece of conduct in the world, economic or otherwise, one cannot, strictly speaking, seek the

> most advantageous employment for one's capital or act to maximize one's returns .... They [formulations of the "Fundamental Assumption" that require the "perfection" of expectation] therefore pass over all the problems of the economy in the world as it is, which may be said to arise from precisely this factor of uncertainty and imperfect foresight. They all make no mention of the question <u>how</u> one is to maximize one's returns. They simply say that it is "rational," "sensible," or "natural" to do this, assuming, presumably, that one <u>knows</u> how this can be done.[124]

Or, in other words, Hutchison believes that the problem of economic science is to explain <u>how</u> one might actually maximize his lot or to explain <u>what</u> economic agents actually do in our uncertain world. The joint assumption of "maximization" and "perfect expectations" assumes away these problems; therefore, the theory of value cannot provide any genuine solution or explanation of the behavior of economic agents.

The apparent absurdity of these joint postulates of economic behavior becomes more clear when put into a methodological context. A goal of classical economics is to provide practical advice to economic agents and institutions (recall Keynes's discussion of this issue; for example, quotation 11). To then assume that all economic agents maximize their lots and have sufficient information to succeed at this task is to "defeat the purpose" of economics or, at least, to avoid the issue. If economic agents actually behave in the manner postulated, then economic science has nothing to offer - the potential value of the science is already actualized. If not, economic science has merely assumed the fulfillment of its purpose without explicating how this goal is or can be achieved. To thus "assume away" the "problem of economics" is no genuine solution and can only confuse the role and use of economic theory.

Hutchison provides a provocative criticism of the usual way that economic theory is formulated. Such criticism can be valuable, particularly in the dogmatic aprioristic methodological climate that then dominated economic thought. Some economists might respond that economics does not need to assume that expectations are "perfect" and have explained economic phenomena by "imperfect expectations."

Hutchison also addresses this issue. "To make particular assumptions about some kind of imperfect expectation is largely question-begging unless supported by empirical investigation."[125] "To make assumptions as to expectations and therefore as to conduct, unless these assumptions are empirically confirmed, is, in dealing with economic problems fundamentally, to beg the question and assume what one wants to find out, which is always just what people's expectations and correlated behavior in different situations are."[126] Hutchison's argument is that if one <u>assumes</u> how expectations are determined he does not solve the problem of how they are in "fact" determined. When the problem for economic science is how expectations are in fact determined, the theory must have some recourse to the empirical data before this problem is solved.

That this view is correct cannot be seriously questioned, yet Hutchison goes too far. He seems to require that the assumptions of "imperfect expectations" be empirically "confirmed" before they can be employed in economic theory. To require the empirical "confirmation" of one's assumptions is quite different than demanding the 'falsifiability' of a theoretical system. Again, Hutchison betrays his logical positivism. Hutchison's argument against theories of "imperfect expectations" is unconvincing. He merely asserts that they are not based upon empirically "confirmed" behavior which is an overly demanding requirement for the assumptions of a theory. All that is necessary for such a theory is that it has some empirically 'testable' implications about the behavior of economic agents.

Yet, even this "ultra-empiric" assertion of Hutchison may have some merit. "Imperfect information" theory has developed since Hutchison's essay and was largely ignored before this time. In the 1930's, the only type of "imperfect expectations" that were explicitly treated concerned phenomena such as the "money illusion," where economic agents have "perfect expectations" for n-1 dimensions and are completely ignorant about one relevant dimension. Then this "fooling" of economic agents can be used to explain "disequilibrium" and such. All these models of "imperfect expectations" are question-begging, if the questions are: How do individuals form their expectations? What economic implications result from the manner in which individuals form their expectations?

Modern economic theory begs these same questions. Many different types of "imperfect information" theories have been developed since Hutchison's essay, yet all assume that economic agents know the relevant distributions without explaining how they come to this knowledge. Thus, if Hutchison is correct to assert that the important question is how economic agents arrive at their expectation of economic consequences, then his criticism of economic theory past and present has some merit.

If, however, this question is not seen as relevant, then Hutchison's criticism is largely beside the point. There is no unique "answer" to the choice of research questions. If the object of economic science is to explain only the behavior of prices and quantities, it is quite possible that a suitable, 'falsifiable' explanation may be found that does not involve the explanation of expectations or any behavior of economic agents.

Misunderstanding the object of economic theory has caused much controversy. The "assumption debate" of the 1940's concerns exactly this issue.[127] The final word of this debate is, perhaps, given by Machlup in his presidential address to the American Economic Association.[128] Machlup provides a good summary of the orthodox position.

> My charge that there is widespread confusion regarding the purposes of the "theory of the firm" as used in traditional price theory refers to this: The model of the firm in that theory is not, as so many writers believe, designed to serve to explain and predict the behavior of real firms; instead, it is designed to explain and predict changes in observed prices (quoted, paid, received) as effects of particular changes in conditions (wage rates, interest rates, import duties, excise taxes, technology, etc.). In this causal connection the firm is only a theoretical link, a mental helping to explain how one gets from the cause to the effect. This is altogether different from explaining the behavior of a firm.[129]

Machlup adds a footnote to the above which asserts that the same holds for the "theory of the household."

Thus, if only explanations of prices and quantities are sought from economic theory, it is inappropriate to criticize economic theory for failing to explain how expectations are formed. Orthodox economists can be criticized on this issue only when their rhetoric indicates that they have explained the behavior of economic agents.

Hutchison is "guilty" of mistaking the purpose of economic theory. He "naively" believes that the "Fundamental Assumption" may be "tested" by the actual behavior of individuals. "Roughly, one can test it simply by asking anyone whether they expect that if they were to employ their money or resources in any other way than in which they are at the moment doing or about to do, they expect that they would increase their returns."[130] There is no doubt that Hutchison is concerned with how individuals actually behave and not only with the subsequent movement of prices.

On this issue there is no clear-cut "winner" or correct view. Both the behavior of economic agents, however defined, and the behavior of prices and quantities are phenomena that science might properly be asked to explain. But which phenomenon should be explained by which theory or discipline is a methodological question and subject to various views. Only if a 'falsifiable' theory is offered that explains prices and quantities as well as traditional price theory and has 'excess empirical content' for the behavior of economic agents is there a clear choice. However, without such a theory, the orthodox view cannot be criticized for the issues it does not claim to address. Here, Hutchison is speaking largely cross-purposely, and his criticism has value only opposed to the error of those who claim that the "Fundamental Assumption" explains the actual behavior of individuals in the economy.

Yet, history has born out, in some sense, the importance of Hutchison's perspective. Much economic theory since Hutchison's essay is devoted to answering the questions that Hutchison raises. And, on the level of the practical significance, which after all is the purpose of economic science, the questions raised by Hutchison have "proved" to be more important than those addressed by traditional "rationality" theories. Is it not "obvious" that questions of how economic agents do or could go about "maximizing" their lots are more important on a practical level than questions of the movement of prices if wages should increase, etc.? The post-war introduction of the practical optimization techniques of operations research, management science, and statistical analysis have caused a substantial influence upon the efficiency of industry, the possibility of which is "assumed away" by orthodox theory. Only by addressing the questions of how economic agents actually behave and what information they actually possess can such practical gains be realized. Although Hutchison's criticism of traditional economic theory can be justly criticized, his vision of the proper questions for economic analysis has born more fruit than price theory.

Hutchison's analysis of the "Fundamental Assumption" does not stop here; he provides us with several additional insights concerning the logic of "maximization." For example, Hutchison is probably the first to recognize that 'perfect expectations' and 'monopolistic' conduct by more than one individual in an interdependent system are logically incompatible.[131] He makes his case by describing a duopoly model where each player knows unconditionally the actions of his rival and can adjust his behavior accordingly.[132] Such a game, asserts Hutchison, simply cannot be played. How can each economic agent know "perfectly" what other agents will do and at the same time be free to adjust his behavior in an interdependent environment? When the actions of agents are interdependent, "perfect expectations" and "maximization" are logically inconsistent. "A game of chess or bridge with all players having perfect expectations of the other's play and then adjusting their own, could not be played."[133]

Here, Hutchison is correct. "Perfect expectation therefore is incompatible, in an interdependent economic system, with people acting in the way they expect will maximize their profits and at the same time more than one person adjusting his conduct in accordance with his (perfect) expectations of the other's conduct - that is, it is incompatible with more than one person acting 'monopolistically' with perfect expectation. Perfect expectation is only compatible with 'competitive' conditions - that is, conditions where no person's conduct can affect the conduct, and the result of the calculations on which it is based, of the other."[134] Hutchison further suggests that his conclusion is consistent with the indeterminant equilibrium results of the classical monopolistic models, particularly those of Pigou and von Stackelberg.[135] However, Hutchison's observation is a criticism to economic theory only in the rare case that all three "assumptions" are being jointly employed.

In his discussion of "equilibrium," Hutchison shows more depth. "It is important to notice that the 'perfect expectation' postulate is not a postulate as to how people under conditions of equilibrium actually behave, but it is introduced simply as an explanation of their behavior .... What is necessary for equilibrium is only that people behave in a certain way, and it is not strictly necessary to go into the question as to how or why they should behave in this way more than any other hypothetical simplified example."[136] Thus, Hutchison does understand the role of "behavioristic assumptions" in economic theory and that such assumptions may be rationalized when the theory's scope is limited. Yet, Hutchison is not satisfied that "equilibrium" or prices and quantities is all there is to explain. He simply desires to explain how this conduct came about and how it can be explained with relation to other things.[137]

It is essentially this desire for "realism" that finally causes Hutchison to proclaim,

> The method of deduction from some "Fundamental Assumption" or "principle" of economic conduct is more or less useless .... Before there can be any more "realistic" analysis some idea must be formed of what the more realistic assumptions are on which it is to be based, unless deductive theorists are simply going to continue building up their analysis on any assumptions - say as to the wage-policy of trade unions when there is a rise in prices - which appeal to them impressionistically a priori, or which are "tractable"; that is, make possible a fascinating display of mathematical or geometrical ingenuity, or which merely fit with their political views.[138]

Part of the emptiness of the "Fundamental Assumption" that Hutchison sees derives from the deductivists' interpretation of it as a definition of economic conduct.

> Finally it came to be openly stated that the "Fundamental Principle" was not conceivably falsifiable and devoid of all empirical content, a circularity, a matter definition, a linguistic proposal. It was assumed or stated that everybody behaves "rationally" or "sensibly," and "rationally" or "sensibly" was defined as how people do in fact behave. All economic conduct is ex definitione rational or sensible. It would be contradictory to speak of "irrational" economic conduct, or, if one does, "one means that one's fellow men do not act as one considers right."[139]

Hutchison references both Joan Robinson and von Mises.

If, as Hutchison suggests, the apriorists define all economic behavior, ex post (or is it a priori?), as "rational," then, of course, the "rationality assumption" has no empirical content, at least not to the deductivists. And, this is generally the apriorists' view. For example, recall von Mises' view that these propositions are the "logic of action and deed" - see quotation 79. Hutchison is only pointing out that the "Fundamental Assumption" - or anything else, for that matter - is empirically empty when so interpreted. When, for example, a proposition is interpreted as "necessary" to or as the "logic" of the situation, it cannot then be regarded as an empirical assertion.

As a criticism of the aprioristic view, or equivalently the "deductive method," Hutchison is only pointing out the obvious. Perhaps mentioning this "obvious truth" has some merit, but Hutchison's plea for more "empirical realism" has even greater value relative to the dominant aprioristic methodological environment. Given such a methodological climate, we might excuse Hutchison's "ultra-empiricism" and his "excess demand" upon economics' "basic postulates." After all, how does one criticize a methodological position that asserts that the entire empirical content of its theories resides in the indubitable "facts" of its "assumptions," as do the deductivists?

Hutchison's analysis is essentially correct as far as it goes. One can only disagree with his "assumed purposes" of economic theory and his logical positivistic mode of analysis. The latter causes the scope of his analysis to be overly limited and its substance to be merely linguistic. Hutchison only argues about the "meaning" and the "logic" of the "Fundamental Assumption." He does little to interpret it as a 'falsifiable' theory, to devise a 'test' of its empirical content, or to assess the state of its 'corroboration,' as would a falsificationist. Yet, he does make some headway upon this falsificationistic road. For example, recall quotation 130 and notice his advocacy of empiricism in the following:

> The objection has been made to statistical investigations, questionnaires to consumers and entrepreneurs, the examination of family budgets and the like, that the result of such arduous researchers are subject to a high degree of inaccuracy, can easily be "cooked," and in any case would not tell us much that we did not know already. The answer to such an objection is quite simple. If, as one is perfectly free to do, one considers that the results obtainable by the only possible scientific method open to one are not of sufficient interest to reward the effort

> of the investigation, then one must give up the scientific handling of these problems altogether and leave them to others of different intellectual tastes.[140]

Such an empirical methodological position must be seen as progressive vis-a-vis the aprioristic view.

In summary, Hutchison makes the following points about the "Fundamental Assumption."

(1) The "Fundamental Assumption" usually requires the further assumption of some type of "perfect expectations."

(2) The "Fundamental Assumption" and "perfect expectations" are logically incompatible in an interdependent economy.

(3) Theories using the "Fundamental Assumption" do not explain the behavior of economic agents or how they actually form their expectations and arrive at the "maximum."

(4) Even when some type of "imperfect expectations" is postulated, the resulting theory begs the same question - of (3) above - unless it is subjected to empirical tests.

(5) The "Fundamental Assumption" is often interpreted as a definition of economic behavior, as such it is only an empty "tautology."

(6) The "Fundamental Assumption" can be empirically testable by asking economic agents about their choices, if interpreted as a genuine statement about the behavior of individuals.

Nonetheless, one might still wonder what a more comprehensive analysis of the "Fundamental Assumption" would tell. To this subject, we shall return in the next section while discussing Friedman's "maximization-of-returns" hypothesis.

The fact that Hutchison was able to identify the limited empirical nature of economic theory in the 1930's is, itself, quite remarkable. His analysis is essentially correct, although his assertions are somewhat too limited and too linguistic. Yet even Hutchison's weaknesses are quite understandable and forgivable when one considers his methodological antagonists. For the sake of contrast and proper perspective, we end our discussion of Hutchison where it begins - that is, with Lord Robbins' aprioristic position of the "Fundamental Assumption."

Robbins gives this "Fundamental Assumption" the preeminent role for economic science. "The most fundamental propositions of economic analysis are the propositions of the general theory of value. No matter what particular 'school' is in question, no matter what arrangement of subject-matter is adopted, the body of propositions explaining the nature and the determination of the relation between given goods of the first order will be found to have a pivotal position in the whole system."[141]

Robbins has so much faith in these "most fundamental propositions" that he all but repeats the famous mistake of Mill in proclaiming that the theory of value is perfect and complete. It seems that Robbins learned something from Mill's mistake.

> It would be premature to say that the theory of this part of the subject is complete. But it is clear that enough has been done to warrant our taking the central propositions as established. We may proceed, therefore, to inquire on what their validity depends.[142]

How has the validity of these propositions been established? After dismissing history and controlled experiments, Lord Robbins queries,

> But on what, then, does it depend? It does not require much knowledge of modern economic analysis to realize that foundation of the theory of value is the assumption that the different things that the individual wants to do have a different importance to him, and can be arranged therefore in a certain order. This notion can be expressed in various ways with varying degrees of precision, from the simple want systems of Menger and the early Austrians to the more refined scales of relative valuations of Wicksteed and Schonfeld and the indifference systems of Pareto and Messrs. Hicks and Allen. But in the last analysis it reduces to this, that we can judge

> whether different possible experiences are of equivalent or greater or less importance to us. From this elementary fact of experience we can derive the idea of the substitutability of different goods, of the demand for one good in terms of another, of an equilibrium distribution of goods between different uses, of equilibrium of exchange and of the formation of prices.[143]

Paraphrasing Robbins' position:

(1) The theory of value depends upon preference theory (which is the "Fundamental Assumption" under a different label).

(2) Preference theory is established by the "elementary fact" that it is possible to arrange one's wants in some order.

(3) From preference theory all sorts of economic ideas flow.

"But this, we have seen already, is really an assumption of one of the conditions which must be present if there is to be economic activity at all. It is an essential constituent of our conception of conduct with an economic aspect."[144]

To Robbins, the "Fundamental Assumption" is an obvious fact and an "essential constituent of our conception of conduct with an economic aspect." From such "factual definitions" of economic activity, the rest of our economic knowledge is deduced.

Unfortunately, from none of the ideas to which Robbins refers nor any statement of content, empirical or logical, can be deduced the possibility nor the "fact" of preference orders. Before anything can be deduced from this "essential constituent of conduct with an economic aspect" all manner of additional assertions must be included. Robbins even neglects to mention that economic agents attempt to maximize their preference. Apparently, this is so obvious that it need not be mentioned.

The factual nature of preference theory is so "obvious" and so generally applicable that we need not worry about the validity of the many necessary auxiliary hypotheses. Economic theory is universally valid, and its applicability is conditional only upon the subsidiary postulates being realized. It would certainly be nice if economic knowledge were as easy to produce as Lord Robbins asserts. But, unfortunately, this hypothetical epistemological world is no more "obvious" nor "factual" than Robbins' assumption of preference.

Hutchison's essay need be appraised in the context of this extreme apriorism of Robbins and his contemporaries, for this is what forms Hutchison's perspective. In such a light, Hutchison's criticisms of specific aprioristic propositions and his empiricism can be seen to further the methodological debate. He bequeathed to later generations of economists at least the language of falsificationism, if not its complete methodology. It remains now to see how these ideas are reflected in one of the most influential, contemporary economic methodologies - Friedman's "methodology of positive economics."

4.4 Milton and Methodology: The Methodology of Positive Economics As If It Were the Method of Economics

> Paradoxically the soft sciences that are still akin to an art benefit more from an explicit awareness of the canons of scientific method . . . than do the hard sciences where doing what comes naturally will protect even a fool from gross methodological error.
>
> (P. S. Samuelson, Stamps Memorial Lecture, Problems of the American Economy, p. 21.)

> More than other scientists, social scientists need to be self-conscious about their methodology.
>
> (Friedman [1966], p. 40.)

> A scholar in economics who is fundamentally confused concerning the relationship of definition, tautology, logical implication, empirical hypothesis, and

> factual refutation may spend a lifetime shadow-boxing with reality. In a sense, therefore, in order to earn his daily bread as a fruitful contributor to knowledge, the practitioner of an intermediately hard science like economics must come to terms with methodological problems.
>
> (Samuelson [1965], p. IX.)

Milton Friedman's methodology of "positive" economics (MPE) has been the source of much discussion in the economics literature, creating an "assumption debate" of its own. The bulk of the comments have been negative. Yet, paradoxically, MPE is the economic methodology that is most used by economic practitioners and most frequently cited in textbooks. It is "as if" contemporary economic methodology were the methodology of "positive" economics.

We shall assume that Friedman's MPE is the received view of economic methodology or, at least, representative of the current practice of economics. Certainly, any such generalization will not be strictly true. There are a multitude of beliefs about economic methodology, but none so pervasive, and many would say persuasive, as is Friedman's view. Admittedly, our assumption is a matter of convenience. Time and space do not permit reference to every economist that has asserted some methodological position. Yet, we make no claim that the convenience of this assumption somehow raises its epistemological status. We simply cannot find any single methodological position that better represents how current economics is practiced.

Nonetheless, there are other methodological views, a few of which need be mentioned for the sake of a well-rounded coverage. The key figure of the period between Hutchison and Friedman is Samuelson. In the _Foundations of Economic Analysis_, Samuelson advocates a form of

Popper's falsificationism. Like Hutchison, Samuelson does not derive his falsificationism from Popper but, instead, from Bridgman's operationalism.[145] Operationalism is a variant of logical positivism that requires the terms of scientific theories to be reducible to some measurement process. Nonetheless, Samuelson somehow manages to interpret operationalism in a manner more consistent with a simple view of falsificationism.

For instance, Samuelson employs Popper's demarcation criterion. "By a meaningful theorem I mean simply a hypothesis about empirical data which could conceivably be refuted if only under ideal conditions."[146] Aside from the positivistic language, "meaningful theorem," this is essentially Popper's demarcation criterion. It is interesting to note that Samuelson makes no reference to Hutchison, in spite of the similarity between their definition and use of this criterion. For Samuelson, too, is quick to demand that a statement be falsifiable without developing tests of theories or analyzing complete theoretical systems.

For example, Samuelson applies the demarcation criterion to the postulate of homogeneity.

> It (the homogeneity of the production function) is a scientifically meaningless assertion that doubling all factors must double product. This is so not because we do not have the power to perform such an experiment; such an objection is of course irrelevant. Rather the statement is meaningless because it could never be refuted, in the sense that no hypothetically conceived experiment could ever controvert the principle enunciated. This is so because if product did not double, one could always conclude that some factor was "scarce."[147] [Parentheses added.]

Samuelson is also critical of the usual aprioristic formulation of consumer theory.

> Thus, the consumer's market behavior is explained in terms of preferences, which are in turn defined only by behavior. The result can very easily be circular, and in many formulations undoubtedly is. Often nothing more is stated than the conclusion that people behave as they behave, a theorem which has no empirical implications, since it contains no hypothesis and is consistent with all conceivable behavior, while refutable by none. Still another "meaningless" theory is held by those writers who speak of behavior in terms of the economic principle, regardless of whether any empirical behavior related to it exists.[148]

Samuelson also introduces the simplist conceivable form of empirical hypothesis to economics: "The usefulness of our theory emerges from the fact that by our analysis we are often able to determine the nature of the changes in our unknown variables resulting from a designated change in one or more parameters. 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."[149] This is the method of qualitative calculus - that is, predicting the sign of a change in an empirically observable variable "resulting from a designated change in one or more parameters." Although a smaller non-zero 'empirical content' is impossible, Samuelson's qualitative calculus represents a substantial improvement over previous economic methods, e.g., casual introspection.

This simple idea of 'testing' is also captured by Friedman's MPE, but not in a manner entirely satisfying to Samuelson.[150] That Samuelson's falsificationism is a progressive methodological development is quite clear, but whether his own work is consistent with his asserted falsificationism is quite unclear. In fact, it seems that Samuelson is much more concerned with formalism than any or all of the results of empirical testing.[151]

Samuelson is an important economic methodologist not only for his introduction of simple falsificationism, but also for his influence upon the formalism of economics. Although Samuelson was not alone in increasing the mathematical sophistication and rigor of economic analysis that has dominated economic science since the 1940's (Arrow, Debrew, Hicks, and many others made substantial contributions), he is probably the most popularly recognized leader of this "formalism." "Formalism" must be considered a methodology of its own. It is the compilation of the many theorems, proofs, and refinements of general equilibrium as well as competitive equilibrium and Pareto optimality. While formalism is the awesome display of mathematical rigor and ingenuity, it has no empirical consequences. No number of theorems or proofs can ever produce a 'testable' proposition. "Formalism" is not scientific nor does it have any implication about the "what is" of economics.[152]

However, "formalism" still occupies much of the published economics literature and is often employed to identify the competence of professional economists. "Formalism" can, at best, help develop the "tools" of economic analysis that someday might provide a convenient language in which to talk about economic phenomena. But it can do no more. Although great intellectual resources have been devoted to economic "formalism," it can be completely ignored as a methodology of science. If it is to make any epistemological claims, it would have to assert the *a priori* "truth" of its sufficient conditions. This, of course, would be some form of apriorism, and we know where that leads. Modern economic formalism is best viewed as the correction and refinement of the language of classical and twentieth-century apriorists.

The conflicting demands of "formalism" and falsificationism are the apparent cause of Samuelson's change of methodological positions. This change can be seen in Samuelson's criticism of Friedman's methodology, Samuelson [1963]. Here Samuelson opts for the descriptive accuracy of theories rather than explanatory power.[153] To replace explanation with description represents a significant methodological retreat. Theories need to be more than mere descriptions of empirical phenomena. They must have informative content beyond "labeling" to produce knowledge or the practical utility of its prediction. Theories which only serve to categorize empirical phenomena can only be linguistic. It is quite unfortunate that an economist who began his career upon such a progressive methodological note retrogressed into the more typical, defensive methodological position.

Probably the most prolific economic methodologist is Fritz Machlup. It would be a difficult task to rigorously demonstrate that Machlup's methodology is only a dogmatic and linguistic defense of traditional theory, if only for the abundance of his words that need be evaluated. But it is quite obvious that Machlup's methodology is both defensive and linguistic. His position seems to be some combination of conventionalism and logical positivism which always results in a defense of the orthodox view and which is more concerned with the <u>words</u> used to describe and discuss economic theory than the content of the theory's explanation.[154]

There is no reason to provide a thorough analysis of Machlup's methodology. We have already adequately discussed "conventionalist stratagems" and would be better off not knowing all the ways to apply them to economic theory. Nor is it necessary to be reminded that it is always possible to find "verifications" of one's favorite theories when one looks for them. In any case, Machlup has taken much of Friedman's position; thus, our discussion of Friedman's methodology will be representative of the better part of Machlup's position.[155]

We turn now to the currently accepted view of economic methodology, Friedman's MPE. Another reviewer of economic methodology also gives MPE much the same role.

> Nothing like an overwhelming consensus has emerged from our survey of postwar economic methodology. But despite some blurring around the edges, it is possible to discern something like a mainstream view. . . . Friedman and Machlup do seem to have persuaded most of their colleagues that direct verification of the postulates or assumptions of economic theory should be judged in the final analysis by their implications for the phenomena that they are designed to explain. At the same time, economics is held to be only a "box of tools," and empirical testing can show, not so much whether particular models are true or false, but whether they are applicable in a given situation. The prevailing methodological mood is highly protective of received economic theory, it is also ultrapermissive within the limits of the "rules of the game": almost any model will do provided it is rigorously formulated, elegantly constructed, and promising of potential relevance of real-world situations. Modern economists frequently preach falsificationism, as we have seen, but they rarely practice it: their working philosophy of science is aptly described as "innocuous falsificationism."[156]

That this is also an accurate descriptive of Friedman's methodology and its influence will be made clear in our following discussion.

4.4.1 Positive Economics As You Like It

Boland has recently presented a review of the literature surrounding Friedman's MPE which explains the apparent anomaly between the fact that Friedman's methodology has received many severe criticisms and yet remains the closest thing to a received view of economic methodology.

Boland asserts, "Every critic of Friedman's essay has been wrong. The fundamental reason why all the critics are wrong is that their criticisms are not based on a clear, correct, or even fair understanding of his essay."[157] He further declares that Friedman's position is instrumentalism, a term not used by Friedman.

It is our conjecture that Friedman's MPE is not clear, coherent, or unambiguous methodology and can be too easily interpreted "as you like it." Thus, our counter-thesis is that all critics of Friedman's essay are correct, for this essay can be reasonably interpreted in almost any manner that you choose. Substantially different interpretations are easily generated by various parts of Friedman's essay, many of which are not eliminated by the rest of the essay.

Evidence for our counter-thesis is best provided by the criticisms themselves and by the manner in which Friedman expresses his views. First of all, Friedman cites no philosopher or philosophy of science. Nor does he use any of the standard labels for identifying methodological positions. The only methodologist to which Friedman refers is J. N. Keynes and then only for his definitions of "positive science," "art," and "normative science." This omission severely limits the clarification of Friedman's views and the 'rational criticism' which naturally follows any provocative methodological thesis.

The result of Friedman's omission is that commentators on Friedman's essay vary widely in their interpretations. To categorize a few examples:

(1) Friedman as a conventionalist:
Latsis [1974], Hutchison [1974], Samuelson [1963]

(2) Friedman as an instrumentalist:
Wong [1973], Boland [1979], Blaug [1980]

(3) Friedman as a falsificationist:
DiMarchi [1974], Blaug [1974]

(4) Friedman as a verificationist:[158]
Rotwien [1959]

These articles are singled out only because they clearly fall into categories. Of particular note are the strong interpretations of Wong and Boland, on the one hand, and the diametrically opposed exegesis of Blaug and DiMarchi, on the other.

> Friedman is not guilty of 'instrumentalism' . . .[159]

> . . . Friedman is simply a Popper-with-a-twist applied to economics.[160]

> That Friedman is an instrumentalist is quite evident.[161]

> For in the influential form of falsification propagated among economists by Milton Friedman . . .[162]

> . . . one cannot understand the particular methodological judgments of Friedman unless one accepts or at least understands his instrumentalism.[163]

Blaug has been on both sides of the divide. Now, he too sees Friedman as an instrumentalist.[164]

It is clear that someone is wrong. The degree and frequency of the misunderstanding of Friedman by otherwise competent economists is strong evidence against the value of this methodology.

The confusion and ambiguity surrounding Friedman's position is itself sufficient to disqualify it as the foundation of any scientific activity - practical, theoretical, or instructional. The purpose of a 'scientific' methodology is to establish clear, normative guidelines which encourage the use of the best practice technology in one's discipline. This purpose cannot be served by a methodology which is essentially ambiguous and has been, is, and can be tractably reduced to science "as you like it."

Although the history of economic theory has shown that it is impossible to establish the motivation behind observed behavior, one cannot help but believe that Friedman's ambiguity is not merely accidental. Such ambiguity serves the purpose of defending one's position against criticism. In the course of our discussion, it should become quite clear that Friedman expresses theories in an ambiguous manner to defend them against criticism. If so, why should Friedman not do the same for his methodology?

Boland presents another challenge to economic methodologists. He suggests that Friedman's methodology is instrumentalism and establishes the grounds upon which instrumentalism can be criticized.[165] While it is our position that Friedman's ambiguity permits almost any interpretation of his methodology, we agree with Boland that the most favorable interpretation of Friedman's classical essay is to see it as an example of instrumentalism. We shall provide a 'rational criticism' of Friedman's instrumentalism as well as Boland's essay in section 4.4.5. Before we present this 'rational criticism,' we shall review Friedman's essay and attempt to establish a reasonable range of possible interpretations of his positions. The breadth of this range of interpretations should be sufficient to establish our first point: Friedman's methodology of "positive" economics is methodology "as you like it."

4.4.2 What is the Methodology of "What Is," Friedman's Methodology of Positive Economics?

Friedman's essay can be viewed in many different lights. Some may see it as a restatement of the classical methodological position in modern language. We shall argue that Friedman's MPE may be considered an elaboration of the classical "art" of the political economy. Like

J. N. Keynes and Lord Robbins, Friedman attempts to sum up the fruits of orthodox economics in an effort to defend it from the outrageous cries of the "unrealism" of its "assumptions." Whether or not Friedman's position is in the classical methodological tradition remains to be seen, yet it is quite clear that it belongs to the tradition of using methodological discussion to defend traditional economic theory and to seek consensus for the evaluation of those theories.

Our main objective of this section is to provide a detailed description of Friedman's methodological position as given in his essay on "positive economics." To see this essay in its best light, an attempt is made to present it in the original sequence and context. Since virtually every commentator of Friedman's essay has been accused of misinterpretation (recall quotation 157 above), it would be wise to present it in the least adulterated fashion. Although such a presentation will necessarily be more lengthy, it is hoped that these costs are balanced by the increases in accuracy, clarity, and objectivity that only a longer discussion in the original context can bring.

While discussing what Friedman's position is, we shall also point out how it is subject to a variety of interpretations. In particular, it shall be shown how aspects of Friedman's essay may be characterized as falsificationism, instrumentalism, conventionalism, probablism, logical positivism, subjectivism, and an apriorism that is constrained to the "art" of political economy. We do not wish to imply that Friedman intended all of these interpretations nor that they are all equally supported by Friedman's words. We claim only that aspects of each of these philosophies of science are found in Friedman's essay and that unsuspecting readers, particularly those who do not specialize in the philosophy of science, may honestly come to very strange and different interpretations of Friedman's MPE.

What makes the difference? you might ask. These philosophical perspectives of science are quite distinct and often inconsistent (recall section 3.3). They do not imply the same or even consistent practices (or methodologies) for economics.

How can an essay that is essentially ambiguous provide the clear guidance that is required of a sound methodology? Can such an essay be used as the intellectual foundation of the current practice of economics called "positive economics"? We ask, in turn, Is Friedman's philosophy so eclectic that his essay is inconsistent? Inconsistency in methodology is as damaging as it is in mathematics. If accepted, the inconsistency implies that all other methodological statements are acceptable - hardly an acceptable situation. To illuminate all these perspectives, we turn now to Friedman's essay.

Friedman's essay begins with the quotation of J. N. Keynes's distinctions of "positive science," "normative science," and "art (see our quotation 4 of section 4.1). Since this is Friedman's only reference to a methodology or to any methodologist, it would be easy to infer too much from this quotation. We wish only to emphasize that the reference indicates that Friedman accepts Keynes's distinctions of these different modes of inquiry.

Friedman's use of "positive" in the "methodology of positive economics" can and has caused some to associate his methodology and neoclassical methodology, in general, with logical positivism.[166] Yet such an association is largely misgiven. There is little in Friedman's essay to substantiate a logical positivistic methodology beyond the superficial resemblance of the label, "positive," and a few word choices.

After citing Keynes, Friedman quickly moves to the purpose of his essay. "This paper is concerned primarily with certain methodological problems that arise in constructing the 'distinct positive science' Keynes called for - in particular, the problem how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the 'body of systematized knowledge concerning what is.'"[167] Here, Friedman clearly sees the role of methodology in sorting out knowledge claims. He also seems to "accept" the philosophical thesis of fallibilism, as evidenced by the word, "tentatively" (recall that fallibilism is associated with falsificationism).

Yet, the problem of "acceptance" or "rejection" of knowledge claims that Friedman speaks of is not a problem in falsificationism. For Popper's falsification, a summary of the 'rational debate,' including empirical 'testing,' is a sufficient description of the third-world status of any theory. "Acceptance/rejection" of a theory always remains a risky choice and a second-world problem. It is not a problem for science or epistemology, per se, but only for application - "art" as Keynes would say or "the purpose at hand" as Friedman would say.

Next, Friedman mentions the popular confusion between "positive" and "normative" issues and uses the remainder of the first section of his essay to clarify this distinction.

> Positive economics is in principle independent of any particular ethical position or normative judgments. . . . Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the prediction it yields.[168]

Friedman asserts the independence of "positive economics" from "normative science" and that the goal of "positive economics" is predictive success. This focus upon predictive success associates Friedman's methodology with instrumentalism and, perhaps, the classical apriorism as well. Recall that Keynes also emphasizes the independence of "positive economics" from normative issues and that the goal of "positive economics" is practical application. Friedman's interest in "generalizations that can be <u>used</u> to make correct predictions about the consequences of <u>any</u> change" clearly places his position in the applied or practical realm. Yet, aside from these loose associations, there is little else to identify Friedman's specific philosophical position. Almost any philosophical or methodological perspective might be consistent with Friedman's statements.

What then is Friedman's position concerning the relationship between "positive" and "normative"? After reading Friedman, one is no further ahead than where Keynes left us. As we previously discussed, Keynes exaggerates the independence of "positive economics" and "normative science." For both see the goal of "positive economics" to be practical "in which ethical considerations are allowed their due weight" and allow "positive" theories to incorporate moral motives.[169] Our position remains, the requirement of the complete independence of "positive economics" from "normative" consideration is as unnecessary as it is inaccurate. All that is necessary is for "positive economics" to be something more than just opinions or ethical judgments.

What does Friedman contribute to this century-old debate? Only ambiguity, for the careful reader cannot even tell whether Friedman believes that "positive economics" is independent of ethics or not.

"Positive economics is in principle independent of any particular ethical position or normative judgment"[170] [emphasis added]. This statement does not even eliminate the possibility that "positive economics" is necessarily dependent upon ethics, and it is consistent with the "principle" that economists can choose which normative thesis to build their theories upon. We do not mean to imply that Friedman is really saying that these two modes of inquiry are interdependent but only that he is quite ambiguous about the issue. The assertion of the possible independence of "positive economics" from a given normative judgment is quite different from an assertion of the complete independence of "positive economics" from "normative science," in general. Unfortunately, careful reading only increases the ambiguity of Friedman's position.

Yet, it is certainly clear that Friedman wishes to claim some type of independence for "positive economics." This is further evidenced by Friedman's assertion of the equality of objectivity among the sciences. "In short, positive economics is, or can be, an 'objective' science, in precisely the same sense as any of the physical sciences."[171] It would appear that Friedman wishes to claim an equal status for economics among the physical sciences, but Friedman is not so definitive. Instead, Friedman succeeds only in creating ambiguity where there need not have been any.

He goes on to concede that the subject matter of economics deals with interpersonal relationships and that the economist is part of his inquiry.[172] Yet, Friedman attempts to negate these adverse subjective considerations by finding in them an advantage. They provide "the social scientist with a class of data not available to the physical scientist."[173] What others have called "casual introspection" is apparently a legitimate source of scientific data to Friedman. Again, we find outselves back in the nineteenth-century classical apriorism.

Friedman is arguing that economics has "special difficulties in achieving objectivity" but also has privy to special information. Together, Friedman concludes, "neither one nor the other is, in my view, a fundamental distinction between two groups of sciences."[174] This apparently means that the differences in the objectivity among the sciences does not imply a significant difference in the nature of those sciences.

Let us review Friedman's argument concerning the relative objectivity among the sciences. First, Friedman asserts the equality in objectivity among the sciences, at least in principle. Next, he gives us evidence of how economics has "special difficulties in achieving objectivity" as well as special subjective data. Then, he offers us an opinion that this evidence does not affect his initial assertion. What are we to conclude from this? Hopefully, this is not an illustration of how Friedman's MPE "works." Do we accept Friedman's "data" or his opinion? It seems clear to us that if we accept either the "special difficulties" or "casual introspection" or both, then we cannot accept the notion that all sciences are equally objective, at least not with respect to their materials.[175]

The careful reader can gain nothing from Friedman's comparison of the sciences except, perhaps, confusion. If "casual introspection" is allowed to be "data" for the science of economics, then the 'objective' status of economics is correspondingly diminished. Any such introspection is not inter-subjectively 'testable' which is a necessary condition for the 'objectivity' of the 'facts.' Yet, we agree with Friedman in that the fact that economics somehow deals with people does not necessarily make economics more "subjective." The objectivity of any inquiry is not conferred by its subject matter but only by the manner in which one chooses to study it - hence methodology.

One must wonder why economic methodologists continually make comparisons with the physical sciences. The status of a science need not be dependent upon the phenomena it studies. The issues that are addressed by these comparisons can potentially be solved by methodological fiat. In the case of the potential subjectivity of economics, we can simply adopt methodological rules which prohibits the use of any subjective considerations in the statement or 'rational discussion' of economic theories. For example, an appropriate methodological rule would be to forbid any reference to introspection in scientific discourse. Another is to likewise forbid references to opinions, beliefs, or judgments of scientists. Without such prohibitions, economists have no grounds upon which to assert the 'objectivity' of their inquiry.

Apparently, Friedman wishes to have it both ways. He claims an objective status for economics while permitting subjective considerations to have a role in the discussion of scientific theories. This permission is seen in his acceptance of the "data" of casual introspection and, as we will see, in his use of "acceptance" and "consensus" in "testing" theories.

To fully understand Friedman's methodology, one must first recognize that Friedman sees all economic inquiry through the spectacles of policy. Although he begins with Keynes's modes of economic inquiry - "normative science," "positive science," and "art" - he soon begins to collapse all into one - "art" or the economics of policymaking. Only such a reduction can explain Friedman's rather peculiar statement that: "Normative economics and the art of economics, . . . cannot be independent of positive economics. Any policy conclusion necessarily rests on a prediction about the consequences of doing one thing rather than another, a prediction that must be based - implicitly or explicitly - on positive economics."[176]

While such a statement may be correct in its reference to the "art" of policymaking, it is certainly incorrect as a description of the relationship between "positive" and "normative" economics. Contrary to what Friedman asserts "normative science" is completely independent of "positive economics." Even if our "positive" inquiry could lead us to the "absolute truth" concerning "what is," it would in no way tell us "what ought to be" nor, for that matter, which economic policy should be employed.

Obviously, Friedman reduces "normative science" to "art" and thus to policy. Where Keynes draws clear and sharp distinctions, Friedman muddles them. In the above passage, Friedman combines "normative science" and "art." In section 4.4.5, we shall argue that he also collapses "positive economics" into "art"; thus, Friedman's entire political economy is merely part of the "art" of policymaking.[177] Friedman further diminishes the role of "normative science" within policy by asserting that it is less of an impediment to consensus about policy than is "positive" economics.

> I venture the judgment, however, that currently in the Western world, and especially in the United States, differences about economic policy among disinterested citizens derive predominately from different predictions about the economic consequences of

> taking action - differences that in principle can be eliminated by the progress of positive economics - rather than from differences in basic values, differences about which men can ultimately only fight. . . . Of course, my judgment that the major differences about economic policy in the Western world are of this kind is itself a 'positive' statement to be accepted or rejected on the basis of empirical evidence.[178]

First of all, Friedman's "empirically testable judgment" is totally irrelevant to questions of philosophy, methodology, or the issues of "positive" vs. "normative" inquiry. Who cares whether people believe that their differences are over questions of ethics or science? What relevance can beliefs have upon the philosophy or methodology of science?[179] Apparently Friedman cares, for he considers this a major reason for drawing the distinctions between "positive" and "normative" in the first place.

> If this judgment is valid, it means that a consensus on 'correct' economic policy depends much less on the progress of normative economic proper than on the progress of a positive economics yielding conclusions that are, and deserve to be, widely accepted. It means also that a major reason for distinguishing positive economics sharply from normative economics is precisely the contribution that can thereby be made to agreement about policy.[180]

Here Friedman reveals one (and perhaps his principal one) of his goals of methodology - to reach consensus over economic policy.

Regardless of Friedman's opinion, questions about the agreement over policy are not methodological or scientific issues. Consensus may well be a concern for the "art" of economics, for the practical mechanics of the adoption of an economic policy in a democratic society may require some type of "consensus." But economic policy is firmly within the "art" of economics and should not be allowed to affect the study of "what is." Herein lies the potential for subjectivism to influence science. If, as Friedman does, we allow the beliefs of the masses (or anyone for that matter) to influence our methodological choices, then the result of our inquiry will contain a corresponding subject element. If considerations of consensus are allowed to influence "fundamental" methodological distinctions, why should they not likewise influence the rules of theory choice? One may think that this latter possibility is a little far-fetched, and we hope that it is. But why does Friedman always discuss the question of the viability or 'truth' of a scientific theory in terms of "acceptance," the same terms he uses to discuss questions of economic policy? In any case, although such questions of beliefs may have "practical" relevance to the economic "artist" who is interested in influencing policy, they have no relevance in drawing methodological distinctions.

What then does Friedman's discussion of "positive" vs. "normative" tell us about his position? We can see only three possible explanations that are consistent with his assertions. Either Friedman reduces all of economics to the "art" of economics, he adopts some type of subjective epistemology, or he is just ambiguous, perhaps reflecting an inconsistent eclecticism. The best interpretative theory of Friedman's position seems to be the reduction of economics to "art." Perhaps Friedman has adopted some "meta-artist" position such that all other modes of inquiry are seen only by the manner in which they are reflected by this meta-position. In any case, Friedman's singular focus upon "art" can explain his focus on "acceptance," "prediction," the "positive" dependence of "normative science," and the role of "consensus" upon methodological choice. It is this interpretation of Friedman's methodology that causes some economists to label it instrumentalism.

Alternatively, these same individual positions could follow from some type of subjectivism. Clearly, a subjective epistemology can explain the importance of "acceptance" and "consensus." Furthermore, given that one has accepted the notion that all knowledge is subjective, it would follow that this person may allow statements about beliefs to be scientific (see our last quotation) and to influence questions of methodology. This, too, would explain Friedman's peculiar position on the "objectivity" of the sciences. Here we do not assert that Friedman's position is subjectivism, but only that the diversity of his statements can be reasonably explained by "assuming" that he is a subjectivist. Or, subjectivism is one explanation of Friedman's position.

Finally, it is also possible that Friedman's position is simply ambiguous. Such ambiguity can "rationally" result when one eclectically chooses individual positions for particular purposes and those individual positions are not consistent in a larger context. Perhaps Friedman subordinates his methodological position to some unstated purpose. Or perhaps he states his purpose, for example to obtain "consensus" over policy. In any case, Friedman cannot be considered to have an 'objective' view of science or epistemology similar to the view that we have attempted to develop in this thesis. Or, if he does, many of his assertions represent serious errors. Friedman's position concerning "positive" vs. "normative" is not even then consistent with Keynes's distinctions. Thus, we are forced to conclude that either one of our interpretations is correct or Friedman is seriously mistaken and confused about issues of methodology. There is little else here to infer Friedman's philosophical position.

Let us review the logical skeleton of Friedman's first section - "The Relation Between Positive and Normative Economics."

(1) "Positive" and "normative economics" are distinct.

(2) "Positive economics" is independent of "normative science."

(3) Statements about peoples' "normative" values are "positive statements to be accepted or rejected on the basis of empirical evidence."

(4) Assuming the acceptability of a given "positive" statement about individual "normative" values, we have a "major reason for distinguishing positive economics sharply from normative economics" - thus, completing the circle.

Given Friedman's conclusion that we should sharply distinguish between "positive" and "normative," should we not distinguish peoples' normative values sharply from "facts"? If so, (3) above evaporates, taking (4) with it. It is a strange conclusion that negates its own premise. Furthermore, (4) by itself is suspect. How can a statement of science, even if 'true,' have methodological implications? It cannot, unless one adds to it some philosophical assertion.

To make sense out of Friedman's argument, we must assume that Friedman's philosophical position gives priority to issues of economic policy. But this only raises more questions. Perhaps such questions and Friedman's ambiguity are resolved in his explicit discussion of "positive economics," Friedman's Section II.

He begins this section with a statement of the purpose of "positive economics." "The ultimate goal of positive economics is the development of a 'theory' or hypothesis that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed."[181] This teleological definition of "positive economics" is consistent with almost any philosophy of science. It sounds similar to falsificationism in that theories are not tautologies (i.e., "not truistic") and should "predict" phenomena as yet unobserved.

This latter goal of scientific inquiry has special importance to Popper. If a theory predicts a completely new type of phenomenon, it deserves special attention. For, what would be a better example of 'excess empirical content'? If, then, this new phenomenon is 'observed' as predicted, the theory receives a strong 'correroration' - for what could be a riskier prediction? But, Friedman does not specify what he means by "not yet observed." Given Friedman's emphasis upon "predictions" for policy purposes, "not yet happened in time" instead of "not yet observed in kind" is probably a sufficient description of what Friedman has in mind. In falsificationism, considerations of time do not affect the appraisal of a theory.

This definition is probably more consistent with instrumentalism with its emphasis upon "predictions," but it is, nonetheless, consistent with logical positivism, probablism, or any of the others that we have mentioned. The "positivistic" connection is strengthened by Friedman's use of "meaningful." Recall that logical positivists searched for "meaningful" statements that were reducible to verified sentences. Is this substantially different from searching for "valid" and "meaningful" predictions? Thus, we still find no unique characterization of Friedman's philosophical or methodological perspective.

The ambiguity of Friedman's position is perhaps best seen by his assertion that theory can be viewed both as "a 'language' designed to promote 'systematic and organized methods of reasoning' . . . (and as) . . . a body of substantive hypotheses designed to abstract essential features of complex reality."[182] When viewed as a language, "theory has no substantive content; it is a set of tautologies. Its function is to serve as a filing system for organizing empirical material . . .; and the criteria by which it is to be judged are those appropriate to a filing system."[183] As discussed in 3.3.3, the "filing cabinet" view of theories is the main characteristic of conventionalism. This is what causes Boland to label Friedman's position as "conventionalism with an instrumental purpose."[184]

In direct opposition to this apparently conventionalistic position, we find a statement which is clearly reminiscent of Popper's falsificationism and which has consequently led others to interpret Friedman as "Popper-with-a-twist":

> Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain." Only factual evidence can show whether it is "right" or "wrong" or, better, tentatively, "accepted" as valid or "rejected.". . . The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted: great confidence is attached to it if it has survived many opportunities for contradictions.[185]

Thus, to Friedman theory can be both a tautological filing cabinet and a substantive predictor. It should be judged by the frequency of the confirmation of its predictions and by its adequacy as a language. Is this a clear methodological position? How does such a prescription serve the important role of providing a set of normative guidelines that promote the "best practice methodology" for economics?[186] If Friedman wished to be clear, would he not have told the reader in which cases a theory can be seen in one way and for which cases it can be viewed in the opposite manner? If the same theory is to be viewed in both ways, the Friedman position is inconsistent. An adequate language cannot be contradicted, since it must speak of all possibilities. Thus, a theory cannot be seen both as a language and as a substantive hypothesis. But, is this not what Friedman is saying?

Friedman's view is consistent with falsificationism in that theories need to be more than tautologies and factual evidence is somehow used to "test" hypotheses. But here the similarity ends. To the extent that Friedman's methodology has contributed to the increased emphasis upon empirical "testing" in economics, we can only applaud it. However, as we shall further discuss, Friedman does not use empirical testing in a critical context, but only as a means for achieving support. With those economists who view Friedman as a falsificationist, we do not wish to argue, for this is the view which we advocate. Yet, we would suggest that those economists read any or all of Popper to avoid misunderstanding and abusing falsificationism.

In Friedman's description of "positive economics" as a body of substantive hypotheses, notice how he focuses upon the "predictive power" of theories. Although he also uses the word "explain," one must interpret it only as a substitute for "predicts." Friedman seems to have some reservations of considering theories as explanations. This is evidenced by his continual quoting of explains (i.e., "explains") throughout his essay. Furthermore, it is clear that Friedman is not at all concerned with truth-like explanations, as is Popper. As we shall show in our discussion of Friedman's examples, he is concerned only with whether a theory "works," that is, "predicts successfully," and the 'truth' of explanation is irrelevant.

Moving to the second sentence of our last quotation, we see that <u>only</u> the "facts" are relevant to Friedman in judging a theory. This, however, is a gross exaggeration. For Friedman elsewhere mentions that logical consistency, "simplicity," and "fruitfulness" are also arbitrators of theory choice.[187] More revealing is Friedman's concern with "acceptance" and "validity." Friedman always uses the term "validity" and never "truth." But his use of "validity" should not be confused with either 'truth' or logical consistency, for it is merely a euphemism for "successful prediction."[188] For example, "the only relevant test of the validity of a hypothesis is comparison of its predictions with experience."[189] The difference is that Friedman seems to have a pragmatic view of truth, a necessary condition for instrumentalism. In pragmatic truth, truth loses all its meaning aside from what is "useful" or "works" or provides a "successful prediction."

Nonetheless, we can appreciate Friedman's recognition of the tentative nature of a theory and its empirical testing as expressed in this quotation. Friedman appears even to understand the asymmetry of empirical "proof." "Factual evidence can never 'prove' a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been 'confirmed' by experience."[190] Unfortunately, Friedman does not extend this logical asymmetry to its best advantage by advocating "testing for falsity." He merely uses it to justify the tentative status of a theory. All this sounds quite falsificationistic, as far as it goes. Friedman simply fails to go far enough for his position to be clearly identified as falsificationism or for his "testing" to be applied.

Nowhere in Friedman's essay can one find an adequate specification of how to empirically "test" a theory. All that Friedman says is that, "the hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted" (from quotation 185). Here as elsewhere, Friedman's position is better identified by what he does not say rather than by what he does.

First of all, Friedman does not explain what it means for a "prediction" to be contradicted. "Predictions" can only be classified into two mutually exclusive groups - "contradicted" or "not contradicted" - when they are qualitative. Otherwise, some methodological rules must be imposed to "judge" the tests of quantitative "predictions." This may seem like a small point, but economics has yet to decide how to empirically choose which empirical correlation or "causation" is "more significant." Since most economic controversies somehow concern which empirical magnitudes are larger, it is important that methodology address this issue. The fact that Friedman's methodology does not address this issue is well illustrated by the Keynesian-monetarist debate of the 1960's. This crusade for the "highest R^2" is probably the best reflection of Friedman's MPE in action.

What constitutes a rejection? Apparently "frequent" or more contradictions than a rival theory is a "rejection." What Friedman does not say is that the "prediction" must be a risky one and that the 'test' must be a serious attempt to 'falsify' if the "test" is to count at all. Nor does Friedman recognize the fact that the value of the 'test,' itself, depends on the 'empirical content' or the degree of 'falsifiability' of a theory. And, he never suggests that we must continually search for theories with even greater 'empirical content' and devise new and more rigorous ways to 'refute' them. Yet, these are all necessary for the achievement of the benefits from 'empirical testing' (recall Popper's view of empirical 'testing' presented in Chapter 2). Instead, Friedman's empirical "testing" seems to be some type of cursory check for consistency with the data.

This view of testing is most accurately seen as some type of probablism (recall 3.3.6), for the accepted theory is merely the one which receives the most "confirmation." "The evidence <u>for</u> a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used. . . ."[191] The logic of "accepting" such theories is clearly inductive. If Friedman were to include some explicit metric that measures the degree of "confirmation," "verification," or probability, then his empiricism would be exactly what we have called probablism. As it is, Friedman's empirical "testing" is closer to probablism than to falsificationism, since it is nearly the opposite of falsificationism. 'Corroboration' results only from serious and failed attempts to 'falsify' a theory. 'Corroboration' is not increased by the mere repetition of old "confirmations" or "applications" but only when new and more demanding 'tests' are devised.

Friedman's complacency about empirical "testing" is further evidenced by his evaluation that "great confidence is attached to it (a hypothesis) if it has survived many opportunities for contradiction." Attaching "great confidence" to any theory can only encourage complacency about further testing and inhibit the production of new explanations, thus inhibiting the growth of knowledge. Furthermore, Friedman does not specify what constitutes an "opportunity for contradiction," nor how many "opportunities" constitutes "confidence." But it seems that any <u>application</u> of a theory qualifies as an "opportunity for contradiction" in Friedman's methodology.[192]

From an empirical point of view, Friedman's idea of "testing" is absurd. There is no trick in finding "confirmations," they are everywhere, particularly when one looks for them or is applying a theory. In practical applications, the "artist" wishes not to be wrong. For in "art" mistakes are costly, while in science they often produce knowledge. Thus, the artist will try his best to use a given theory in its best light, he will use all manner of adjustments and corrections that his judgment indicates, and he will tend to rationalize away any discrepency between his "art" and "reality." Such applications cannot in any reasonable sense be called "tests." Thus, contrary to Friedman, neither evidence nor confidence accumulates with each new application.

Friedman's subjectivism is again seen in his use of "confidence." "Confidence" is clearly a second-world phenomenon that has nothing to do with the 'objective' content of a theory. The important distinction between 'objective,' third-world knowledge and subjective, second-world knowledge is completely absent in Friedman's essay. Not only does Friedman fail to make this distinction, he continually utilizes second-world terms to describe issues which are best seen as 'objective.'

As Klappholz and Agassi note,

> The absence of successful refutation of a hypothesis certainly does not prescribe belief or 'confidence' in it. Confidence in a hypothesis is purely a matter of subjective estimate, and there are no rules or theorems which relate absence of a refutation to desirable confidence in a hypothesis. . . . We mention it in order to substantiate our claim that Friedman, in spite of his avowed intentions, does not wholeheartedly accept the critical approach. . . . The doctrine that the absence of refutation imposes a desirable degree of confidence in a hypothesis can be an impediment to critical argument. . . . It is difficult to produce a rival hypothesis if it is claimed that existing unrefuted hypotheses must command confidence. . . . Thus the doctrine of desirable

> confidence . . . may be a hindrance to scientific argument and progress and a weapon for the defense of the scientific status quo.[193]

One cannot "reject" the notion that Friedman uses his "doctrine of confidence" solely as a means to dogmatically defend the status quo of economic theory. Others have voiced the same suspicion. As we have seen (quotation 156), Blaug complains that as a result of Friedman's methodological prescriptions, "the prevailing methodological mood is highly protective of received economic theory." Indeed, Friedman himself seeks to use his methodological position to defend the economists' ubiquitous acceptance of the "maximization-of-returns" hypothesis: "the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and widely accepted, is strong indirect testimony of its worth."[194] However, such a defense carries no weight, for as Blaug reminds us, "the age of a maintained hypothesis and the absence of a widely accepted rival do not provide 'strong indirect testimony to its worth,' to quote Friedman's own words: every fallacious doctrine that was ever held has been defended on such grounds."[195]

What then is Friedman's methodology? There is no clear answer. The clearest statement that can be made is that Friedman is not attempting to proclaim any form of falsificationism. Or, if he is, he does so quite poorly. His view of "substantive" hypotheses and testing seems closest to instrumentalism, at least to the extent that it sees the truth of theories in their use, and his view of theory as a language is conventionalism. Yet, Friedman's methodology can also be seen as some form of logical positivism, probablism, or even a classical apriorism that focuses upon the "art" of economics. The problem is that Friedman's explicit description of his MPE does not sufficiently define any particular methodology or philosophical position. Clearly, Friedman's view of empirical "testing" is some type of probablism. Otherwise, the merit of a theory could not increase by the mere repetition of a "successful prediction." Such a view would be subject to Hume's criticism of induction, recall Chapter 2. Merely because a hypothesis has repeatedly made "successful predictions" (e.g., "the sun will rise in the next twenty-four hours"), there is no guarantee or justification that it will work equally well, or at all, in the future.

Friedman's MPE may be less clearly seen as logical positivism. Recall from 3.3.5 that the logical positivists accepted both the criterion of falsifiability and some form of probablism as a result of Popper's criticisms. Thus, Friedman's view of empirical "testing" may have a logical positivistic heritage. Beyond the connection with probablism, the only indications of logical positivism are contained in Friedman's use of the words "positive" and "meaningful."

Finally, Friedman's methodology may be seen as a development of the practical side of the nineteenth-century British methodology. This methodology (what Keynes calls the "deductive" method and we call "apriorism") was anti-empirical and dogmatic about its general economic theories (many of which Friedman still defends); yet its application to empirical questions was seen as part of the practical "art" (recall 4.1). Perhaps such is Friedman's view of economics; thus, he seeks to further the "art" of political economy by stating its empirical methodology.

Friedman merely fills a gap left by the nineteenth-century methodologists who largely ignored the empirical aspects of theory. We do not wish to assert that Friedman's MPE is just another version of dogmatic apriorism such as the views expressed by Robbins or von Mises. We only wish to suggest that Friedman's essay may be consistently interpreted as the methodology of the art of political economy (at least by Keynes's use of the terms). Perhaps the true "F-twist" is Friedman's twist of "positive economics" into the "art" of political economy or the twist of classical apriorism into some type of modern empiricism. It should be noted that if Friedman only uses his empirical method to find confirmations of orthodox theories and fails to criticize other theories on the basis of empirical evidence, then Friedman's position is in action and in result the same as the nineteenth-century apriorists. In such a case, he only finds a new way to state their arguments and to draw their conclusions. Thus, we find it difficult to discover just what Friedman's methodology or his empirical testing is.

Friedman himself recognizes that his methodology is insufficient, at least as it concerns empirical "testing."

> The validity of a hypothesis in this sense is not by itself a sufficient criterion for choosing among alternative hypotheses. Observed facts are necessarily finite in number; possible hypotheses, infinite. If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are.[196]

What is the point of mentioning that there are infinite hypotheses consistent with the evidence? This nonuniqueness of "acceptable" theories induces Friedman to invoke additional criteria for theory choice - "simplicity" and "fruitfulness." "A theory is 'simpler' the less the initial knowledge needed to make a prediction within a given field of phenomena; it is more 'fruitful' the more precise the resulting predictions, the wider the area within which the theory yields predictions, and the more additional lines of further research it suggests.[197]

Two important questions emerge. Why are these methodological criteria needed? And, how are they used?

One may notice a superficial resemblance between Friedman's "simplicity" and "fruitfulness" and Popper's notion of 'empirical content' or with Popper's definition of "simplicity" as the degree of 'falsifiability.' Yet this similarity is <u>only</u> superficial, for Friedman uses his notions in quite a different manner.

In the first place, Friedman views "simplicity" and "fruitfulness" as something apart from empirical "testing." In Friedman's methodology, one can check a theory for consistency with the data and then choose the "simpler" one. To Popper, however, "simplicity" or 'empirical content' (recall our discussion in 3.3.3) is the set of "potential falsifiers," and these must be considered before the result of an empirical 'test' can be evaluated. Friedman views all three concepts - "testing," "simplicity," and "fruitfulness" - separately, while Popper subsumes all these concepts, including logical consistency, into one - 'empirical content' or the degree of 'falsifiability.' To view these concepts independently is highly anti-falsificationistic. In fact, Friedman never mentions the possibility of a trade-off between "simplicity" and "fruitfulness." This is particularly odd when one considers Friedman's definitions. Loosely speaking, "simplicity" is the "input" into the "prediction function" and "fruitfulness" is the "output." Thus, we would expect "technically efficient" theories to exhibit a trade-off between "simplicity" and "fruitfulness." That Friedman neglects to mention the possibility of such a trade-off and how to evaluate it makes one wonder whether he is truly serious in proposing these methodological criteria in the first place.

Friedman even admits that these notions are somewhat subjective.[198] As we discussed in section 3.3.3, Popper finds the conventionalist notion of "simplicity" subjective and arbitrary; thus, he proposes an 'objective' definition of "simplicity." Not only does Friedman have a different concept in mind, he brings us squarely back to the ambiguous and subjective, conventionalistic notion of "simplicity." Friedman's methodological criteria of "simplicity" and "fruitfulness" are not equivalent to Popper's 'empirical content' and are best seen as the old conventionalist notion of "simplicity."

Why does Friedman propose these additional criteria? Apparently, it is due to the problem of choosing a unique theory. "But why must we have a single theory?" reason asks. If more than one theory has survived all of our empirical 'tests,' all the better. For multiple "unrejected" hypotheses would serve as a stimulus for even more severe 'testing' and a hedge against experimenter bias.[199] Thus, there is no 'scientific' reason to worry about multiple 'corroborated' theories or to require criteria beyond empirical 'testing.'

Why then does Friedman wish to eliminate the multiplicity of "acceptable" theories? Two hypotheses suggest themselves. Perhaps Friedman is aware that his view of testing is not very demanding and would lead to an unreasonable number of "acceptable" theories. Or, Friedman's desire for consensus over economic policy is so strong that he cannot tolerate a wealth of scientific explanations, or both. Friedman's frequent reference to "consensus" and his focus upon policy applications tends to support the second of these explanations. Since it makes little sense to base practical economic policy upon a multiplicity of economic theories (especially when they are inconsistent), the economic "artisan" must find some means to single out a theory. For this reason, Friedman gives us two additional criteria.

In any case, reference to these ambiguous notions of "simplicity" and "fruitfulness" does nothing to further the 'rational criticism' of science and can all too easily be selectively used as justifications for the dogmatic defense of virtually any theory. A methodology that advocates theory choice by reference to the joint criteria of "predictive success," "simplicity," and "fruitfulness" (not to mention the additional criteria that are needed to "judge" theory as a language) is being everything but clear. Is it not to be expected that a theory which ranks high on one of these measures will also rank low by another? But Friedman never discusses or makes clear by word or example how to jointly evaluate a theory. With three nebulous criteria to choose from, it is "empirical testing" "as you like it." Can anyone continue to see Friedman's lack of clarity as unintentional oversight?

Nonetheless, Friedman's methodology is beginning to emerge. First a theory's "predictions" are checked against the "facts." If we find more than one theory consistent with the facts, then we invoke "simplicity" or "fruitfulness" to choose amongst them. This process is likely to lead to the theory with the smallest non-zero 'empirical content.' Theories with little content are more likely to pass the empirical "tests" and will tend to be "simpler" by Friedman's definition. Thus, Friedman's methodology will tend to result in the reverse ordering of theories from falsificationism.[200] Friedman's examples, as we shall discuss, provide ample evidence of his preference for trivial theories.

Next, Friedman offers some opinions concerning the methodology of the natural sciences. His comments about "controlled" experiments and his reference to astronomy are highly misleading and are best ignored.[201] But his reference to the crucial experiment reveals much about Friedman's MPE.

> The denial of economics of the dramatic and direct evidence of the "crucial" experiment does hinder the adequate testing of hypotheses; but this is much less significant than the difficulty it places in the way of achieving a reasonably prompt and wide consensus on the conclusions justified by the available evidence. It renders the weeding out of unsuccessful hypotheses slow and difficult. They are seldom down for good and are always cropping up again.[202]

The fact that empirical "testing" is diminished by the lack of recourse to the "crucial" experiment is much less important to Friedman than its effect upon "consensus." Here we have it, empirical "testing" is of lower significance in Friedman's epistemology or opinion than "consensus." In the scientific methodology of falsificationism, there is no trade-off between empirical 'testing' and "consensus," for "consensus" is completely irrelevant. And, the crucial experiment is the most desired and informative result of scientific activity - not that we are always or often so fortunate. We must also wonder whether the "fact" that economic hypotheses "are seldom downed for good and are always cropping up again" does not have something to do with the fact that economists have not "accepted" falsificationism.

Friedman's monetarism provides another insight for illustration of his methodology. He sees the high correlation between money and prices as "evidence that is about as direct, dramatic, and convincing as any that could be provided by controlled experiments."[203] Certainly, such evidence is "convincing" when you already have your mind made up; otherwise, it has little, if any, relevance. Mere correlation can result from practically anything. This is one of the major problems with Friedman's methodology; it gives us no possibility of distinguishing between spurious correlation and explanation or between correlation and 'corroboration.' It seems that a high correlation is the highest achievement for a theory in Friedman's MPE.

What is Friedman's methodology of "positive" economics? Again, we must say that his explicit discussion of "positive" economics is not sufficient to be "convincing." We have made a case that Friedman's MPE is not falsificationism, or, if it is, it is a distorted reflection of it. Yet, if someone cares to add a number of Poppper's methodological rules of empirical testing, MPE could be transformed into falsificationism. "A silk purse can be made from a sow's ear," if only one adds a sufficient quantity of silk.

With less silk, Friedman can be interpreted as an instrumentalist or a conventionalist. His conventionalism is seen in his view of theory as a language and his methodological criteria of "simplicity" and "fruitfulness." Even his view of empirical testing can be reconciled by conventionalism, for radical conventionalism accepts the notion that our file cabinet needs to be changed from time to time as our "facts" become more difficult to sort. It is only Friedman's concentration upon "predictions" and application that makes his position more easily seen as instrumentalism.

Although Friedman is probably best interpreted as an instrumentalist, he neither uses this term to describe his position nor explicitly states the sufficient conditions for this philosophy of science. That is, a pragmatic view of "truth" and the belief that theories are <u>only</u> instruments for predictions define instrumentalism. Without these axioms instrumentalism would not be a separate philosophy of science. Nonetheless, Friedman does come close: "The only relevant test of the validity of a hypothesis is a comparison of its predictions with experience."[204]

As we have seen, Friedman is primarily concerned with policy. This is also reflected in his preference for methodology's implications upon professional consensus above its epistemological implications. This association of Friedman's MPE with policy issues is also indicative of instrumentalism. For, what is policy if not "practical application"? Thus, we tend to agree with Boland that Friedman is least ambiguously seen as an instrumentalist. Although we agree that this is the closest to a consistent interpretation of Friedman's essay, it is not the only reasonable interpretation.

We can find only two statements about Friedman's position that can be stated with any degree of certainty.

(1) Theories need to be more than mere tautologies.

(2) Empirical testing is somehow important.

They are useful methodological views, and they are consistent with nearly any philosophy of science. Without any modifications the above tenets are consistent with forms of logical positivism and probablism. And, Friedman's essay may be viewed as a specification of the practical side of the classical aprioristic methodology, the side that Keynes calls "art."

In the first eleven pages of Friedman's essay, one can find evidence which supports at least seven different interpretations of the underlying philosophy of science. To summarize, the methodology of positive economics may be seen as:

(1) Falsificationism: Friedman's methodology is associated with falsificationism to the extent that it emphasizes the tentative and empirical nature of science. At this level, we can only commend Friedman's position. Beyond this broad argreement, there is only disparity between MPE and Popper's views.

(2) Instrumentalism: Friedman's MPE appears to be instrumentalism in its focus upon "successful predictions," policy and other application, and, in general, the pragmatic aspects of economics. Since these aspects take a principal position in Friedman's essay, one may see instrumentalism as the most consistent interpretation of MPE. Friedman's instrumentalism is most clearly seen in his applications to specific examples (see 4.4.4 and the appendix) and is further supported by the fact that none of his explicit assertions contradict it.

(3) The Methodology of the "Art" of Political Economy: Since the "art" of political economy is precisely that branch of economics concerned with practical application, Friedman's MPE is the methodology of the "art" of political economy to the same extent that it is instrumentalism. Thus, this interpretation is not independent of the preceding one. It differs only by additionally relating Friedman with the classical apriorists and MPE with Keynes's three branches of political economy. These additional connections are evidenced by Friedman's reference to Keynes's distinctions, by his defense of classical theories, and by his acceptance of "casual introspection."

(4) Conventionalism: Friedman's MPE is conventionalism in its view of economics as a language and its use of "simplicity" for theory choice. Recall (3.3.3) that conventionalism is consistent with the tentative nature of theories and with some use of empirical testing.

That "truth" is only "truth by convention" may explain Friedman's use of "truth" as "validity." MPE fails to be conventionalism only by the magnitude of its empiricism and by its goal of practical application. Yet, even these characteristics only make MPE an extreme form of conventionalism, namely, instrumentalism (recall 3.3.4).

(5) Subjectivism: Friedman's methodology is a form of subjectivism to the extent that it allows the data from introspection to have any scientific role, that it seeks "acceptance," "consensus," and "confidence," that it sees peoples' "basic values" as scientific "facts," and that it uses such beliefs to make methodological distinctions. Beyond this, one finds only a superficial resemblance between subjectivism and Friedman's word choice (e.g., "purpose in mind," "judgment," "acceptance," "confidence," "consensus," "design," . . .).

(6) Probablism: Friedman's methodology of empirical testing is some form of probablism. This is seen in the fact that a "positive" theory is "accepted" merely because its "predictions" are frequently "successful" or more so than some rival theory and that a mere repetition of a "confirmation" increases the "confidence" one may have for a theory. The main difference between MPE and other types of probablism is that MPE is not explicit in specifying the type of inductive logic or probability metric that is being used.

(7) Logical Positivism: The "methodology of positive economics" superficially appears to be related to logical positivism. However, there is no direct evidence that this appearance is more than superficial. This interpretation is the weakest of all the ones mentioned and is not to be seriously maintained. We mention it only because some readers have come to this interpretation; thus, it too represents a possible "misinterpretation" of MPE.[205]

Thus, we have no definitive conclusion concerning Friedman's methodology other than it is ambiguous. In our view, this ambiguity is inexcusable, for it would have been so easy to avoid. A simple reference to the literature or a single explicit statement could have avoided the decades of confusion and senseless debate that this essay has generated.

Throughout this thesis we have advocated 'critical debate' as a practical means for the growth of knowledge. But, debating what someone did or did not say or meant to have said is not 'rational criticism.' This is the same as arguing about the "meaning" of words. Unless a position is sufficiently explicated to be identifiable, it is not worthy of discussion. For 'rational discussion' is not possible when you do not even know what it is that you are talking about. Yet, this is how we see Friedman's methodology.

Nonetheless, perhaps we have been hasty and should "judge" MPE by its use. After all Friedman is highly concerned about practical applications, and methodology is meant to be a practical means for the production of knowledge. Fortunately, Friedman provides us with many examples of how he applies his methodology to various scientific theories. Perhaps, here we will be able to adequately identify his methodology. Yet, before we can discuss these examples, we must attempt to clarify an ambiguity which is contained in most of Friedman's examples. Is he illustrating his methodological position, or is he trying to establish his "irrelevance-of-the-assumptions" thesis. Since these examples serve both purposes, we turn first to the "irrelevance-of-the-assumptions" thesis.

4.4.3 The Irrelevance of the "Irrelevance-of-the-Assumptions" Thesis

The instrumentalist interpretation of Friedman's essay can be used to shed light upon Friedman's controversial methodological view regarding the "realism of the assumptions." If one were to "assume" that Friedman's MPE is instrumentalism, then he could "predict" that Friedman would say that the "assumptions do not matter." Since instrumentalists are interested only in "successful predictions" which may be generated by nothing more than correlation, the "truth" or "realism" of one's "assumptions" makes no difference (at least not beyond that which is contained in their "implications" or "predictions"). If, on the other hand, we wish to explain why certain market events occur or how various behaviors of economic agents are interrelated, then the "truth" or "realism" of at least some of the "assumptions" is quite relevant. Beyond clarifying Friedman's instrumentalism, the "irrelevance-of-the-assumptions" thesis adds nothing to economic methodology other than confusion and a precedent for sloppy scientific practice.

One of the purposes of Friedman's essay, if not its major one, is to settle the century-old "assumptions debate." Since the nineteenth century, orthodox economic theory has been attacked by all manner of "realistic" views. J. N. Keynes made an earlier attempt to set the record straight, then Lord Robbins; and yet the issue was not settled to the satisfaction of all concerned. In the 1940's the "assumptions debate" turned towards the theory of the firm. Friedman's "irrelevance-of-the-assumptions" thesis is an attempt to finally resolve this debate or, at least, to defend against the latest attack upon the theory of firm. Friedman's solution is simple: A theory's "assumptions" need not be "realistic"; thus, they are irrelevant in the evaluation of a theory. Since the critics had focused largely upon the "assumptions" of the theory of the firm, this is seen by many to answer the critics' challenges.

Though "simple," Friedman's thesis is irrelevant in more than one sense. In the first place, it has little relationship with the content of the historical debate. In Keynes's time and after, everyone agreed that "assumptions" should be "realistic." They only disagreed about the "realism" of particular "assumptions" and the manner in which "realism" is determined (recall our discussion of Keynes). The deductivists (or apriorists), including those of the 1930's, have consistently regarded the premises of economic theory as "facts," often labeling them "indubitable facts" of experience. What could be more "realistic" than the "facts"? From Ricardo to Samuelson orthodox economic theory was built and defended by the deductive method. Since this method rests only upon the validity of deduction and the "factness" of its premises, what other recourse is there for growth or for change other than to question the "realism of the assumptions"?

Therefore, not only is it relevant to question the "realism of the assumptions" of deductive economic theories, it is the only way to criticize such theories. In fact, the apriorists, themselves, were continually questioning the "realism" of these "assumptions," for how else could their theories change form over time? If the criticism of the "assumption" is forbidden, then the deductivist's position becomes completely dogmatic and unable to progress. From a historical perspective, it is not only legitimate to criticize the "realism of the assumptions," it is necessary for the progress of economic knowledge.

Nonetheless, past criticisms of the "realism of the assumptions" were often inappropriate and sometimes quite abusive. Members of the German historical school, Marxists, radical economists, and American institutionalists have seemed to demand the complete and exhaustive "realism of the assumptions" of orthodox economic theory. If an economic theory leaves out some historical, institutional, or social consideration which some "inductivist" deemed important, the theory was proclaimed "unrealistic" and dismissed, at least by the "inductivist." Such an excessive demand for the exhaustive, descriptive accuracy of the "assumptions" is, perhaps, as unreasonable as forbidding any criticism of a theory's "assumptions." As Blaug aptly summarizes this aspect of the "assumptions debate,"

> It is as though generations of physicists had ridiculed Newton's theory of gravity on the grounds that he committed himself to the patently unrealistic assumption that the masses of moving bodies are concentrated at their center, which might well have induced Newton to reply that predictions are everything and assumptions nothing. Faced with the accusation that no theory with counterfactual assumptions can be taken seriously, the thesis of the irrelevance of assumptions is almost excusable.[206]

Friedman's "irrelevance-of-the-assumptions" thesis may perhaps provide a quick retort to the most abusive demands for "realism." But, beyond this, his thesis says both too much and too little. To say that the "realism-of-the-assumptions" is irrelevant is much too strong relative to the deductive method. For then economics would be a completely dogmatic doctrine. It is also too strong relative to empirical science, in general, for as we will discuss shortly, there are times when science seeks the "realism of its assumptions." And, Friedman's thesis says too little in that it does not solve the important issue of the "assumptions debate."

Yet, the "assumptions debate" has a simple solution. The original debate is best resolved by the adoption of some type of explicit empirical methodology, such as falsificationism. The controversy of the 1940's can be seen as a difference or misunderstanding over which phenomena economic theory explains.[207] As such all that is required is a specification of the appropriate phenomena. Generally, the orthodox view is that economic theory explains various relationships of prices, quantities and certain "parameters"; while the critics assert that economic theories do not explain the "behavior" of economic agents (for example, the formation of "expectations" or the process leading to "maximization") or that this explanation is erroneous. Thus, this latest version of the "assumptions debate" is resolved when each side makes clear which phenomena they wish to explain and each understands that a given explanation is not deficient merely because it does not explain what others wish it to.

Our position on this issue is that:

(1) Friedman goes too far in asserting the "irrelevance-of-the-assumptions."

(2) The "assumptions debate" can be decided by the explication of which phenomena a hypothesis explains and by keeping one's language consistent with these phenomena.

(3) Traditional economic theory can be defended from inappropriate criticism by merely pointing out that exhaustive, descriptive realism is not a necessary condition for the adequacy of a theory--"abstraction is O.K."--and that a theory is not best criticized by empirical evidence which the theory makes no pretense to explain.

(4) It is impossible to determine Friedman's position on this issue; he is completely ambiguous.

One might imaginatively interpret Friedman's position as (2) or (3) above. If so, we have no quarrel with the "implications" of such an "assumption." Yet, we can find no clear statement of these positions in Friedman's essay, and it seems to us that he is saying something more than (2) and (3). For example, Boland suggests that, "Friedman argues again that the falsity of the assumptions does not matter _if the conclusions are true_."[208] Which leads us to retort if only the conclusions matter, then one should make no "assumptions"--it can only confuse the matter. Our conclusion is that Friedman's "irrelevance-of-the-assumptions" thesis should be completely ignored. It is hopelessly ambiguous, tells us nothing new, and only confuses the issue--thus, irrelevant. The only use we find for Friedman's thesis is to support the instrumentalism interpretation of Friedman's MPE, and it provides a clear "disconfirmation" of a falsificationism interpretation.

Friedman's first allusion to his "irrelevance-of-the-assumptions" thesis falls in his discussion of substantive hypotheses. "As I shall argue at greater length below, the only relevant test of the _validity_ of a hypothesis is a comparison of its predictions with experience."[209] Friedman then develops this point into one of his major considerations for "positive economics." For example, Friedman believes that it is incorrect "to suppose that hypotheses have not only 'implications' but also 'assumptions' and that the conformity of these 'assumptions' to 'reality' is a test of the validity of the hypothesis _different from or additional to_ the test by implications. This widely held view is fundamentally wrong and productive of much mischief."[210]

This position leads Friedman to the gross exaggeration that the "unrealism" of the "assumptions" is a positive advantage:

> Truely important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions. . . . To be important, therefore, a hypothesis must be descriptively false in its assumptions. . . ."[211]

Samuelson dubs this thesis the "F-twist."[212]

Friedman defends this methodological principle by reference to the criterion of "simplicity."[213] To see Friedman's position in its best light, we must interpret his point as the obvious assertion that scientific explanations are abstract and that "good" explanations go beyond mere descriptions (recall our section 3.1). If Friedman is only saying that it is "O.K." to use abstractions in economic theory and that it is not necessary to employ an exhaustively accurate description of the economy in one's "assumptions," then his thesis is quite valid and a reasonable response to the more abusive criticisms of economic theory. In context, this is clearly part of Friedman's point.[214] But, beyond this obvious point it does not follow that the "unrealism" of the "assumptions" is somehow advantageous or that "better" theories are somehow more "unrealistic." Such absurd assertions can only be a provocation to senseless debate, as witnessed by Samuelson's discussion of the "F-twist."

Our disagreement with Friedman's thesis begins at the point where he goes beyond defending the abstract nature of the "assumptions." "Assumptions" can be abstract and at the same time "realistic"--witness Einstein's "assumption" of the non-Euclidean nature of space. To the extent that Friedman is asserting that the "assumptions" need not be "realistic," in general, he is "wrong." Is this not how his position sounds? In any case, Friedman's position is thoroughly ambiguous due to his liberal and unspecified use of the term "assumptions."

Friedman even admits to this ambiguity by phrases like: "In so far as a theory can be said to have 'assumptions' at all",[215] "the very concept of 'assumptions' is surrounded with ambiguity,"[216] "in so far as the assumptions can themselves be regarded as implications of the hypothesis."[217] Yet, Friedman continues to operate with the double quoted term "assumptions" without making clear to what he is referring. As Blaug states it, "Suffice it to say that the entire thesis of the irrelevance of assumptions has been bedeviled from the outset by the indiscriminate use of the term <u>assumptions</u>."[218]

In science or 'rational discourse,' one need not be pedantic about the definitions of his terms. If some point depends crucially upon some specific meaning of a term, the author need only point out how his intended meaning differs from the general connotation. Although Friedman is clearly not satisfied with the term "assumptions," he gives no alternative definition nor does he point out his intended meaning. In such a case, we can only <u>assume</u> that Friedman wishes to imply the full connotation of the term "assumption." What choice do we have? Yet, such an interpretation creates a problem, for there are many different types of statements that are called "assumptions," some of which are quite reasonably asked to be "realistic," while it would be unreasonable to demand the realism of others.[219]

To cite some examples without attempting to give a taxonomy, there are implicit definitions, background knowledge, and 'initial conditions.' The latter demands "realism" in the strongest sense of the word, 'corroboration,' while the former need not be "realistic" in any sense. For example, it is not valid to criticize the "assumptions" of an "electron" (as Robbins seemed to), or "gravity," or the concept of a "line" (in either "pure" or "physical" geometry) for their "unrealism." These "assumptions" are only convenient labels for some set of theoretical properties and brief descriptions of where to look for certain phenomena. Their "meanings" are only inferred by the use of the entire theoretical/ empirical system to which they belong; in isolation they are undefined. Therefore, their "realism" can be considered only in reference to the "realism" of the entire theoretical system.

The "individual firm" plays such a role in economic theory. The firm is usually thought to be some business enterprise that produces a single product. It is inappropriate to criticize the theory of the firm on the basis that we do not find such firms in the "real world" or, at least, very few. Of course, "real firms" often produce a variety of products, and the modern conglomerate grossly violates the "assumptions" of the "individual firm" by producing across many different "industries." Such criticism is similar to questioning the "assumption" of an "electron" on the grounds that we do not find individual "electrons" in the "real world" but, instead, conglomerations of large numbers of "electrons," "protons," "atoms," and "molecules."

Yet the implicit definitions and theoretical terms do have a role to play. They tend to restrict the phenomena which is being explained and point to where one might find this phenomena. In quantum mechanics, the "assumption" of an "electron" forbids the explanation of the gross behavior of macro-objects (e.g., a table), nor can we use such phenomena to 'test' quantum mechanics. "Electron" tells us to look at extremely "microphenomena" and their consequences, perhaps upon macro-objects. The same holds for the "assumption" of the "individual firm." It restricts our view to microeconomic phenomena and forbids the explanation of aggregate macroeconomic behavior or even the aggregate behavior of conglomerates (unless, of course, additional hypotheses are added which are, hopefully, independently 'testable'). Due to this restriction, it may be necessary to "artificially split" large corporations into individual subsidiaries or product groups when 'testing' the theory of the firm, just as the 'testing' of quantum mechanics sometimes requires the "artificial splitting of the atom." In any case, one cannot 'test' nor legitimately demand the "realism" of theoretical terms or implicit definitions.

At the other extreme are 'initial conditions.' Examples of 'initial conditions' include the initial positions, velocities, and masses of physical objects for Newtonian dynamics (recall section 2.3), the growth of money supply for monetary theory, and the direction of change for any "disturbing factor" in economics' qualitative calculus. All of these entities may be considered "assumptions." They are logically prior to any 'testable' "implication" or "prediction," and they are commonly called "assumptions." For would not someone claim that Newton's theory "assumes" that the positions, velocities, and masses are known, that monetary theory "assumes" that money supply growth is known, or that monetary policy "assumes" that money supply can be controlled?

To 'test' or "predict," one must first empirically specify these 'initial conditions.' 'Initial conditions' are what the instrumentalist would call "predictors." Thus, it is obvious that Friedman is also concerned about the "realism" of these 'initial conditions,' for, otherwise, the resulting "predictions" would not be accurate (or even "predictable").

'Initial conditions' must be in the strongest sense "realistic," perhaps more so than the resulting "implications." If we assume that we have a "perfectly true" theory, the resulting explanation and "prediction" can be no more accurate or "realistic" than 'initial conditions.' When the theory is not quite "perfect," the predictions may well be less accurate than our specification of the 'initial conditions.' Newton's theory works precisely in this manner; the predictions it implies are always somewhat less accurate than our ability to "realistically" specify the necessary 'initial condition' (due to Lorentz contraction).

Between these extremes, one may find many other types of "assumptions" (we label this "gray area," "background knowledge"). For some, "realism" may be an important concern; for others, less so. This issue of "realism" is even more problematic for "background knowledge." Friedman's lack of clear exposition can only leave one in a quandary concerning the role of "realistic background knowledge." For either accurate "predictions" or valid empirical 'testing' the "background knowledge" must be "realistic," although not necessarily in the sense of descriptive accuracy. When viewing scientific practice, it is easy to overlook the "background knowledge." Often such "assumptions" are not explicitly stated when 'testing' a theory and especially not when using a theory for "prediction." Many applications merely assume these conditions to "hold" without any empirical investigation.

To clarify, let us look closer at Newtonian physics. The explanation or "prediction" of the motions of any physical system requires an "assumption" that there are no "outside" influences acting upon the system. In other words, a line must be drawn somewhere such that the relative motions of all objects inside the line can be explained by the interactions of gravity and inertia of only those objects. Yet such an assumed "background knowledge" may well be "unrealistic" and lead to corresponding inaccuracies in the "predicted" motions - for example Uranus without Neptune (recall section 3.2.2).

To a falsificationist this "background knowledge" plays an important and necessary role in scientific 'conjecture' and 'refutation.' Whenever a potential 'falsification' is observed, the veracity of the "background knowledge" comes into question. Changes in the "background knowledge" may become a "falsifying hypothesis" to the conjunction of the theory and the old "background knowledge." In this manner, the "Neptune assumption" is a "falsifying hypothesis" to the previous Newtonian system. The "realism" of Neptune is of paramount importance both to the falsificationist who wishes to 'test' various versions of Newtonian theory as well as the instrumentalist who wishes only to accurately "predict" the motions of the planets. The "assumption" of Neptune not only has independently 'testable' implications, it also changes the "predictions" and thus affects their accuracy.

The obscurity of "background knowledge" and its role in science is due largely to applied scientists. They merely assume that the necessary "background knowledge" is adequately specified without an explicit discussion or empirical investigation. In all practical applications, many such "assumptions of convenience" are made, usually without notice or discussion. However, this oversight of applied scientists does not diminish the importance of the "realism" of our "background knowledge."

Perhaps another example will clarify. In order for the close correlation between money supply and prices to exist, monetarism requires that the velocity of money be constant, or at least relatively so.

Whenever a monetarist makes a macroeconomic prediction some "assumption" about the velocity of money is being made, and the resulting "prediction" can be no more accurate than the "realism" of this "assumption." To most monetarists, the constancy of the velocity of money is merely part of an accepted body of "background knowledge."[220] Thus, monetarists perceive little need or effort to empirically "verify" this constancy with each new application. Yet should the constancy of the velocity of money change, so would the "success" of the monetarists' "predictions." In spite of Friedman's methodological assertion of the irrelevance of the "assumptions," it appears the "realism" of this particular "assumption" is quite relevant.[221]

So what is the point? Every scientific prediction is based upon all types of "assumptions," many of which demand "realism" if the predictions are to be accurate. Thus, contrary to Friedman's methodological thesis, the "assumptions" usually require "realism." This is true even if the object is _only_ "successful prediction" or simply practical application. Either Friedman is quite "wrong" about the role of "assumptions" in science, or he has something entirely different in mind when speaking of them.

Since we have not presented a taxonomy of "assumptions" nor wish to, a few more words may be needed to explain how others view the "assumptions." Archibald mentions four types of economic assumptions: "assumptions about motivation," "empirical assumptions of the existence and stability of functional relationships," the _ceteris paribus_ clause, and boundary conditions; while Melitz discusses only the dichotomy: "generative assumptions" and "auxiliary assumptions."[222]

In orthodox economics, "generative assumptions" are usually "assumptions about motivation," often involving "maximization." In conjunction with "auxiliary assumptions," these "generative assumptions" may be used to derive empirical statements for either 'testing' or "prediction." The ceteris paribus clause is the common assumption that all "outside influences" are constant, and boundary conditions refers to where a theory is applicable or, in other words, to which phenomena a theory explains. Of all the types of "assumptions" mentioned, only "generative assumptions" or equivalent "assumptions about motivation" can be reasonably considered independent of "realism."

Why are "generative assumptions about motivation" exempt from the need for "realism"? They need not be "realistic" simply because they are not genuine statements about the behavior of anything. These "assumptions" represent only means for deriving "substantive hypotheses" about prices and quantities; they do not say anything about how economic agents actually behave.[223] If orthodox economics makes no claim to explain this behavior, "maximization" cannot be properly criticized for its lack of "realism" Such "generative assumptions" play no 'scientific' role, as we have discussed it, nor do they "predict" anything. They can be used only to derive "predictive equations" or scientific 'conjectures.'

For Friedman's thesis about the "realism" of the "assumptions" to be correct, we must restrict it to these "generative assumptions." Perhaps, this is Friedman's implicit intention. Is it not clear that the role of the "irrelevance-of-the-assumptions" thesis is to defend the "generative assumptions" of the "maximization-of-returns"?[224] However, to generalize Friedman's "irrelevance-of-the-assumptions" thesis beyond these "generative assumptions about motivation" (particularly "maximization") clearly leads to an erroneous position. For, all the other types of "assumptions" need generally be "realistic."

To reiterate, the _ceteris paribus_ clause needs to be "realistic," as well as fully specified, or "disturbing influences" will cause "predictions" to be inaccurate and disqualify any potential refutation for lack of proper control. Boundary conditions need be "realistic"; otherwise, a theory will be applied outside the range for which it "holds." In such a case, "predictions" are likely to be inaccurate and empirical 'testing disqualified. Obviously, the same can be said of our "background knowledge" or Archibald's "assumptions about the existence and stability of functional firms." If such "assumptions" turn out to be "unrealistic," then "predictions" based upon them will be unreliable and probably inaccurate. Thus, the irrelevance of "realism" for all the above types of "assumptions" cannot be seriously maintained, and Friedman's position is "wrong" when applied to such "assumptions." One should also notice that the above argument is not based upon any particular philosophy of science. It holds equally well if the goal of science is "truth-like" explanations of the interactions of complex phenomena or if one wishes only to "predict successfully." Thus, Friedman's position about the "realism" of the "assumptions" is wrong if extended beyond "generative assumptions" and implicit definitions.

Perhaps Friedman wishes only to apply his thesis to "generative assumptions." Such an implicit intention seems to be further evidenced by how Friedman links the "irrelevance-of-the-assumptions" thesis inextricably with the issue of specifying the range of phenomena that a theory explains. Friedman first mentions this "explanation range" when discussing "positive economics." "For the test to be relevant, the deduced facts must be about the class of phenomena the hypothesis is designed to explain, and they must be well enough defined so that observations can show them to be wrong."[225]

Thus, Friedman appears to equate this "solution" to the problem of specifying which phenomenon a theory explains to the "irrelevance-of-the-assumptions" thesis.

> The reason this evidence is indirect is that the assumptions or associated implications generally refer to a class of phenomena different from the class which the hypothesis is designed to explain; indeed, as is implied above, this seems to be the chief criterion we use in deciding which statements to term 'assumptions' and which to term 'implications.'[226]

In other words, "implications" are defined to be statements about phenomena we wish to explain, and "assumptions" are statements about other phenomena.

Given these definitions, Friedman's "irrelevance-of-the-assumptions" thesis becomes equivalent to his solution to the problem of identifying which phenomena a theory explains. Do we finally have an answer to what the "assumptions" are and a specification of which phenomena a theory explains? Not from Friedman; the reader must supply his own answer to one of these questions, for Friedman only states that the questions are interdependent.

Friedman tells us indirectly that the "assumptions" are irrelevant because they do not refer to the class of phenomena that the theory was "designed to explain." But, what phenomena is that? Or, which phenomena are not explained? As usual, we can find no clear answer in Friedman's essay.

If we add our own answer to which phenomena traditional economic theory explains and which it does not, then we can break the circle. The "assumptions debate" is resolved only after one accepts that economic theory does not explain the behavior of economic agents (recall section 4.3). In economics, such behavior of economic agents always involves "generative assumptions about their motivation." The apriorists' "generative assumption" is the "Fundamental Assumption," "maximization." These then are the things Friedman considers "assumptions" and which may be independent of "realism." So if we apply our own knowledge about which phenomena a theory is responsible in explaining, we also have a solution to Friedman's question of the "realism" of the "assumptions."

From either direction we have been led to the notion that "generative assumptions" require special consideration in economics. Apparently, they need not be "realistic" nor are they part of theory's explanation. If Friedman wishes to restrict the "irrelevance-of-the-assumptions" thesis to "generative assumptions," why does he not say so explicitly? For then a great deal of unnecessary controversy could have been avoided, and perhaps economics would have experienced some resolutions to methodological confusion decades ago.

However, one might ask, "If the assumptions concern an entirely different class of phenomena than the class of interest, then why talk about them at all?" The whole purpose, as we see it, of scientific explanation is to find the interconnections among different phenomena. The explanation is precisely the connection between those different phenomena. In Friedman's language, it is the connection between the "assumptions" and the "implications." Thus, it is impossible to 'test' a scientific explanation without testing both the "assumptions" and the "implications." It makes no sense to 'test' only the "bottom line" of some explanation and suggest that this can provide evidence, for or against, some mechanism of explanation. If the "assumptions" are taken to be about some completely different phenomena and only the "implications" allowed to be "tested," then the "assumptions" are indeed irrelevant. For they are neither a part of the scientific theory nor its explanation. Friedman, himself, recognizes the possibility of eliminating the "assumptions" altogether. "For example, a hypothesis is formulated for a particular class of behavior. This hypothesis can, as usual, be stated without specifying any 'assumptions.'"[227] What then is Friedman's point?

His point is more clearly seen when we accept Boland's interpretation of Friedman's "irrelevance-of-the-assumptions" thesis. He believes that Friedman is asserting that 'false' assumptions may be considered part of the explanation of empirical phenomena; thus the "as if" euphemism.[228] But this is absurd by our notions of 'truth' and scientific 'knowledge.' Friedman's "as if" clause can make sense only if we accept that Friedman's philosophy is either conventionalism or its extreme form, instrumentalism. Only these philosophies of science completely ignore the "truth" of the hypothesized explanation and view such an explanation only as a means to an end - "successful predictions" for instrumentalism.

Although some may believe that logic permits "false assumptions," such a view is largely misgiven. While it is true that an "if-then" statement is valid when the premises are false and the conclusions are true, if we know the premises to be false, we have no license to use such a theorem. In such a case (when the premises are known to be false or must always be false) the theorem is said to hold vacuously; thus, it has no content. The same holds true for scientific theories and explanations. If we know the "assumptions" to be 'false,' the theory of which they are a part is, at best, an empty explanation without content, incapable of being meaningfully applied, and, we would say, 'falsified.'

To allow "false assumptions" to be part of scientific explanation is to make absurd the notion of scientific knowledge. "False assumptions" cannot be in any sense responsible for 'true' conclusions, no matter how well the latter are 'corroborated.' "False assumptions" are incapable of saying more than, "Do not look here, if you are looking for the truth." If a methodology makes irrelevant the 'truth' or 'falsity' of the "assumptions" of an explanation, then the "explanation" is equally irrelevant and should be completely ignored. Apparently it is scientific explanation that is irrelevant to Friedman's methodology of "positive" economics. Since instrumentalism is concerned only with "prediction," it can "explain" how or why Friedman ignores the role of scientific explanation and advocates the "irrelevance-of-the-assumptions" thesis. Only Friedman's instrumentalism adequately explains his extreme view of the "assumptions."

In Popper's science, if we know that some "assumption" is 'false' and the conclusions are 'true' or "work" or whatever, then the proper response is to eliminate the assumption completely from our theoretical system or formulate a new one. 'False' assumptions can serve no role other than to confuse the issue and to make "false pretenses." If an assumption is only "partly false," then we need to find a more general theory that can completely bypass this assumption or tells us under which conditions the assumption can be considered 'true' and for which it cannot. The best interpretation of an "irrelevance-of-the-assumptions" thesis is that 'false' assumptions are irrelevant to current scientific explanations and knowledge. But they are not irrelevant to the growth of knowledge, for knowledge grows only when we find such 'falsities.'

If Friedman is willing to accept "false assumptions," and thus false explanations, why must we have "assumptions" at all? Friedman answers,

> The 'assumptions of a theory' play three different though related, positive roles: (a) they are often an economical mode of describing or presenting a theory; (b) they sometimes facilitate an indirect test of the hypothesis by its implications; and (c) as already noted, they are sometimes a convenient means of specifying the conditions under which the theory is expected to be valid.[229]

Taking these "positive" roles in turn, we can ignore the first. It only speaks of the convenience of exposition which has no content or scientific role. Yet, Friedman's discussion of this "positive" role is characteristic of his erroneous and instrumentalist argumentation. For he uses a false choice to justify the use of assumptions as an economical mode of description. Either we use a "false assumption" - "leaves seek to maximize the sunlight they receive" - or we must provide a set of exhaustive rules for describing the phenomena.[230] Yet, even in terms of exposition there are many other choices, some of which may even be more convenient, for example, phototropism or the more general theory that Friedman mentions, "sunlight contributes to the growth of leaves."[231]

Here again Friedman's instrumentalism appears. Clearly, Friedman's "maximization-of-sunlight" hypothesis is not meant as a serious explanation of how trees actually behave or even why leaves come to occupy the positions that they are observed to occupy. Such a hypothesis or "assumption" may be used to "generate" "predictions" but then nothing more. Yet to an instrumentalist this may be enough. However, Friedman appears to realize that this hypothesis is inadequate. "We are inclined to 'explain' its validity on the ground that sunlight contributes to the growth of leaves. . . . This alternative hypothesis is more attractive . . . because it is part of a more general theory that applies to a wider variety of phenomena. . . ."[232] While this may, in some sense, be true, a better reason to prefer the "general theory" is that it contains an explanation of the arrangement of leaves - it is something more than a "prediction." Or, one might rationally prefer the more "general theory" since it is potentially 'falsifiable,' thus having greater 'empirical content' than the alternative. The "maximization-of-sunlight" hypothesis is 'irrefutable' since every arrangement of leaves that is seen to deviate from the "optimal arrangement" would be "explained" by adding some new constraint.

Nor does the second "positive" reason for "assumptions" make any sense when we allow "assumptions" to be 'false.' The "indirect testing" of a hypothesis involves deriving other "implications" from the "assumptions" and "testing" them. Friedman believes that evidence for one set of "implications" can thereby lend support for another set of "implications."[233] But if the "assumptions" of each set of "implications" are allowed to be 'false,' then one cannot say that the "success" of either set of "implications" is related to the theoretical system to which the "assumptions" belong.

The only definitive result that such "indirect testing" can hope for is to show that the "assumptions" are 'false.' But if the 'falsity' of the "assumptions" is "acceptable," then we cannot learn from such "testing." If either set of "implications" is 'falsified' by empirical evidence, this implies only what we already know or do not care about; namely, that the "assumptions" are 'false.' If, on the other hand, the empirical tests tend to be supportive, we have no license to infer any greater support merely because each can be hypothetically derived by some set of statements with common "assumptions." When the 'truth' of the "assumptions" is irrelevant so must be the results of such "indirect tests." At best, "indirect testing" could tend to support the 'truth' of the "assumptions." But since this does not matter to Friedman, the "indirect testing" which uses the "assumptions" as an intermediate step is equally irrelevant.

Friedman seems to want it both ways. The 'falsity' of the "assumptions" is irrelevant for our "successful implications," yet any additional support that one may find for the "truth" of the "assumptions" - "indirect testing" - increases our "confidence" in the original "implications." What then is Friedman's purpose in using the "assumptions as an indirect test of a theory"?

> For example, a hypothesis is formulated for a particular class of behavior. This hypothesis can, as usual, be stated without specifying any "assumptions." But suppose it can be shown that it is equivalent to a set of assumptions including the assumption that man seeks his own interest. The hypothesis then gains indirect plausibility from the success for other classes of phenomena of hypotheses that can also be said to make this assumption; at least, what is being done here is not completely unprecedented or unsuccessful in all other uses.[234]

Is it not clear that this "indirect plausibility" is merely superficial support for the "Fundamental Assumption"? That "indirect testing" is just another source of all those many "confirmations" that one can always find when one looks for them? This is the only possible interpretation, since Friedman mentions no possibility that "indirect testing" can have a "negative" impact upon our evaluation of the results of "direct testing." It appears that "indirect testing" is only a euphemism for finding what you wish to see.

Friedman, himself, does not seem to be entirely convinced that "indirect evidence" can lend support to a given "implication." For, why else does he refer to the precedent of such argumentation? While it is true that there is a precedent for using such "indirect evidence" to support a theory, there is also a precedent for any other type of "bad" methodology. In falsificationism, "indirect testing" is productive only when it opens our original theoretical system to even more severe criticism, when it makes even riskier predictions. Only then can unsuccessful 'tests' (that is, those which fail to 'falsify' our theoretical system) provide 'corroborative' evidence. But falsificationism considers the 'truth' or the 'falsity' of the "assumptions" - in an explanatory sense - of paramount importance.

Nonetheless, Friedman's "use of the assumptions as an indirect test of a theory" tells us at least one thing. Namely, Friedman, too, considers the "irrelevance-of-the-assumptions" an exaggeration. Is it our imagination, or is there an inconsistency between allowing "the use of assumptions as an indirect test of a theory" and the assertion that "the only relevant test of the <u>validity</u> of a hypothesis is comparison of its predictions with experience?" From this, one can conclude that either Friedman is completely ambiguous or inconsistent and/or he erroneously evaluates empirical evidence "both ways" to support orthodox economic theory. In any case, Friedman's discussion is best forgotten.

Finally, we come to Friedman's third "positive" reason for "assumptions." Here, Friedman wishes to use "assumptions" to specify under which conditions a theory holds. Fine, but such "boundary conditions" need to be "realistic," at least in the sense that the phenomena being explained or "predicted" actually fall inside them. Thus, such conditions need to be 'corroborated' before empirical 'testing' to justify one's confidence in his "predictions." However, if the "assumptions" need to be 'corroborated,' then their 'realism' - in the strongest sense of the term - is quite relevant, which negates Friedman's thesis.

"Boundary conditions" are quite important, particularly for applied science. In fact, we find that the explicit specification of "boundary conditions" is the solution to the more recent "assumptions debate." While we can only applaud Friedman's advocacy of the use of "boundary conditions," we are not entirely convinced that their use is consistent with the rest of Friedman's MPE. If Friedman were serious about stating which phenomena a theory explains, would not one of his many examples contain at least one such explicit statement? Yet, Friedman never explicitly limits the applicability of any hypothesis. And if he were serious about stating which phenomena a theory actually explains, would that not contradict his assertions that the "realism" of the "assumptions" does not matter.

In the course of discussing the "positive" reasons for using "assumptions," Friedman negates the strong version of his own "irrelevance-of-the-assumptions" thesis. This gives us only two choices. Either Friedman is simply inconsistent, or he never meant to imply that the "assumptions" were totally irrelevant. Giving him the benefit of the doubt, we can conclude that the "assumptions" are sometimes irrelevant and sometimes not. But this hardly clarifies the issue. Yet here it must stand, for Friedman does not explicate under which conditions or situations the "assumptions" are relevant and for which they are not. Apparently, there is an unspecified ceteris paribus clause or "boundary condition" appended to Friedman's general thesis which, as Hutchison reminds us, makes one's statements "hopelessly ambiguous."[235] The closest that Friedman comes to such a specification is to equate relevancy to "use." It is not relevant to criticize a theory by using its "assumptions" - the "irrelevance-of-the-assumptions" thesis - but it is relevant to use the "assumptions" to support a theory - "indirect testing." Does one need to comment upon the type of methodology that this implies?

As we have already pointed out, the demand for an exhaustive, descriptive accuracy of a theory or its "assumptions" is an inappropriate criticism and can be dismissed. Clearly, this is one of the senses in which Friedman wishes to invoke his "irrelevance-of-the-assumptions" thesis. Only with this restriction does the "irrelevance-of-the-assumptions" thesis have any merit. However, it seems that Friedman wishes to apply this thesis beyond this restriction, and to the extent that he does, his position is erroneous.

Where does this leave us? The real issues at stake concern the "acceptability" of orthodox economic theory or how to handle the criticism of the theory of the firm. This "assumptions debate" completely evaporates when defenders of economic theory remind the critics that:

(1) the lack of complete descriptive accuracy is not a 'rational criticism' nor a deficiency of a theory,

while the critics demand that the defenders

(2) specify which phenomena a theory explains and keep their language well within this range of explanation.

Friedman's essay provides us with neither of these resolutions. His "irrelevance-of-the-assumptions" thesis is considerably more general than (1). Such an overgeneralization only adds to the profession's previous confusion over the "assumptions." Nor does Friedman's "phenomena the theory is designed to explain" qualifier imply (2). To say that a theory only explains a certain class of phenomena is not the same as saying which phenomena that is.

Friedman fails to tell us which phenomena the theory of the firm explains (or any other theory, for that matter). Yet he argues that criticism derived from studying the behavior of businessmen is irrelevant. If the "maximization-of-returns" hypothesis is not meant to explain such phenomena, then Friedman obfuscates when he says, "Individual firms behave _as if_ they were seeking rationally to maximize their expected returns . . . _as if_, that is, they knew the relevant cost and demand functions. . . ."[236] Although Friedman is careful to add the _as if_ qualification, it clearly sounds "as if" the "maximization-of-returns" hypothesis has something to do with the behavior of firms or businessmen.

Friedman does not bring us any closer to a resolution of the "assumptions debate" of the 1940's. As we mentioned in the previous section, the central issue of this controversy concerns which phenomena the theory of the firm explains. The defenders of the theory of the firm wished only to explain the interrelations among certain prices, quantities, and given parameters, while the critics were interested in various behaviors of the firm. When the defenders, such as Friedman, make use of some behavior of the firm as an apparent "explanation" of these price-quantity relationships, the critics are quite justified to empirically 'test' any additional implication of this "assumed" behavior of the firm and to regard this test as a 'test' of the proposed explanation. For example, the implication that "knowledge" has a zero price provides a legitimate 'test' of Friedman's "maximization-of-returns" hypothesis. This 'test' is not some directly perceived "realism of the assumptions," but a 'test' of the only logical implication of Friedman's "maximization-of-returns" hypothesis. And, this 'test' results in a 'falsification' of the entire 'empirical content' of Friedman's "maximization-of-returns" hypothesis (more is said on this subject in the next section).

If the defenders of the theory of the firm do not wish to have their theory subjected to such criticism, they have a simple escape. They can reformulate the theory of the firm such that it does not refer to any behaviors but only uses the entities - price, quantities, etc. - by which they wish their theory to be 'tested.' This is what we mean by our previous point, (2). If, however, they continue to use some type of behavior to explain prices and quantities, then empirical 'testing' of all implications of that behavior is proper 'rational criticism.'

Friedman's solution is, of course, to have it both ways. That is, to use behavior as a rhetorical "explanation" - "firms behave _as if_"[237] - and to prohibit any criticism that may be derived from this "assumed" behavior. Both Friedman's "irrelevance-of-the-assumptions" thesis and his "designed to explain" clause can only be used to limit and restrict 'rational criticism.' Neither can open any theory to harsher empirical testing or extend a theory to broader regions. It is obvious that theories can be _explicitly_ restricted by using 'boundary conditions' and such. So what is the purpose of being reminded that a theory may be designed to explain only certain phenomena? Friedman's instrumental logic would suggest that in its use lies its truth. By such reasoning these theses become merely dogmatic axioms, since they can do naught but defend and restrict. If we allow Friedman to have it both ways, then his ability to selectively find "predictive successes" will encase the theory of the firm in steel, and we will never be able to learn about the economy.

In conclusion,

(1) Friedman's "irrelevance-of-the-assumptions" thesis is erroneous if extended beyond the irrelevance of complete descriptive accuracy.

(2) Friedman's solution to the specification of which phenomena a theory explains is unsatisfactory. The qualifier "the phenomena the hypothesis is designed to explain" cannot have any reasonable methodological role to play, other than to add another defense against criticism or another prohibition to 'empirical testing.'

(3) Friedman's position is quite ambiguous because he does not explicate how far we are to take the theses associated with (1) and (2), above.

(4) He does not resolve the "assumptions debate," but merely seeks to defend the theory of the firm from criticism.

In view of these points, we assert that, at best, Friedman's "irrelevance-of-the-assumptions" thesis is irrelevant to issues of scientific methodology and best forgotten. Popper's methodology of falsificationism and his view of 'testing' clearly resolve all of the issues that Friedman is, in any way, attempting to address. Thus, Friedman's can only contribute confusion where there is already clarity.

Again we find ourselves in agreement with Blaug when he writes,

> Looking back at the entire debate surrounding Friedman's essay, we cannot help being struck by the lack of methodological sophistication that it displayed. The notion that theories can be neatly divided into their essential components and that the empirical searchlight is to be directed solely at the implications and never at any other parts of a theory can only be understood as a reaction to a century of critical bombardment of orthodox theory. . . .[238]

As Blaug further suggests, given the abusive nature of some of this criticism, the "irrelevance-of-the-assumptions" thesis is <u>almost</u> excusable. But, dogmatism is never an excusable reaction to criticism, no matter how inappropriate the criticism may be.

4.4.4 Maximization-of-Returns: Its "Truth" in Its Use

Much of Friedman's essay is a defense of the "Fundamental Assumption" or "maximization-of-returns" (recall 4.3). The "irrelevance-of-the-assumptions" thesis is one defensive tactic. Use as "truth" is another - "evidence for maximization-of-returns hypothesis is experience from countless applications. . . ."[239] Since methodology is the practical side of epistemology, the evaluation of a methodology's applicability is always important. When the methodology gives usefulness and application preeminent roles, the evaluation of the methodology's use becomes essential. For this reason, a comprehensive review of Friedman's MPE must analyze and discuss the many examples and illustrations that are found in his essay. In this section we discuss only the "maximization-of-returns" hypothesis; Friedman's other examples are relegated to the appendix.

After presenting several absurd examples of "scientific" hypotheses (see appendix), Friedman asserts that:

> It is only a short step from these examples to the economic hypothesis that under a wide range of

> circumstances individual firms behave *as if* they were seeking rationally to maximize their expected returns (generally misleadingly called 'profits') and had full knowledge of the data needed to succeed in this attempt; *as if*, that is, they knew the relevant cost and demand functions, calculated marginal cost and marginal revenue from all actions open to them, and pushed each line of action to the point at which the relevant marginal cost and marginal revenue were equal.[240]

What is the content of Friedman's "maximization-of-returns" hypothesis? Our position is that the "maximization-of-returns" hypothesis is, at best, a 'falsified' proposition. At worst, it is part of economic methodology. It is a statement which appears to be a theory but is only a technique that can generate almost any statement about the economy. Whether intentionally or inadvertently, Friedman offers a non-empirical statement and yet lauds its ability to solve concrete problems.

This position is further supported by Friedman's use of the defensive devices of "as if" and "under a wide range of circumstances" in the above quotation. Both devices have the effect of reducing the role of empiricism. The "as if" phrase renders that part of a theory empirically empty, for it asserts that it is only "true by convention"; thus, its empirical 'truth' is irrelevant. Perhaps this is the real message of Friedman's assertion that the realism of the assumptions is irrelevant. If we accept that the "assumptions" are only true by convention, then their realism is indeed irrelevant. One consistent interpretation of Friedman's essay is to view MPE as giving only a conventional nature to the "assumptions." Then Friedman's "irrelevance-of-the-assumptions thesis" makes sense as well as his concentration upon "use" instead of 'truth.' But this interpretation makes MPE a form of conventionalism.

Even more damaging is Friedman's use of "under a wide range of circumstances." If taken seriously, this "application clause" forces any theory to be a second-order tautology. The reason is simple. If one finds some cases where "individual firms do not behave as if they were seeking rationally to maximize their expected returns," someone can always explain this apparent anomaly by merely asserting that such cases are not among the "wide range of circumstances." And, the serious scientist has no recourse, for there is no way to identify whether or not the "falsifying instances" are a member of this unspecified "wide range." Friedman's "application clause" works the same way as an unspecified ceteris paribus clause. It simply makes empirical 'testing' impossible and the content of theories is rendered "hopelessly ambiguous."

Such considerations do not inhibit Friedman's ability in finding confirming evidence. It is revealing to observe that Friedman sees this evidence everywhere but is incapable of specifying any.

> An even more important body of evidence for the maximization-of-returns hypothesis is experienced from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted. This evidence is extremely hard to document; it is scattered in numerous memorandums, articles, and monographs concerned primarily with specific concrete problems rather than submitting the hypothesis to test. Yet the continued use and acceptance of the hypothesis over a long period and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony of its worth. . . . It tends to become part of the tradition and folklore of a science revealed in the tenacity with which hypotheses are held rather than in any textbook list of instances in which the hypothesis has failed to be contradicted.[241]

Is it too much to ask for one shred of this evidence, a statement of any 'testable' implication, or, perhaps, a reference to where one might find one of these "countless applications"? Or is it too critical to wonder how applications which did not intend to submit this hypothesis to test can be considered evidence for it? We can only agree with Blaug when he says,

> This is without a doubt the most frustrating passage in Friedman's entire essay because it is unaccompanied by even a single instance of these "countless applications." No doubt, when the price of strawberries rises during a dry summer, when any oil crisis is accompanied by a sharp rise in the price of oil, when stock market prices tumble after the threat of a switch to a hard money policy, we may take comfort in the fact that the implications of the maximization-of-returns hypothesis have once again failed to be refuted. However, given the multiplicity of hypotheses that could account for the same phenomena, we can never be sure that the repeated failure to produce such refutations is not a sign of the reluctance of economists to develop and test unorthodox hypotheses. It would be far more convincing to be told what economic events are excluded by the maximization-of-returns hypothesis, or better still, what events, if they occurred, would impel us to abandon the hypothesis. . . . It suggests that Friedman, despite what he says elsewhere, is not really interested in testing the maximization-of-returns hypothesis and is instead seeking to confirm it. As we know, there is no hypothesis so strange but that it is confirmed by evidence all around us.[242]

Friedman's style of argumentation begs to be hoisted upon its own petard, for, as Blaug also notices, Friedman says one thing and yet argues by another. Friedman's defense of the "maximization-of-returns" hypothesis is completely anti-empirical. In this example Friedman mentions not one 'testable' implication and insists upon using evidence from applications which never attempted to 'test' this hypothesis. If this source of evidence is so important, why does not Friedman mention the almost countless applications that did not "work" (i.e., predicted counterfactually)? Empirically, one cannot be important without the other. In fact, falsificationism would give a higher weighting to such "unsuccessful applications."

But, are not potential contradictions considered by Friedman? Not more than verbally, since the "maximization-of-returns" suffers an obvious 'refutation,' one of which Friedman is aware. Taken as a whole, and not as an isolated "assumption," Friedman's "maximization-of-returns" hypothesis contains only one potentially 'testable' implication. The conjunction of the assertions that a firm maximizes expected returns and that the firm has "full knowledge of the data needed to succeed in the attempt" implies that the "knowledge" of the data has zero value or price, at least for the "individual firm." Or, if these firms have all the requisite information to maximize "expected returns" and if the maximization of "expected returns" is the objective of the firm, then a firm will not be willing to exchange any resource (including money) with non-zero "expected return" for "knowledge." Notice that this is an implication of the "maximization-of-returns" hypothesis and its only logical implication. The quantity of "knowledge" that an individual firm will demand at a positive price is zero, or alternatively an individual firm will pay no positive price for "knowledge." All that remains is to correlate "knowledge" to some empirically observable phenomenon.

"Knowledge" may be thought of as the resources or services that a firm uses to help make its decisions. For example, "knowledge" could be interpreted as the professional services of economists, statisticians, decision scientists, and the information systems used in decision making. By so doing, is not the result obvious? Would we not find many firms paying non-zero prices for these services? If so, the "maximization-of-returns" hypothesis is 'falsified.' We even have a "falsifying hypothesis" that is 'well-corroborated.' Namely, knowledge is a scarce resource and commands a non-zero price. Thus, when the "maximization-of-returns" hypothesis is interpreted as a genuine statement, it is 'falsified.' Now we 'know' that the "maximization-of-returns" hypothesis is not an adequate explanation of economic phenomena. Thus, 'knowledge' grows.

But this cannot be; it is too easy; and it ignores the fact that economists, including Friedman, are aware of the reality of the scarcity of knowledge - are the predictable responses.[243] Clearly, economists are well aware of this actual "scarcity of knowledge" that can be used to 'falsify' the "maximization-of-returns" hypothesis. Nonetheless, many still hold onto this hypothesis or something quite similar. How? Why? "I do not know - I only guess."[244]

Our guess is that economists do not view "maximization" as a scientific hypothesis or explanation, at all. By its use, it is clear that economists simply do not regard the "maximization-of-returns" hypothesis as 'testable.' "Maximization" is held as some type of tautology or definition of economic behavior. Or, perhaps economists are aware that such hypotheses are factually false (due to the "imperfection" of information) but maintain such propositions within economic theory for their "convenience" and ability to generate "hypothetical" interrelationships. Yet, this latter possibility is equivalent to regarding the "maximization-of-returns" hypothesis as a first-order tautology. Since most economists are aware of this refutation, one can only conclude that economists choose to interpret "maximization" as a tautology or a definition of economic behavior. Although such choices are methodological privilege, to regard "maximization" in this manner is to enshrine it within our methodology.

In such a case, economists behave "as if" they are apriorists (or deductivists). They, in effect, see "maximization" as the "Fundamental Assumption" of behavior with an economic aspect - as a definition, a means for generating statements about specific events, a situational logic, or as a methodology. By this view, "maximization" is in no way subject to empirical contradiction. Empirical application becomes but an "art" which is not permitted to reflect "negatively" upon such an important and "fundamental theory."

Our only point is that the "Fundamental Assumption" is not employed in any empirical manner to economic theory. Economists have chosen to interpret "maximization" as having no empirical content. "Maximization" is then part of economic methodology and not its science. It is merely a tool which is used to generate specific models of the economy that makes no genuine statement about the economy itself.

A critical reader might think that such a conclusion is unwarranted or, at least, premature, for the "maximization-of-returns" hypothesis is analyzed in isolation from the complete theoretical system to which it belongs. As we discussed in 4.3, one needs to view the entire theoretical system before its merits may be truly evaluated. Yet, we have two reasons for providing this incomplete analysis. First, if series of statements are always found to be zero or first-order tautologies, they can have no 'scientific' role to play even in conjunction with other statements. As Hutchison and Wittgenstein note, "From a tautology only tautologies follow."[245] If a substantive hypothesis (not a "rule of correspondence") is added to the "maximization-of-returns" hypothesis, the resulting 'empirical content' is attributable to the added hypothesis and not "maximization." Economists should be the first to realize that "you do not get something for nothing." Secondly, we are not the ones who have isolated the "maximization-of-returns" hypothesis for analysis. Friedman explicitly chooses this formulation to defend against the criticism of the antimarginalists and to illustrate his methodology, MPE.

It is Friedman's isolation of the "maximization-of-returns" hypothesis that makes it so vulnerable to philosophical criticism yet so resilient to empirical contradiction. However, "maximization-of-returns" is never employed by itself. At a minimum, auxiliary hypotheses about production functions, market structures, and "equilibrium" conditions are required before 'testable' consequences result. Friedman's failure to specify any of these necessary auxiliary hypotheses serves only to obfuscate any rational appraisal of the "maximization-of-returns" hypothesis.

When one's position is left inadequately specified, infinite degrees of freedom are available to defend the position which one claims to hold. Thus, Friedman can use any observation to support the "maximization-of-returns" hypothesis, for he has total freedom in specifying which auxiliary hypothesis to use. In fact, Friedman's methodology permits one to use contradictory hypotheses to "explain" different observations of the same economic agent. A firm can be both "perfectly competitive" and "monopolistic" depending only upon the circumstances surrounding the "application."[246] Is it any wonder that Friedman can find "confirmations" from "countless applications"?

Friedman's empiricism is biased. Every "application" adds support, since there will always exist some "artful" manipulation of "maximization" that results in the "correct prediction." On the other hand, any "unsuccessful prediction" can be blamed upon the "model's specification."

Such an "empirical" method is equivalent to that of the nineteenth-century apriorists. It is a dogmatism of certain central ideas that are only superficially discussed empirically. Friedman's application of MPE to the "maximization-of-returns" hypothesis is just as Lakatos describes in his MSRP - "methodology of scientific research programmes." "Maximization-of-returns" (as well as "perfect competition" and "monopoly") are part of the "hard core" of othrodox economics. As such, it is 'irrefutable' and a dogmatism that supports the continuity of economic thought and not its growth. The "protective belt of auxiliary hypotheses" can be continually added ad hoc to bear the brunt of any apparent empirical refutation.[247] Thus, empiricism becomes completely one-sided, and a potential scientific theory becomes nothing more than a methodology or "tool box" for making predictions.

By its use, then, the "maximization-of-returns" is methodology. Methodology can be used to aid the derivation of specific theoretical systems or "models," and it is not allowed to be falsified by the failure of its applications. Yet it is seen to gain support by a history of "successful" theories. Friedman reference to the folklore and tenacity of "maximization-of-returns" only adds testimony to our interpretation of "maximization" as methodology. For, if its supporters held "maximization-of-returns" as a scientific theory, the last two centuries of economics might have been characterized with the development and empirical testing of alternatives to "maximization."

Have we 'falsified' the "maximization-of-returns" hypothesis, proved it to be "wrong," or otherwise showed it to be inadequate? No, to make any such appraisal, a comprehensive analysis of economics would be required. It would include a review of all of the major theoretical systems that include "maximization," their 'testable' implications, and all the empirical evidence that might be brought to bear. Although such an exercise is beyond the scope of this thesis, the beginning of this appraisal may be found in Blaug [1980], Chapters 6-9, and Latsis [1974].

What then have we accomplished in this section? Our only objective is to show that Friedman's MPE is defensive and anti-empirical as he chooses to employ it. For Friedman's most important example of what he considers a scientific hypothesis, the "maximization-of-returns" hypothesis, he fails to find any contradictory evidence or "unsuccessful applications" but is able to see "confirmations" everywhere. He utilizes two anticritical devices - "as if" and "under a wide range of circumstances." These adjunct strategems can serve no useful purpose; they can only put up a dogmatic defense. Furthermore, Friedman states the "maximization-of-returns" hypothesis in a manner that is either 'falsified' (by the "scarcity of knowledge") or is not 'falsifiable' (since no substantive auxiliary hypotheses are given). Thus, what Friedman intimates is an empirical hypothesis is, in fact, 'irrefutable' methodology. In spite of Friedman's avowed intentions, the "methodology of positive economics" is not an empirical methodology, at least not as Friedman applies it.

If all of this were not "bad" enough, Friedman, like his aprioristic predecessors, commits the "factual fallacy" (recall 4.1 and 4.2). "Confidence in the maximization-of-returns hypothesis is justified by evidence of a very different character. This evidence is . . . the process of 'natural selection.'"[248] Whatever the relationship between this "Darwinian survival" hypothesis and "maximization" or its wisdom, it cannot be used as "evidence."[249] Again, we find a methodologist of economics attempting to employ one theory as "empirical" support for another. Wherever we look, we find Friedman misusing empiricism and marshalling all manner of erroneous arguments to defend his choice of theories.

Although Friedman's rhetoric appears to put orthodox economic theory upon a more firm empirical foundation, his application of the methodology of positive economics adds little beyond the traditional, dogmatic methods of the classical apriorists. Friedman's MPE results in little improvement over Keynes's deductive method. Both loudly proclaim the importance of empirical evidence, yet neither is capable of seeing "disconfirmation" of economic theory.

In Friedman's examples (also see the Appendix), we find significant evidence of dogmatism. "For the dogmatic attitude is clearly related to the tendency to verify our laws and schemata by seeking to apply them and to confirm them, even to the point of neglecting refutations, whereas the critical attitude is one of readiness to change them - to test them; to refute them; to falsify them, if possible.[250] In the course of Friedman's discussion, he invents at least three devices that can serve no function other than to defend hypotheses and to inhibit growth or change - "as if," "under a wide range of circumstances," and the "irrelevance-of-the-assumptions" thesis. He does not make a single attempt to change, test, refute, or falsify any theory. For each hypothesis that Friedman offers, there is an explicit attempt to verify or confirm it, and he neglects refutations. Yet nothing seems to inhibit Friedman's ability to apply his hypotheses. Finally, Friedman continually neglects to mention any 'testable' implications of his hypotheses - implications which, if they proved counterfactual, would cause the rejection of his theory. What can the critical reader infer from all of this? Friedman seeks only to dogmatically defend his beliefs? Or, is Friedman critically open to all hypotheses - his own and others? Which of these hypotheses would be more frequently confirmed, or more importantly, which would be refuted less often?

4.4.5 Friedman's Methodology As If It Were Instrumentalism

A growing awareness that Friedman's essay is best seen as an application of instrumentalism is emerging. This is most clearly seen in Wong's criticism of Samuelson, in Boland's defense of Friedman's essay, and, most recently, in Blaug's reversal of his previous verdict that Friedman is innocent of instrumentalism.[251] We agree that Friedman's methodology is best interpreted as instrumentalism, for this maximizes the coherence of his essay. However, this does not mean that: "His methodological position is both logically sound and unambiguously based on a coherent philosophy of science," as Boland asserts.[252]

In this section we shall assume that Friedman's methodology is clearly instrumentalism. Yet before we analyze the implications of an unambiguous instrumental methodology, we must reiterate our doubts that Friedman's methodology is unambiguous. The "assumption" of instrumentalism causes MPE to be inconsistent, thus logically negating this "assumption."

"Proof":

(1) "Assumption": Suppose that Friedman's methodology is sharply and unambiguously instrumentalism.

(2) "Implication": Then Friedman's methodology concerns only the "art" of political economy.

(3) "Inconsistency": Friedman's methodology is "the methodology of positive economics," and yet it concerns only "art."

Although this argument follows almost directly from definitions, a few additional words may make the necessary connections sharper. Our argument is composed of only three premises: that Friedman's methodology is pure instrumentalism, that Friedman accepts Keynes's distinctions between "art" and "positive science," and that Friedman's essay concerns the methodology of "positive" economics. These last two assertions are the clearest positions contained in Friedman's essay. Friedman's acceptance of Keynes's distinctions is his introduction and defines his stated purpose, while "The Methodology of Positive Economics" is the essay's title.[253] When combined with the instrumental interpretation, even these positions of Friedman become inconsistent.

The only missing step involves the understanding of instrumentalism (recall 3.3.4). Instrumentalism concerns only the practical; what is "useful," what "works," and not "what is." It is best seen as the method of engineering or technology and not as science as we have discussed it. For instrumentalism views science as merely an instrument for practical application and nothing more.[254] This prohibitive focus upon application and pragmatism is precisely what Keynes calls "art." Thus, instrumentalism concerns only "art." "Positive science" is something entirely different; to Keynes, it is the study of "what is."

Now if Friedman accepts Keynes's distinctions, his methodology is inconsistent. Friedman's MPE cannot be the methodology of "positive" economics and, at the same time, not concern "positive" economics (since it concerns only "art"). The assumption that Friedman's methodology is unambiguously instrumentalism leads to inconsistency and thus to its own denial.

Either Friedman collapses all of "positive" economics into "art" or his methodology is not unambiguously instrumentalism. The former remedy makes absurd all of Friedman's discussion about "positive vs. normative" and thus much of his essay. Yet this is exactly what Friedman does, at least in part. In 4.4.2 we show that Friedman collapses "normative" economics into "art" because of his concentration upon policymaking. By the same token, it is clear that Friedman also collapses much of "positive" economics into "art." What can we conclude from this? Either that Friedman's methodology is not instrumentalism, that Friedman sees all of economics as the "art" of political economy, or that Friedman is simply inconsistent. While we agree with Boland that the best interpretation of Friedman's MPE is instrumentalism, we cannot unconditionally accept an interpretation which makes absurd the clearest portions of the very essay to which it is meant to apply.

If Friedman's methodology is instrumentalism, then consistency would demand that he reject Keynes's distinctions or state that there is no "positive science" in Keynes's sense but only "art." As it is, the instrumental interpretation makes Friedman's methodology inconsistent with his stated purpose of dealing with problems concerning a "distinct positive science" and "the body of knowledge concerning what is."[255] "What is" is not "what is useful," and Keynes's distinctions do not permit practical application (instrumentalism) to be seen as "positive science." If Friedman's methodology is instrumentalism, pure and simple, then it is inconsistent with his own stated purpose. Is this "good art"?

How can anyone interpret Friedman's essay as a consistent and unambiguous "application" of instrumentalism to economics? Nonetheless, this is precisely what Boland claims.[256] But then Boland finds significant aspects of other philosophies in Friedman's essay. He claims that Friedman's methodological position incorporates an "inductivist" distinction, an "inductivist" claim, several conventionalist criteria, and various versions of conventionalism.[257] Is this unambiguous? In 4.4.2 we attempt to show that Friedman's explicit statements about methodology are subject to a "wide range" of interpretation. Our position is that if Friedman's methodology is instrumentalism his essay is no simple or clear presentation of this view. In fact, the "assumption" of instrumentalism causes at least one significant inconsistency.

Nonetheless, Boland does present a number of severe challenges to any potential critic. First, he demands that: "Any effective criticism must deal properly with Friedman's instrumentalism."[258] In addition, Boland states a number of requirements for an adequate "refutation" of Friedman's instrumentalism. To satisfy all of Boland's requirements is an onerous task. Yet to provide such a comprehensive "refutation" of Friedman's methodology is our intention and a principal thesis of this dissertation.

To meet Boland's requirements, our criticisms of instrumentalism are restated (recall 3.3.4). Next, Boland's interpretation of Friedman's instrumentalism is discussed along with its specific implications. Finally, Boland's requirements for an adequate criticism of Friedman's instrumentalism are met point by point.

In general, instrumentalism holds that scientific theories are only instruments for prediction. It is grounded upon pragmatism where truth is but use and "predictive success" is the only arbitrator of knowledge. As we have previously discussed, the philosophy of instrumentalism cannot account for the difference between applied and "pure" sciences ("art" and "positive science" in Keynes's words), the growth of scientific knowledge, nor the "excess empirical content" of the "better" examples of scientific theories.[259] The inability of instrumentalism to address these issues represents a serious weakness for a philosophy or theory of knowledge. However, the inadequacy of instrumentalism is best seen from its own perspective. Can instrumentalism, in fact, solve the problems it poses? Does "predictive success" lead to "useful" knowledge? Would a totally instrumental science bring about the utility that its pragmatism demands?

At the bottom, instrumentalism's problems lie in its regulative concept, "predictive success." Predictive success is not adequately defined, and any adequate definition is inconsistent with the philosophy of instrumentalism. What is "predictive success"? Friedman's implicit definition is as good as any. To him, "predictive success" is simply the result of comparing predictions with experience. If such a comparison fails to be contradicted more often than alternative theories, then the theory is "successful." Furthermore, the more often a theory is applied and not contradicted, the greater is its "predictive success."[260] How does one know whether a given application is "successful"? When it "works," of course. Yet, any such answer only begs the question.

The first weakness with "predictive success" is that there is no reasonable way to say how close a "prediction" and the associated "fact" need to be in order to proclaim the "prediction" a "success." This is particularly troublesome when making predictions of unique events. Although one can solve this problem by methodological fiat (e.g., alpha = .05), it remains more problematic to instrumentalism than falsificationism. Falsificationism reduces this problem by allowing only reproducible or rèplicable experience to count as a 'test' (recall 2.3). Statistical tests are always asymmetric, and as such the acceptance of a "predictive success" will be inconclusive. To assume that "predictive success," in general, increases as the number of noncontradicted applications increases is to assume some manner of definitive conclusion for each "confirmation." Thus, "predictive success" demands what statistics or science cannot give - definitive "confirmation."

Even more problematic is the reliance of "predictive success" upon the "truth" of the observations. If they do not assume the "truth" of their observations, there would be no way to know that the prediction was "successful" or how large the magnitude of error. Any metric of the difference between "prediction" and "fact" that might be used to measure a theory's quality or "confirmation" assumes both the "truth" of those "facts" and some method of induction. Otherwise, today's verdict of "predictive success" may be overturned tomorrow. If instrumentalism cannot live up to its own criterion - that is, predict the future success of a theory that has been labeled "successful" - then it would be inadequate on its own terms. "How can this be?", you might ask.

First let us assume that the "facts" are not tentative nor problematic. "The facts are the facts and that is all you need to know." Then it would be possible to measure "predictive success" by methodological convention. If, in addition, there were some method of induction, then the repetition of "success" would increase our "confidence" in the theory. Without induction we have no ground to increase our measure of a theory's "success" merely through repeated applications. Yet this is precisely what Friedman claims; thus, he is assuming some type of induction.[261]

Unfortunately, there is no valid method of induction and the "facts" are not necessarily "true" or unproblematic. As we have repeatedly discussed, "facts" are theory-impregnated and tentative. It is interesting to note that Friedman, too, accepts the theoretical nature of the "facts." "A theory is the way we perceive 'facts,' and we cannot perceive 'facts' without a theory."[262] If this is the case, then it is possible that a new theory will come along and see the "facts" in the manner that makes past "success" a failure. Such a new theory could then be the most "successful" and, at the same time, reverse our evaluations of the "success" of past "successful" theories. When the "facts" are theoretical and tentative we cannot predict that our currently "successful" theory will remain as "successful" nor can alternative theories be ranked or chosen by their "predictive success." If the "facts" depend upon our theory, then alternative theories may well see the "facts" differently. Thus, theories would not be comparable, at least not in terms of "predictive success," and "predictive successes" would not provide an adequate means for theory choice.

Such circumstances pose a serious dilemma for instrumentalism. If the "facts" are tentative and theoretic, then the instrumentalist cannot even predict that his "successful" theory will remain "successful." Can such theories be called "practical"? Will their application be "useful"? Thus, instrumentalism cannot engender its theories to provide the practical utility that its pragmatism demands.

On the other hand, an instrumentalist could, as a philosophical axiom, assert that the facts are true and immutable. Then a theory once "successful" would always remain at least as "successful." But the pragmatic view of truth sees truth only as use. To assume that "facts" are true in some absolute or unproblematic sense is to "refute" the philosophical basis of instrumentalism. Nor can the instrumentalist "successfully" define the "facts" by what is "useful." "Useful" might be seen as whatever makes the theory in question "work." Since "usefulness" is a teleological notion, it could be defined relative to whatever one chooses, including one's personal gain or the "purpose at hand." In any case, "fact" as "utility" would lead to absurd scientific practice, particularly when the goal of instrumental science is to discover "successful" or "useful" theories.

With the goal of science being "successful" theories, there are incentives to propose theories with little "empirical content" and to "test" them but gently. Thus, instrumental science would tend to lead to the trivialization of knowledge and dogmatism, for not only are there no incentives to "refute" a "successful" theory, such an activity would seem to defeat the instrumental purpose. Instrumentalism will also have a tendency to interpret the "facts" in light of its theories, for what more practical way is there to find "successful predictions." Instrumentalism can promote only a degenerate empirical science.

In falsificationism there are no guarantees. Neither theories nor facts can be established or chosen for once and for all. But then falsificationism does not desire to do so. The biases that instrumentalism has towards dogmatism and trivialization of knowledge are minimized by falsificationism. When the goal is to "falsify" theories, the incentives to interpret the "facts" in light of the theory, to offer theories with little content, or to hold on to past "success" are minimized. What better way is there to produce knowledge and practical utility than by severely "testing" theories, by trying our best to find any weakness, and by offering bold and imaginative conjectures whenever they occur to us?

Instrumentalism fails upon its own ground. It cannot produce utility or practical value. Its ability to produce practical utility rests upon the fallacy of induction. For a theory to have practical value (from an instrumental perceptive), it must be capable of producing "successful predictions" of the future. A theory which "worked" only in past applications and will not "work" in future applications is neither "practical" nor "useful." Any such claim assumes that the problem of induction has been solved. The logic of instrumentalism is subject to the full force of Hume's criticism of causation and induction. No amount of past success implies future success, nor does it make future success probable.[263] Without any guarantee of future success, how can anyone say that past applications or past "success" produces anything like "confidence" or practical utility? Without induction, there simply is no reason to such statements.

The fact that the logic of instrumentalism depends upon some absolute notion of the "facts" and upon induction from these facts is a fatal criticism of a philosophy of knowledge. Thus, we have the nearest substitute to a "refutation" for a philosophical position. Yet without these assumptions, instrumentalism cannot be applied. For then there is no basis upon which to choose a "successful" theory for the purpose of future application. The truth of the "facts" and the existence of a valid method of induction are untenable philosophical positions. Thus falls instrumentalism. By the philosophy which demands that the world be viewed in terms of practical utility and application, the inability to produce practical value with any probability or reason for "confidence" must be seen as empty; thus, instrumentalism is self-destructive.

Instrumentalism can "work" only if one assumes knowledge as given. If we know something "is" or "is not," then such knowledge may be usefully applied. Without assuming such knowledge, instrumentalism is empty and has no value by its own view. Instrumentalism can be seen as a philosophy or methodology for engineering or "art" when scientific knowledge is taken as a given. But instrumentalism cannot produce scientific knowledge or utility without it.

Theories are most likely to be useful for practical application when they have been severely 'tested' for 'falsity.' Only if our best efforts are devoted to 'falsifying' our theories, can we have <u>any reason</u> to suspect that they might be usefully applied, and, then, there are no guarantees. Such 'testing' is not consistent with instrumentalism's search for "predictive success." If someone attempts to solve the inadequacies of instrumentalism by incorporating Popper's empiricism, we can only applaud this change and his rejection of instrumentalism. "It makes no good sense to speak of modified instrumentalism, since the bold idea that science is merely instrumental for technology with the slightest modification becomes rather trite."[264] If one is to accept Popper's view of empirical testing, one must also accept his falsificationism to be consistent.

Falsificationism is not inconsistent with the hope or objective of practical application. We believe that falsificationism and applied science are complements. Without well-tested explanatory theories concerning "what is," practical application is not likely to be "useful." Without the prospect of practical application, the search for truth is likely to grow sterile.

We do not wish to imply that the desire for practical value is somehow wrong or misconceived but only that without "knowledge" practical application is empty and self-defeating. We see no reason to criticize an instrumental view of engineering or "art." In fact, such a view is at least partially necessary to apply scientific knowledge to the everyday world and to develop new technology. However, "art" can have no value without "knowledge." We only quarrel with the substitution or replacement of falsificationism by instrumentalism as the methodology of "pure" science.

If Friedman's methodology is interpreted as instrumentalism, then it inherits all these criticisms. What is Friedman's particular version of instrumentalism? Since interpretations can differ, we shall use Boland as our guide to Friedman's instrumentalism. This choice is dictated by the fact that Boland states the most defensible version of Friedman's methodology and that Friedman, himself, does not directly state his instrumentalism.

From Boland's paper, we may identify Friedman's instrumentalism as composed of the following theses.

1. Theories are chosen for their "predictive success."

   > Appreciating the success orientation of Friedman's view is essential to an understanding of his methodological judgments. For Friedman, an instrumentalist, hypotheses are chosen because they are successful in yielding true predictions. . . . It is his assumption that there has been a prior application of *modus tollens* (by evolution, see Friedman, 1966, p. 22) which eliminates unsuccessful hypotheses (ones that yield false predictions), and which allows one to face only the problem of choosing between successful hypotheses.[265]

2. Empirical testing is application for the purpose of verification.

   > Friedman explicitly rejects the necessity of requiring the "testing" of substantive hypotheses before they are used simply because it is not possible. . . . Throughout his essay "testing" always

means "testing for truth (in some sense)." It never means "testing in order to reject," as most of his critics seem to presume. That is, for Friedman a successful test is one which shows a statement (e.g., an assumption, hypotheses, or theory) to be true. . .[266]

3. The past success of a theory supports its future success.

   Finally he unnecessarily adds the false conventionalist theory of confirmation that says that the absence of refutation supports the (future) truth of a statement.[267]

4. The purpose of theory is only policy application.

   Namely, that theories are only constructed to be instruments of policy. Those economists who do not see policy application as the only purpose of theorizing can clearly argue with that judgment.[268]

5. Theory choice for application is a matter of judgment.

   Basically Friedman's solution (to the problem of induction) is that our acceptance of a hypothesis for the purposes of policy application should be made a matter of "judgment." . . . Friedman notes that the problem of choosing models can be seen as a problem of explaining when the model is applicable. To solve the latter version of this problem, he says that to any model of a theory or hypothesis one must add "rules for using the model." These required rules, however, are not mechanical. He says that "no matter how successful [one is in explicitly stating the rules] . . . there inevitably will remain room for judgment in applying the rules." Unfortunately, the "capacity to judge" cannot be taught, as each case is different. . . . However it can be learned, "but only by experience and exposure in the 'right' scientific atmosphere". . . This seems to bring us back to the inductive problem that his version of instrumentalism was intended to solve.[269]

6. "Finally, and most importantly, I think it essential to realize that instrumentalism is solely concerned with (immediate) practical success."[270]

The above clearly reveals that Friedman's instrumentalism is a methodology of practical application and not a methodology for learning "what is." We can find nothing in this instrumentalism that can produce "knowledge," in our sense. It is but a method for "art" or technology.

Clearly Friedman's instrumentalism is even more extreme than the general form of instrumentalism. As such, Friedman's MPE is 'rationally refuted' by our general argument against instrumentalism:

(1) "Predictive success" is an inadequate regulative principle, since it requires the immutability of the facts and a valid method of induction.

(2) Therefore, instrumentalism has no basis upon which to "predict" the success of an accepted theory.

(3) It cannot produce practical utility.

(4) That is, any claim of "confidence," "success," or practical value from a purely instrumental science presupposes the possession of the same knowledge that science seeks to discover and instrumentalism is unable to produce.

(5) The teleological nature of instrumentalism makes it all too easy for anyone to insert his own values or beliefs.

Thus falls instrumentalism in the "vacuum" of its own "success."

What then is left in Friedman's instrumentalism to choose theories and to support their application? Only judgment and past applications (recall (2) and (5) above) remain. While it may be true that theory choice is, at the bottom, a matter of judgment (in some sense), it is not useful to leave this choice to some unspecified "judgment." Proper methodology must distinguish methodological judgments (those concerning the use of logic, legitimate sources of empirical data, the choice of the statistical level of significance . . .) from the more general notion of judgment which includes values and beliefs. Friedman makes no such distinction, and the reference to learning proper judgment in the "right scientific atmosphere" provides no clarification.[271] Friedman's logic is only circular and empty. To refer to the "right scientific atmosphere" merely begs the original question of methodological judgment by generating further questions concerning what is "right" and who decides. If anything, it appears to us that Friedman's discussion reduces the potentially "objective" notion of methodological judgments to personal and political ones.

A methodology of science or its application which solves the problem of choosing theories or predictions for application by reference to unspecified judgment is not saying anything. It is thus empty. At the least, one has every right to demand that an adequate methodology posit some methodological rules or conventions for such choices. For example, one might wish to "apply" Popper's falsificationism to "art." In such a case, it would be reasonable to assert the methodological rules that only the best 'corroborated' theories be applied and that 'falsified' theories can be used only if the application in question is within the range for which the 'falsified' theory is 'well-corroborated.' Such methodological rules give guidance to the practitioner and restrict his choice of applications.

In contrast, Friedman provides no restrictions nor any real guidance for science or its application. He only says that: "The gains from greater accuracy, which depend on the purpose in mind, must be balanced against the costs of achieving it."[272] Alright, but how is one to apply this principle? It is a license for "anything goes." There is nothing in this principle, or the rest of Friedman's essay, which restricts how one defines the "purpose in his mind" or how one measures the costs. In fact, this principle can provide an excuse for the "worst practice technology." If some specific application is questioned, the defender can merely assert that the cost of some alternative procedure exceeds its benefits for the "purpose in mind." From such a rationalization the well-intentioned critic has little recourse. How can one 'rationally criticize' the defender's unspecified "judgment" or his "costs" other than by being swept into a debate over values and opinions?

Furthermore, Friedman's essay contains a number of bad examples of "judgment" in stating and applying theories. For example, Friedman's discussion of Galileo's law sets a poor precedent in choosing theories for application. His discussion clearly indicates that it is his judgment that Galileo's law is suitable for application. Since no scientific theory is more 'falsified' or less accurate relative to its well-known alternatives, one might wonder for what purposes is it suitable? We cannot see any. In choosing to apply either Galileo's or Newton's theory, there is only a <u>small</u> difference in the computational ease, a difference which cannot be reasonably considered a significant cost. Yet there are always forgone benefits from lost accuracy when one applies Galileo's law.

The same is true when considering whether to incorporate "friction" into the prediction. The difficulty of computation is not what needs to be minimized. Such considerations are completely trivial to applied science except in extreme cases. Instead, it is the <u>opportunity costs</u> of the chosen application which need to be minimized. Are not the forgone benefits the largest component of the cost of application? To mention the obvious consideration of cost in practical application can serve no useful function other than to provide solace to poor practice.

In the evaluation of methodology, it is necessary to consider its potential abuse. A major theme to Popper's choice of methodological rules is to prohibit or restrict the anticritical tactics of conventionalism in order to permit the growth of knowledge through refutation. What is there in Friedman's instrumentalism which so limits or prohibits abuse? We can find nothing. His references to unspecified and confounding "judgment" cannot fulfill this need. And, it does no good to suggest that such "judgments" can only be learned in the "right scientific atmosphere."

Yet, all of Friedman's methodological regulations and limitations are contained in the unspecified concepts - "judgment," "predictive success," "cost," "purpose in mind," or, in general, "utility." In disciplines known to engage in normative disputes often disguised as "scientific," it is highly dangerous to choose theories or applications solely upon teleological criteria that have practically unavoidable normative connotations. If some economist seeks personal gain or to promote his own personal political philosophy, what is there in Friedman's instrumentalism to prohibit or, at least, restrict his use of applied economics? Could he not always find some way to make a prediction that supports his normative views?

By itself, applied economic methodology allows its users virtually unlimited degrees of freedom in "adjusting" the specification of their "models." When Friedman's teleological criteria are added to such a "free trade" methodology, "anything goes." Even if we impose the added restriction that all economic applications must be based upon some "accepted" economic hypothesis, we are no further along. For, as Friedman tells us, the "maximization-of-returns" hypothesis is "accepted," and it can be used to derive <u>any</u> "prediction" or application. This follows from the fact that Friedman does not sufficiently specify the "maximization-of-returns" hypothesis, allowing one to derive any prediction he desires by merely filling in the blank "objective function" and by adding sufficient "constraints."

Since Friedman's instrumentalism fails to supply any limits to application, it is reasonable to wonder how will the actual choices be made. When only "judgment" arbitrates choice, it is as likely to be determined by "normative" considerations of personal values or political philosophy as not. While it is true that applied economics, "art," is necessarily dependent upon "normative" or ethical ends, as J. N. Keynes recognized, the proper role of methodology is to minimize the dependence upon opinion and personal values.

Thus far, we have been unable to find any reasonable way to choose theories or to engender application with practical value by using Friedman's instrumentalism. Is there a way out of this dilemma? Might not "predictive success" supply the necessary substance to Friedman's "judgments"? After all, is not "predictive success" empirical support? Here, too, Friedman fails to supply "predictive success" with the necessary substance to make it useful. Friedman's instrumentalism does not provide any reasonable means for appraising "predictive success." As Boland asserts, Friedman assumes that choice is restricted to "successful" hypotheses (recall quotation 265). However, this leaves the most important question to an instrumental science unanswered.

In order to use "predictive success" as an 'objective' criterion of theory choice or application, one must have answers to the following questions. How can one decide whether the predictions of a given theory are "successful"? How much support is provided by an additional "successful prediction"? How can two "successful" hypotheses be relatively assessed? In our general discussion of instrumentalism, we have discussed how instrumentalism fails to give adequate answers to the first two questions. Friedman "solves" these problems by assuming them away, for he wishes only to deal with "successful" hypotheses. He further transforms the last question to one of choosing between hypotheses for purposes of application. From this perspective, specific applications are chosen by their convenience - "cost" and "accuracy" - considered relative to one's "purpose in mind" and in reference to one's "judgment." Thus, again we do not find the necessary methodological precepts for using "predictive success." Instead, choice is situationally relative and dependent on unspecified "judgments."

Friedman's only answer to the problem of identifying "successful" theories is that by evolution the "unsuccessful" are somehow eliminated. That is, by repeated application only the "successful" theories survive. However, this is absurd or, at best, wishful thinking in the context of the history or "evolution" of economic theory. Prior to Friedman's essay, economics was dominated by apriorists or conventionalists, with few exceptions. When was economic theory rejected for its "unsuccessful" predictions or what economic theory ever rested upon its "predictive success"?

The apriorists, in opposition to Friedman's methodological assertions, were primarily concerned with the "realism" of their "assumptions." It was the "assumptions" that gave substance and justification to economic theory. Predictions were not allowed to reflect negatively upon theory (recall section 4.1), for there are always "disturbing causes" operating in the actual economy. A "predictive failure" was seen more as the failure of the applied economist to properly account for the disturbing influences than as a reflection of the merit of the theory. With such methodological views, how can anyone believe that "surviving" theories will tend to be more "successful" in prediction? Nor do the conventionalists with their ability to reinterpret the evidence and other anticritical strategems provide a more reasonable mechanism for the "survival of the fittest."

The fact that certain economic theories have survived does not, in itself, lend any support to a claim of their "predictive success." It has only been in relatively recent times that prediction carried any weight, and then one would be hard pressed to demonstrate that the lack of "predictive success" was ever responsible for the "death" or "fall from fashion" of an economic theory. The tenacity of economic theories is much more adequately explained by the dogmatism and absence of empiricism of economic methodology than by the "success" of their predictions.

Evolution is neither necessary nor sufficient for the identification of a theory's "success." It cannot solve the problem of finding "successful" theories. Thus, Friedman's instrumentalism fails to give substance or a means to apply its most important notion, "predictive success." Again, we find Friedman's instrumentalism empty. There is no need to "refute" Friedman's methodology, for it is empty.

In summary, Friedman's instrumentalism is an inadequate methodology. It is susceptible to all of the general criticisms of instrumentalism, and furthermore it suffers from its own weaknesses. The ideas that past "success" lends support for future "success" or that verification imbues support are untenable theses. Neither tradition nor evolution can reasonably be assumed to provide the necessary basis for useful application. While Friedman's instrumentalism depends crucially upon "judgment" and "predictive success," neither are sufficiently specified or restricted to be applied or to provide a methodological basis. The use of verification, evolution, or statistical correlation simply does not "work" - to use the instrumentalists' term. Applications based upon these principles are as likely to be "costly" as "useful." An instrumental methodology that cannot give its applications the practical value that it demands is inadequate on its own terms. Such a methodology is empty from its own point of view and in general. Friedman's methodology simply fails to make the necessary restrictions to provide substance. Friedman's instrumentalism is inadequate not so much because it is "wrong" but because it is empty and fails to solve any of the problems it poses. Friedman's instrumentalism need not be refuted, it need only be ignored.

Yet few economists, if any, have recognized that the view which can only consider practical utility is incapable of producing it. As we have discussed, the completely pragmatic view of instrumentalism cannot produce the type of scientific knowledge that practical application requires. We conjecture that the reluctance of economists over the past century to deny the practicality of their field has caused them to be complacent towards reductions of economic inquiry to pragmatism. But such a reduction is unnecessary for practical application.

Our view is one of "roundabout production." The most efficient means of producing the practical utility which the pragmatists seek is to first produce the necessary intellectual capital, scientific knowledge. Then together, capital and labor - or scientific knowledge and "art" - can more effectively satisfy society's demand for practical value. Since such a technology "works" for the economy, why should it not "work" for economics?

It is interesting to note that Boland all but sees the inherent emptiness of instrumentalism. "It has been argued in this paper that Friedman's essay is an instrumentalist defense of instrumentalism. . . . The repeated attempts to refute Friedman's methodology have failed, I think, because instrumentalism is its own defense and its *only* defense."[273] What Boland fails to say is that if instrumentalism is its *only* defense, as he asserts, then it is an empty philosophical or methodological position.

This is one of the central difficulties of instrumentalism. It is based upon pragmatism; it seeks practical "success"; and it has eyes only for practical "success." Its only justification is based on an affirmative answer to the question: Is it not practical to adopt a methodology that is concerned only with practical application? While many would acquiesce to such a justification, it is false. Instrumentalism involves only a circular system of thought. Practical value requires some external substantiation and an injection of knowledge. The mere assertion that instrumentalism is a useful means of producing practical value is itself not useful. Though consistent, the framework of instrumentalism is empty, and we must look elsewhere for a viable economic methodology.

Instrumentalists view theories as financial instruments which possess right to cash (predictions) under stipulated conditions. Since Friedman is unable to specify any restrictions for either his methodology or his theories (e.g., the "maximization-of-returns" hypothesis), his instrumentalism is a blank check. It is as if Friedman has given his power of attorney to anyone who wishes to draft a blank check for the purpose of "cashing in" predictions. Unfortunately, this is one of the meanings of "*successful*" prediction.

What then is Friedman's instrumentalism? Friedman's instrumentalism is to methodology as the "maximization-of-returns" hypothesis is to science. The "maximization-of-returns" hypothesis has no content; it can be applied in any manner one chooses; and, by default, it has become a mere methodological convention rather than the scientific explanation that it claims to be. Likewise, Friedman's instrumentalism has no content, for it fails to limit the practice of science and can thus be applied in any manner one chooses. By default, then, Friedman's instrumentalism is not methodology, at all. Instead, it is some type of philosophical or metaphysical system of beliefs.

At that level, Friedman's instrumentalism is little more than philosophical pragmatism. To fulfill Friedman's wish for policy application, one must then develop a means (methodology) of producing the knowledge that policy demands. Thus, we have come full circle, back to the beginning of methodological inquiry. Here we find Popper's falsificationism as the best means of producing the knowledge that instrumentalism requires. There simply are no better alternatives to falsificationism for the production of knowledge or the "practical success" of that knowledge.

What does our discussion of Friedman's essay accomplish? We have provided a robust criticism of Friedman's methodology. No matter how one interprets Friedman's position, we have shown it inadequate. What are the central conclusions of this exercise? To the extent that Friedman's methodology promotes empiricism and a greater synthesis of economic theory and empirical evidence, it serves a useful purpose. However, Friedman's brand of empiricism is but the tip of an iceberg. The growth of economic knowledge will not truly begin until Popper's falsificationism is employed. Beyond Friedman's advocacy of some type of empiricism, his methodology has nothing to offer and should be ignored.

What opinion have we formed concerning the proper label for Friedman's methodology? We agree that Friedman's essay is best unified and most favorably interpreted as instrumentalism. However, we do not think that this label completely describes the methodological position expressed in Friedman's essay.

There are too many extraneous theses and comments in Friedman's essay for him to be reflecting only instrumentalism. Even the strongest advocate of the instrumentalist interpretation, Boland, finds significant aspects of conventionalism and inductivism in Friedman's essay. Our general interpretation of the morass which surrounds Friedman's essay is that Friedman's methodology is eclecticism. We believe that Friedman proposes an eclectic methodology in an effort to persuade us of his views on economic policy issues. This interpretation can explain the many parts of different methodologies one finds in Friedman's essay, the diverse interpretations of Friedman's methodology in the literature, the preeminence that Friedman gives to policy applications, his use of the disjunctive form of argument, the apparent subjectivism of his essay, and his concern about "consensus" and "acceptance."

Furthermore, we believe that Friedman's eclecticism is demonstrated by its inconsistency. His "methodology of positive economics" as instrumentalism is inconsistent with his acceptance of Keynes's distinctions among "art," "positive science," and "normative science." Whenever one eclectically chooses a methodological or philosophical system for some external reasons, it is almost certain to contain a fundamental inconsistency. Like Lakatos' methodology of scientific research programs, Inconsistency is the fate of Friedman's methodology of positive economics.

4.5 Summing Up: Aggregate Economic Methodology

In the aggregate, economics is still nonempirical and irrefutable. The normal practice of economic science is to state and hold the traditional "Fundamental Assumptions" in a manner that deflects criticism; to add, with each new investigation, all manner of ad hoc auxiliary hypotheses; and to report any type of empirical support for traditional economic theory while ignoring the preponderance of "negative" empirical evidence. At best, an auxiliary hypothesis is discarded from time to time when the empirical evidence is overwhelming. Such activity does not seem worthy of the title empirical science. We see the neoclassical research programme as "degenerating," to use Lakatos' phrase. While it is beyond the scope of this thesis to make an irreproachable argument for the above appraisal, it is obvious that economics is not falsificationistic and that economists fail to use 'refutation' to its best advantage.

Has economic methodology progressed? Almost one hundred years ago, Keynes characterized the principal economic method as "positive, abstract, and deductive" (recall quotation 18). This "deductive method" is casually empirical in "deriving" its assumptions and empirically dogmatic about its implications. Whenever economic "facts" seemed to be consistent with a given deductive theory, those facts "counted" and gave support to the economic theory. Whenever these facts contradicted economic theory, those "facts" were "discounted" as "disturbing causes" or attributed to "bad art" (recall 4.1). While Keynes is insistent that the deductive method is empirical, it is an empiricism that refuses to learn.

The twentieth century apriorism of Lord Robbins and von Mises is even more dogmatic than its predecessors. In theory and in method, there is no significant difference between these economic methodologies. Robbins and von Mises distinguish themselves mostly by the clarity and forthrightness of their dogmatism.

The first real advocacy of empiricism is made by Hutchison. He uses Popper's demarcation criterion and logical positivism to criticize economic apriorism. As a criticism of the empirical dogmatism of economics, Hutchison's words still ring true. However, the linguistic appraisal of economic statements as empirical or tautological does not a methodology make. Because Hutchison's original criticisms were a bit too narrow and naive, economists withdrew into conventionalism or instrumentalism and avoided making the substantive changes that falsificationism requires.

Some see Friedman's methodology as a turn towards falsificationism. Yet the "methodology of positive economics" (instrumentalism) is nearly the opposite of falsificationism. Friedman's methodology is as empirically dogmatic as nineteenth century economic methodology. In preceding section 4.4, the "methodology of positive economics" is clearly shown to be inadequate for economic science or the growth of its knowledge.

What then is the status of economic methodology? We can find no economic methodology that can be deemed adequate, that makes the best use of empirical evidence, or that is capable of advancing the growth of economic knowledge. Throughout the history of economics one sees the use of defensive strategems, casual empiricism, and dogmatism about certain "Fundamental Assumptions."

From our study of economic methodology, a strange theme emerges. It seems that empirical inquiry has always been seen as something separate from economics proper - where economic theory and knowledge reside. The classical British economists saw all empirical study as part of the "art" or "applied" economics. If empirical evidence appeared to refute economic theory, the inability of the applied economists in properly allowing for "disturbing causes" or in specifying the ceteris paribus clause would then be questioned - not the "positive" economic theory. In that way, economic theory could always be "positively" defended. "Disconfirmations" are dismissed as "bad art," while "verifications" are exalted as demonstrating the validity or applicability of economic theory.

Such empirical naivete of the nineteenth century might easily be excused, but this biased empiricism remains. Although it is easy to point to improvements in our empirical techniques (e.g., statistical hypothesis testing, regression analysis, computer assisted data analysis, . . .), it would be extremely difficult to show any substantive change in the methodological relationship between economic theory and fact. Are not empirical investigations currently considered "applied economics"? Whenever the principal economic theories are applied to specific cases or, in other words, used to derive empirical implications, this activity is seen as part of "applied economics" and as something different from economic theory.

Friedman's instrumentalism takes this separation of theory and fact to its absurd limit. If taken seriously, Friedman's instrumentalism reduces economic science to "art," and his "methodology of positive economics" becomes a methodology for the "art" of political economy. To

Friedman, only the successful application of economic theory matters, and the study of "what is" vanishes. However, Friedman's instrumentalism involves the same empirical inversion as does nineteenth century methodology. His instrumentalism applies the same deductive theories and regards them, in practice, as 'irrefutable.' Furthermore, only accumulations of successful applications are permitted to reflect upon theory; thus, Friedman preserves the bias towards verification. Perhaps the real "F-twist" is Friedman's twist of all empirical economics into "art" and orthodox economic theory into methodology.

The "methodology of positive economics" provides a framework in which to view empirical investigations as applications of theory. Such a methodological perspective would quite naturally lead to the impression that empirical criticism is "a poor carpenter who blames his tools."[274] When theory is regarded as methodology (or crudely, as a "tool box"), then empirical "disconfirmations" reflect only upon the practitioner.

Such a view eliminates useful empirical feedback. It seems that economics, as practiced, is as Lakatos describes - that is, economics consists of a "hard core" of 'irrefutable' propositions surrounded by a "protective belt" of auxiliary hypotheses.

The absurdity of Friedman's instrumentalism becomes particularly apparent when compared to Keynes's methodological observations. Keynes recognized that applied economics needs knowledge of both "what is" and "what should be" (recall 4.1). By reducing empirical economics to "art," Friedman cuts off the "positive" knowledge that practical application requires. Furthermore, Friedman's desire for consensus causes him to ignore the other necessary type of knowledge for application, "normative" knowledge concerning "what should be." It appears that

Friedman turns Keynes's optimistic appraisal of applied economics upon its head. "It may be added that although in the past there may have been a tendency with a certain school of economists to attempt the solution of practical economic questions without adequate recognition of their ethical aspects, there is, at the present time, no such tendency discernible amongst economists who have any claim to speak with authority."[275]

Friedman's authority and his "methodology of positive economics" "twists" Keynes's three branches of economics into just one, the "art" of political economy. Yet without knowledge of "what is" and "what should be" applied economics cannot solve any practical problems. Thus, Friedman's instrumentalism makes empty the study of "positive economics," even in its practical aspect. It is unable to discover the types of knowledge that practical application demands or to learn from its mistakes. Hence, Friedman's "methodology of positive economics" is no real improvement over the nineteenth century practice.

What has this thesis accomplished? Have we succeeded in establishing a rock-hard foundation for economics? Do we have an unfailing method for the production of knowledge? Must all economic inquiry follow our prescription? While the answers to all these latter questions are negative, this thesis makes some contribution to economic thought.

In general, we have provided a critical analysis of the philosophy of science and the methodology of economics. From the philosophy of science, one view emerges as the most promising in identifying scientific knowledge and in promoting its growth - Popper's falsificationism. Although there are no guarantees, scientific knowledge and methodological decisions remain fallible, falsificationism is the best technology for the production of economic knowledge.

Our general perspective is optimistic. We believe that economic knowledge will show a substantial growth when economists become more explicitly aware of proper methodology and adopt Popper's falsificationism. We do not insist that all economic inquiry adopt our epistemological framework; there will always remain room for the development and refinement of "pure" theory, analytical tools, normative understanding, and the "art" of political economy. Yet, there should be a little room left within the political economy for the rigorous search for truth that we advocate. Without an earnest attempt to discover "what is" - the actual interrelationships among economic phenomena - the other aspects of economic inquiry are likely to grow stale.

More specifically, insights to many economic theories and problems are given throughout our discussions - the most important of which concerns the understanding that economists have chosen to view "maximization" as methodology. Finally, this thesis is a comprehensive criticism of Friedman's "methodology of positive economics," a response to Boland's "critique of Friedman's critics," and a resolution of these "assumption debates."

While there is not certitude nor demonstrative knowledge, knowledge can grow. The application of rational criticism, critical fallibilism, and methodological falsificationism to economics can advance the growth of economic knowledge. What more can be asked?

Notes

[1] Blaug [1980], p. 55.

[2] Keynes, J. N. [1955], p. 1.

[3] *Ibid.*, p. 2.

[4] *Ibid.*, pp. 34-35.

[5] *Ibid.*, p. 34, n. 1.

[6] Senior [1966b], pp. 18-19 and 23-27. Blaug [1980], p. 59, also attributes this distinction to Senior.

[7] *Ibid.*, p. 18.

[8] *Ibid.*, pp. 18-19

[9] *Ibid.*, p. 63.

[10] Keynes, J. N. [1955], pp. 37-46.

[11] *Ibid.*, p. 47.

[12] Senior [1966b], p. 18.

[13] Keynes, J. N. [1955], p. 60.

[14] *Ibid.*, p. 57.

[15] *Ibid.*, pp. 60-61.

[16] *Ibid.*, pp. 61-62.

[17] *Ibid.*, pp. 63-65.

[18] *Ibid.*, pp. 9-10.

[19] Blaug [1980], p. 82, gives a similar interpretation of Keynes's position.

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

[21] *Ibid.*, p. 13.

[22] *Ibid.*, p. 14.

[23] *Ibid.*, pp. 15-16.

[24] *Ibid.*, p. 16 and n. 1.

[25] *Ibid.*, pp. 230-231.

[26] *Ibid.*, pp. 15-16.

[27] See Senior [1966b], pp. 62-67.

[28] Senior [1966a], pp. 10-11.

[29] See Blaug [1980], pp. 60-62.

[30] Mill [1967], pp. 321-325. Also quoted in Senior [1966b], pp. 57-61.

[31] See Cairnes [1965], p. 68.

[32] Keynes [1955], p. 88.

[33] *Ibid.*, p. 89.

[34] Cairnes [1965], pp. 95-100.

[35] *Ibid.*, p. 87.

[36] *Ibid.*, p. 103, n.

[37] *Ibid.*, pp. 92-94.

[38] Keynes [1955], p. 227.

[39] *Ibid.*, p. 228.

[40] *Ibid.*, p. 229.

[41] Mini [1974], pp. 14-127.

[42] *Ibid.*, p. 53.

[43] Keynes [1955], p. 230.

[44] *Ibid.*, p. 231.

[45] *Ibid.*, pp. 233-234.

[46] *Ibid.*, p. 234, n. 1.

[47] Changes in the mathematical forms of a theory are not changes in the 'theory.' It is merely a translation of symbols. Usually such theory translations create only technical "puzzles," but leave the

explanation and problem-situation unchanged. Our question in the text is not meant to imply that these economic theories are somehow "wrong," although that is a possibility. We are only suggesting that the dogmatism of the deductive method is a sufficient explanation of the tenacity of many economic theories, at least for the period up to the 1940's when more empirical methodologies began to take hold. It is unlikely that theories, whether "right" or "wrong," will be changed by a methodology that has no means to incorporate or promote change and, thus, growth.

[48] Keynes [1955], pp. 225-226.

[49] Hofstadter [1980], pp. 183-187.

[50] See Blaug [1980], pp. 73-77, for an evaluation of the practice of classical methodology.

[51] Robbins' first chapter is completely concerned with the definition of economics and the remainder of his essay is usually couched in terms of definitions.

[52] Robbins [1962], pp. 4-5.

[53] See, for example, Robbins [1962], pp. 4-5, 16-17, and 21, n. 1. The essentialism of Robbins will be discussed shortly.

[54] See our discussion of logical positivism, section 3.3.5, and its problem of meaning. Also see Popper [1963], pp. 19-21, and Popper [1972], pp. 119-125.

[55] Ibid., pp. 78-79.

[56] Ibid., p. 76. This quotation refers explicitly only to the assumption of preferences in the theory of value. Yet it is not inappropriate to use this statement as characteristic of Robbins' view of the "facts."

[57] Ibid., pp. 116-118.

[58] *Ibid.*, p. 117.

[59] *Ibid.*, p. 120.

[60] *Ibid.*, p. 106.

[61] See *Ibid.*, pp. 75-76.

[62] *Ibid.*, pp. 104-105.

[63] *Ibid.*, p. 111. Notice there how the following is used to support his argument. "The fact that we *can* arrange our preferences in an order is a fact of so much greater generality ..." [Emphasis added.]

[64] Robbins attributes his argument concerning "electrons" to Cairnes; see *Ibid.*, p. 111. In the next chapter we shall point out that Friedman, too, makes this mistake. A complete accounting of the economists that have made similar arguments would include the majority of economists that have addressed methodological issues.

[65] *Ibid.*, p. 105.

[66] See Robbins [1971], p. 149.

[67] See Popper [1062], pp. 18-21, 103-107, and 114-119, for a brief outline of Popper's criticism of essentialism.

[68] *Ibid.*, p. 104.

[69] *Ibid.*, pp. 105-107.

[70] *Ibid.*, pp. 19-21.

[71] Robbins [1962], pp. 147-151.

[72] *Ibid.*, pp. 142-143.

[73] See the "Preface to the Second Edition" of Robbins [1962] for an outline of this criticism and Robbins' response.

[74] *Ibid.*

[75] *Ibid.*, pp. 54-59.

[76] *Ibid.*, pp. 56-57.

[77] _Ibid_.

[78] Robbins [1971], pp. 149-150.

[79] Von Mises [1960], pp. 12-13.

[80] _Ibid_., p. 25.

[81] _Ibid_., pp. 27-28.

[82] _Ibid_., p. 28 and pp. 9-10.

[83] _Ibid_., p. 30.

[84] _Ibid_.

[85] _Ibid_.

[86] For evidence of this assertion notice the following quotation.

[87] _Ibid_., p. 25-27.

[88] _Ibid_., p. 17.

[89] _Ibid_., pp. 17-22.

[90] _Ibid_., pp. 15-16.

[91] _Ibid_., p. 16.

[92] Hutchison [1960], p. 9.

[93] This point will soon be made clearer.

[94] For examples, see Hutchison [1974] and Preface to Hutchison [1960].

[95] _Ibid_., p. 19, n. 8.

[96] _Ibid_., p. 13.

[97] _Ibid_., p. 18.

[98] _Ibid_., pp. 26-27.

[99] Popper [1959], pp. 81-82.

[100] Hutchison [1960], p. 19, n. 8.

[101] _Ibid_., pp. 19-20, n. 4-14; pp. 46-48, n. 1-19. Of particular interest is p. 19, n. 8 and p. 46, n. 5 and 6. All these are references

to the logical positivists on issues of demarcation, while Popper is not referenced at all, on this issue. Popper is referenced only later for one of his more popularizing phrases and then somewhat out of context; p. 35, and p. 48, n. 19.

[102] See Kraft [1974] and our discussion of the "Popper Legend" in our section on logical positivism.

[103] Hutchison [1960], p. 161.

[104] *Ibid.*, p. 23.

[105] It should be noted that diminishing marginal returns can have empirical content if "maximization" is further postulated. Then if an increase in marginal product is observed, it would "refute" this combined theoretical system. Yet even in this case, the common economic practice would reduce this system to a second-order tautology. If marginal product is observed to increase, would not someone assert that technology has also increased, or, equivalently, that the production function has "shifted"? This last assertion is made more clear in the following paragraphs.

[106] Hutchison [1960], pp. 40-46.

[107] *Ibid.*, p. XVII.

[108] *Ibid.*, p. 162.

[109] *Ibid.*, pp. 36-40.

[110] *Ibid.*, pp. 36-37.

[111] *Ibid.*, p. 40.

[112] *Ibid.*, pp. 136-137.

[113] *Ibid.*, p. 132.

[114] *Ibid.*, pp. 142-143.

[115] *Ibid.*, p. 143 and p. 148.

116 Machlup [1955], pp. 7-8.

117 Hutchison [1960], p. 146.

118 *Ibid.*, pp. 143-153.

119 Or at least he *was* optimistic; see Hutchison [1974].

120 Hutchison [1960], p. 53.

121 *Ibid.*, p. 54. That Hutchison considers Robbins' definition of economics as one of these "authoritative definitions" is unquestionably stated in the interviewing text.

122 See Hutchison [1960], pp. 83-85.

123 Friedman [1953], p. 21.

124 Hutchison [1960], pp. 85-87, quoted in reverse order.

125 *Ibid.*, pp. 162-163.

126 *Ibid.*, p. 113.

127 The main battlefield of the debate is captured in Lester [1946], Lester [1947], Machlup [1946], Machlup [1947], Stigler [1946], and Stigler [1947] as one of the disputants claims. See Machlup [1967], p. 1.

128 See Machlup [1967].

129 *Ibid.*, p. 9.

130 Hutchison [1960], p. 111.

131 *Ibid.*, p. 163.

132 *Ibid.*, pp. 97-99.

133 *Ibid.*, p. 97.

134 *Ibid.*, pp. 98-99.

135 *Ibid.*, p. 124, n. 35.

136 *Ibid.*, p. 95.

137 Ibid., p. 96.

[138] *Ibid.*, pp. 118-120 and intervening text.

[139] *Ibid.*, pp. 115-116.

[140] *Ibid.*, p. 120.

[141] Robbins [1962], p. 73.

[142] *Ibid.*

[143] *Ibid.*, p. 75.

[144] *Ibid.*, p. 76.

[145] See Blaug [1980], pp. 99-103, for a brief discussion of Samuelson's operationalism.

[146] Samuelson [1965], p. 4.

[147] *Ibid.*, p. 84.

[148] *Ibid.*, pp. 91-92 and n. 3.

[149] *Ibid.*, p. 7.

[150] See Samuelson [1963] for his criticism of Friedman's methodology.

[151] See Blaug [1980], pp. 209-215, for a discussion of Samuelson's anti-empirical trade theory.

[152] Also see Blaug [1980], pp. 187-192, for a brief review of the content of general equilibrium theory.

[153] Samuelson's descriptivism is discussed in Wong [1973].

[154] See Latsis [1974], pp. 9-12, for a brief outline of the defensive nature of Machlup's methodology. The subject of Machlup [1975] and the first essay of Machlup [1978] are entirely concerned with words.

[155] See Machlup [1978], pp. 145-149 and 495-499.

[156] Blaug [1980], pp. 127-128.

[157] Boland [1979], p. 503.

[158] Verificationism is one of the types of probablism (recall 3.3.6). Verificationists believe that a theory gains credibility as

confirmations accumulate.

159 Blaug [1978], p. 703.

160 Blaug [1974], p. 107.

161 Wong [1973], p. 314.

162 DiMarchi [1974], p. 107.

163 Boland [1979], p. 518.

164 See Blaug [1980], pp. 104-120, particularly pp. 113, 119, and 223.

165 Boland [1979], pp. 520-522.

166 For example, Hollis and Nell [1975].

167 Friedman [1966], p. 3.

168 Ibid., p. 4.

169 Recall the discussion of Keynes's view on this subject in section 4.1. That Friedman also permits "positive" theories to incorporate normative and ethical motivations will be shortly discussed and is clearly seen on p. 5, Friedman [1966].

170 Ibid.

171 Ibid.

172 Ibid.

173 Ibid., pp. 4-5.

174 Ibid., p. 5.

175 Of tangential interest is that Friedman offers a footnote to support his opinion (see Friedman [1966], p. 5, n. 3). Here, Friedman seems to use the "indeterminacy principle" and Gödel's theorem as evidence of a similar subjectivity in physics and mathematics. That Friedman mentions such deep issues of the philosophy of mathematics and physics is commendable, but this reference is so superficial that it

contributes nothing to the methodological issue being discussed. Neither the "uncertainty principle" nor the "undecidability" of formal systems necessarily imply any subjective quality to physics or mathematics. In fact, Popper has spent his career showing that quantum mechanics can be consistently interpreted as an 'objective' theory having nothing to do with the second world of the "observer" (for example, see Popper [1974a], pp. 71-77). While Gödel's theorems are related to the "paradoxes" of self-reference in formal systems, it would be quite hasty to infer that logic is therefore less 'objective' or somehow "subjective." These issues which Friedman mentions have nothing directly to do with subjectivity. Instead, they more directly indicate the fallibility of our knowledge and its potential limitations.

176 Friedman [1966], p. 5.

177 To understand Friedman, the reader should keep in the back of his mind our thesis that all of Friedman's economics is "art." This thesis explains many of the discrepancies between our view of science and the assertions made by Friedman. For the moment, we shall assume that Friedman is speaking of science in our sense. For after all, he claims to give a methodology for the study of "what is." Only when we get to Friedman's instrumentalism shall we change this initial assumption.

178 Friedman [1966], pp. 5-6.

179 What are peoples' "basic values" other than their beliefs concerning morals, ethics, and perhaps religion? Although the issues of "basic values" may not be completely resolved by reason (they are at the bottom, matters of faith), there are alternatives to fighting. More is said of this example of a "positive" statement in the appendix.

180 Friedman [1966], pp. 6-7.

[181] Friedman [1966], p. 7.

[182] Ibid.

[183] Ibid.

[184] Boland [1979], p. 510.

[185] Ibid., pp. 8-9.

[186] Note that Friedman's phrase, "is to be judged," is still a normative, prescriptive statement, even though he artfully avoids the use of the word "should." This is as it needs to be, for methodology is necessarily prescriptive relative to science.

[187] Boland [1979], pp. 9-10.

[188] Boland [1979], pp. 510-511, interprets Friedman's "validity" somewhat less restrictedly. We accept his interpretation "not inconsistent with facts," but we do not find as close of an association between Friedman's "validity" and "truth" that Boland seems to imply. But, then, we see "truth" as objective correspondence with the 'facts' and Friedman's "validity" as theory that "works." The latter is more consistent with Boland's interpretation of Friedman's instrumentalism.

[189] Friedman [1966], pp. 8-9.

[190] Ibid., p. 9.

[191] Ibid., p. 23.

[192] This association of the application and the use of a hypothesis with a "test" is definitively made in Friedman's discussion of the "maximization-of-returns" hypothesis. Ibid., pp. 22-23.

[193] Klappholz and Agassi [1959], pp. 67-68.

[194] Friedman [1966], p. 23.

[195] Blaug [1980], p. 117.

[196] Friedman [1966], p. 9. Friedman adds the note: "The qualification is necessary because the 'evidence' may be internally contradictory, so there may be no hypothesis consistent with it" (n. 8). This note shows Friedman's logical incompetence and questions his understanding of epistemology. If our "evidence" or 'facts' are inconsistent, then all hypotheses are consistent with the evidence. From a contradiction, any statement may be proven true. Also we must question the wisdom of considering the possibility of "internally contradictory evidence." 'Facts,' themselves, cannot be inconsistent or contradictory. They can be so only in the context of some given 'observational theories' and/or explanatory theories. If under well-controlled conditions we sometimes observe a particular "fact" while at other times we observe a completely opposite "fact," we need not have any contradiction. Such a contradiction results only in the context of a deterministic theory that demands that the observation comes out one way or the other. Yet, a probablistic theory may be consistent with and explain these apparently "contradictory" facts. It appears that Friedman does not understand either the logical or the epistemological consequences of "internal contradictions."

[197] Ibid., p. 10.

[198] Ibid.

[199] See Chamberlin [1965] for an insightful discussion of the benefits of multiple working hypotheses.

[200] There is a possible exception to this tendency. If one uses the criterion of "fruitfulness" instead of "simplicity," then the empirical content of surviving theories may increase. Yet, in none of his examples does Friedman use "fruitfulness" and he reveals a preference for "simplicity." See the appendix for a fuller discussion of Friedman's examples.

201 Friedman [1966], p. 10.

202 *Ibid*., p. 11.

203 *Ibid*.

204 *Ibid*., pp. 8-9.

205 See Hollis and Nell [1975].

206 Blaug [1980], p. 120.

207 Recall our previous discussion (section 4.3, particularly pp. 320-323).

208 Boland [1979], p. 512.

209 Friedman [1966], pp. 8-9.

210 *Ibid*., p. 14.

211 *Ibid*., pp. 14-15.

212 See Samuleson [1963].

213 Friedman [1966], pp. 14-15.

214 *Ibid*., pp. 14-15 and 32-33.

215 *Ibid*., p. 14.

216 *Ibid*., p. 23.

217 *Ibid*., p. 28.

218 Blaug [1980], p. 108.

219 See Blaug [1980], pp. 104-111, for a similar analysis of the "irrelevance-of-the-assumptions" thesis.

220 More sophisticated versions of monetarism could allow for changes in the velocity of money. Yet for our discussion this does not matter. For then, one would be forced to assume the constancy of some other parameters of which the "realism" is of equal importance. Monetarism is not alone in its dependency upon the constancy of some "background

knowledge." All economic theory must assume the constancy of some parameters or the stability of some functional relationship to make "predictions" or to be empirically 'testable.'

[221] See Friedman, M. and Meiselman, P. [1963].

[222] Archibald [1959], pp. 64-65, and Melitz [1965], p. 42. We do not believe that this list is complete nor the best way to define "assumptions." It is merely beyond the scope of this thesis to attempt a taxonomy of "assumptions." Nor is such a study necessary to make our points about Friedman's methodology.

[223] Recall our discussion of the "Fundamental Assumption" in section 4.3. More is said about "maximization" in 4.4.4.

[224] Notice how Friedman's section, "Can a Hypothesis Be Tested by the Realism of Its Assumptions," builds up to the "maximization-of-returns" hypothesis and how Friedman attacks those who believe that the "maximization-of-returns" should be "realistic." See Friedman [1966], pp. 30-33 and 38-39.

[225] _Ibid_., p. 13. We do not wish to be overly pedantic about terms, but Friedman's repeated use of phrases like "deduced facts" can only confuse the complex methodological issues being addressed and cause misinterpretation of Friedman's "designs." In Popper's falsificationism and by our use of these terms, "deduced facts" is an oxymoron. In context, it appears that Friedman does not wish to imply anything with this, but the frequency of these ambiguous and seemingly inconsistent phrases makes a reasoned interpretation of this essay virtually impossible.

[226] _Ibid_., p. 28.

[227] _Ibid_., pp. 28-29.

[228] Boland [1979], pp. 512-513.

[229] Friedman [1966], p. 23.

[230] Ibid., p. 24. Also see p. 20 where Friedman accepts the apparent falsity (without quotes) of this "assumption."

[231] Ibid., p. 20.

[232] Ibid.

[233] Support for this interpretation of Friedman's "indirect testing" comes primarily from pp. 28-29, and it is further strengthened by Friedman's application of this principle to the "maximization-of-sunlight" hypothesis - p. 20, last sentence, first paragraph. Friedman's explication of "indirect testing" is considerably obscured by his involved example on p. 27. First of all, this example is an illustration of "the possibility of interchanging 'implications' and 'assumptions'" (p. 27). In this role Friedman's example is quite confused and "wrong," since Friedman's (b) is not equivalent to (c) nor are (a) and (d) equal (p. 27). Only after a convoluted exercise in rhetoric does Friedman get to the relationship with "indirect testing." "Suppose that the hypothesis works for the first purpose. . . . It clearly does not follow that it will work for the second purpose. . . . Yet, in absence of other evidence, the success of the hypothesis for one purpose - in explaining one class of phenomena - will give us greater confidence than we would otherwise have that it may succeed for another purpose - in explaining another class of phenomena" (pp. 28-29). Friedman clearly states that the success of one application has no connection to success for some completely different application. Yet, such considerations of logic or epistemology do not inhibit Friedman from finding increased confidence for one from the other. Again, we see Friedman's instrumental logic in

use. Even in this example, it is clear that Friedman's "indirect testing" merely concerns the use of "successful" application in one field to support the "prediction" of some separate phenomena.

[234] *Ibid.*, pp. 28-29.

[235] See the Preface of Hutchison [1960].

[236] Friedman [1966], p. 21.

[237] *Ibid.*

[238] Blaug [1980], pp. 119-120.

[239] Friedman [1966], p. 22.

[240] *Ibid.*, pp. 21-22.

[241] *Ibid.*, pp. 22-23.

[242] Blaug [1980], pp. 116-117.

[243] Friedman's knowledge of this "scarcity of knowledge" is evidenced by his added footnote concerning choice under uncertainty. Friedman [1966], p. 21, n. 16.

[244] Popper [1963], p. 234.

[245] Hutchison [1960], p. 116 and p. 126, n. 56.

[246] See Friedman [1966], pp. 34-38, and Blaug [1980], pp. 106-107 and n. 25.

[247] For a similar interpretation of "maximization" see Latsis [1974].

[248] Friedman [1966], p. 22.

[249] See Blaug [1980], pp. 114-120. Blaug clearly shows that Friedman's "evidence" is an independent theory and an alternative to the "maximization-of-returns" hypothesis.

[250] Popper [1963], p. 50

[251] Recall our introduction to Friedman's methodology. Also see Wong [1973]; Boland [1979]; Blaug [1978], p. 703; Blaug [1974], p. 107;

and Blaug [1980], pp. 104-120, particularly pp. 113, 119, and 223.

[252] Boland [1979], p. 503.

[253] Friedman [1966], p. 3. Also see section 4.4.2.

[254] Recall our quotation from Agassi [1974], quotation 218 of Chapter 3.

[255] Friedman [1966], p. 3.

[256] Recall quotations 157 and 163.

[257] Boland [1979], pp. 510-511, 513, and 515.

[258] Ibid., p. 21.

[259] Refer back to 3.3.4 for the details of this discussion. Here we will only repeat the major points and elaborate upon them.

[260] Friedman [1966], pp. 8-9 and 22-23.

[261] Ibid., and see Boland [1979], p. 513.

[262] Friedman [1966], p. 34.

[263] Recall quotation 260 from Popper and Hume in section 3.3.6.

[264] Agassi [1974], p. 693.

[265] Boland [1979], p. 511.

[266] Ibid., pp. 510-511.

[267] Ibid., p. 513.

[268] Ibid., p. 512.

[269] Ibid., pp. 510 and 514. The quotation is from Friedman [1966], p. 251.

[270] Ibid., p. 521.

[271] Friedman [1966], p. 25.

[272] Ibid., p. 17.

[273] Boland [1979], pp. 510-511.

[274] Recall Kuhn's view. See quotation 20, Chapter 3.

[275] Keynes [1955], p. 61.

BIBLIOGRAPHY

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Agassi, J. [1974]: "Modified Conventionalism is More Comprehensive Than Modified Essentialism," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Archibald, G. C. [1959]: "The State of Economic Science," The British Journal for the Philosophy of Science, pp. 58-69.

Baldwin, J. M. [1909]: Darwin and the Humanities.

Bar-Hillel, Y. [1974]: "Popper's Theory of Corroboration," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Barrett, W. [1979]: The Illusion of Technique.

Blaug, M. [1968]: Economic Theory in Retrospect, 3rd edition.

Blaug, M. [1974]: "Kuhn versus Lakatos or Paradigm versus Research Programme in the History of Economics," in S. J. Latsis (ed.): Method and Appraisal in Economics.

Blaug, M. [1980]: The Methodology of Economics: Or How Economists Explain.

Bohr, N. [1934]: Atomic Physics and the Description of Nature.

Boland, L. A. [1979]: "A Critique of Friedman's Critics," Journal of Economic Literature, 17, pp. 503-522.

Burrt, E. [1932]: The Metaphysical Foundations of Modern Physics.

Bury, R. [1969]: Outlines of Pyrrhonism.

Cairnes, J. [1965]: The Character and Logical Method of Political Economy.

Caldwell, B. [1980]: "Positivist Philosophy of Science and the Methodology of Economics," Journal of Economic Issues, 14, pp. 53=76.

Campbell, D. T. [1974]: "Evolutionary Epistemology," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Capra, F. [1975]: The Tao of Physics.

Carnap, R. [1953]: "Testability and Meaning," in H. Feigl and M. Brodbeck (eds.): Readings in the Philosophy of Science.

Chamberlin, T. C. [1965]: "The Method of Multiple Working Hypotheses," Science, 148, pp. 754-759.

Coats, A. W. [1974]: "Economics and Psychology: The Death or a Resurrection of a Research Programme," in S. J. Latsis (ed.): Method and Appraisal in Economics.

DeMarchi, N. [1974]: "Anomaly and the Development of Economics: The Case of the Leontief Paradox," in S. J. Latsis (ed.): Method and Appraisal in Economics.

Einstein, A. [1953a]: "Geometry and Experience," in H. Feigl and M. Brodbeck (eds.): Readings in the Philosophy of Science.

Einstein, A. [1953b]: "The Fundaments of Theoretical Physics," in H. Feigl and M. Brodbeck (eds.): Readings in the Philosophy of Science.

Einstein, A. [1954]: Ideas and Opinions.

Feyerabend, P. K. [1970]: "Consolations for the Specialist," in I. Lakatos and A. E. Musgrave (eds.): Criticism and the Growth of Knowledge.

Friedman, M. [1966]: "The Methodology of Positive Economics," Essays in Positive Economics.

Gödel, K. [1965]: "On Formally Undecidable Propositions of the Principia Mathematica and Related Systems," in M. David (ed.): The Undecidable.

Grumberg, E. [1966]: "The Meaning of Scope and External Boundaries of Economics," in S. Krupp (ed.): The Structure of Economic Science.

Hausman, J. A. [1978]: "Specification Tests in Econometrics," Econometrica, 46, pp. 1251-1270.

Heisenberg, W. [1958]: Physics and Philosophy.

Hicks, J. R. [1974]: "Revolutions in Economics," in S. J. Latsis (ed.): Method and Appraisal in Economics.

Hofstadter, D. [1980]: Gödel, Escher, Bach: An External Golden Braid.

Hollis, M. and Nell, E. [1975]: Rational Economic Man: A Philosophical Critique of Neo-Classical Economics.

Hume [1888]: Treatise of Human Nature.

Hutchison, T. W. [1938]: The Significance and Basic Postulates of Economic Theory, 2nd edition, 1960.

Hutchison, T. W. [1974]: "On the History and Philosophy of Science and Economics," in S. J. Latsis (ed.): Method and Appraisal in Economics.

Johnson, H. [1971]: "The Keyesian Revolution and the Monetarist Counter-Revolution," American Economic Journal, pp. 1-14.

Kant, I. [1782]: A Prolegomena to Any Future Metaphysic, P. Carees (tr.).

Keynes, J. N. [1955]: The Scope and Method of Political Economy.

Keynes, J. M. [1973]: A Treatise on Probability.

Klapphole, K. and Agassi, J. [1959]: "Methodological Prescriptions in Economics," Economica, 26, pp. 60-74.

Kmenta, J. [1971]: Elements of Econometrics.

Kraft, V. [1974]: "Popper and the Vienna Circle," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Kuhn, T. S. [1962]: The Structure of Scientific Revolutions.

Kuhn, T. S. [1970a]: "Logic of Discovery or Psychology of Research," in I. Lakatos and A. Musgrave (eds.): Criticism and the Growth of Knowledge.

Kuhn, T. S. [1970b]: "Reflections on my Critics," in I. Lakatos and A. Musgrave (eds.): Criticism and the Growth of Knowledge.

Kuhn, T. S. [1970c]: The Structure of Scientific Revolutions, 2nd edition.

Lakatos, I. [1971]: "History of Science and Its Rational Reconstruction," in R. C. Buck and R. S. Cohen (Eds.): Boston Studies in the Philosophy of Science.

Lakatos, I. [1978a]: The Methodology of Scientific Research Programmes.

Lakatos, I. [1978b]: Mathematics, Science, and Epistemology.

Latsis, S. J. [1974]: "A Research Programme in Economics," in S. J. Latsis (ed.): Method and Appraisal in Economics.

Leijonhufvud, A. [1974]: "Schools, 'Revolutions,' and Research Programmes in Economic Theory," in S. J. Latsis (ed.): Method and Appraisal in Economics.

Leontief, W. [1937]: "Implicit Theorizing: A Methodological Criticism of the Neo-Cambridge School," Quarterly Journal of Economics.

Lester, R. [1946]: "Shortcomings of Marginal Analysis for Wage-Employment Problems," American Economic Review, 36, pp. 63-82.

Lester, R. [1947]: "Marginalism, Minimum Wage, and Labor Markets," American Economic Review, 37, pp. 135-148.

Machlup, F. [1946]: "Marginal Analysis and Empirical Research," American Economic Review, 36, pp. 519-540.

Machlup, F. [1947]: "Rejoinder to an Ultramarginalist," American Economic Review, 37, pp. 148-154.

Machlup, F. [1952]: The Economics of Sellers' Competition: Model Analysis of Sellers' Conduct.

Machlup, F. [1955]: "The Problem of Verification in Economics," The Southern Economic Journal, 22, pp. 1-21.

Machlup, F. [1967]: "Theories of the Firm: Marginalist, Behavioral, Managerial," American Economic Review, 57, pp. 1-33.

Machlup, F. [1975]: Essays in Economic Semantics.

Machlup, F. [1978]: Methodology of Economics and Other Social Sciences.

Masterman, M. [1970]: "The Nature of a Paradigm," in I. Lakatos and A. Musgrave (eds.): Criticism and the Growth of Knowledge.

Mayer, T. [1980]: "Economics as a Hard Science: Realistic Goal or Wishful Thinking," Economic Inquiry, 18, pp. 165-178.

Militz, J. [1965]: "Friedman and Machlup on the Significance of Testing Economic Assumptions," Journal of Political Economy, 73, pp. 37-60.

Mill, J. S. [1967]: Collected Works, Essays on Economy and Society, vol. 4.

Mini, V. [1974]: Philosophy and Economics.

Mises, L. von. [1960]: Epistemological Problems of Economics.

Musgrave, A. [1974]: "Objectivism of Popper's Epistemology," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Nagel, E. [1961]: The Structure of Science.

Oppenheimer, J. R. [1954]: Science and Common Understanding.

Pareto. [1909]: Le Maneul d' Economie Politique.

Platt, J. [1964]: "Strong Inference," Science, 146, pp. 346-353.

Poincaré, H. [1921]: Science and Hypothesis: The Value of Science.

Poincaré, H. [1953]: "Non-Euclidean Geometries and the Non-Euclidean World," in H. Feigl and M. Brodbeck (eds.): Readings in the Philosophy of Science.

Popper, K. [1938]: "A Set of Independent Axioms," Mind, 47, pp. 275-277.

Popper, K. [1955]: "Two Alternative Axiom Systems for the Calculus of Probabilities," The British Journal of Philosophy, 6, pp. 51-57.

Popper, K. [1959]: The Logic of Scientific Discovery.

Popper, K. [1962]: The Open Society and Its Enemies.

Popper, K. [1963]: Conjectures and Refutations: The Growth of Scientific Knowledge.

Popper, K. [1970]: "Normal Science and Its Dangers," in I. Lakatos and A. Musgrave (eds.): Criticism and the Growth of Knowledge.

Popper, K. [1972]: Objective Knowledge: An Evolutionary Approach.

Popper, K. [1974a]: "Autobiography of Karl Popper," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Popper, K. [1974b]: "Replies to My Critics," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Putman, H. [1974]: "The 'Corroboration' of Theories," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Reichanbach, H. [1953]: "The Philosophical Significance of the Theory of Relativity," in H. Feigl and M. Brodbeck (eds.): Readings in the Philosophy of Science.

Rescher, N. [1973]: The Coherence Theory of Truth.

Robbins, L. [1932]: The Nature and Significance of Economic Science, 2nd edition.

Robbins, L. [1962]: An Essay on the Nature and Significance of Economic Science.

Robbins, L. [1971]: Autobiography of an Economist.

Rosser, J. B. [1939]: "An Informal Exposition of Proofs, of Gödel's Theorem and Church's Theorem," The Journal of Symbolic Logic, 4, pp. 53-60.

Rotwein, E. [1959]: "On 'The Methodology of Positive Economics,'" Quarterly Journal of Economics, 73, pp. 554-575.

Russell, B. [1953]: "On the Notion of Cause, With Application to the Free-Will Problem," in H. Feigl and M. Brodbeck (eds.): Readings in the Philosophy of Science.

Samuelson, P. A. [1963]: "Problems of Methodology: Discussion," American Economic Review, 63, pp. 313-325.

Samuelson, P. [1965]: Foundations of Economic Analysis.

Schlick, M. [1953]: "Are Natural Laws Conventions," in H. Feigl and M. Brodbeck (eds.): Readings in the Philosophy of Science.

Senior, N. [1966a]: "An Introductory Lecture on Political Economy (1827)," Selected Writings on Economics.

Senior, N. [1966b]: An Outline of the Science of Political Economy.

Settle, T. [1974]: "Induction and Probability Unfused," in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Simon, H. A. [1974]: "From Substantive to Procedural Rationality," in S. J. Latsis (ed.): Method and Appraisal in Economics.

Stigler, G. [1946]: "The Economics of Minimum Wage Legislation," American Economic Review, 36, pp. 358-365.

Stigler, G. [1947]: "Professor Lester and the Marginalists," American Economic Review, 37, pp. 154-157.

Tarski, A. [1956]: Logic, Semantics, Metamathematics.

U. S. Department of Commerce. [1975]: "Accuracy of Data for Selected Housing Characteristics as Measured by Reinterviews," 1970 Census of Population and Housing, PHC(E)-10.

Ward, B. [1972]: What's Wrong with Economics.

Watkins, J. [1970]: "Against 'Normal Science,'" in I. Lakatos and A. Musgrave (eds.): Criticism and the Growth of Knowledge.

Weinberg, J. and Yandell, K. [1971]: Metaphysics.

Wheeler, J. A. [1973]: The Physicist's Conception of Nature.

Wisdom, J. O. [1974]: "The Nature of 'Normal Science,'" in P. A. Schilpp (ed.): The Philosophy of Karl Popper.

Wittgenstein, L. [1922]: Tractatus Logico Philosophicus.

Wong, S. [1973]: "The F-twist and the Methodology of Paul Samuelson," American Economic Review, 63, pp. 313-325.

Worland, S. [1972]: "Radical Political Economy as a Scientific Revolution," Southern Economic Journal, pp. 274-284.

APPENDIX

APPENDIX

The purpose of this appendix is to present a review of Friedman's examples. Obviously, such an analysis should help to clarify Friedman's view of science, particularly since Friedman's MPE deals with application. There are two objectives to this appendix. First, we wish to register our disagreement with Friedman's methodology as he chooses to apply it. There is a considerable difference between Friedman's MPE and Popper's falsificationism. More important is the fact that Friedman misses every opportunity to find or use empirical "disconfirmations." Although his examples of "scientific" hypotheses are stated in a manner that makes them 'irrefutable,' he shows no difficulty in finding "supporting" evidence. The central realization that emerges from the analysis of Friedman's examples is that MPE, at least as applied, is neither critical nor empirical in any substantive sense.

(1) Friedman's first example of a scientific theory is a "positive statement" that is also a judgment that "differences about economic policy among disinterested citizens derive predominantly from different predictions about economic consequences . . . rather than fundamental differences in basic values."[1] Does this not seem more like wishful thinking than scientific hypothesis? The scientific character of this hypothesis is quite questionable. How can "basic values" be empirically measured? What 'facts' could <u>conceivably</u> 'falsify' this assertion? To equate one's "basic values" with the "facts" seems even more tenuous

than Lakatos' use of "basic value judgments" as "facts" (recall 3.2). In any case, it is an unfortunate choice for the first example of what one wishes to call science.

Not only does Friedman fail to provide an indication of how one might 'falsify' his statement, reading between the lines makes one wonder whether Friedman would allow his assertion to be 'falsified.' For he has no difficulty in interpreting the evidence in the light of his assertion. In the same paragraph as his assertion, he supports it with the example minimum-wage legislation. He accepts the notion that everyone has the goal of achieving a "living wage" (although he has some doubt concerning what this means) and asserts that differences in opinions concern the relative merits of means to this end. Friedman reduces the differences in opinions to differences in predictions of unemployment and under-employment and suggests that if we had better information about these predictions the controversy would largely vanish.

Yet there is a potential fallacy to Friedman's argument. How is unemployment related to the "living wage"? Does not the goal of a "living wage" refer only to the wage rate? If so, there is no controversy concerning the predicted effect of minimum wage increases upon the wage rate. Yet disagreement about minimum wage legislation remains. The fact that no one argues about the predicted effect upon wages when many disagree about minimum wages implies that there is not the agreement about the social objectives of this legislation that Friedman wishes us to believe. In such a case, it tends to disconfirm his "positive statement."

Clearly, unemployment will affect poverty. But it is far from clear that unemployment is directly related to "living wage." Friedman is liberally interpreting the policy goal, "living wage," in order to find supporting evidence. We are merely pointing out that Friedman is interpreting the data in the light of his hypothesis. While this can always be done, it is the antithesis of scientific activity.

Friedman finds another example to support his hypothesis about economic predictions.[2] For this case, he finds an apparent counter-example. But this potential "falsifying instance" is quickly dismissed by his belief that "at the bottom" it would be consistent to his view.[3] In Friedman's first example of a "positive statement," he shows the reader only how to favorably interpret the data without a hint of how one might meaningfully 'test' his assertion.

Furthermore, this example makes explicit Friedman's view that value judgments can be used as the "raw material" of economic science. His use of "basic values" clearly specifies that "positive economics" regards ethics and values as explanatory variables and observable states of affairs. Otherwise, Friedman is contradicting his own stated methodology when he labels his assertion a "positive statement."

While we agree with the possibility of observing human values, we see a need to emphasize the difficulty of devising intersubjectively 'testable' methods of observation. The fact that Friedman provides no qualification to his hypothesis about "basic values" seems to thrust "positive economics" back into the "normative" sphere. In any case, this example only confuses the methodological issues of "positive" vs. "normative" which, after all, is the topic being discussed.

(2) Friedman's first unambiguous example of a scientific hypothesis is Galileo's law.[4] Yet, Friedman's discussion of Galileo's law is far from scientific. He uses this example as a vehicle for his answer to the

question, "Can a hypothesis be tested by the realism of its assumptions?" Or, at least, this is what his section title states. Thus, this example is linked with Friedman's "irrelevance-of-the-assumptions" thesis. Keeping this in mind, "we may start with a simple example, the law of falling bodies. It is an accepted hypothesis that the acceleration of a body dropped in a vacuum is a constant - g, or approximately 32 feet per second per second on the earth - and is independent of the shape of the body, the manner of dropping it, etc."[5]

Accepted? By whom? This is a 'false' statement. The acceleration of gravity is not a constant, not even in a vacuum. Both the theories of Einstein and Newton are in direct opposition to Galileo's law, and the 'corroboration' of these theories 'falsifies' Galileo's law. Except for pedagogical purposes, physicists have long "rejected" Galileo's explanation. The true value of Galileo's law is not that it is somehow "approximately correct" but that it is a successful "falsifying hypothesis" to Aristotle's law which asserts that the falling speed is directly proportional to the masses of bodies.

It is hard to imagine that anyone writing about scientific methodology can regard such a definitively 'falsified' theory as correct. Yet it appears that Friedman does. "The formula $S = 1/2\ gt^2$ is valid for bodies falling in a vacuum and can be derived by analyzing the behavior of such bodies. It can therefore be stated: under a wide range of circumstances, bodies that fall in the actual atmosphere behave _as if_ they were falling in a vacuum."[6] Since "valid" is Friedman's euphemism for 'truth,' the above reflects Friedman's belief in Galileo's law in his strongest language. He even implies that Galileo's law is somehow established by induction. For what else could "can be derived by analyzing the behavior of such bodies" mean?

There is no logic or theory of falling objects that implies Galileo's law, and both Newton's and Einstein's theories imply statements that contradict Galileo's law. Thus, Galileo's law cannot be "derived by analyzing the behavior of such bodies." Friedman even mentions a "falsifying hypothesis" to his generalization of Galileo's law but, of course, does not use it as such. The "falsifying hypothesis" is that bodies dropped in the actual atmosphere will eventually reach a terminal velocity.[7] The phenomenon of terminal velocity is a "falsifying hypothesis" to the constancy of acceleration since it implies that acceleration is zero when the terminal velocity is reached and positive before. Thus, even though Friedman is aware of the 'falsity' of Galileo's law, he is willing to accept it as part of the "body of systematized knowledge concerning what is."

This example shows quite clearly that it is methodologically correct, in Friedman's MPE, to accept hypotheses known to be 'false' into the "body of systematized knowledge concerning what is." Galileo's law is 'false' both within and without a vacuum. While Friedman is unambiguously arguing that it is "acceptable" in both cases. "The formula is accepted because it works, not because we live in an approximate vacuum - whatever that means."[8]

What is particularly appalling about Friedman's "observed methodology" is its illustration of a completely anticritical approach to scientific inquiry - contrary to whatever Friedman may appear to say in his "positive economies." This example of Galileo's law affords many opportunities to use empiricism as a critical tool. The fact that Friedman expresses no concern about the 'falsity' of Galileo's law and reformulates this theory to dismiss some potential empirical problems is a strong testimony of his anticritical methodology. Clearly, Friedman substitutes "use" for 'truth' and adopts some type of pragmatic view of truth. This aspect of Friedman's essay is what causes Boland and Wong to call Friedman's methodology, instrumentalism.

(3) Friedman's next example concerns the biological phenomena of phototropism which Friedman "designs" as an analogue of hypotheses in the social sciences. "I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbors, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and would move rapidly or instantaneously from any one position to any other desired and unoccupied position."[9]

Friedman finds no difficulty in obtaining supporting evidence for this hypothesis. The observed density of leaves on a tree can be interpreted to conform to this hypothesis under a variety of circumstances, and this "direct evidence" is strengthened by the "indirect evidence" which is consistent with a more general theory.[10] The more general theory to which Friedman refers is "that sunlight contributes to the growth of leaves."[11] One can only wonder how empirical support for this latter hypothesis provides increased support to the "direct evidence" concerning the density of leaves. If the more general theory has more 'empirical content' and is better 'corroborated' than Friedman's formulation, then we have some good reasons for 'rejecting' Friedman's hypothesis and not increase support for it. Again, we see that Friedman is incapable or unwilling to find any "disconfirming" evidence for his hypotheses. Instead, he only dismisses the obvious "unrealism" of his "assumptions" by suggesting that it is not relevant whether leaves do not "consciously seek," "have not been to school and learned the relevant laws of science or the mathematics required to calculate the 'optimum' position, and cannot move from position to position."[12]

Here, too, Friedman demonstrates how it is possible to interpret the evidence in the "light" of one's hypothesis. He clearly believes in his formulation. "The constructed hypothesis is presumably valid, that is, yields 'sufficiently' accurate prediction about the density of leaves, only for a particular class of circumstances."[13] Why must we presume its "validity"? Why does not Friedman report its accuracy of prediction so that we may "judge" whether it is "sufficient"? Although Friedman asserts the importance of specifying the circumstances for which a theory "works" and the magnitude of error for those circumstances, at each opportunity he fails to do so.

(4) Friedman's "maximization-of-sunlight" hypothesis seems to serve only as a transition to the even emptier example of the billiard player. "(T)he hypothesis that the billiard player made his shots as if he knew the complicated mathematical formulas that would give the optimum direction of travel, could estimate accurately by eye the angles, etc., describing the location of the balls, could make lightening calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas."[14]

Of what possible use is such a hypothesis? What phenomena might it explain? What empirical evidence might 'falsify' it - or support it, for that matter? To all such reasonable questions, Friedman is silent. The apparent purpose to this absurd "hypothesis" is to make the reader complacent about "unrealistic assumptions" and similar hypotheses. Our reason for this conclusion is simply that the "hypothesis" of the billiard player cannot explain any phenomenon.

To make our case, we must first recognize which mathematical formulas are being used. Presumably, they are the formulas of physics which describe the results of the collisions of massive bodies under conditions of significant friction. For what other formulas would reasonably apply to the making of a billiard shot? But such laws of physics can play no explanatory role for the phenomenon of "successful" billiards. Here, the laws of physics can be used only tautologically. That is, either a given shot will be "successful" because of its obedience to the "laws of physics" or it will be "unsuccessful" due equally to its obedience to these same laws.

Friedman's hypothesis cannot explain why some shots are more likely to be missed than others or why shots are missed at all. It seems to us that the fact that even "expert" billiard players miss some shots is "falsifying evidence" to his hypothesis - that is, if this hypothesis is to be regarded as having any content. If the billiard players behaved as if Friedman's hypothesis assumes, would this not imply (or "predict") that they will make every shot? Ironically, a more "useful" hypothesis for the "prediction" of billiard shots is, more or less, the opposite of Friedman's assumption. Suppose that billiard players have great difficulty in calculating and understanding the laws of physics and further that their errors of calculation are proportional to the complexity (defined by the number of interactions of the physical system) of the formulas. Such a hypothesis correctly "predicts" that more complicated shots will be missed more frequently than less complicated ones. Thus, shots using "banks," "English," or several balls in combination will be missed more frequently than those that do not. Friedman's hypothesis simply cannot explain any of the variation in billiards playing, contrary to what he claims.[15]

This empirically empty example along with the "maximization-of-sunlight" hypothesis only serve the purpose of lulling the reader into accepting similar hypotheses - for example Friedman's "maximization-of-returns" hypothesis (recall 4.4.4). Again Friedman fails to find critical evidence, while tautological argumentation is allowed to support his "hypothesis." "Our confidence in this hypothesis is not based on the belief that billiard players, even expert ones, can or do go through the process described; it derives rather from the belief that, unless in some way or other they were capable of reaching essentially the same results, they would not in fact be _expert_ billiard players."[16]

This argument is circular. It can serve only to implicitly define "expert" as someone for whom this hypothesis "works." Apparently, "confidence" (as used by MPE) increases with one's ability to _define_ the "facts" consistent with his hypothesis. Such practice is a good illustration of conventionalism in action.

(5) The "maximization-of-returns" hypothesis (see 4.4.4).

(6) The last part of Friedman's essay contains several more examples, most of which are of minor significance and of major ambiguity. For the sake of completeness and clarity, we shall discuss only two additional illustrations of what Friedman considers scientific theory.

> An example may help clarify this point. Suppose the problem is to determine the effect on retail prices of cigarettes of an increase, expected to be permanent, in the federal cigarette tax. I venture to predict that broadly correct results will be obtained by treating cigarette firms _as if_ they were producing an identical product and were in perfect competition. . . .

> On the other hand, the hypothesis that cigarette firms would behave as if they were perfectly competitive would have been a false guide to the reactions to price control in World War II, and _this would doubtless have been recognized before the event_. Costs of the cigarette firms must have risen during the war. Under such circumstances perfect competitors would have reduced the quantity offered for sale at previously existing price. But, at that price, the wartime rise in the income of the public _presumably_ increased the quantity demanded. Under conditions of perfect competition strict adherence to the legal price would therefore imply not only a "shortage" in the sense that quantity demanded exceeded quantity supplied but also an absolute decline in the number of cigarettes produced. The facts contradict this particular implication: there was reasonably good adherence to maximum cigarette prices, yet the quantities produced increased substantially. The common force of increased costs _presumably_ operated less strongly than the _disruptive_ force of the desire of each firm to keep its share of the market, to maintain the value and prestige of its brand name, especially when the excess-profits tax shifted a large share of the costs of this kind of advertising to the government. For this problem the cigarette firms cannot be treated _as if_ they were perfect competitors. (All but the last emphasis added.)[17]

What is this an example of? "Everything depends on the problem; there is no inconsistency in regarding the same firm as if it were a perfect competitor for one problem and a monopolist for another . . ."[18] As such, it is a good example of Friedman's economics in practice. If one hypothesis does not "work" for a particular situation, use another. But, in no case, should one suggest that either hypothesis is somehow discredited. Friedman's claim that everything depends upon the problem can be an apt qualifier, yet, as usual, Friedman misapplies it. The "problem" is not characterized by some particular circumstance. Theories are meant to explain all observations of a given phenomenon. The "problem" is merely to explain some given phenomenon. Are not both cases to which Friedman refers observations of the same phenomenon? Is not the relevant question, "How can the price and quantity changes of cigarettes be explained?" Thus, the "problem" and the phenomenon in both cases are the same, and only one theory is required. In both cases even the stimuli are quite similar. Tax increases are quite similar to cost increases. The price controls of the second case are the only real difference. Still both cases seem quite suited to the usual explanation of prices and quantities by supply and demand. The inability for "supply and demand" to explain the increases in production in the second case represents a real weakness of economic "theory."

Of course, this is not how Friedman sees it. Instead he believes that he can have it anyway he wishes. For his method allows one to "predict" that quantity will decrease or increase as a response to the same stimulus depending on how one wishes to see the industry or specific firms within the industry. The only way that Friedman's method can be considered a legitimate theory is if he can specify 'testable' conditions that identify, a priori, when to "use" each theory. Otherwise, Friedman is merely "assuming a can opener."

Although Friedman admits the desirability of a "more general theory," he only criticizes others for their attempt to develop one.[19] Friedman's only answer to the problem of specifying when a theory is applicable is to refer to "judgment." "The capacity to judge that these are or are not to be disregarded, that they should or should not affect what observable phenomena are to be identified with entities in the model, is something that cannot be taught; it can be learned but only by experience and exposure in the 'right' scientific atmosphere, not by rote."[20] Once learned, they apparently cannot be stated.

In this example, Friedman claims the market share behavior of the cigarette industry "would doubtless have been recognized before the event." But this claim is quite unconvincing. How or by what grounds? Friedman does not answer. By using market share to explain the wartime behavior of the cigarette industry, is he not implying that he accepts the "maximization-of-market share" hypothesis? If so, does this not contradict his "maximization-of-returns" hypothesis? If not, then Friedman could not have predicted the increase in the quantity of cigarettes produced during the war before the fact. It seems that something is inconsistent.

Again we see Friedman's lack of empiricism. Friedman's theory of "multiple market structure" cannot be 'refuted' nor empirically 'tested.' Yet, ironically, it logically requires an investigation of the "realism of the assumptions" to be applied. Are not the conditions that describe "perfect competitors" or monopolies the "assumptions" of those theories? The "realism" of these conditions must somehow be adduced before one can decide which theory to apply when using Friedman's method. How else can we know, a priori, which model to apply? Or, would Friedman wait until the price-quantity effects are observed before making a "prediction"? Thus, it seems that we have come full circle by attempting to follow Friedman. The "realism of the assumptions" is irrelevant and impossible to "test," yet we must somehow know in which situations those "assumptions" are "realistic" (at least in some unspecified sense) before our economic theories can be applied.

(7) Although Friedman is unable to specify the necessary conditions for deciding which theory to use, he is quick to criticize one such specification. Here we are referring to Friedman's discussion of monopolistic competition.

> The theory of imperfect or monopolistic competition developed by Chamberlin and Robinson is an attempt to construct such a more general theory. Unfortunately, it possesses none of the attributes that would make it a truly useful general theory. . . .
>
> The deficiencies of the theory are revealed most clearly in its treatment of, or inability to treat, problems involving groups of firms - Marshallian "industries." So long as it is insisted that differentiation of produce is essential - and it is the distinguishing feature of the theory that it does insist on this point - the definition of an industry in terms of firms producing an identical product cannot be used. By that definition each firm is a separate industry. Definitions in terms of "close" substitutes or a "substantial" gap in cross-elasticities evades the issue, introduces fuzziness and undefinable terms into the abstract model where they have no place, and serves only to make the theory analytically meaningless - "close" and "substantial" are in the same category as a "small" air pressure. . . .
>
> The theory of monopolistic competition offers no tools for the analysis of an industry and so no stopping place between the firm at one extreme and general equilibrium at the other. It is therefore incompetent to contribute to the analysis of a host of important problems: the one extreme is too narrow to be of great interest; the other, too broad to permit meaningful generalization.[21]

The most striking aspect of Friedman's criticism of monopolistic competition is that it is totally nonempirical. Friedman does not even address the issue of whether monopolistic competition can be "usefully applied." Is he not breaking his own rules in criticizing this theory's assumptions? After all does he not say, "Only factual evidence can show whether it is 'right' or 'wrong.'"[22] What factual evidence (or for that matter, empirical implications) does Friedman mention? None.

Friedman is inconsistent when he criticizes theories by the same means he refuses others. How is "close substitutes" less clear or potentially less applicable than "under a wide range of circumstances"? Friedman's argument is simply unfair. Since Friedman recognizes some similarity between Galileo's law and monopolistic competition, ("close" and "substantial" compared to "small" air pressure) why does he not provide a similar restatement for monopolistic competitions. Surely by adding such phrases as "under a wide range of circumstances" and "as if" Friedman could have found a way to state monopolistic competition that would have dismissed his problems with its terms. After all, the problematic nature of the term "vacuum" did not stop Friedman from using it and Galileo's 'false' law. For some reason, Friedman seems unwilling to view monopolistic competition in the same light that he claims to throw upon other examples.

Apparently the reason has to do with some perceived illogic of monopolistic competition. Friedman does not seem to like the definitions used in monopolistic competition. He sees some type of logical inconsistency in an industry in which different firms produce different products (recall the second paragraph of the above quotation). The only inconsistency involved in such a notion arises when one defines an industry as a group of firms which produce an identical product. Just because this is Marshall's or Friedman's definition does not make other definitions invalid or unuseful. There is no point to argue over a theory's definitions. To Friedman, it would seem that the proper question to ask is whether the theory which uses such definition "works." The point is that Friedman's criticism of the logic of monopolistic competition is not consistent with Friedman's explicitly stated methodology.

Why then does Friedman change his own mode of argumentation to criticize monopolistic competition? He reveals his position earlier in his essay.

> The theory of monopolistic and imperfect competition is one example of the neglect in economic theory of these propositions. The development of this analysis was explicitly motivated, and its wide acceptance and approval largely explained, by the belief that the assumptions of "perfect competition" or "perfect monopoly" said to underlie neoclassical economic theory are a false image of reality. And this belief was itself based almost entirely on the directly perceived descriptive inaccuracy of the assumptions rather than on any recognized contradiction of predictions derived from neoclassical economic theory.[23]

Apparently, Friedman does not believe in monopolistic competition not because of any "recognized contradictions of predictions" but because of his perception that its supporters believe that neoclassical theory is "unrealistic." How is his criticism of monopolistic competition really any different than the criticisms of neoclassical theory which he berates? If the above does indeed represent Friedman's reason for the rejection of "monopolistic competition," it is completely ludicrous. What others may or may not believe about some theory is totally irrelevant to the evaluation of that theory or its alternative. What can be more subjective than to reject a theory based upon what one believes that others believe?

If Friedman had really adopted the critical approach or if he was interested in the growth of economic knowledge, he would not simply reject monopolistic competition based upon its internal logic. Instead, he would have offered suggestions for its improved specification and application. Even the falsificationists do not reject a new theory based upon difficulties in developing a satisfactory initial explication. In falsificationism, the critic should attempt to improve the specification through "positive criticism" of such new theories, at least until it can be determined just what 'empirical content' the new alternative has. How else will we ever discover what new alternatives have to offer?

Friedman's impatience and nonempirical rejection of monopolistic competition can only show that he is not interested in the critical approach or in finding potentially better alternatives to traditional economic theory. Is it any surprise that there has been a "failure of any coherent self-consistent alternative (to the "maximization-of-returns" hypothesis) to be developed and widely accepted"[24] [parentheses added]. Friedman gives us at least one observation that tends to confirm Blaug's fear that "we can never be sure that the repeated failure to produce such refutations is not a sign of the reluctance of economists to develop and test unorthodox hypotheses."[25]

Friedman's examples dramatically illustrate a different view of science than the ones which we have presented in the previous chapters. At nearly each turn in Friedman's discussion, we find an erroneous argument or a misconceived notion of science, at least when compared to science as 'rational' and 'empirical criticism.' Each of Friedman's examples shows some type of anticritical and nonempirical argumentation.

Friedman reduces all empirical content to insignificance. By adding the phrases "as if" or "under a wide range of circumstances" what little content Friedman's hypotheses might have had is further diminished. Such practice can only be interpreted as a defense against potential criticism and thus as an unwillingness to promote the growth of knowledge. Each example represents a very trivial statement, at least empirically. Even when we ignore Friedman's defensive devices, his hypotheses have little or no 'empirical content.' The only notable exception to this generalization is Friedman's use of Galileo's law. It is the only example that has a possibility of making a quantitative "prediction," yet, ironically, its quantitative "predictions" are all 'false.' It is "as if" Friedman considered the alternative explanations of some given phenomenon and chose the one which minimizes the 'corroborated empirical content.'

Our suspicion that Friedman's MPE would gravitate to the acceptance of the most trivial, non-empty theories is well "confirmed" by Friedman's examples (recall our discussion in section 4.4.2). In fact, we see at least two cases in which Friedman even breaks his methodological rule that hypotheses must be capable of contradiction - the billiard player and the "maximization-of-returns" hypothesis.

Friedman's use of "empirical evidence" is completely lopsided, and we think backwards. In all his discussions he does not mention one case where empirical evidence is falsifying or in anyway reflects "negatively" on a hypothesis. Friedman's antiempirical bias is further illustrated by his consistent interpretation of the evidence in the light of his belief. Friedman only "confirms" Popper's observation that, "It is easy to obtain confirmations, or verifications, for nearly every theory - if we look for confirmations."[26] Although in Friedman's explicit statement of MPE he shows a recognition of the asymmetry of empirical evidence, his methodology in practice makes no use of this invaluable truth. Furthermore, Friedman shows no ability to use 'rational criticism,' in general. His examples illustrate only a defensive, dogmatic attitude. He "criticizes" only the critics and the definition of monopolistic competition's terms.

Friedman's application of MPE reveals a strong emphasis upon applied science and thus practical technology, rather than knowledge. If there was any doubt concerning whether Friedman has adopted some form of falsificationism, it is unquestionably removed by his examples. His concentration on what "works" and other practical aspects of science clearly associates his methodology with instrumentalism. Although we have seen some significant conventionalism in Friedman's MPE, his examples appear to be more indicative of instrumentalism.

It is interesting to note that Friedman does not give one example about how knowledge comes about or how a theory "success" or magnitude of error comes to be known. The closest that Friedman comes to describing this most important aspect of methodology is during his defense of the "maximization-of-returns" hypothesis. "The evidence <u>for</u> a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its very nature is difficult to document at all comprehensively."[27] But such a response addresses none of the relevant questions. How is the theory's magnitude of error identified and used? More importantly, is there any incentive to honestly report or be concerned with the error after one has already applied the theory? Yet, without accurate knowledge of the error, Friedman's applications of scientific theory cannot even "predict" their own "success." This weakness reflects a totally destructive consequence of Friedman's instrumentalism. It can "work" only if there is known and given knowledge; that is, it must assume the existence of a "useful" theory before it has anything to do. And yet, instrumentalism cannot produce such knowledge on its own. (A more comprehensive development of these points is presented in 4.4.5.)

Nonetheless, the silence of Friedman's methodology on the production of knowledge leads us to another realization. In none of his examples does he show how one theory may be chosen after another. Although Friedman mentions that "predictive success" of a theory needs to be balanced against the "cost" of "using" it, he gives no hint about how either of these can be determined in practice nor how the "balancing act" can be successfully accomplished. Yet, the stated purpose of Friedman's essay is to address the question of theory choice. "This paper is concerned primarily with . . . the problem how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the 'body of systematized knowledge concerning what is.'"[28] In contrast, all of his examples concern how to "use" a given theory or how to defend it from criticism. The only answer to the question that Friedman regards as his purpose is: "The hypothesis is rejected if its predictions are contradicted ('frequently' or more often than predictions from an alternative hypothesis). . . ."[29] Even with this "all-but-empty" methodological principle, Friedman cannot be consistent. For, he accepts Galileo's law as a valid hypothesis. Few, if any, theories have had their "predictions" contradicted more often or relative to more theories than has this one. In other examples, he violates his principle that a theory must be susceptible to contradiction. In none of Friedman's applications do we find the reasonable use of empiricism or 'rational criticism.' Friedman misses every opportunity to find and use "disconfirming" empirical evidence or to state hypotheses in an empirically 'refutable' manner. These examples illustrate the fact that Friedman is not at all interested in "what is" but rather "what works." Thus, instrumentalism is the best description of how Friedman applies his methodology.

Notes

[1] Friedman [1966], p. 5.

[2] *Ibid*., p. 6.

[3] *Ibid*., p. 6, n. 4.

[4] *Ibid*., pp. 16-19.

[5] *Ibid*., p. 16.

[6] *Ibid*., p. 18.

[7] *Ibid*., p. 17. Although Friedman mentions this phenomenon, he does not claim its generality.

[8] *Ibid*., p. 18.

[9] *Ibid*.

[10] *Ibid*., p. 20.

[11] *Ibid*.

[12] *Ibid*.

[13] *Ibid*.

[14] *Ibid*., p. 21.

[15] *Ibid*.

[16] *Ibid*.

[17] *Ibid*., pp. 36-37.

[18] *Ibid*., p. 36.

[19] *Ibid*., pp. 38-39. We shall soon discuss Friedman's criticism of monopolistic competition.

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

[21] *Ibid*., pp. 38-39.

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

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

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

[25] Blaug [1980], p. 117.

[26] Popper [1963], p. 36.

[27] Friedman [1966], p. 23.

[28] *Ibid*., p. 3.

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

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