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THE METHODOLOGY OF MAINSTREAM ECONOMICS AND ITS IMPLICATIONS FOR CHINA'S ECONOMIC RESEARCH

by

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ABSTRACT OF THE DISSERTATION

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This study provides a detailed survey and critical analysis of the methodology of mainstream economics. From this investigation some radical implications are drawn for current Chinese economic research. The purpose of this study, from epistemological and methodological perspectives, is to explain why economics is so advanced in the Western countries and why economics is so backward in the former socialist countries. It is shown that mainstream Western economics is epistemologically and methodologically founded on modern logical empiricism. With the detailed analysis of the historical texts, I have carefully identified where and how each of the leading economic methodologists (Morgenstern, Hutchison, Machlup, and Friedman) has been dynamically influenced by the logical positivist philosophy. I also use three-case studies (the proof of the existence of general equilibrium, the construction of expected utility theory, and the development of experimental economics) to demonstrate that the development of modern economic science has been generally in keeping with the positivists' methodological prescriptions in both formal and empirical aspects. It is argued that positive economics cannot be separated from positivist philosophy and empiricist methodology. The important implication for Chinese economic research is that economics as a science in China cannot be developed and cultivated in the soil of metaphysical Marxism.

Table of Contents

Chapter 1. Introduction and Overview

1.1 Economics in the West and the East

Edmund Burke called the eighteenth century "the age of the economist," and our century can be more appropriately called "the age of economics becoming a strict science." Economics has gained the status of a science especially since the 1930s. Of course, heterodoxies such as Institutionalists and the New Left Marxists have frequently asked the question "why economics is not yet a science?"[1], but the majority of the practitioners have never doubted the scientific (cognitive) status of economics.

First, positive and normative economics, facts and value, "what it is" and "what it ought to be", have been strictly dichotomized; normative values and ethical judgments have been excluded from positive economics. Most economists have agreed with John Naville Keynes's statement that the separation between positive economics and normative economics is not only *possible*, but also *imperative*.[2] Mainstream economists, from George Stigler to Milton Friedman, have spoken with one voice: there is no value judgment in positive economics, "economics as a positive science is ethically--*and therefore politically*-- neutral."[3]

Second, since positive economics is essentially concerned with *quantifiable* and *measurable* relations such as quantity and price of commodities, modern mathematical tools have been widely used in the theoretical and empirical research. The mathematical tools include not only differential calculus and metrics algebra, but also topology, linear and dynamic programming, game theory,[4] and mathematical logic.

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[1]Alfred Eichner (1983), ed., Why Economics is not yet a Science?

[2]John Neville Keynes (1890), The Scope and Method of Political Economy.

[3]G. Stigler, "The Politics of Political Economists," Quarterly Journal of Economics, 1959, 73, p. 522. Italics original. Also see M. Friedman (1953), Essays in Positive Economics, p. 3.

Large parts of microeconomics such as choice theory and general equilibrium theory have been mathematically formalized and axiomatized. Macroeconomists have applied the advanced mathematical techniques (e.g. the recursive methods) to analyze monetary dynamics and public policy issues. Even the topics in the old fields such as industrial organization and international trade have been rigorously treated and much analytic insight has been gained by using mathematical tools. James Buchanan, a Nobel laureate, writes that what economists can learn from one of its neighbors, mathematics, is "*a* language;"[5] Paul Samuelson, another Nobel laureate, goes even further: "mathematics *is* language [itself]."[6]

Third, laboratory experimentation has become an increasingly important tool in economic empirical investigations; economics is widely recognized as an *experimental* discipline much like the physical and biological sciences. Thus, the conventional view (held by Mill, Marx, and Robbins) that "experiment is a resource from which the economist is debarred"[7] has been fundamentally challenged, and Auguste Comte's positivist prophecy that the experimental method employed in natural science can be extended to the study of human behavior has been finally fulfilled. Economists are not concerned with the "final cause," nor the "first principle" of economic systems, but only with the understanding of individual and group observable *behavior*; economics,

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[4]This year the Nobel Prize in Economics is awarded to three game theorists: Harsanyi (University of California-Berkeley), Nash (Princeton University), and Selton (University of Bonn).

[5]In his article "Economics and its Scientific Neighbors," J. Buchanan asks "what can economics learn from its neighbors?" His answer is : "from Engineering--a warning; from History--hope; from Humanities--inspiration; from Law--a framework; from Mathematics--a language; from Physical Science--a morality; from Political Science--Data; from Psychology--a damper; from Statistics--design." This article is included in Krupp (1966), The Structure of Economic Science, p. 171.

[6]Paul Samuelson says that he has only one objection to "the great" Willard Gibbes's statement ("mathematics is a language"): "I wish he had made it 25 per cent shorter--so as to read as follows: 'mathematics *is* language.'" See Samuelson's "Economic Theory and Mathematics: An Appraisal," in Machlup (1991), Economic Semantics, p. 350.

[7]Cf. Neville Keynes (1890), The Scope and Method of Political Economy, in Hausman (1984), p. 76.

as a behavioral science, can and must use the experimental approach. Most economists now believe that laboratory experimentation plays a significant part in theory suggestion, hypothesis testing, fact finding, and policy recommendation. The new approach to the subject matter eventually marks the end of all metaphysical speculations in economic science.

Fourth, *analytical training* has become increasingly important for the economists' education because economic research has been highly *professionalized* and *technically* oriented.[8] Through formal, professional training, students are required to have a solid understanding of conceptual frameworks, analytic tools, and even technical jargons. In the nineteenth century, intellectual giants such as David Ricardo, John Stuart Mill, Karl Marx, and Stanley Jevons were largely self-taught economists. However, this is simply not the case for the twentieth century economists. Holding a Ph.D degree has become a major license to practice teaching and research in Anglo-American universities.

Fifth, it has been widely acknowledged among economics professors and graduate students that a knowledge of the history of economic thought is not a necessary condition for understanding current conventional economic theory.[9] The twentieth century indeed has been "the age of analysis" for philosophers as well as for economists.

Sixth, it is true that there are many differences of opinion and even many disputes in the economics profession; economists have frequently joked about the old professor who asked the same exam questions but always changed the answers.[10] But

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[8]See Mark Perlman on the professionalization in economics, Lecture Notes, 1993.

[9]Cf. Donald Gordon, "The Role of the History of Economic Thought in the Understanding of Modern Economic Theory," American Economic Review, 55 (1965), p. 46.

the disagreements among professional economists have been frequently exaggerated by the heterodoxies and the lay public. There is significant *consensus* on the fundamental principles of the discipline among the mainstream economists, e.g. New Classicists, New Keynesians, and Monetarists. Disagreements have largely arisen from theory applications, policy recommendations, and value judgments.

Seventh, prominent philosophers of science, such as Karl Popper, Imre Lakatos, and Thomas Kuhn, hold mainstream economics in almost as great esteem as they hold physics. Economics has shown that "one social science at least has gone through its Newtonian revolution," while "sociology and psychology are riddled with fashion and with uncontrolled dogmas" and they do "not seem as yet to have found their Galileo."[11]

Undoubtedly, economics in the Western World is a science; economics is more advanced than political science, sociology, and psychology[12], and it has been regarded as the "queen of social sciences."[13] Surprisingly, economics in China, the former Soviet Union, and the Eastern Bloc countries is so backward that it can hardly be called a science. The entire history of economic research in these countries had simply been a series of interpretation of Karl Marx's Capital (1867, 1885, 1894), Lenin's Imperialism, the Highest Stage of Capitalism (1917), and Stalin's Socialist Economic Problems in the USSR (1952). The primary and fundamental task of the economist was

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[10]The joke has been frequently quoted in seminars, papers, and textbooks. See Daniel Fusfeld, The Age of the Economist (Glenview, Illinois, Scott, Foresman and Company, 4th ed., 1982), p. 2. Also see Alan Blinder, "Keynes, Lucas and Scientific Progress," American Economic Review, 1987, May.

[11]Cf. Popper, The Poverty of Historicism (1960), pp. 1 & 16; also The Philosophy of Karl Popper (1974), ed. by Schilpp, pp. 16 & 60.

[12]Of course, I do not deny the fact that advances in the study of the brain are so rapid that psychology may gain the same status as neuro-physiology.

[13]The Nobel Memorial Prize in Economics was instituted by the Central Bank of Sweden in 1968 to reward the professional economist who "rendered mankind the greatest benefit."

no more than to provide a theoretical justification for the government policy. The economic research in these countries had been greatly affected by political and ideological forces. Free scientific inquiries into social phenomena became impossible in the communist regime. External conditions such as political persecution and ideological intolerance had thus been unfavorable to the progress of economic science. But I believe that besides these factors, Marxian metaphysical, dogmatic philosophy and its methodology also had a negative impact upon the economic research. The reason is that external (e.g. political) conditions for scientific inquiries have been significantly improved in the last fifteen years in these countries, but economic research and its results are not very promising.

1.2 The Thesis of the Present Work

In this study I will provide a detailed survey and critical analysis of the methodology of mainstream economics. From this investigation some radical implications will be drawn for current Chinese economic research. This study, from epistemological and methodological perspectives, will partly explain why economics as a science is so advanced in the Western countries (especially in America) and why economics is so backward in the former socialist countries. My thesis is that mainstream Western economics is epistemologically and methodologically founded on modern logical empiricism, and the empirical (and theoretical) research of mainstream Western economists has been dynamically influenced by logical empiricist philosophy and empiricist economic methodology. It will be argued that the development of mainstream economics has been largely due to its epistemological foundations and its methodology. Positive economics has not been, and cannot be, separated from positivist philosophy and empiricist methodology. It is my firm conviction that epistemology and methodology deeply matter especially when economics as a science

has gone to a wrong direction. The radical implication drawn from this argument is that economics in China cannot be a science until it has a secured epistemological foundation (i.e. empiricist philosophy) and an empiricist methodology. It will be shown that positive economics as a science in China cannot be developed and cultivated in the tradition of the Chinese metaphysical philosophy, nor in the soil of the dogmatical, speculative Marxism.

1.3 The Epistemological Foundations of the Mainstream Economics

Epistemologically mainstream Western economics has been based on logical empiricism. It will be shown that the methodology of mainstream economics is the result of the direct application of logical empiricism (chapter 3) and that the research of mainstream working economists has been in line with the spirit of logical empiricism (chapter 4). So we will first (chapter 2) investigate the epistemological foundations of mainstream economics: logical empiricist philosophy.

Logical empiricism as a philosophical movement was originated in the 1920s among the members of the "Vienna Circle" in Vienna. This philosophical movement was led by "scientifically trained philosophers and philosophically interested mathematicians and scientists."[14] The logical empiricist movement had close associations with many of the leading figures in the exact sciences, such as Albert Einstein, P. W. Bridgman, Bertrand Russell, John von Neumann, and Kurt Gödel. It also developed with those schools that shared its general outlook, e.g. Operationalism, Pragmatism, and Behaviorism. Since the 1930s, logical empiricism has been the dominant philosophy in Scandinavia, Great Britain, and the United States. In the 1950s and 1960s logical empiricism, facing increasing attacks more from its friends (those

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[14]Feigl (1969), "The Origin and Spirit of Logical Positivism," in Achinstein and Barker (1969), The Legacy of Logical Positivism, p. 3.

trained in the analytic tradition of empiricism) rather than from its enemies (metaphysical and moral philosophers), radically revised its original position and developed its philosophy of science. Logical empiricism has had a dynamic impact not only upon such areas of philosophy as ethics, epistemology, and metaphysics; but also upon social sciences such as psychology, sociology, and economics.

Logical empiricism has revolutionized philosophy and provided the philosophical community with a new research program. First, logical empiricists discovered and emphasized logic as the "new method" of philosophizing. This method is not the older Aristotelian logic, but modern mathematical logic. Since the second half of the 19th century logic has been reconstructed and expanded in a way that extends it far beyond traditional logic. This reconstruction was initially effected by Boole, Frege, Peano, and Schröder, and comprehensively achieved in Whitehead and Russell's monumental book Principia Mathematica (1910-13). Logical empiricists attached great significance to the new logic for philosophy. They believed that "the study of logic becomes the central study in philosophy: it gives the method of research in philosophy, just as mathematics gives the method in physics."[15] The positivist/empiricists' application of the new method to philosophy leads to a positive and to a negative result. The *positive* result is that various scientific concepts, laws, and theories are critically analyzed and clarified. The *negative* result is worked out in the domain of metaphysics and value theory: all alleged statements in this domain are entirely meaningless.[16]

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[15]This summarizing characterization of the philosophical method, from Russell's Our Knowledge of the External World, as a Field For Scientific Method in Philosophy, was frequently quoted in the writings of logical empiricists. Cf. The Philosophy of Rudolf Carnap, ed. by Schilpp (1963), p. 13.

[16]Cf. Carnap's "The Elimination of Metaphysics Through Logical Analysis of Language," in Ayer (1959), Logical Positivism, p. 60.

Second, logical empiricism radically revised the old empiricism, represented by Mill, Spencer, and Comte. This is especially true in the understanding of logic and mathematics. Old empiricism reasonably rejected the theses of rationalism and apriorism, but claimed that we can derive all knowledge (including that of mathematics and logic) and science from our experience. Indeed, "the understanding of logic and mathematics has always been the main crux of empiricism."[17] Logical empiricists, influenced by Wittgenstein, took the propositions of logic and mathematics as analytical-tautological. ("The propositions of logic are tautologies." "The propositions of logic therefore say nothing. They are the analytical propositions." "From tautology only tautologies follow."[18]) Mathematics and logic contain not the fundamental laws of the world, but the fundamental laws of representing or symbolizing the world. Thus the core of empiricism is preserved and the empiricist thesis restricts itself to factual knowledge. All factual knowledge comes neither from "pure thinking," nor from "pure intuition," but only from experience. All synthetic judgments have to be validated only by experience. Logical empiricists thus rejected the fundamental question that Kant asked in the Critique of Pure Reason (1787): "How *a priori* synthetic judgments are possible?"[19] For, they held that there are no such judgments.

Third, logical empiricists rejected all metaphysical, ethical, and theological statements as meaningless. Like David Hume, they claimed that all genuine, *cognitively* meaningful propositions are either analytic or synthetic but not both. Analytic propositions, concerning "relations of ideas," belong to the domain of logic or

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[17]Hans Hahn, "The Significance of the Scientific World View, Especially for Mathematics and Physics," in Hahn (1980), Empiricism, Mathematics, and Logic, p. 21.

[18]All from Wittgenstein, Tractatus Logico-Philosophicus, 6.1; 6.11; 6.126.

[19]Kant took this question as "the proper problem of pure reason." Cf. Kant, Critique of Pure Reason, translated by Norman Kemp Smith (New York: St, Martin's Press, 1965), p. 55.

pure mathematics. They are necessary and certain because they are devoid of factual content; they do not make any assertion about the empirical world, thus they cannot be refuted by experience. Synthetic propositions, concerning of "matters of facts," can be judged true or false according to the *verification principle*: "the meaning of a proposition is the method of its verification" (Wittgenstein). The analytic and synthetic propositions, as Ayer indicates, are supposed to be exhaustive: if a statement does not express something that could be empirically tested nor express something that is formally true or false, it does not express anything at all. Metaphysical (ethical, or theological) statements might have some poetic merit or even might "express some exciting or interesting attitude to life," but they contribute nothing to the increase of our *knowledge*, and thus they are cognitively meaningless.[20]

Fourth, logical empiricists completely rejected the prevailing view in the German-speaking countries (today still prevailing everywhere) that there is a radical distinction between the natural sciences and social sciences. They believed that various sciences can be unified in concepts, laws, and methodology. Carnap and Neurath maintained that all scientific concepts (terms) can be reduced to the *physicalist* observational language. As Carnap wrote: "there is a unity of language in science, viz., a common reduction basis for the terms of all branches of science."[21] Moreover, certain laws can be shown to be logically derivable from others, the laws of psychology and social science are reduced to biology, the laws of biology in turn reduced to those of chemistry and physics.[22] Finally, sciences are unified in procedure or method.

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[20]Cf. Ayer's Introduction to Logical Positivism, Ayer (1959), p. 10.

[21]Carnap, "Logical Foundations of the Unity of Science," in International Encyclopedia of Unified Science, vol. 1, p. 61.

[22]It is interesting to note that logical empiricists did not view psychology as a branch of social sciences, but a branch of behavioral sciences.

Carl Hempel and Paul Oppenheim proposed the hypothetico-deductive model of scientific explanation and prediction.[23] They claimed that all truly scientific explanations and predictions have a common logical structure: an explanans that contains a general law and a statement of relevant initial conditions, and an explanadum that is deduced from the explanans by the aid of the rules of deductive logic. Hempel has characterized this type of unity: "the thesis of the methodological unity of science states, first of all, that, notwithstanding many differences in their techniques of investigation, all branches of empirical science test and support their statements in basically the same manner, namely, by deriving from them implications that can be checked intersubjectively and by performing for those implications the appropriate experimental or observational tests. This, the unity of method thesis holds, is true also of psychology and the social and historical disciplines."[24]

1.4 The Methodology of the Mainstream Economics

From the 1930s to the 1960s, logical empiricism had an enormously great success; it became the dominant philosophy in the English-speaking countries. Roughly at the same time, mainstream economics undertook a *methodological revolution* in the direction of logical positivism (see chapter 3). These decades can be called "the years of *grand* methodology." This revolution was led by some prominent young economists. Some of them had close affiliations with the Viennese logical empiricist philosophy (Oskar Morgenstern and Fritz Machlup), some were trained in the related traditions of empiricism, pragmatism, and operationalism (Terence

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[23]Hempel and Oppenheim (1948), "Studies in the Logic of Scientific Explanation." Also see Hempel (1965), Aspects of Scientific Explanation and other Essays in the Philosophy of Science.

[24]Hempel (1969), "Logical Positivism and the Social Sciences," in Achinstein and Barker (1969), The Legacy of Logical Positivism, p. 191.

Hutchison, Milton Friedman, and Paul Samuelson). All of them, directly or indirectly, were influenced by logical empiricism (including pragmatism and operationalism). They transformed logical empiricist philosophy into economic methodology, and wrote various methodological prescriptions for working economists. These methodological prescriptions, though frequently criticized by philosophers of science, have been largely accepted and practiced by many working economists.

Oskar Morgenstern has been constantly acknowledged by the economics profession as a distinguished scholar in game theory, economic theory, and mathematical economics. He had long affiliations with leading figures who shared the empiricist outlook of logical positivism in mathematics and physics, such as Einstein, Gödel, and von Neumann, in Vienna and Princeton. Morgenstern was also a frequent participant in the meetings of the Vienna Circle and in Karl Menger's Mathematical Colloquium.[25] It was thus not surprising that there was always a positivist strain in his life work. Morgenstern's work on economic methodology stems from his positivist belief that the development of modern economics as a science was intimately related to how well it incorporated mathematics (e.g. mathematical logic) and laboratory experimentation. Morgenstern is the first economist who recognized the importance and applicability of modern mathematical logic in economic analysis. In his "Logistics and the Social Sciences" (1936) which makes frequent references to the writings of logical positivist philosophers (Carnap, Gödel, Hahn, and Menger), Morgenstern calls economists' attention to the "new logic," symbolic logic, because "one of the most powerful and impressive steps forward that the human spirit has made in the last two generations has up to now apparently been totally overlooked by the social sciences."[26]

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[25]See Morgenstern (1976b), "The Collaboration between Oskar Morgenstern and John von Neumann on Theory of Games," in JEL, 14, p. 806.; also cf. Stephan Boehm (1984), "The Private Seminar of Ludwig von Mises."

The new logic, in his view, is "profound, indeed fundamental for the social sciences," especially for economics: the so-called theory of types, the method of axiomatization, and the creation of a scientific language. First, "the problem of knowledge or expectation" cannot be treated adequately without using the theory of types. Second, the axiomatic method "can be applied to all sciences provided they are sufficiently developed" (p.396), including economics; Morgenstern believes that "it is quite possible to axiomatize economics." Third, the new logic provides a scientific *language*, "only in a formal language is it possible to examine whether one proposition actually *follows* from another one and what that means anyway" (p.398). In another article "Experiment and Large Scale Computation in Economics" (1954), Morgenstern stresses the importance of laboratory experimentation in economics. For a long time, economists (e.g. John Stuart Mill, Alfred Marshall, and Ludwig von Mises) had questioned "such a possibility in economics." Indeed economics had been regarded as a non-experimental subject by the majority of economists, even though laboratory experiments in psychology and education were conducted as early as in the late 19th century. But Morgenstern believes that "there exist great opportunities for direct experiments now and in the future." He indicates that the possibilities of controlled "direct experiments" (i.e. laboratory experiments) in the economy as a whole are very numerous: in individual decision-making and business organizations' activities. Morgenstern also shows the possibility of "indirect experiments" (what we would call "social experiments" or "field experiments").

All these views on the importance of mathematical logic and laboratory experiment were extremely modern and radical. It was revolutionary for Morgenstern to put them forth at a time when the economics profession was dominated by German

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[26]Morgenstern (1976b), Selected Economic Writings of Oskar Morgenstern, p. 389.

institutional and historical economics. His views were definitely inspired by logical empiricist philosophy.

Today Hutchison is probably the best-known historian of economic theory in the world. In economic literature in English, his Significance and Basic Postulates of Economic Theories (1938) is the first and most important mark of the union of logical positivism and the methodology of economics. Following logical empiricists, Hutchison claims that all propositions of economic science can be classified in two categories: *analytic* and *synthetic*. Propositions of pure economic theory are analytical-tautological. They are certain and necessary, but lack empirical content: they are independent of all facts, they cannot tell us anything new "in the sense of telling us new facts about the world" (p.30). Although the analytic propositions of pure theory are devoid of empirical content, they afford us a sharp clear-cut language and enable us to pass from one empirical-synthetical proposition to another. Propositions of applied economic theory are synthetical. Thus they must "conceivably be capable of testing or be reducible to such propositions by logical or mathematical deductions" (p.9). Hutchison claims that "all propositions with *scientific* sense are either conceivably falsified by empirical observation or not, and none can be both" (p.27). The fundamental purpose of Hutchison's making the sharp distinction between analytic and synthetic propositions of economics is to eliminate all non-scientific (metaphysical or ideological) statements from economic science. He indicates that it is the *principle of empirical testing* of propositions which keeps sciences separate from pseudo-science," "scientific" propositions from "philosophical" or "metaphysical." He demands that the "fundamental proposition" of maximizing behavior be subject to empirical test and investigation. Hutchison suggests that "the economic scientist is *transgressing* the frontiers of his subject whenever he resorts to propositions which can never conceivably be brought to an intersubjective empirical test, and of which one can never

conceivably say that they are confirmed or falsified, or which cannot be deduced from propositions of which that conceivably can be said" (p.10). It makes no difference to such a transgression whether the proposition is an expression of ethical uplift or persuasion, political propaganda, poetic emotion, psychological association, or metaphysical intuition or speculation. The principle of empirical test was later again emphasized in Samuelson's Foundations of Economic Analysis (1947). In the book he claims that economists should seek to discover "*operationally meaningful* theorems;" such a theorem is defined as "a hypothesis about empirical data which could conceivably be *refuted* if only under the ideal conditions."[27]

In the mid-1950s Machlup (a former president of the American Economic Association) wrote two papers to discuss "the problem of verification in economics."[28] These articles show his through understanding of modern philosophy of science: the sophisticated form of logical empiricism. He frequently makes references to the writings of logical empiricists such as Richard Braithwaite, Felix Kaufmann, and Ernest Nagel. For example, he quotes often from Braithewaite's Scientific Explanations (1953) and makes central use of the phrase "hypothetico-deductive system."[29] The fundamental question he asks in these articles is "Should all propositions in economics be verifiable?". To use Braithwaite's terminology, Machlup distinguishes in a hypothetico-deductive system between "higher-level" and "lower-

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[27]Samuelson, Foundation of Economic Analysis (Harvard University Press, 1947), p. 11.

[28]Machlup: "The Problem of Verification in Economics," and "Rejoinder to a Reluctant Ultra-Empiricist." Both published in Southern Economic Journal, 1956, (22), pp. 1-21., and pp. 482-93.

[29]Braithwaite's contribution in this book is his view that the deductive system of scientific theories has a hierarchical structure: "the propositions in a deductive system my be considered as being arranged in an order of levels, the hypotheses at the highest level being those which occur only as premises in the system, those at the lowest level being those which occur only as conclusions in the system, and those at intermediate levels being those which occur as conclusions of deductions from higher-level hypotheses and which serve as premises for deductions to lower-level hypotheses." Scientific Explanations, p. 12.

level" generalizations, between fundamental hypotheses (which are not independently testable) and specific assumptions (which are supposed to correspond to observed facts or conditions). Machlup argues that the fundamental assumptions of economic theory are not subject to a requirement of independent empirical observations. But he insists on strict independent verification of the assumptions selected as "assumed change" and of the conclusion derived as "deduced change." As he says: "In principle we want both assumed change and deduced change to be capable of being compared with recorded data so that the correspondence between the theory and the data can be checked" (p.14). He claims that this understanding of testability holds for both the natural and social sciences. Thus, Machlup's view on verification is directly based on the 1950s philosophy of science dominated by logical empiricism. Logical positivist philosophers of that time argued that scientific theory forms a hypothetico-deductive system. Such a system will normally contain some general laws and theoretical terms (unobservable entities) that are not empirically testable. So by positivist standards these are in danger of counting as meaningless. To solve this problem, the indirect-testability hypothesis was formulated: instead of testing each sentence in a theory for cognitive significance, we test the theory as whole. If the theory as a whole is confirmed, the sentences containing theoretical terms are indirectly meaningful. Though postulates are empirically untestable, the deduced theorems and predictions are empirically testable. All of these are compatible with Machlup's methodological views. Today most economists have accepted his characterization that economic theory forms a hypothetico-deductive system and his indirect-testability hypotheses.

Friedman's essay "The Methodology of Positive Economics" (1953) is by far the most influential methodological statement of the twentieth century in economics.[30]

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[30]This paper is included in the Essays in Positive Economics, Friedman (1953).

Though he was frequently criticized, the essay "survived to become the one article on methodology that virtually every economist has read at some stage in his career."[31] While Friedman does not explicitly refer to contemporary philosophy of science, he attempts to show that positive economics does satisfy empiricist standards. First, Friedman, like J. S. Mill and John Naville Keynes (the father of Maynard Keynes), makes a sharp distinction between positive and normative economics.[32] Positive economics is "in principle independent of any particular ethical position or normative judgments" because it deals with "what is", not with "what ought to be." 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 predictions it yields. In short, "positive economics is, or can be, an objective science, in precisely the same sense as any of the physical sciences" (p.4). Second, Friedman claims that the ultimate goal of a positive economics "is the development of a theory or hypothesis that yields valid and meaningful predictions about the phenomena" (p.7). Criteria for the acceptability of hypotheses follow: theories should be logically consistent and contain categories which have meaningful empirical counterparts; theories must also advance "substantive hypotheses" which are capable of testing; "the only relevant test of the validity of a hypotheses is comparison of its predictions with experience" (p.9). Third, Friedman emphasized the importance of indirect testing of hypotheses in economics; a theory cannot be tested by the realism of its assumptions. Although Friedman's article

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[31]Mark Blaug, "Paradigms versus Research Programmes in the History of Economics," in Hausman (1984), The Philosophy of Economics, p. 360.

[32]John Neville Keynes's The Scope and Method of Political Economy (1890) has become a "classic" of economic methodology. It was frequently used as a supplementary to Alfred Marshall's The Principle of Economics (1890) in the early century.

has been widely criticized by economists and philosophers, his methodological prescriptions have been accepted by the majority of working economists.

1.5 The Practice of the Mainstream Working Economists

It is now clear that mainstream economics has been epistemologically founded on logical empiricism and that the methodology of mainstream economics is the direct application of logical empiricist philosophy. Economic methodologists have written many methodological prescriptions for the working economists. These methodological prescriptions have been focused on two aspects of economic research: the *formal* and the *empirical*. In the formal aspect, economic methodologists have constantly emphasized the importance of applying formal techniques (e.g. mathematics, axiomatic method, game theory) to economic investigations. In the empirical aspect, they have demanded that working economists derive empirically testable (falsifiable or verifiable) hypotheses. It will be shown that the development of modern economic science has been in these two directions.[33] Thus chapter 4 exclusively concerns what working economists *actually have done* since the 1930s, or how working economists have *behaved*. Have working economists been influenced in their theoretical and empirical research by logical positivist /empiricist philosophy? Is this influence due to the working economist's self-consciously following such methodological prescriptions? My answers are affirmative.

First, there is no doubt that mathematical formalization has played a leading role in the development of modern economic analysis since the post-World War II

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[33]As Samuelson has said: "In connection with the exaggerated claims that used to be made in economics for the power of deduction and *a priori* reasoning--by classical writers, by Carl Menger, by the 1932 Lionel Robbins (first edition of The Nature and Significance of Economic Science), by disciples of Frank Knight, by Ludwig von Mises--I tremble for the reputation of my subject. Fortunately we have left that behind us" (American Economic Review, Sept. 1964, p. 736.)

period. Mathematics has become an essential tool and language of economic theorists. Most working economists have argued that economics cannot be truly scientific without a continuous cultivation of mathematical techniques. It is commonly recognized that the benefits that mathematics brings to economics are at least threefold: mathematics makes assumptions explicit, it prevents unnoticed assumptions that will slip into the deduction; mathematics also makes presentation of theory more precise and more concise; finally, mathematics allows economists to deal more easily with more than two-dimensional economic problems.[34] Of course, the mathematical techniques that have been used by working economists are far beyond the calculus and linear algebra used by the pioneers of mathematical economics such as Johann von Thünen (1783-1850), Augustin Cournot (1801-1877), Leon Walras (1834-1910), and Vilfredo Pareto (1848-1923). They include topology, linear programming (an offspring of game theory), dynamic programming, and mathematical logic. It is mainly the influence of von Neumann and Morgenstern's work that has freed mathematical economics from its traditions of differential calculus and made modern logic prominent. The major breakthrough in microeconomics in this century was the successful proof of the existence of the general equilibrium by Arrow and Debreu, employing an advanced technique, i.e., the Fixed Point Theorem;[35] the most celebrated Arrow's Impossibility Theorem in social choice theory was proved by using very simple techniques of symbolic logic.[36] Perhaps the most robust tool of modern economic analysis is the

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[34]Cf. Robert B. Ekelund and Robert F. Hebert, A History of Economic Theory and Method (McGraw-Hill Publishing Company, 3rd ed., 1990), p. 585.

[35]Their work has been named the Arrow-Debreu Model of General Equilibrium. Gerald Debreu was awarded the Nobel Prize for his contribution to mathematical economics, especially to his proof of the existence of the general equilibrium. See Arrow and Debreu, "Existence of an Equilibrium for a Competitive Economy." Econometrca. 22, 1954, 265-90.

mathematical technique called game theory. It was initially invented by mathematicians and was (and still is) a branch of modern mathematics. But in their monumental book Theory of Games and Economic Behavior (1944), Morgenstern and John von Neumann for the first time applied game theory to economic analysis. Then John Nash used the Fixed Point Theorem (S. Kakutani) to successfully prove that every *n-person* game has an equilibrium point.[37] Since the 1970s, game theory has been widely applied to various branches of economics such as industrial organization, labor economics, international trade, and public policy analysis. In search of the *micro*-foundations of macroeconomic behavior, macroeconomists have applied the basic ideas and methods of *dynamic programming* (proposed by Richard Bellman in 1957) to studying dynamic optimization problems (in consumption, saving, and investment) since the mid-1970s.[38] The dynamic tool often provides analytical insights and computational simplicity. Indeed, modern mathematics and modern logic have great power in analytic transformation and empirical investigations.

The *axiomatization* of economic theory is part of the mathematical formalization of economics. Axiomatization of theory was advocated by logical positivists and by economic methodologists (Morgenstern and Samuelson). One tenet of logical empiricism is that a scientific theory can be formulated *axiomatically* in terms of first-order mathematical logic. Morgenstern believes that "any theory ultimately will have to be axiomatized" because "the axiomatic method is simply a superb technique for

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[36]The Impossibility Theorem was first stated (and proved) in Arrow's Social Choice and Individual Values (1951): "There can be no constitution simultaneously satisfying the conditions of Collective Rationality, the Pareto Principle, the Independence of Irrelevant Alternatives, and Non-Dictatorship."

[37]J. Nash, "Equilibrium Points in *N*-Person Games," Proceedings of the National Academy of Science of the U.S.A., 36, pp. 48-49, 1950.

[38]Richard Bellman's Dynamic Programming (1957) is the classic. The application of dynamic programming to economic analysis, see G. Chow: Econometric Analysis by Control Methods (New York: Wiley, 1981).

summarizing our knowledge in a given field and for finding further knowledge deductively."[39] One aspect of the development of mathematical economics is that working economists have attempted to axiomatize economic theory since the 1920s. The first *set* of axioms in economics was presented by Ragnar Frisch (the first Nobel Laureate in economics) in 1926 and Franz Alt (a participant of Karl Menger's Mathematical Colloquium) in 1936.[40] But the first axiomatic *system* was constructed by Morgenstern and von Neumann in 1944. Thereafter, Gerald Debreu presents an axiomatic treatment of economic equilibrium in his Theory of Value (1959). Recent applications of the axiomatic method have extended to bargaining theory, to social choice theory, and to consumer theory.[41]

Empirical investigation has also played a leading role in the development of modern economic analysis. The modern field of empirical economics has been called "*econometrics*," a new discipline which incorporates mathematics, statistics, and economics. The object of econometrics, according to its founder Ragnar Frisch, is "to subject abstract laws of theoretical political economy or 'pure' economics to experimental and numerical verification, and thus to turn pure economics, as far as is possible, into a science in the strict sense of the word."[42] Econometric study thus has realized the dream of Stanley Jevons (a founder of the Marginal Revolution in the

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[39]Cf. Morgenstern (1976a), Selected Economic Writings of Oskar Morgenstern, pp. 269 & 453.

[40]Frisch, "On a Problem in Pure Economics," and Alt, "On the Measurability of Utility." Both were collected in Preferences, Utility, and Demand, ed., by J. Chipman, L. Hurwicz, M. Richter, H. Sonnenschein (Harcourt Brace Jovanovich, Inc., 1971), pp. 389-434.

[41]For Roth's axiomatic treatment of bargaining theory, see his Axiomatic Models of Bargaining (Berlin: Springer-Verlag, 1979). Norman Schofield, Social Choice and Democracy (Berlin: Springer-Verlag, 1985); and Bernt Stigum, Toward a Formal Science of Economics. The Axiomatic Method in Economics and Econometrics (MIT Press, 1990).

[42]R. Frisch (1926), "On a Problem in Pure Economics," in Chipman (1971), Preferences, Utility, and Demand, p. 386.

1870s): to *measure* the variations in the marginal utility of economic goods. Specifically, by employing statistical techniques, econometricians have attempted to test economic theory using historical data and to predict future economic events utilizing economic theory and historical data. Econometrics can provide both *explanations* and *predictions* of economic behavior within the context of economic theory. It is in empirical economics (i.e. econometrics) that testability, a central tenet of logical empiricism, shows its power and its success. The principle of testability was frequently dismissed by many economic theorists in the 1930s, but the development of modern econometrics has brought a wide recognition of the principle of testability among working economists. We will review some economic literature to show how the empirical researchers have followed the principle of testability, the prescription advocated by logical empiricists and economic methodologists.[43]

Laboratory experimentation, as a species of empirical investigations, has been widely used in economic research. The first experiment was conducted by Edward Chamberlin at Harvard in 1948.[44] Then Maurice Allais (the French Nobel laureate), Vernon Smith (a Harvard graduate student participating in Camberlin's initial experiment), and several game theorists continued (though only rarely) to use laboratory experimentation to test economic theory.[45] Since the mid-1970s, laboratory

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[43]Cf. Gerhard Tinter, Methodology of Mathematical Economics and Econometrics, in Foundations of the Unity of Science, vol 2.

[44]Chamberlin's market experiment was briefly mentioned in a footnote in the 8th edition of his classic The Theory of Monopolistic Competition; then independently published, "An Experimental Imperfect Market," in Journal of Political Economy, 56, pp. 95-108.

[45]Allais's experimental test on the Subjective Expected Utility Theory was discussed in Expected Utility Hypothesis and the Allais Paradox: Contemporary Discussion of Decisions under Uncertainty with Allais's Rejoinder, ed. by M. Allais and O. Hagen, 1979, Reidel. V. Smith reported two experiments he conducted in 1962 and 1964 in "An Experimental Study of Competitive Market Behavior," Journal of Political Economy, 70, pp. 113-137.; and "The Effect of Market Organization on Competitive Equilibrium," Quarterly Journal of Economics, 78, pp. 181-201. In the 1950s and 1960s, some game experiments were conducted by psychologists, game-theorists, and economists to investigate behavior in

experimentation has been transformed "from a seldom encountered curiosity to a systematic investigation."[46] Many laboratory experiments have been conducted in many different fields such as individual decision-making (Kahneman and Tversky), bargaining (Roth), auction (Kagel), public goods (Marwell, Kim, and Walker), coordination problems (Oches), financial and asset markets (Sunder), and market equilibrium (Smith, and Plott).[47] It is now widely accepted that economics, like the physical and biological sciences, is an experimental discipline: the experimental method can be used in fact finding, theory suggesting, and policy recommendation. Laboratory experimentation, along with game theory, has become a powerful tool in economic investigations. The experimental approach in economics was not only prophesied by economic methodologist Morgenstern, but is in line with the spirit of logical positivism, which emphasized the important role of experiments in testing theory and suggesting hypotheses (Carnap and Hempel). As Abraham Kaplan said: "the argument that we cannot experiment in behavioral science because the problems are too complex is no more than a blanket rationalization of our ignorance as to what experiments to perform, and how to go about performing them."[48]

1.6 Radical Implications for China's Economic Research

We have argued that the methodology of mainstream economics cannot be

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the context of the "prisoner's dilemma." See Sidney Siegel and Lawrence Fouraker, Bargaining and Group Decision-Making (NY: McGraw-Hill, 1960); James Friedman, "Individual Behavior in Oligopolistic Markets: An Experimental Study," Yale Economic Essays, 3, pp. 359-417., and "An Experimental Study of Cooperative Duopoly," Econometrica, 35, pp. 379-397.

[46]Roth (1987), Laboratory Experimentation in Economics, p. 1.

[47]These laboratory experiments were largely conducted in the University of Arizona, University of Pittsburgh, and California Institute of Technology. For reports of these experiments, see the bibliography of this work.

[48]A. Kaplan (1964), The Conduct of Inquiry, p. 166.

separated from logical empiricist philosophy, and that the development (or practice) of modern economic analysis cannot be separated from its methodology and its epistemological foundations. From this investigation, some radical implications can be drawn for China's economic research (chapter 5). Of course, intellectual freedom, political tolerance, and liberal ideologies play an important part in the development of science, including economics. In the last forty years, external conditions have thus been unfavorable for the development of economic science in China. Besides these factors, I believe, there is something (e.g. philosophical tradition, methodology) much deeper and more fundamental, that impedes the progress of economics in China.[49] We should not ignore the fact that China, unlike the Western countries, does not have the analytic and experimental tradition (see Joseph Needham's classic treatment of Chinese history); metaphysical and speculative philosophies have been dominant for more than two thousand years. Many important concepts in Chinese philosophy, such as "Tao", "Yin Yan", and "Qi", have not been, and probably will never be, clearly analyzed and understood. Vagueness is the fundamental feature of the Chinese traditional philosophy. Marxism has replaced traditional Chinese philosophy and become dominant since the Communist Party took power in 1949. But Marxism itself is also metaphysical and speculative in ontology, epistemology, and its conception of history. Empiricism cannot flourish under the Chinese Communist regime, since Lenin in his Materialism and Empirio-criticism (1908) declared war against all varieties of empiricism. Empiricist philosophers such as Richard Avenarius, Helmholtz, Mach, and Poincaré were severely attacked. Politically, empiricist philosophy was proclaimed as a form of bourgeois idealism. But Lenin forgot a fundamental fact: that the founders of Marxism were firmly in a line with empiricism and positivism (Karl Marx was listed

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[49]This was shortly discussed in my article "Ten Major Changes in China's Economic Research," in Beijing Review, (49), 1985, pp. 17-20.

as a precursor of logical empiricism in the manifesto of the Vienna Circle). It was Marx who undertook massive empirical, scientific investigations of modern capitalist economic systems and ancient societies; it was Marx's collaborator, Friedrich Engels, who claimed that what remains for philosophy is logic and epistemology after the rejection of all traditional metaphysical philosophies. Unfortunately, Materialism and Empirio-criticism, which radically deviated from Marx's positivist and empiricist spirit, had become a "Philosophical Bible" for about half century in China (and in the former Soviet Union and Eastern Bloc countries). Consequently, the positivist movement in which logical empiricists strove to clarify the foundations of knowledge penetratingly, to accomplish the task of logical rigorousness and cogent argumentation has never been initiated in China. In the last fifteen years, some Chinese philosophers have even attempted to resurrect the older metaphysics. It seems to me that this is again a philosophical dead end. The backwardness of China's economic research is at least partly due to the tradition and the influence of speculative philosophy. Positive economics must be epistemologically founded on positivist/empiricist philosophy.

Besides the philosophical tradition, there are some methodological issues here. First, most Chinese economists have a firm belief that there is a radical distinction between economics, as a social science, and natural science. They have denied any methodological unity of science. This belief led them for a long time to reject the application of mathematics in economic research. A popular slogan in China was that "you cannot put human nature in the mathematical equation." But as we have already argued, the increased respect for economics as a separate scientific discipline since World War II has been largely due to the massive application of rigorous mathematical and statistical tools. Logic and mathematics not only make the presentation of economic theory more concise and more precise, but also are powerful tools in the analytical transformations because "the human mind is too weak, to recognize

immediately all implications in a given set of propositions."[50] This firm belief also has led (and still leads today) most Chinese economists to reject the possibility and significance of laboratory experimentation in economics. Of course, laboratory experiments in economics, as in any other science, have their limits because of the idealized situations, but one cannot deny their significance in theory testing, theory suggesting, and policy recommendation. Today, experimental economics is the most promising field in economics; it has already taken off on the Continent, in Great Britain, and in the United States. Second, most Chinese economists, influenced by Marxian economics, have not made a sharp distinction between positive and normative economics, facts and values, "what is" and "what it ought to be." We have already seen that the mainstream economists (such as Friedman, George Stigler, and Gottfried Haberler) have persistently drawn such a distinction and exclude any normative judgments from positive economics; they have attempted to "purify" economic science from any ideological or ethical "pollution." Third, unlike the mainstream economists who argue that improved predictions of economic behavior and events are the main or primary task of the economist, most Chinese economists have insisted that the main task of the economist is to provide explanations for economic behavior. It seems to me that the central weakness of Chinese economics is its reluctance to produce theories that yield unambiguously testable implications.

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[50]Morgenstern (1936), "Logistics and the Social Sciences," in Selected Economic Writings of Oskar Morgenstern, ed. by Andrew Schotter (1976), p. 393.

Chapter 2. A Philosophical Revolution: The Origin, Evolution, and Spirit of Logical Positivism

2.1 The Origin and Evolution of Logical Positivism

The philosophical movement known as logical positivism was originated in the 1920s among the members of the "Vienna Circle," or "Der Wiener Kreis," in Vienna.[51] The circle was established in 1922 when Moritz Schlick, succeeding philosophically interested physicists Ernst Mach and Ludwig Boltzmann, took over the chair for the Philosophy of the Inductive Sciences at the University of Vienna. It is historically understandable why this philosophical movement originated and was cultivated in Vienna rather than elsewhere. Vienna was the place where liberalism became the dominant political current after the second half of the nineteenth century. In this liberal atmosphere, intellectual thinking flourished. Literature, art, and music were first-rate in Vienna. Renown scholars occupied leading positions in psychoanalysis (Sigmund Freud), individual psychology (Alfred Adler), theory of law (Hans Kelsen), and economics (Carl Menger and his Austrian School: Friedrich von Wieser, Eugen von Böhm-Bawerk, Joseph Schumpeter, Ludwig von Mises, and Friedrich von Hayek); the philosophical achievement of logical positivism was only one of many. Moreover, by contrast with Germany, where speculative philosophies such as Kantianism and Hegelianism had been dominant since the early 18th century, Vienna had a long tradition of positivism and empiricism. Franz Brentano, a Catholic priest, who considered the critique of language as the starting-point in philosophy, taught at the University of Vienna (1874-80). It was this educational institute that created a

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[51]The phrase "Der Wiener Kreis" was coined by its leading member, Otto Neurath. Since Moritz Schlick was the founder and the center of the Vienna Circle, it was also called the "Schlick Kreis" in Vienna.

chair for the philosophy of the inductive sciences for the physicist-philosopher Mach, succeeded by Boltzmann and Adolf Stöhr; all of them held decidedly empiricist views and took antimetaphysical attitudes. Finally, the Viennese intellectual life in the early decades of this century was characterized by the establishment of various circles, seminars, and colloquia in mathematics, economics, and philosophy. For example, the mathematician Karl Menger (the son of Carl Menger) formed a Mathematical Colloquium (participants including Franz Alt, Gustav Bergmann, Kurt Gödel, Hans Hahn, Oskar Morgenstern, John von Neumann, and Abraham Wald); the Austrian economist Ludwig von Mises founded a Private Seminar (participants: von Hayek, Fritz Machlup, Gottfried von Haberler, Felix Kaufmann, Morgenstern, Rosenstein-Rodan, K. Menger, Alfred Schütz, Richard von Strigl, Karl Schlesinger, Gerhard Tintner, 1923-34)[52]; others included Hans Mayer's Seminar and Hayek's Circle. Schlick's circle was just one of them.

What distinguishes this philosophical movement from any other (e.g. Kantianism, Hegelianism) is the striking fact that it was led not by *professional* philosophers. In fact, almost all of them denied that they were philosophers. The Vienna Circle consisted of no "pure" philosopher; all its members were *scientists* by training or profession.[53] Schlick, the founder and the leader of the circle, had specialized in theoretical optics; his doctoral dissertation was written under the direction of Max Planck in Berlin and his monograph "Space and Time in Contemporary Physics" (1917) provided the first philosophical interpretation of the Theory of Relativity. Schlick also enjoyed personal contacts with leading figures in the

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[52]Cf. Stephan Böhm (1984), "The Private Seminar of Ludwig von Mises," pp. 1-4.

[53]The Vienna Circle was described as follows in its manifesto, The Vienna Circle of the Scientific Conception of the World: "[n]ot one of the members is a so-called 'pure' philosopher; all of them have done work in a special field of science." See Neurath (1973), Empiricism and Sociology, p. 304.

exact sciences such as Planck, Albert Einstein, and David Hilbert.[54] Rudolf Carnap and Philipp Frank, like Schlick, came to philosophy through physics. Carnap had been trained as a physicist and mathematician (under the guidance of the great mathematician, unfortunately not well-known at that time, Gottlob Frege in Jena); his doctoral dissertation, Der Raum (1921), was on the philosophical foundations of geometry: the nature of space. Frank, as the successor of Einstein, was professor of physics at the University of Prague. Kurt Gödel, Hans Hahn, and Karl Menger were professional mathematicians; Hahn was a professor, Gödel and Menger were lecturers, of mathematics at the University of Vienna; Menger himself became a member of the circle after 1927 when he worked with the famous mathematician L. E. J. Brouwer (his most celebrated Fixed Point Theorem has been widely applied in modern economic analysis, e.g. in the proof of the existence of the general equilibrium in microeconomics by Arrow and Debreu and of the Nash equilibrium in game theory by John Nash and others). Bergmann (a student of Menger), Herbert Feigl and Friedrich Waismann (distinguished students of Schlick), also had extensive, advanced training in the formal and physical sciences. Other members who had done work in fields of social sciences included: Felix Kaufmann, a lawyer, mathematician and methodologist of social science; Victor Kraft, a historian at the University of Vienna; and Otto Neurath, an economist and sociologist.

It is also interesting to note that members of the Vienna Circle did not share any distinct philosophical *system* or *doctrine*.[55] This is not surprising because they

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[54]In a letter to Schlick (August 10, 1921), Einstein wrote: "[t]his morning I read your article about [Ernst] Cassirer with true enthusiasm. I have not read anything so clever and true in a long time." See J. Coffa, The Semantic Tradition from Kant to Carnap (Cambridge University Press, 1991), p. 189. Also cf. Kraft (1953), The Vienna Circle, p. 3.

[55]This was confirmed in Joergensen (1940), The Development of Logical Positivism, in the Foundations of the Unity of Science, vol. 2, p. 847; Kraft (1953), The Vienna Circle, p. 15; Blanshard (1962),

originally came from different disciplines of scientific inquiry and from different philosophical orientations. There were no masters nor disciples in this philosophical movement. All of them were critical, independent, and tough-minded thinkers. In the circle there was a "left [radical] wing," formed by Neurath, Carnap, and Hahn, and a "right [more moderate] wing" by Schlick and Waismann.[56] There was no uniformity of views on the theory of probability: the only general agreement was that the calculus of probability is a branch of pure mathematics. Nor was there a common attitude toward the thesis of Ludwig Wittgenstein: Schlick and Waismann were his enthusiastic followers, while Neurath and Carnap quite often took a critical attitude to the decided "metaphysical tendencies" of the Tractatus Logico-Philosophicus (1922).[57] What unified logical positivists in a philosophical movement was a set of *attitudes*, *distastes*, and *approaches*.[58] First, all of them were scientists and admirers of science. They

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Reason and Analysis, p. 93; and Stegmüller (1960), Main Currents in Contemporary German, British, and American Philosophy, p. 259.

[56]Cf. Neurath's "Unified Science and Psychology," where he said: "the author of the present paper has taken the *radical* position within the framework of the 'Vienna Circle' that one cannot make even preliminary remarks in anything other than a physicalistic language" (see McGuinness ed., Unified Science, p. 275. Italcs added). This was confirmed in the article "Ayer and the Vienna Circle" by Tscha Hung, who was a member of the Vienna Circle (the article was collected in The Philosophy of A. J. Ayer, ed. by Lewis Edwin Hahn, Illinois, Open Court, 1992, p. 288); also cf. Kraft (1953), The Vienna Circle, p. 15.

[57]In the article "Unified Science and Psychology," Neurath wrote: "[i]t is possible to accept Wittgenstein's theory of truth and truth-functions and his extremely fruitful approach to the radical analysis of language and nevertheless to reject decisively and without reservation his attempt to legitimize at least provisionally some form of idealistic, even mystical metaphysics in an indirect way, via preliminary elucidations. A few sentences from the Tractatus should be sufficient proof of these tendencies: 'Objects make up the substance of the world. That is why they cannot be composite...A picture is a model or reality...The simplest kind of proposition, an elementary proposition, asserts the existence of a state of affairs...The sense of the world must lie outside the world.'" (see McGuinness, Unified Science, pp. 274-75.)

[58]This was again confirmed in the manifesto: "[t]he scientific world conception is characterized not so much by theses of its own, but rather by its basic *attitude*, its points of view and *direction* of research." "[o]ver the years a growing uniformity appeared; this too was a result of the specifically scientific *attitude*: 'what can be said at all, can be said clearly;' if there are differences of opinion, it is in the end possible to agree, and therefore agreement is demanded." See Neurath (1973), Empiricism and Sociology, pp. 305-306, 304. Italics added.

tended to rank science over the knowledge of philosophy. "Whereas philosophy in the middle ages was to become the handmaid of theology, it is now to become the handmaid of science," as one critic remarked.[59] Philosophy is no longer conceived as "the Queen of Sciences" or "the Science of Sciences." Philosophy is not even a science, but a servant of sciences. The principal task of philosophy is no more than the clarification of the concepts, laws, and theories of science, ranging from logic and mathematics, through physics, chemistry, and biology, to psychology, sociology, and economics. Influenced by Wittgenstein, they insisted that philosophers do not make any propositions, but "make propositions clear." "The object of philosophy is the logical clarification of thoughts."[60] Second, all of them completely rejected speculative and metaphysical philosophies. They avoided unnecessary ontological commitments or epistemological mysteries. Metaphysics, once regarded as the queen of philosophy, was again brought before the bar of reason. The metaphysician was no longer regarded as a wise man who revealed the secret of the Universe to the lay public, but treated as the criminal, as well as the victim, of logical error or misuse of language. They believed that philosophy had made little progress whatsoever for it traditionally had suffered from conceptual confusion and the metaphysical "disease." Third, they entirely agreed with Russell that "the study of logic becomes the central study in philosophy: it gives the method of research in philosophy, just as mathematics gives the method in physics."[61] The logic here, of course, is not the old logic, the Aristotelian logic, but the new logic, modern mathematical logic. As Russell said: "the old logic put thought in fetters, while the new logic gives it wings."[62] All of them

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[59]Quoted in Blanshard (1960), Reason and Analysis, p. 93.

[60]Wittgenstein (1922), Tractatus Logico-Philosophicus, 4.112

[61]Quoted in Schilpp (1963), The Philosophy of Rudolf Carnap, p. 13.

held the view that the most appropriate tool for the philosophical analysis of empirical science, as well as the best weapon against speculative metaphysics, is modern mathematical logic. In short, there is something fundamental and essential uniting them: philosophy must be *scientific*.

The last significant feature of this philosophical movement, compared with any other in the history of philosophy, is that members of the Vienna Circle had a much clearer idea of the fundamental difference between a revolution *on paper* and a revolution *in philosophy*, and of how to go about effectively transforming the former into the latter.[63] At the very beginning, the circle was no more than a local debating *club*: members found they had a "common interest" in, and a "similar approach" to, a certain set of philosophical problems and met regularly once a week (on Thursday evening) to discuss them in an institute of the University of Vienna. These philosophical problems mainly included the foundations of logic and mathematics, and the logical structure of empirical science, especially physical sciences. Occasionally behavioral sciences such as psychology and sociology were also a part of the subject matter. But by 1928 logical positivists had become convinced of their "philosophical *mission* in the world."[64] From that time it had been gradually transformed from a local club into a *well-organized* movement through a series of self-consciously cooperative and collaborative campaigns.

First, the Ernst Mach Society ("Verein Ernst Mach"), with Schlick as its president, was founded (mainly by Neurath) in November 1928, and it was from this society that the Vienna Circle spoke to a wider public. The society, as stated in its

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[62]Russell, Our Knowledge of the External World (Open Court Publishing Company, 1915), p. 59.

[63]This idea was originated from and pushed further by sociologist Otto Neurath.

[64]Herbert Feigl (1974), "Not Pot of Message," in Mid-Twentieth Century American Philosophy: Personal Statements, p. 127.

program, wished to "further and disseminate the scientific world-conception," and to "create the intellectual instruments of modern empiricism." It organized lectures and publications "about the present position of the scientific world-conception, in order to demonstrate the significance of exact research for the social sciences and the natural sciences."[65] The basic orientation of the society was "science free of metaphysics." With the establishment of the Mach Society, the positivist movement first became more *organized* and *institutionalized.*

Second, The Scientific Conception of the World: The Vienna Circle, the manifesto or "the declaration of independence" of the logical positivist movement, was published in 1929.[66] It is a product of teamwork. The pamphlet was written by Neurath, cooperated in by Carnap, revised by Hahn, and aided by Feigl and Waismann. It was dedicated to Schlick, who returned from his visiting professorship at Stanford University, as a "token of gratitude and joy at his remaining in Vienna." In this manifesto, the Vienna Circle proclaimed the birth of a new philosophy ("scientific world-conception") and the end of all traditional philosophies. This small programmatic pamphlet gave a brief account of the intellectual heritage (e.g. Einstein, Russell, and Wittgenstein were listed as "the leading representatives" of the Circle's scientific outlook) and the philosophical position of the Vienna Circle, as well as a brief review of some foundational (i.e. philosophical) problems in mathematics, physics, biology, psychology, and social sciences. Thus, it provided the philosophical community with a new world-conception (modern empiricism), a new method of philosophizing (modern mathematical logic), and a set of foundational problems

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[65]All are from the manifesto, collected in Neurath (1973), Empiricism and Sociology, p. 305.

[66]Wittgenstein was not happy about the manifesto. He wrote to Waismann: "'Renunciation of Metaphysics!' As if *that* were something new! What the Vienna School has achieved, it ought to *show* and not *say*...." Cf. Wittgenstein and the Vienna Circle, conversation recorded by Friedrich Waismann, ed. by Brian McGuinness (Basil Blackwell, 1979), p. 18.

stemming from the formal and empirical sciences; in a word, a new philosophical *research program*, or *paradigm*.

Third, the Vienna Circle, joined with the Society for Empirical Philosophy (also called the Berlin Group, led by the physicist-philosopher Hans Reichenbach and the mathematician Richard von Mises), organized its first international congress at Prague in 1929; the further congresses were held for the epistemology of the exact sciences at Königsberg in 1930, for the scientific philosophy at Paris (1935), for the problem of causality at Copenhagen (1936), and for the planned international encyclopedia of unified science at Cambridge, England (1938). The last congress, for the unity of science and methodology of special science, was held in 1939 at Harvard University, Cambridge, Massachusetts. These congresses led to many valuable contacts and exchanges with some groups and individuals that shared the general scientific outlook of the Vienna Circle, such as the Berlin Group, the Lwow-Warsaw Group, the Uppsala School, the Münster Group, British analytic philosophers, and American pragmatists and operationalists.

Fourth, in 1930 the periodical Erkenntnis, jointly edited by Carnap and Reichenbach, became the principal *organ* of the logical positivist movement. It published many important and influential papers, such as Carnap's "The Elimination of Metaphysics Through Logical Analysis of Language" and "The Old and the New Logic," Schlick's "The Turning Point in Philosophy," Hahn's "Logic, Mathematics and Knowledge of Nature" and Neurath's "Protocol Sentences," to name but a few. It was mainly through this medium that the ideas of logical positivism were spread over the whole academic world. In the middle 1930s, a group of monographs collectively entitled Unified Science, edited by Schlick and Frank, was published. In 1938 the International Encyclopedia of Unified Science, under the editorship of Neurath, Carnap, and Charles Morris, also began publication at the University of Chicago. All

these extensive publishing activities contributed greatly to the transformation of Viennese positivism into an international philosophical movement.

Finally, logical positivism was internationally disseminated also through *personal* channels. Members of the Vienna Circle visited many Western countries with the missionary spirit. In 1930 Feigl visited Harvard and studied with P. W. Bridgman, the great physicist and the founder of Operationalism; Schlick visited Stanford in 1929, and Berkeley and the University of London in 1931; Carnap also visited the University of London in 1933. Roughly at the same time, many philosophers who were attracted to Viennese logical positivism returned to their native countries with the program of the new philosophy. They included Carl Hempel from Germany, Morris, Ernest Nagel, W. V. Quine, and Albert Blumberg from the United States, Alfred Ayer from England, Eino Kaila from Finland, Alfred Tarski from Poland, H. A. Lindemann from Argentine, and Tscha Hung from China. Ayer's Language, Truth and Logic (1936) provided an authoritative and a most popular exposition of "the classical position" of logical positivist doctrine. The book has been described as possessing "almost the status of a philosophical Bible" in Great Britain.[67] All of these activities sketched above furthered the ambition of the Vienna Circle, transformed a revolution on paper into philosophy, and developed logical positivism as an international movement.

But the Vienna Circle started to dissolve as it reached its highest peak in the early 1930s. First, Feigl accepted a professorship in the University of Iowa in 1931 and subsequently in the University of Minnesota. Then, Hahn, a leading member of the circle, died prematurely in 1934. Finally, Schlick, the leader of the circle, was tragically murdered by a mentally deranged student, and Carnap, the most gifted member of the circle, accepted a position in the University of Chicago, in 1936. The

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[67]Quoted in Joad (1950), A Critique of Logical Positivism, p. 15.

meetings of the circle were discontinued in 1936, and the circle itself, as well as the Mach Society, ceased to exist in 1938 when Austria was forcibly annexed to Nazi Germany. Most members of the circle (some being Jews) were forced into exile for political reasons. Neurath took refuge in Holland, at the Hague, to continue the organization of the International Unity of Science movement; Waismann (first at Cambridge, then Oxford) in England and Karl Popper (who was loosely affiliated with the circle) at the Canterbury University College in Christchurch in New Zealand; Frank (Boston, then Harvard), Gödel (Princeton), Hempel (Yale), Menger (Illinois Institute of Technology), and Kaufmann (the New School for Social Research) in the United States. The heyday of the Vienna Circle was past by 1936. But the tradition, spirit, and program of logical positivism were continued in England, Scandinavia, the United States, and many other countries. In Great Britain logical positivism developed alliance with the Oxford movement of linguistic analysis and Cambridge analytic philosophy. Logical positivism found its real home in the United States, where it was allied with the Pragmatism (C. I. Lewis), Operationalism (Bridgman), and Behaviorism (John Watson and Clark Hull).

2.2 The Synthesis of the New Logic and Empiricism

The philosophical position (or "the scientific world-conception") of the Vienna Circle can be best described as "*logical empiricism.*"[68] This expression has been suggested and used at least by its leading members such as Schlick, Carnap, and Neurath. It is *logical* because they took modern mathematical logic as the "rigorous,

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[68]The term "logical positivism" was coined by Feigl, in collaborating with Blumberg, in the well-known article "Logical Positivism: A New Movement in European Philosophy" (1931). Other labels have also been used: "consistent empiricism" (Schlick, Hahn), "scientific empiricism" (Carnap), "neo-positivism" (Kraft, Richard von Mises). But most members of the circle prefer the characterization "logical empiricism."

scientific method of philosophizing;"[69] it is the *method* of logical analysis that "essentially distinguishes recent empiricism and positivism from the earlier version that was more biological-psychological in its orientation."[70] It is *empiricism* because they recognized that knowledge comes neither from "pure thinking" nor from "pure intuition," but only from our experience. They denied the possibility of any factual *à priori* knowledge. This philosophical position was clearly proclaimed in the manifesto--The Scientific Conception of the World: The Vienna Circle: "[w]e have characterized the *scientific world-conception* essentially by *two features*. *First*, it is *empiricist and positivist*: there is knowledge only from experience, which rests on what is immediately given. This sets the limits for the content of legitimate science. *Second*, the scientific world-conception is marked by application of a certain method, namely *logical analysis*. The aim of scientific effort is to reach the goal, unified science, by applying logical analysis to the empirical material."[71] Of course, these two distinct features can be historically traced to the older empiricism (represented by Hume, Mill, and Comte) and the new logic (contributed by Frege, Russell, and Wittgenstein). But logical positivism is not simply a combination of both. It needs to radically *revise* the doctrine of the older empiricism and draw significantly some philosophical *implications* from modern logic. We begin with modern mathematical logic.

Modern logic, mathematical logic, played a leading role in the formulation of logical positivism. We must note first that professional mathematicians (e.g. Gödel, Hahn, Menger, and Radakovic) formed a large portion of the Vienna Circle, and that other members of the circle, such as Carnap, Feigl, Kaufmann, Neurath, Schlick,

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[69]Carnap (1931), "The Old and the New Logic," in Ayer (1959), Logical Positivism, p, 133.

[70]Cf. Neurath (1973), Empiricism and Sociology, p. 306.

[71]The manifesto is included in Neurath (1973), Empiricism and Sociology, p. 309. Italics original.

Waismann, and Zilsel, were also mathematically competent.[72] This background reinforced their tendencies towards logical rigor and precision. The regular group discussion of the circle was highly mathematically oriented. Hahn organized a seminar to study the Principia Mathematica, and Wittgenstein's Tractatus Logico-Philosophicus was discussed page by page among the members of the circle.[73] Logical positivists initiated and undertook most of the *logical analysis* of the formal and empirical sciences (i.e. mathematics, physics, and psychology) and the traditional philosophical problems. It is also surprising to note that many of the mathematical logic textbooks and monographs were first written by logical positivists.[74] This is why the new logic had great significance for the members of the Vienna Circle: modern logic is "*the method* of philosophizing." Thus, the philosophy of the Vienna Circle has been given the various labels: "*logistic* neo-positivism," or "*logical* positivism," or "*logical* empiricism."

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[72]Cf. Neurath (1932), "Unified Science and Psychology," in Unified Science, ed. by McGuinness (1987), endnote 2, p. 275. "All members of the Vienna Circle were strongly influenced by logicians: Schlick, Hahn, and Frank especially by Russell, Carnap by Frege, and Neurath by Gregorius Itelson and Schröder. Of the younger members, Waismann was influenced primarily by Wittgenstein, and Feigl especially by Schlick, while Gödel, as a mathematician, has made important contributions to the Circle by pursuing certain logical investigations, which are of importance to the question whether a language about language is possible within a given language." As for Neurath's knowledge of modern logic, I should mention that a paper jointly written by him and Olga Hahn (Hahn's blind sister, later Neurath's wife) was collected in A Survey of Symbolic Logic, ed,. by C. I. Lewis (Berkeley, 1918). Lewis ranked the paper among those "that are considered the most important contributions to symbolic logic" (see Hahn, Empiricism, Logic, and Mathematics, introduced by K. Menger, p. xviii). As for Kaufmann's knowledge of logic, see his The Infinite in Mathematics. Logico-mathematical Writings. Ed. by McGunness, Intro. by E. Nagel. Vienna Circle Collection # 9.

[73]Cf. K. Menger's introduction of Hahn (1980), Empiricism, Logic, and Mathematics, p. xiv.

[74]Carnap, "The Old and the New Logic" (1931); Introduction to Semantics (1941); Formalization of Logic (1942); Introduction to Symbolic Logic and Its Applications (1958). Joergensen, A Treatise of Formal Logic (1931). Menger, The New Logic (1933). Quine, Mathematical Logic (1940). Reichenbach, Elements of Symbolic Logic (1966). Waismann, Introduction to Mathematical Thinking (1951).

Since the middle of the 19th century logic has been reconstructed far beyond traditional logic, the older Aristotelian logic. The reconstruction of logic was inspired by the need for a critical re-examination of the foundations of mathematics, e.g. the appearance of logical *antinomies* in mathematics. It was initially effected in George Boole's development of the *calculus of classes* (also called algebra of logic or calculus of logic) in his Mathematical Analysis of Logic (1847) and Laws of Thought (1854); then greatly extended in Frege's formulating a complete axiomatization of the *first-order logic* (Conceptual Notations, 1879) and his precise formalization of the *logic of relations* (Foundations of Arithmetic, 1884); and finally comprehensively achieved in Whitehead and Russell's monumental book Principia Mathematica (1910-13), for all further contributions to the new logic depend upon this work, either as supplements or revisions. Thus, mathematical logic is historically a branch of mathematics, and at the beginning it was cultivated by mathematicians rather than philosophers. The new logic has been further cultivated by students of Russell (Wittgenstein, Ramsey), by the Göttingen School (Hilbert, Ackermann, Bernays, Behmann), by the Warsaw School (Lukasiewicz, Lesniewski, Chwistek, and Tarski), by American philosophers (Quine, Nagel, and Goodman), and of course by logical positivists.

The new logic has been differently named "mathematical logic," or "symbolic logic," or "logistics."[75] It is *new* because it is fundamentally different from the old logic in *form* and *content*, rather than in subject matter. First of all, the new logic appears in symbolic garb, it employs a *symbolic language*. In the older logic, symbols have been used only as abbreviations or auxiliaries. The use of symbolic forms in modern logic appears similar to those of mathematics. In fact, it is itself a branch of mathematics. In logic, the advantage of the symbolic method of representation over

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[75]Cf. D. Hilbert and W. Ackermann, Principles of Mathematical Logic, ed., by Robert Luce (New York: Chelsea Publishing Company, 1950), p. 1.

verbal language has been commonly acknowledged. It makes presentation more concise and more precise. The sentence "if one number is multiplied by another, the result is the same as that obtained by multiplying the second by the first" can be more clearly and more rigorously presented in symbolic form "(x, y).x.y=y.x." Moreover, symbolic logic has greater power in analytic transformations. Suppose we are given the problem: "if Peter were five years younger, he would be twice as old as Paul was when he was six years younger, and if Peter were nine years older, he would be thrice as old as Paul, if Paul were four years younger."[76] This problem can be easily solved by writing down the equations, resulting from the symbolization of it (solution: Peter is 21 years old, Paul 14). Finally, it makes assumptions and premises *explicit*; it guarantees that "no unnoticed assumptions will slip into the deduction, a thing which it is very difficult to avoid in a word-language."[77] In short, the *rigor* of deductions and inferences can only be obtained by employing symbolism in logic.

The new logic is essentially distinguished from the old not only by symbolic form, but also by *content*. The old logic is significantly poorer in content because its exclusive concern is statements with a very *simple structure*: subject-predicate form, e.g. "All Greeks are men," or "All men are mortal." It maintains that all relational sentences can be conceived as the sentences of predicative form. Strangely enough, Aristotle and his disciples have never recognized this limitation of the old logic, which was only pointed out after nearly two thousand years by Leibniz. In the old logic, from two premises "all men are mortal" and "Socrates is a man," we can draw the conclusion "Socrates is mortal." However, the old logic does not permit us to draw inference from the premise "All horses are animals" to the conclusion "All heads of

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[76]This example was given in Reichenbach's The Rise of Scientific Philosophy (University of California Press, 1953), p. 219.

[77]Carnap (1931), "The Old and the New Logic," in Ayer (1959), Logical Positivism, p. 136.

horses are heads of animals"; from "James is the father of John (Stuart Mill)" to "John is the son of James." But one important domain of modern logic is the *theory of relations*. It concerns pairs (triplets or groups) of members in addition to individual members themselves. The theory of relations is especially indispensable for the sciences that deal with series and orderings such as arithmetic (number series), geometry (point series), and physics (all scales of measurement).[78]

The significance of mathematical logic for philosophical analysis was initially discovered by Frege and Russell. The new logic, in Russell's opinion, has "introduced the same kind of advance into philosophy as Galileo introduced into physics, making it possible at last to see what kinds of problems may be capable of solution, and what kinds must be abandoned as beyond human powers."[79] But it was emphasized and pushed furthered by logical positivists. They believed that mathematical logic is indispensable for scientific philosophy just as mathematics for modern physics. By the methods of mathematical logic, analysis can show that "many philosophical concepts do not satisfy the higher standards of rigor; some have to be interpreted differently and others have to be eliminated as meaningless."[80] More specifically, the logical positivists' application of the new method to philosophy leads to a negative result and a positive result.[81] The *negative result* is worked out in the domain of metaphysics and value theory: the alleged statements in the domain are deemed entirely meaningless (see section 3). The *positive result* is that various scientific concepts, laws, and statements are critically analyzed and clarified (section 4).

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[78]Ibid., p. 138.

[79]Russell (1915), Our Knowledge of the External World, p. 59.

[80]Carnap, "The Old and the New Logic," in Ayer (1959), Logical Positivism, p. 137.

[81]The "negative result" was discussed in great detail in Carnap (1932), "The Elimination of Metaphysics Through Logical Analysis of Language," in Ayer (1959), Logical Positivism, p. 60.

In his search for the foundation of mathematics, Frege had already come to the conclusion that arithmetic is a branch of logic. According to him, arithmetic, without the need of any axioms, can be deduced from pure logic. Thus, Kant's assertion that "7+5=12" is synthetic is disproved. The idea that arithmetic is part of logic was confirmed in the Principia Mathematica. But Frege's work was not final, because it "applied only to arithmetic, not to other branches of mathematics."[82] Moreover, his premises "did not exclude certain contradictions to which all past systems of formal logic turned out to be liable." These two defects were remedied by Whitehead and Russell in their book. They showed that "from certain ideas and axioms of formal logic, by the help of the logic of relations, all pure mathematics can be deduced, without any new undefined ideas or unproved propositions." Specifically, it was shown (1) every mathematical *concept* can be derived from the fundamental concepts of logic, and (2) every mathematical *sentence* can be derived from the fundamental statements of logic.[83] Since mathematics is part of logic, attention was naturally focused on the nature of the propositions of *logic*.

It was Wittgenstein, in his celebrated Tractatus Logico-Philosophicus, new in conception and cryptic in form, who discovered the *tautological* nature of logic and mathematics. According to him, logic contains not the fundamental laws of being, but the fundamental laws of *symbolizing* the world. Thus, the propositions of logic are "*analytic*," in the Kantian sense that they "express nothing in the predicate but what has

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[82]This quote and the following two are from Russell's "Logical Atomism," in Ayer (1959), Logical Positivism, p. 33.

[83]Arithmetic concepts (e.g. numbers) and the concepts of analysis (e.g. limit, derivative, integral, continuity) can be derived from the fundamental concepts of logic, such as "negation", "or", "and", "all", "some", "identical". Also the arithmetic sentence "2+2=4" can be derived from a sentence of pure logic (i.e., "If a property f has the cardinal number 2 and a property g has the cardinal number 2, and f and g are mutually exclusive, and if the concept h is the union of f and g, then h has the cardinal number 4."). See Carnap, "The Old and the New Logic," in Ayer (1959), Logical Positivism, p. 141.

been already actually thought in the concept of the subject" (e.g. "all bodies are extended," "all bachelors are unmarried men").[84] "The propositions of logic therefore say nothing," i.e. they add nothing to the *content* of our cognition, they are "tautologies;" and "from a tautology only tautologies follow."[85] As Hahn later observed: "[t]o me, the Tractatus explained the role of logic," the central insight of Wittgenstein's book is that "all logical thought is tautological; it can only help us to say in another way what has already been said, and it can never help us to say anything new."[86] Hence, "there can *never* be surprises in logic," asserts Wittgenstein.

What is true for logic is also true for mathematics because mathematics is part of logic. The propositions of mathematics do not say anything about the empirical world; they cannot be derived from experience; they are tautologies. Every mathematical proof is a succession of tautological transformations. "Mathematical propositions express no thoughts."[87] At the first glance it is very difficult to believe the *tautological* character of mathematics because we invest so much labor to derive mathematical theorems and these theorems so often surprise us. But the term "tautological" implies nothing derogatory to mathematics, for the tautological transformation is extremely significant for our knowledge. The mathematician Hahn gave a good explanation: "we are not omniscient." "An omniscient being, indeed, would at once know everything that is implicitly contained in the assertion of a few propositions."[88] Contrary to Plato, God never does mathematics, nor does it even

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[84]Kant, Critique of Pure Reason, p. 12.

[85]All from Wittgenstein, Tractatus Logico-Philosophicus, 6.1; 6.11; 6.126; 6.1251.

[86]Hahn, "On the Significance of the Scientific World View, Especially for Mathematics and Physics," in Hahn (1980), Empiricism, Logic, and Mathematics, p. 21.

[87]Wittgenstein, Tractatus, 6.21

need for. "An omniscient being has no need for logic and mathematics. We ourselves, however, first have to make ourselves conscious of this by successive tautological transformations, and hence it may prove quite surprising to us that in asserting a few propositions we have implicitly also asserted a proposition which seemingly is entirely different from them, or that we do mean the same by two complexes of symbols which are externally altogether different."

It is this new understanding of logic and mathematics that constitutes a fundamental *revision* of empiricism. Old empiricists such as Mill, Spencer, and Comte reasonably rejected the theses of rationalism and apriorism, but incorrectly claimed that we can derive all knowledge (including that of mathematics and logic) and science from experience. Old empiricists believed like Kant that a mathematical proposition such as "7+5=12" is a synthetical judgment; it increases the given cognition and contributes to the growth of our knowledge. They failed to see the analytical-tautological nature of the propositions of logic and mathematics. Principia Mathematica successfully showed that Kant's example (i.e. "7+5=12") can be reduced to tautology by using Peano's axiomatization of arithmetic. Indeed, "the understanding of logic and mathematics has always been the main crux of empiricism."[89] Mathematical knowledge is *universal*, but experience can *not* provide us with universal knowledge. On the other hand, rationalists discovered the analytical-tautological nature of mathematics and logic, but unfortunately concluded that all our knowledge and science come from "pure reason" or "pure intuition," they are independent of experience. For example, Leibniz claimed that propositions of mathematics are analytic. In a word, they are *à priori*. Kant, the founder of the Critical Philosophy, had a much clearer idea about the problems of both

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[88]This and the following quotes are from Hahn's "Logic, Mathematics and Knowledge of Nature," in Ayer (1959), Logical Positivism, p. 159. Also see Hahn's article, "The Significance of the Scientific World View, " in Hahn (1980), p. 23.

[89]Hahn, "The Significance of the Scientific World View, " in Hahn (1980), p. 21.

empiricism and rationalism. He revolutionized philosophy by his "*transcendental turn*:" a shift from metaphysics to epistemology; or put it in Lewis White Beck's words: "the substitution of epistemic for ontological concepts and principles;" or as Hegel indicates: "the critical philosophy has indeed turned metaphysics into logic."[90] But the fundamental question that Kant asked in the Critique of Pure Reason (1787) is very misleading: "How are *a priori* synthetic judgments [like those of mathematics] possible?"[91]

The propositions of mathematics, according to logical positivists, are neither synthetic nor *à priori*, but simply tautologies. Only logical positivists knew how to combine the insight into the analytic nature of mathematics and logic with the core of empiricism. The rationalist thesis now restricted itself to *formal* knowledge (i.e. mathematics and logic), and the empiricist thesis to *factual* knowledge (i.e. empirical sciences). The conflict between rationalism and empiricism in the history of philosophy was thus dissolved. Empiricism and rationalism (logicism) had been synthesized into a very influential philosophy: *logical empiricism*. Schlick characterized the insight into the nature of logic and mathematics, into "the relationship of logic [and mathematics] to reality and experience," as the "most important step in philosophy."[92] Carnap claimed that the insight into the nature of logic itself, together with Russell's logicism, made it "possible for the first time to combine the basic tenet of empiricism with a satisfactory explanation of logic and mathematics."[93] Hahn indicated that it is this new understanding of logic and mathematics that for the first

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[90]R. Hahn, Kant's Newtonian Revolution in Philosophy (Carbondale: Southern Illinois University Press, 1988), p. 133; also cf. R. Soloman, In the Spirit of Hegel (Oxford University Press, 1983), p. 64.

[91]Kant, Critique of Pure Reason, tran. by N. K. Smith (St. Martin's Press, 1965), p. 55.

[92]See Kraft (1953), The Vienna Circle, p. 196.

[93]Carnap, "Intellectual Autobiography," in Schilpp (1963), The Philosophy of Rudolf Carnap, p. 47.

time makes a "consistent empiricism" (or "pure empiricism" or "scientific empiricism") possible.[94] Finally, Ayer writes: "we come finally to settle the conflict between Idealism and Realism, the dispute between rationalists and empiricists of which we have now finally disposed."[95]

2.3 The Elimination of Metaphysics

Logical positivism has gained a reputation, as well as incurring disrepute, for its complete rejection of metaphysics. It stigmatized all metaphysical statements as devoid of cognitive meanings, including all metaphysical systems of German speculative philosophies, such as Leibniz's "Metaphysical Idealism," Kant's "Transcendental Idealism," Fichte's "Subjective Idealism," Schelling's "Objective Idealism," and Hegel's "Absolute Idealism." The term "metaphysics" used here is to be understood in a wider sense that not only denotes a theory of supernatural objects, but also includes all philosophies that purport to obtain factual knowledge or normative judgments by pure intuition or pure reason. Thus, logical positivism, rejecting all metaphysical, ethical, and theological statements as meaningless, has been labeled "*Neo-Positivism*" (Richard von Mises).

Of course, the idea of rejecting all metaphysics is not very much original in the history of philosophy. The anti-metaphysical attitude can be traced to the skeptics of ancient Greece and the nominalists of the middle ages. The modern empiricist, David Hume, provides an excellent statement of positivism in his Enquiry Concerning Human Understanding (1748). He divides all meaningful statements into two categories, those concerning "relations of ideas" and those concerning "matters of facts." Metaphysics,

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[94]These phrases have been used by the leading members of the Vienna Circle, such as Schlick, Neurath, Carnap, and Hahn.

[95]Ayer (1936), Language, Truth and Logic, p. 32.

containing neither "abstract reasoning concerning quantity or number" nor "experimental reasoning concerning matter of fact and existence," should be committed "to the flames," for it contains nothing but "sophistry and illusion." As Kant observes: "since the origin of metaphysics so far as we know its history, nothing has ever happened which could have been more decisive to its fate than the attack made upon it by David Hume."[96] Hume's refutation of metaphysics is based on his psychological arguments that all our ideas are copies of *impressions*, all knowledge is derived from experience, and metaphysics, the knowledge of the ultimate origin and nature of the universe, is therefore impossible.

Kant also argues that metaphysics as a science is *impossible*, metaphysics is "sophistical *pseudo-science.*"[97] The reason he gives is that "percepts and concepts constitute the elements of all our knowledge." Percepts without concepts are blind, concepts without percepts are empty. The principles of metaphysics *transcend* the limits of experience, "they are no longer subject to any empirical test."[98] "Things-in-themselves" such as Ego or God can not be perceived by the experience, thus we cannot have universal knowledge of anything non-perceivable. It seems to Kant that though metaphysics as a science is *impossible*, it nevertheless remains *necessary*; although we can never have knowledge of the existence of God, freedom, and personal immortality, we have to presuppose them in the realm of practical reason. Kant's fundamental concern is to provide a reconciliation of scientific knowledge and practical values. As he says: "I have therefore found it necessary to deny knowledge in order to make room for faith."[99] In short, Kant destroyed metaphysics in the domain of pure

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[96]Kant, Prolegomena to Any Future Metaphysics (Indianapolis: Hackett Pub. Co., 1977), p. 3.

[97]Kant, Prolegomena to Any Future Metaphysics, p. 107.

[98]Kant, Critique of Pure Reason, p. 7.

reason, but at the same time he attempted to save metaphysics in the domain of practical reason.

Finally, Auguste Comte (1798-1857), the founder of positivism, rejected metaphysics based on historical generalization. According to Comte, each branch of knowledge in its development "is necessarily obliged to pass through three different theoretical states: the theological or fictitious state; the metaphysical or abstract state; and the scientific or positive state."[100] Metaphysics is only the transition stage from theology to positive science. Since the nature of the human intellect had already passed the first two stages and been rendered positive--astronomy, physics, chemistry, and physiology--since social and "moral" studies were approaching the third stage, metaphysical speculation would become obsolete in the last stage (positive science), like the mythical thinking of the first stage. Comte's philosophy reflected the optimism of the Enlightenment that reason had eventually come into its own, and he anticipated the Young Hegelian optimism that "all that is *rational* is *real*." Comte's argument, however, was based neither on logical analysis nor on empirical investigations, but largely on his historical speculation which is fundamentally incompatible with the spirit of modern positivism.

Contemporary logical positivists essentially hold the same antimetaphysical attitude as the earlier positivists did, but their weapon against traditional metaphysics is taken from the arsenal of modern science, mathematical logic. The enemy is still the same but the sword to execute him is sharper. With the techniques of modern mathematical logic, as Carnap says, "a radical elimination of metaphysics is attained, which was not yet possible from the earlier antimetaphysical standpoints."[101] Logical

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[99]Kant, Critique of Pure Reason, p. 29.

[100]A. Comte, Cours de Philosophie Positiv, in Auguste Comte and Positivism: The Essential Writings. Ed. and intro. by Gertrud Lenzer (Harper Torchbooks, 1975), p. 29.

positivists did not declare that the doctrine of metaphysics is "false" or "contradictory," rather asked a simple but tough question: "what do you *mean* by your statement?" The *meaning* (or sense) of the statement is the fundamental concern of logical positivists. As Ayer says: "[t]he originality of the logical positivists lay in their making the impossibility of metaphysics depend not upon the nature of what could be *known* but upon the nature of what could be *said.*"[102]

Logical positivists, like David Hume, claimed that all genuine, *cognitively* meaningful propositions are either analytic or synthetic but not both.[103] Analytical propositions, like those of logic or pure mathematics (e.g. arithmetic, geometry), concern "relations of ideas." They are devoid of factual content because they say nothing about the empirical world. They are true solely by virtue of their form, they are tautologies. Synthetical propositions, concerning "matters of facts," belong to the domain of the empirical sciences. They can be judged true or false according to the criterion known as the *verification principle*. Following Wittgenstein, logical positivists insisted that "the *meaning* of a proposition is the method of its *verification.*" "The sense of a proposition is the way it is verified....To say that a statement has sense means that it can be verified."[104] The verification principle was explained by Schlick, as he says, "the question of what a sentence means is identical with the question: *How*

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[101]Carnap (1932), "The Elimination of Metaphysics Through Logical Analysis of Language," in Ayer (1959), Logical Positivism, p. 61.

[102]Ayer's Editor's Introduction to Logical Positivism (1959), p. 11. Italics mine.

[103]Cf. Carnap's "The Elimination of Metaphysics Through Logical Analysis of Language." He says: "(Meaningful) statements are divided into the following kinds. First there are statements which are true solely by virtue of their form (tautologies)....Secondly there are the negations of such statements (*'contradictions'*)....With respect to all other statements the decision about truth or falsehood lies in the protocol sentences. They are therefore (true or false) *empirical statements* and belong to the domain of empirical science." See Ayer (1959), Logical Positivism, p. 76.

[104]Cf. Wittgenstein and the Vienna Circle, conversation recorded by Friedrich Waismann, ed. by McGuinness (1979), pp. 227, 224-44.

is this statement verified?".[105] He distinguishes between "*empirical impossibility*" (i.e. a proposition is not verifiable for lack of technical means for deciding its truth or falsity) and "*logical impossibility*" (a proposition is in principle in no way whatever empirically decidable). If a proposition fails to be verifiable in this latter (logical) sense, it must be a "pseudo-proposition."

Metaphysics is now held to be *impossible* not because the knowledge of the ultimate origin and nature of universe is beyond the *ability* of human understanding, as Kant rhetorically argued; nor because it is an historical *necessity* that metaphysics will eventually be replaced by positive sciences, as Comte firmly believed. It is simply because metaphysical statements, though offering strong emotive appeal and moral inspirations, fail to meet the empiricist criterion of cognitive meaning--the verification principle--and thus they are pseudo-statements.

Since metaphysical statements lack objective testability, an almost unsolvable problem of *communication* arises: metaphysical concepts cannot be *intersubjectively* understandable. In short, "a proposition that cannot be verified in any way has *no* sense."[106] For logical positivists, "there are no unanswerable questions," there are no such things as the "riddles of the universe" (Ernst Haeckel)!

Since a language consists of a *vocabulary* (a set of words) and a *syntax* (rules of sentence formation), there are, Carnap showed, two kinds of pseudo-statements occurring in metaphysics: (1) "they contain a word which is erroneously believed to have meaning;" (2) the constituent words are meaningful, but they are combined in a way that violates the rules of syntax, thus they do not yield a meaningful statement.[107]

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[105]Schlick, Philosophical Papers, Vienna Circle Collection #11, vol. 2, p. 131. Also see his "Positivism and Realism" in Ayer (1959), Logical Positivism, pp. 87-89.

[106]Cf. Wittgenstein and the Vienna Circle, p. 245.

[107]Carnap (1932), "The Elimination of Metaphysics Through Logical Analysis of Language," p. 61.

First, many metaphysical terms such as "principle," "God," "the Absolute," "the being of being," "absolute spirit," and "the Ego" are devoid of cognitive meaning because there are no empirical criteria that can be given, there is no way to specify under what conditions a sentence that contains such words is supposed to be true, and under what conditions false. Carnap analyzed the metaphysical term "principle" in great detail. This term is widely used because metaphysicians have offered various answers to the question which is the highest "principle of the world" (e.g. water, number, the idea, life). In order to know the meaning of the term "principle," we must ask the metaphysician "under what conditions a statement of the form 'x is the principle of y' would be true and under what conditions it would be false."[108] In short, a definition of "principle" must be given. The metaphysician would reply that "x is the principle of y" means "y arises out of x." This means that we can observe that things or processes of kind x are frequently followed by things of kind y. But the metaphysician says that he does not mean this "empirically observable" relationship (a relation of temporal and causal sequence), which is what the word ordinary means. In this case no criterion is specified for any other meaning. Consequently, the alleged "metaphysical meaning" simply does not exist.

The second kind of metaphysical statements consists of meaningful words, but they violate the syntactical rules. The sentence "Caesar is a prime number" is an example: it looks like a statement yet is not a statement, does not assert anything, expresses neither a true nor a false proposition; this word sequence is a "pseudo-statement." This shows that the grammatical syntax of natural language does not fulfill the task of eliminating senseless combinations of words in all cases. Carnap indicates that many metaphysical statements are not easily recognized as "pseudo-statements"

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[108]Ibid., p. 65.

because of the formation of meaningless sequences of words without violating the rules of grammar. But they nevertheless violate the rules of *logical* syntax. Metaphysical utterances such as Heidegger's "What about this nothing?--The Nothing itself nothings" and Hegel's "pure Being and pure Nothing, therefore, are one and the same" are examples of the violation of logical syntax.

Since the statements of metaphysics are devoid of cognitive meaning, a question that people would naturally ask is how we can explain why "so many men in all ages and nations, among them eminent minds, spent so much energy, nay veritable fervor, on metaphysics if the latter consisted of nothing but mere words, nonsensically juxtaposed?"[109] Also how can we explain that metaphysical books, if containing only combinations of senseless words or only senseless statements, have exerted a strong influence on readers? The reason, as Carnap explains, is that our spiritual activity is not confined to pure reason, but also encompasses such practical reasoning as occurs in art and religion. Metaphysics is "the expression of the general attitude of a person towards *life*." Historically, metaphysics arose from the need to give expression to a man's attitude in life, "his emotional and volitional reaction to the environment, to society, to the tasks to which he devotes himself, to the misfortunes that befall him."[110] Metaphysics is a mixture of science and art, but it blurs the significant difference between science and art. It contributes nothing to the increase of our knowledge, nor does it, compared with art, adequately express our basic attitudes. Metaphysicians are musicians without musical ability. Thus, logical positivists rejected any (e.g. Kant's) attempt to keep metaphysics in the domain of practical reason.

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[109]Ibid., p. 78.

[110]Ibid., p. 78.

What has been said of metaphysics is also largely applicable to traditional value theory, ethics, and aesthetics. All logical positivists wished to dissociate scientific philosophy from moral preaching. They took all value judgments also as cognitively meaningless. Carnap writes: "[e]ither empirical criteria are indicated for the use of 'good' and 'beautiful' and the rest of the predicates that are employed in the normative sciences, or they are not. In the first case, a statement containing such a predicate turns into a factual judgment, but not a value judgment; in the second case, it becomes a pseudo-statement. It is altogether impossible to make a statement that expresses a value judgment."[111] According to the definition of meaningfulness in terms of verifiability, only descriptive sentences can be verifiable; ethical statements are meaningless in the sense that they have no theoretical (cognitive) content. "The objective validity of a value or norm is not empirically verifiable nor deducible from empirical statements; hence it cannot be asserted (in a meaningful statement) at all."[112]

Philosophy traditionally includes such areas as metaphysics, ethics, and epistemology. With the rejection of all metaphysical, ethical, and theological statements, there is not much left for traditional philosophy. The big cake of philosophy is incredibly shrunken. The problems of traditional philosophy can be formulated either as empirical problems, in which case they belong to special sciences; or as metaphysical, ethical or theological questions, in which case they are devoid of cognitive meanings and must be completely eliminated; or as problems of language, meaning, and syntax, in which case they need to be critically analyzed. Thus, what remains for philosophy is *epistemology*, or the theory of knowledge, or what Carnap

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[111]Ibid., p. 77.

[112]Ibid., p. 77.

called "the logic of science."[113] Logical positivism indeed delimited the subject of philosophy, epistemology is the only *legitimate* subject.

With the rejection of metaphysics, not only has the subject of philosophy been delimited as epistemology, but also our conception of the nature (or function) of philosophy has radically changed. With regard to this aspect, logical positivists were chiefly influenced by Wittgenstein. Unlike traditional philosophers who thought that philosophy was "the pursuit of truth," he conceived it as a kind of "*therapy*," to prevent us from talking about nonsense and going astray by problems for which there is no real solution. Wittgenstein maintained that "philosophy is not a theory but an activity."[114] "The totality of true proposition is the total natural science," but "philosophy is not one of the natural sciences." Thus philosophy does not make any propositions at all. "Most propositions and questions, that have been written about philosophical matters, are not false, but senseless." "The object of philosophy is the logical clarification of thoughts." The result of philosophy is not a number of "philosophical propositions," but "to make propositions clear." "All philosophy is 'Critique' of language."

This view of philosophy was immediately accepted by Schlick, Carnap, and other members of the Vienna Circle. Philosophy is neither a theory, like science, nor a way of life, like theology or ethics or art; philosophy is no longer regarded as doctrine embodying "wisdoms" or "a pursuit of truth." Carnap writes: "[w]hat remains is not statements, nor a theory, nor a system, but only a *method*: the method of logical analysis."[115] Schlick also expressed the same idea in the article "The Turning Point in

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[113]Carnap (1934), "The Task of the Logic of Science," in Unified Science, ed. by McGuinness, p. 46.

[114]All from Wittgenstein, Tractatus Logico-Philosophicus, 4.112, 4.11, 4.111, 4.003, 4.112, 4.0031.

[115]Carnap, "The Elimination of Metaphysics Through Logical Analysis of Language," p. 77.

Philosophy:" "[t]he great contemporary turning point is characterized by the fact that we see in philosophy not a system of cognitions, but a system of *acts*; philosophy is that activity through which the meaning of statements is revealed or determined."[116] "Philosophy is not a system of statements; it is not a science." Philosophy, according to Schlick, cannot compete with science, for it does not describe the behavior of physical, biological, social, or even mental, objects; philosophy is only concerned with meaning, while science is concerned with truth. Schlick was hoping that in future "no more books will be written about philosophy, but all books will be written in a philosophical manner,"[117] that is, they will be written "*clearly* and *meaningfully*."[118]

2.4 The Logical Analysis of the Structure of Science

Logical positivists claimed that logical analysis is the new, scientific way of philosophizing. It leads to a negative and a positive result. The *negative* result, shown in the last section, is a radical rejection of traditional metaphysics: all alleged statements of metaphysics are pseudo-statements because they are devoid of cognitive significance. The *positive* result is worked out in the domain of empirical science: it consists in the logical analysis of the statements and concepts of empirical science. This section exclusively concerns positivists' analysis of the structure of scientific theories.

First of all, logical positivists divided all activities of science into two contexts: "the context of *discovery*" and "the context of *justification*." This distinction was first

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[116]This and the following quotes are from Schlick (1931), "The Turning Point in Philosophy," in Ayer (1959), Logical Positivism, p. 56.

[117]Schlick, Communication to the 7th International Congress of Philosophy, 1930.

[118]"It will no longer be necessary to speak of 'philosophical problems' for one will speak philosophically concerning all problems, that is: clearly and meaningfully." Cf. Schlick, "The Turning Point in Philosophy," in Ayer (1959), p. 59.

clearly made in Carnap's Logical Structure of the World (1928): "[i]t must be possible to give a *rational* foundation for each scientific thesis, but this does not mean that such a thesis must always be discovered *rationally*, that is, through an exercise of the understanding alone. After all, the basic orientation and the direction of interests are not the result of deliberation, but are determined by emotions, drives, dispositions, and general living conditions. This does not only hold for philosophy but also for the most rational of sciences, namely physics and mathematics. The decisive factor is, however, that for the *justification* of a thesis the physicist does not cite nonrational factors, but gives a purely empirical-rational justification."[119] The terms "context of discovery" and "context of justification" were first officially introduced by Reichenbach in Experience and Prediction (1938). He also argued that the act of discovery escaped logical analysis (i.e. rational reconstruction): "there are no logical rules in terms of which a 'discovery machine' could be constructed that would take over the creative function of the genius."[120] The task of epistemology is "to analyze the relation between given facts and a theory presented to him with the claim that it explains these facts." Epistemology (or logic) is concerned only with the context of justification. The fundamental motivation for separating the task of psychology from that of epistemology is that "many false objections and misunderstandings of modern epistemology have their sources in not separating these two tasks."[121] As Carnap indicates in the Philosophy and Logical Syntax (1935): "*epistemology* or theory of knowledge in its usual form contains both psychological and logical questions. The psychological

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[119]Carnap, The Logical Structure of the World and Pseudoproblems in Philosophy (1928), trans. by Rolf A. George (University of California Press, 1967), p. xvii. Italics mine.

[120]This and the following quotes from Reichenbach's The Rise of Scientific Philosophy (University of California Press, 1951), p. 231.

[121]Reichenbach, Experience and Prediction (University of Chicago Press, 1938), p. 6.

questions here concern the procedure of knowledge, that is, the mental events by which we come to know something. If we surrender these questions to the psychologist for his empirical investigation, there remains the logical analysis of knowledge, or, more precisely, the logical analysis of the examination and verification of assertions, because knowledge consists of positively verified assertions." Psychology deals with the issue of "how" we come to know something, while from the perspective of logical positivists this issue has little philosophical significance. Epistemology addresses the issue of rational justification and logical validity.

Logical positivists have attempted to separate psychology from epistemology by introducing the distinction between the context of discovery and the context of justification. On the other hand, they have eliminated metaphysical problems and doctrines from the theory of knowledge by introducing the verification principle. Philosophy thus has been reduced to logic alone, and epistemology is "*purified*" from psychologism and metaphysical pollution. But logical positivists soon discovered that there was a serious problem for the *verification principle.* Scientific laws cannot be logically reduced to elementary statements of experience, theoretical statements that contain theoretical terms (terms for unobservable entities such as electrons, fields) cannot be empirically testable. If consistently applied, the criterion of meaningfulness (the verification principle) would reject these scientific laws and theoretical statements as meaningless. "Positivists, in their anxiety to annihilate metaphysics, annihilate natural science, along with it."[122] Even Carnap himself in 1936 admitted that "if verification is understood as a complete and definitive establishment of truth then a universal sentence, for example, a so-called law of physics or biology, can never be

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[122]K. Popper (1935), The Logic of Scientific Discovery, Engl. trans. (NY: Harper & Row, 1959), p. 36.

verified, a fact which has often been remarked."[123] This shows clearly that the problem of the meaningfulness of theoretical statements cannot be solved without investigating the *internal structure* of scientific theories. Thus logical positivists had to develop a very sophisticated conception of scientific theories. This they did from the early 1930s to the late 1950s. The resulting conception has been called "The Received View on Theories" since the early 1960s.[124]

First, a scientific theory was conceived as "an *axiomatic* system."[125] It is formulated in the calculus of first-order mathematical logic. We may note first that Frege formulated a complete axiomatization of the first-order logic and Hilbert constructed an axiomatic system of geometry in the late 19th century. Then Whitehead and Russell (1910-13) axiomatized much of mathematics in terms of mathematical logic. Finally, John von Neumann in his Mathematical Foundations of Quantum Mechanics (1932) formalized quantum mechanics and this new field of physics was put on a secure mathematical base.[126] All of these developments convinced logical positivists that "the mathematical statements of scientific laws and also the definitions of theoretical terms could be given in terms of mathematical logic."[127] In fact, Carnap in his Foundations of Logic and Mathematics (1939) attempted to investigate the issues

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[123]Carnap (1936), "Testability and Meaning," collected in Feigl and Brodbeck (1953), Readings in the Philosophy of Science, p. 48.

[124]The phrase "the Received View" was coined by Hilary Putnam in his "What Theories Are Not," in Logic, Methodology, and Philosophy of Science: Proceedings of the 1960 International Congress. Ed. by Nagel, Suppes, and Tarski (Stanford University Press, 1962), pp. 240-51.

[125]As for the development of the received view on theories, see Carnap (1939), The Foundation of Logic and Mathematics, in the International Encyclopedia of Unified Science (University of Chicago, 1939), pp. 139-213.

[126]The book was described as "not only a classic, but still remains the bible on the subject" by S. Ulam in 1958. Cf. John von Neumann and Modern Economics, ed. by Mohammed Dore, Sukhamoy Chakravarty, and Richard Goodwin (Oxford: Clarendon Press, 1989), p. 118.

[127]F. Suppe (1977), The Structure of Scientific Theories, p. 12.

of applying mathematical logic (logical calculi) to geometry and physics; later, in Introduction to Symbolic Logic and Its Applications (1954) he attempted actually to construct axiomatic system for geometry, physics, and biology. The axiomatization and formalization of scientific theories have a great advantage over verbal language, which was nicely put by Patrick Suppes: "it provides the best objective way we know to convince an opponent of a conceptual claim." More specifically, (1) formalizing a connected family of concepts is one way to bring out their meaning in an *explicit* fashion; (2) formalization results in the *standardization* of terminology and the methods of conceptual analysis; (3) formalization provides a degree of *objectivity*; (4) formalization makes it clear exactly what is being assumed; and finally, (5) formalization enables one to determine "what are the minimal assumptions necessary for statement of the theory."[128]

Second, since a scientific theory is mathematically formalized in terms of a first-order logic, the vocabularies of an empirical science can be divided into *logical* (including all of mathematics) and *nonlogical* terms. The nonlogical terms include *theoretical* terms and *observational* terms. The theoretical terms refer to unobservable qualities and things such as "electron," "gene," or "dream." The observational terms refer to "publicly observable" physical objects and the qualities of these objects, such as "red," "touches," or "stick." Of course, no sharp boundary separates the observational terms (O-terms) and the theoretical terms (T-terms) because "they lie on a continum." But from a practical point of view, as Carnap argues, the distinction is usually evident. "Everyone would agree that words for properties, such as blue, hard, cold, and words for relations, such as warmer, heavier, brighter, are O-terms, whereas

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[128]P. Suppes (1968), "The Desirability of Formalization in Science," pp. 651-664.

electric charge, proton, electro-magnetic field are T-terms referring to entities that cannot be observed in a relatively simple, direct way."[129]

Third, theoretical terms are given partial observational interpretations by correspondence rules (or "operational rules" or the "dictionary"). There are two types of laws in science: empirical laws and theoretical laws (or abstract or hypothetical laws). The theoretical laws contain only theoretical terms while the empirical laws contain only observable terms. In order to deduce an empirical law from a theoretical one, we must have a set of rules connecting the theoretical terms with the observable terms. Carnap gave an example: "If there is an electromagnetic oscillation of a specified frequency, then there is a visible greenish-blue color of a certain hue."[130] Here something observable is connected with a nonobservable microprocess. With the introduction of the correspondence rule, the theoretical statement that contains theoretical terms is guaranteed to be cognitively meaningful.

Fourth, the essential feature of a science consists in its deductive system and hierarchical structure. A scientific system consists of a set of hypotheses which form a *deductive* system; that is, this deductive system "is arranged in such a way that from some of the hypotheses as premises all the other hypotheses logically follow."[131] Moreover, the deductive system has a *hierarchical* structure: "the propositions in a deductive system may be considered as being arranged in a order of levels, the hypotheses at the highest level being those which occur only as premises in the system, those at the lowest level being which occur only as conclusions in the system, and those at intermediate levels being those which occur as conclusions of deductions from

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[129]Carnap, Philosophical Foundations of Physics (NY: Basic Books, 1966), p. 259.

[130]Carnap, "The Nature of Theories," p. 167.

[131]All from Richard Braithwaite (1953), Scientific Explanation, pp. 12 & 13.

higher-level hypotheses and which serve as premises for deductions to lower-level hypotheses." Lower-level hypotheses, i.e., deductive consequences of the theory, describe observable phenomena. The empirical testing of the deductive system is "affected by testing the lowest-level hypotheses in the system. The confirmation or refutation of these is the criterion by which the truth of all the hypotheses in the system is tested. The establishment of a system as a set of true propositions depend upon the establishment of its lowest-level hypotheses."

Through the logical analysis of scientific theories, logical positivists were confident that all sciences are unified. They fundamentally rejected the prevailing view at the time in the German-speaking countries that there was a radical distinction between natural sciences and social sciences, a cleavage between Geisteswissenschaften (or Kulturwissenschaften) and Naturwissenschaften that was classically formulated by Dilthey. Logical positivists believed that this could eventually realize Mach's dream of the unification of science. As Mach says: "I only seek to adopt in physics a point of view that need not be changed the moment our glance is carried over into the domain of another science; for, ultimately, all must form one whole."[132] It is not surprising that Mach was regarded as one of the "spiritual ancestors of the unity of science movement and particularly the real master of the Vienna Circle."[133]

Logical positivists held that all sciences can be unified on three fronts. First, all scientific concepts (or terms) are reducible to the physicalist observational language. Thus, all concepts of empirical science, including those of behavioral and social sciences, would be assured of cognitive significance. As Neurath says: "*Metaphysical terms divide--scientific terms unite.*"[134] In Neurath's index *verborum prohibitorum*, he

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[132]Mach (1914), The Analysis of Sensation, p. 34.

[133]P. Frank, Modern Science and its Philosophy (Harvard University Press, 1950), p. 79.

listed "transcendental," "categorical imperative," "intuition," "reality," "appearance," and so on. Carnap writes: "there is a *unity of language* in science, viz., a common reduction basis for the terms of all branches of science."[135] Second, certain scientific laws can be logically derived from others, and it was hoped that "gradually there would emerge a deductive hierarchy in which the laws of psychology and social science are reduced to those of biology, the laws of biology in turn reduced to those of physics and chemistry."[136] The unity of scientific laws was recognized only as a "future prospect" rather than as an established achievement. Third, all sciences are unified in methods or procedures. Carl Hempel and Paul Oppenheim proposed the hypothetico-deductive model for scientific explanation and prediction. They claimed that all truly scientific explanations and predictions have a common logical structure: an explanans that contains a general law and a statement of relevant initial conditions, and an explanandum that is deduced from the explanans by the aid of the rules of deductive logic. As Hempel characterized: "[t]he thesis of the methodological unity of sciences states, first of all, that, notwithstanding many differences in their techniques of investigation, all branches of empirical science test and support their statements in basically the same manner, namely, by deriving from them implications that can be checked intersubjectively and by performing for those implications the appropriate experimental or observational tests. This, the unity of method thesis holds, is true also of psychology and the social and historical disciplines."[137]

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[134]Neurath (1932), Unified Science and Psychology, in Unified Sciences, p. 23.

[135]Carnap, "Logical Foundations of the Unity of Science," in IEUS, vol. 1, p. 61.

[136]Cf. L. Smith, Behaviorism and Logical Positivism (Stanford University Press, 1986), p. 63.

[137]Hempel (1969), "Logical Positivism and Social Sciences," The Legacy of Logical Positivism, p. 191.

Chapter 3. The Methodology of Mainstream Economics

3.1 Introduction

From the 1930s to the 1960s, logical empiricism had an enormously great success; it became the dominant philosophy in the English-speaking countries. Roughly at the same time, mainstream economics undertook a methodological revolution in the direction of logical empiricism. This revolution was led by some prominent young economists. Some of them had close affiliations with the Vienna logical empiricists philosophy (Oskar Morgenstern and Fritz Machlup), some were trained in the related traditions of empiricism, pragmatism, and operationalism (Terence Hutchison, Milton Friedman, and Paul Samuelson). All of them, directly or indirectly, were influenced by logical empiricism (including pragmatism and operationalism). They adapted logical empiricist philosophy to economic methodology and wrote various methodological prescriptions for working economists. This chapter will show that their positivist views *collectively* constituted the basic *core* of the methodology of mainstream economics, though each of them focused on the different aspects of economic methodology.

3.2 Morgenstern On Formal and Empirical Research

This section is devoted exclusively to Oskar Morgenstern's contributions to the methodology of economic science. The fundamental motivations are twofold. First, Morgenstern's view on the methodology of economics was very influential and typified the logical positivist orientation that caught on in mainstream economic theory. He has had long, close associations with the leading figures in mathematics and physics who shared the general outlook of logical positivism, with men such as Einstein, Gödel, and von Neumann, in Vienna and Princeton, and especially with von Neumann in writing

the monumental work The Theory of Games and Economic Behavior (1944). Moreover, Morgenstern in the late 1920s and early 1930s "frequently" participated in the meetings of the Vienna Circle and Mathematical Colloquium.[138] The former was led by the physicist-philosopher Schlick and the latter was presided over by Karl Menger, the mathematician son of the famous economist Carl Menger. It is thus not surprising that there is always a positivist strain in Morgenstern's work. He was a strong advocate of the methodological principle of logical positivism, that methods used in natural science such as mathematical techniques and laboratory experimentations can and should be employed in the social sciences, particularly in economics. However, recent publications on the impact of logical positivism upon the social and behavioral sciences have completely ignored the fact that Morgenstern's writings were dynamically influenced by the logical positivist philosophy.[139] Thus, this section fills a gap in the histories of analytic philosophy and economic thought.

Second, it has been widely acknowledged that Morgenstern, along with Hayek, Haberler, and Machlup, is a prominent member of the third generation of the Austrian School of Economics. Morgenstern was educated and worked in Vienna, the native home of the Austrian School, where he had had close associations with its leading members; in the 1920s/30s he was a participant in Ludwig von Mises's Private Seminar and Hayek's circle.[140] Morgenstern himself admits that he was "a product of the

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[138]Cf. Morgenstern (1976b), "The Collaboration between Oskar Morgenstern and John von Neumann on Theory of Games," Journal of Economic Literature (14), p. 806; also see Boehm (1984), "The Private Seminar of Ludwig von Mises," p. 12.

[139]Mark Blaug's Methodology of Economics (1980), the most popular and influential book on the subject (reprinted: 81, 82, 83, 84, 85, 86, 87, 88, 90), does not even mention Morgenstern's work in this field. Nor do the writings of philosophers of economics like Daniel Hausman (The Inexact and Separate Science of Economics, 1991) and Alexander Rosenberg (Economics--Mathematical Politics or Science of Diminishing Returns?, 1992).

[140]Machlup, "Friedrich Von Hayek," International Encyclopedia of Social Sciences, (18), p. 805.

Austrian School of Economics."[141] It will be shown in this section, however, that Morgenstern's positivist theory and practice deeply conflicted with the extreme anti-positivist, anti-empiricist methodology of the Austrian School, for he has persistently held the view that there is no limit to the use of mathematics in economics, and that economics is ultimately an empirical, experimental science. After thorough and careful investigation, I am led to the conclusion that Morgenstern, who both advocated and practiced the doctrine of logical positivism, can be hardly included in the Austrian School of Economics.

In this section, I will first present Morgenstern's view on the application of modern mathematics, especially of mathematical logic, to economic investigations; then present his conception that economics is an empirical, experimental science; and finally show that Morgenstern's methodology has uncompromisingly conflicted with that of Austrian economics.

3.2.1 Morgenstern on Economics and Mathematics. Morgenstern was the first 20th century economist who recognized the importance and applicability of modern mathematical logic in economic analysis. Of course, those who are familiar with the development of modern economics would not be surprised to find that there had existed close relations between economics and logic in the nineteenth century. Great economists such as John Stuart Mill, Stanley Jevons, and John Neville Keynes (the father of Maynard) had also been prominent logicians; they have done significant work in both fields. Mill's System of Logic (1843) was more successful than his economics books; students of logic and philosophers of science today are still impressed by his canons of induction, which were conceived by him as the *only* path to the new knowledge. Jevons, who inaugurated the Marginal Revolution in economics in

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[141]Morgenstern (1976b), "The Collaboration Between Oskar Morgenstern and John von Neumann on the Theory of Games," p. 805.

England, taught and published several logic books.[142] But unlike Mill, Jevons really belonged to the group of *modern* logicians such as Boole, Venn, and Peirce, and he succeeded in constructing a logical machine in 1869.[143] Finally, Neville Keynes had provided the most popular exposition of the classical logic in his Formal Logic (1884) which was widely used as a textbook.[144] But what Morgenstern emphasizes here is the building of a bridge between economic science and modern mathematical logic.

The new logic was constructed in the late 19th century by mathematicians to re-examine critically the foundations of mathematics. The techniques of mathematical logic have been used in such various fields of scientific inquiries as mathematics, physics, and philosophy of science. But up to the mid-1930s, the new tool, modern logic, had not been employed in economics, nor in any other field of social sciences. Regrettably, the closer relation between economics and logic that had existed in the 19th century became loose. Thus, in his "Logistics and the Social Sciences" (1936) Morgenstern first calls economists' and other social scientists' attention to "the new logic," mathematical logic, because "one of the most powerful and impressive steps forward that the human spirit has made in the last two generations has up to now apparently been totally overlooked by the social sciences" (p.389). It was Morgenstern more than any other economist who attached great significance to mathematical logic for economic analysis: he had long, close associations with modern logicians such as Gödel, Hahn, and Menger; he was a frequent participant in the meetings of the Vienna Circle of logical positivism and Menger's Mathematical Colloquium. It is thus not

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[142]Jevons was named "Professor of Logic, Mental and Moral Philosophy, and Political Economy" in 1866 at the Owens College. His works on logic include: The Substitution of Similars (1869); Elementary Lessons in Logic (1870); and The Principles of Science (1874).

[143]Cf. William and Martha Kneale, The Development of Logic (1962), p. 421.

[144]Neville Keynes, Formal Logic. 1st ed., 1884; 2nd., 1887; 3rd., 1894; 4th ed., 1906.

surprising that he makes frequent references to the writings of logical positivist philosophers and to their predecessors, Russell and Wittgenstein.[145]

The new logic, according to Morgenstern, is "of profound, indeed fundamental, importance for the social sciences," especially for economics. First, "problems of expectation" cannot be treated adequately without using the "theory of types." The theory of types was created by Russell to offer a way out of logical paradoxes. One paradox, the Liar, was invented by ancient Stoic philosophers: "A man says that he is lying. Is what he says true or false?" Other paradoxes such as that of the set of all sets and that of the class of all classes were discovered respectively by Cantor and Russell at the turn of the century.[146] A way out of the paradox developed by Russell is as follows: all concepts are classified according to "types."[147] A distinction is made between individuals which are not properties (0 level), properties of individuals (1st level), properties of properties of individuals (2nd level), and so on. Take bodies to be individuals, for example, then "square" and "red" are properties of the first level, "spatial property" and "color" are properties of the second level. The theory of types says: "a property of the first level can be attributed or denied only to individuals but cannot apply to properties of the first or higher levels; a property of the *second* level can be attributed or denied only to properties of the *first* level but cannot apply to individuals or to the properties of the second or higher levels." (Carnap, p.139). If *a* and *b* are bodies, the sentences "*a* is square" and "*b* is red" are either true or false, in

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[145]Morgenstern cites Russell's Introduction to Mathematical Philosophy (1919); Wittgenstein's Tractatus Logico-Philosophicus (1922); Karl Menger, The New Logic (1933) and Morality, Decision, and Social Organization (1934); L. S. Stebbing, A Modern Introduction to Logic (1933); Hans Hahn, Logic, Mathematics, and Knowledge of Nature (1933); Carnap, The Logical Structure of the World (1928) and The Logical Syntax of Language (1934). All quotes are from Selected Economic Writings of Oskar Morgenstern, ed. by A. Schotter.

[146]William and Martha Kneale, The Development of Logic, pp. 114 and 652.

[147]The following is from Carnap (1931), "The Old and New Logic," in Ayer (1959), p. 140.

either case are meaningful. Further, the sentences "Squareness is a spatial property" and "Red is a color" are true. But "*a* is a spatial property" or "squareness is red" are neither true nor false but meaningless, and thus pseudo-sentences. They can be avoided if a property of the *nth* level is applied only to concepts of level *n-1*.

Traditionally, economists (e.g. Knight, Fisher, Keynes, and Hicks) have assumed the condition of "full foresight" or "perfect foresight" for the general competitive equilibrium; they argue that the theoretical perfection of equilibrium could not be obtained without the assumption of complete foresight. Perfect foresight is so understood here that all persons concerned correctly foresee the relevant events, i.e. "not only the change in objective data but also the behavior of all other persons" (p.171). This means that "a calculation of the effects of one's own future behavior always rests on the expected future behavior of others, and *vice versa*" (p.173). So full foresight leads to "contradiction," or "paradox." The remedy, according to Morgenstern, lies in employment of a theory analogous to the theory of types in modern logic. "This would mean that on the basis of the assumed knowledge by the economic subjects of theoretical tenets of Type I, there can be formulated higher propositions of the theory; thus, at least, of Type II. On the basis of information about tenets of Type II, propositions of type III, at least, may be set up, etc. But, obviously, one has to start from below in order to build up; one cannot begin at once with the highest type" (p.176).

Second, modern logic, according to Morgenstern, provides an *axiomatic method* to formalize economic theory.[148] Although the techniques of axiomatics were already used in Euclid's Elements and Spinoza's Ethics, it was Hilbert who first achieved

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[148]A mathematician H. Weyl defines the axiomatic method as follows: "The axiomatic method means simply to collect completely the fundamental concepts and the fundamental facts from which all concepts and theorems of a science can be derived by definitions or conceptually" (Morgenstern 1976b, p. 395).

complete understanding of the axiomatic method. Hilbert not only investigated the properties of an axiom system such as consistency, completeness, and independence, but also constructed an axiomatic system of geometry. Whitehead and Russell axiomatized much of mathematics in terms of the new logic. Von Neumann formalized quantum mechanics in terms of mathematics. All these successful applications of the axiomatic method convinced logical positivists that a scientific theory can be axiomatized in terms of the first-order mathematical logic. For instance, Carnap constructed some axiomatic systems of geometry, physics, and biology; Menger had in fact provided a logico-mathematical treatment of ethical theory in Morality, Decision, and Social Organization (1934).

Morgenstern, closely associated with Menger and other members of the Vienna Circle, was familiar with developments of all these ideas. He had a firm belief that the axiomatic method could be applied to all sciences, whether formal or empirical, "provided they are sufficiently developed" (p.395). Thus he cites Hilbert: "anything that can at all become object of scientific thought, as soon as it is ripe for the formation of a theory, becomes subject to the axiomatic method and thereby indirectly to mathematics. In the sign of the axiomatic method, mathematics appears to be destined to assume a leading role in science" (p.397). Morgenstern claims that "it is quite possible to axiomatize economics," though it is not certain that the entire economic theory can be formulated axiomatically. The role of the axiomatic method in economics is that it provides "a superb technique" for systematizing our knowledge of economic science, for "finding further knowledge deductively," and for treating any problem of the subject rigorously. He quotes Hilbert again: "the axiomatic method is indeed and remains the indispensable tool, suited to our mind, for all exact sciences, no matter what field it may be: it is logically unassailable and yet fruitful; it gives research complete freedom of movement. To proceed axiomatically is in this sense nothing but

to think consciously" (p.397). Almost four decades later Morgenstern still holds the same view in his "Thirteen Critical Points in Contemporary Economic Theory: An Interpretation" (1972): "the axiomatic method is the most powerful and demanding way of stating a theory."[149] Morgenstern also practiced what he preached: he axiomatized utility theory, the leading part of microeconomics, in collaboration with von Neumann in their Theory of Games and Economic Behavior (see chapter 4).

Third, modern mathematical logic, in Morgenstern's view, creates a *scientific language* for economics. It seemed to Morgenstern that the origin of most difficulties in the social sciences, especially in economics, "resides in the lack of rigor in the language" (p.400); the confusion of language in many areas of economic theory has caused "so much trouble." This has been frequently shown in the "terminological confusions" and more importantly, in the violation of the syntactical rules of modern logic. He emphasizes that "particularly in economic theory much could be accomplished from a cleanup of language" (p.398). "Only in a formal language is it possible to examine whether one proposition actually 'follows' from another one and what that means anyway" (ibid). Morgenstern's distrust of the ordinary language and his eulogy to the formal language were obviously influenced by logical positivists, who conceived mathematical logic, a formal system, as the ideal language for all sciences. The article shows his strong interest in Carnap's work (1934) on the logical syntax of language and especially his great respect for Karl Menger's formal treatment (1934) of questions of morality. He believed that Menger's "Logic of Ethics" demonstrates that "exact methods can be applied in the fields of the social sciences and in a manner that differs from prevailing views" (p.402). Morgenstern's claim that modern logic creates

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[149]Cf. Morgenstern (1976b), p. 269. Morgenstern also makes this point in "Limits to the Use of Mathematics in Economics" (1963) and "Descriptive, Prescriptive and Normative Theory" (1972). See Morgenstern (1976b), pp. 441-473.

a scientific language for economics has been widely accepted by economists: mathematics is not *a* language, "mathematics *is* language [itself]."[150]

Morgenstern extends his view on mathematical logic to mathematics itself in the "Limits to the Uses of Mathematics in Economics" (1963) and "Descriptive, Predictive and Normative Theory" (1972), in which he strongly advocates the application of mathematics to economic investigation. It seems to him that the backwardness of social sciences is partly shown by the fact that there has frequently been violent opposition to mathematics; and this opposition is based on the many widespread misunderstandings of mathematics itself. "Among them is the idea that mathematics could not be used to analyze psychological factors in value theory; that human nature cannot be described mathematically, because it is 'too rich' to be represented; that, though curves or other graphs may be appropriate, the use of exactly the same information in the form of equations is unwarranted; that there is no infinite divisibility of goods and 'hence' no use of differential calculus; that utility is not measurable; that people do not behave rationally, and so forth. All these 'objections' lead to trivially false statements which need no refutation, because they reveal nothing about the issue under consideration. They merely show that their proponents do not know what mathematics is about and how a mathematical model of economic phenomena is to be constructed" (p.444). These misunderstandings had contributed to slowing down the development of mathematical economics early in this century; they are still frequently met in the literature of economics and other social sciences. Hence, it seems to me, they *need* to be refuted.

First, those who claim that mathematics cannot be used to analyze the psychological factors in value theory have been ignorant of the development of

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[150]Samuelson, "A Note on Mathematics," in Machlup (1991), p. 300.

economic science. Psychological factors such as consumers' *preferences* and individual decision-making process have already been analyzed and presented mathematically by prominent economists such as Francis Edgeworth, Vilfredo Pareto, and Gerald Debreu.[151] Second, human nature *can* be described mathematically. For example, rational expectations can be conceived as part of human nature, because only human beings have expectations while animals don't. Yet rational expectations have been successfully described in mathematical forms by New Classical economists Robert Lucas and Thomas Sargent since the 1970s.[152] Third, it is true that there is no infinite divisibility of economic goods, but "no use of differential calculus" does not follow; for even in the physical world there is no infinite divisibility, and yet physicists do not generally object to the use of differential calculus in their field. Fourth, the development of modern microeconomics has successfully shown that utility can be numerically measurable, whether in the situation of certainty or of uncertainty. Fifth, rational economic behavior can be mathematically modeled, but it does not follow that irrational behavior cannot be mathematically modeled. In short, these statements against the application of mathematics to economics are either based on ignorance of historical facts or commit logical errors. They are no more than, to use a prominent mathematical economist's words, "kindergarten clichés."[153]

Morgenstern firmly believes "the impossibility of stating any limits to the use of mathematics" in economics. This is shown by the fact that the development of modern economic analysis has been largely due to the applications of advanced mathematical

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[151]Pareto (1906), Manual of Political Economy, Edgeworth (1881), Mathematical Psychics, and Debreu (1959), Theory of Value.

[152]Lucas (1975), "An Equilibrium Model of the Business Circle," and Sargent and Wallace (1976), "Rational Expectations and the Theory of Economic Policy."

[153]Cf. William Baumol, "Economic Models and Mathematics," in Krupp (1966), p. 92.

techniques such as topology, linear and dynamic programming, game theory, and mathematical logic; and some young economists are eager to await new mathematical techniques to solve complex economic problems. "If we were to ask today what the limitations of mathematics are in physics, both mathematicians and physicists would be baffled by the question, brush it aside as meaningless, and go on with their work" (p.444). That this question is not asked is "a sign of the maturity of physics and a consequence of the tremendous success mathematics had in developing that science--nay, indeed, in developing together with physics." But that the question has been frequently asked in economics (and other social sciences), it seems to Morgenstern, shows that economic science, compared to physics, still has a long way to go to reach its *maturity*.

3.2.2 Morgenstern on Economics and Laboratory Experimentation. In the economics profession there are many who overestimate the importance of mathematical formalization but underestimate the significance of empirical research, and there are many who make the opposite error. But Morgenstern not only has urged his fellow economists to apply the formal techniques of modern mathematics and logic to economic analysis, he also has persistently emphasized the importance of conducting *empirical* research. "Two souls reside within his breast." As he puts it: economics is "ultimately an empirical science," it has to "describe and explore the given problems" and to predict future economic events.[154] Particularly, he declares that "economics is definitely an experimental science in the wider sense as physics [sic]."[155] In this respect as well, Morgenstern was also strongly influenced by the positivist tradition, in

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[154]Morgenstern (1972a), "Thirteen Critical Points in Contemporary Economic Theory," p. 1164.

[155]Morgenstern (1972), "Descriptive, Prescriptive and Normative Theory," p. 710.

which both its founding father and its modern representatives have favored the employment of experimental methods in all sciences (see 4.3.1).

Morgenstern was the first economist who recognized the significance of experimental methods in economics. In the article "Experiment and Large Scale Computation in Economics" (1954), Morgenstern investigates the possibility and importance of experiments in economics. He indicates that if someone were thinking "along conventional lines of methodology in the social sciences" he would naturally question "whether there is such a possibility of experiments in economics" (p.484). Morgenstern believes that "there exist great opportunities for direct experiments now and in the future." But experiments mentioned here are real, physical, experiments, they should be distinguished from the so-called "thought experiments" that have been widely used in all sciences, including economics. Thought-experiments involve "imaging conditions that differ from the known conditions and then attempting to identify the proper factor to which the imagined variations could be ascribed" (ibid). They have played an important role in leading to the discoveries of new facts and new theories, but it is extremely difficult to conduct them in practice because the procedure of thought-experiments involves long chains of deduction and is restricted to qualitative considerations. The thought (or mental) experiment is the only sort of experiment that Marx and von Mises recognized in economic science. Morgenstern has gone far beyond the conventional way of thinking; his "main new interest is for the physical, i.e., real, experiment in economics." (ibid)

Experimentation is defined as "any planned and controlled observation or measurement." According to Morgenstern, direct experiments are "similar to experiments in the physical sciences involving the preliminary knowledge or supposed knowledge of a phenomenon whose precise properties are wishes to determine and to measure" (p.497). Experiments could be made by individual business organizations to

investigate how management is affected by wage rates, work hours, and payment schedules. Controlled direct experiments on the economy as a whole can also be conducted such as Fourier's "Phalanxes" in France (18th century), Owen's organizations in Scotland (19th century), and "Social Credit" in Canada (1930's). This kind of experiments upon large aggregates is more difficult to conduct than the experiments in business organizations' activities because the experiment has limited access to the subjects of economic experiment. But on the other hand, experiments of this kind are extremely important in government policy recommendations, for they can "provide significant quantities of new information not available so far" (p.421). It is interesting to mention here that in the 1960s/70s the U.S. government spent a huge amount of money (several billions!) to conduct the Negative Income Tax Experiment in New Jersey and several other states in the effort to gain information for dealing with the policy issues.

Besides the experiments on business organizations' activities and on the economy as a whole (better named "social experiments"), there are also some experiments that are "performed more nearly under laboratory conditions, cost far less money and deal with very specific issues that can be narrowly defined" (p.423). Morgenstern mentions several controlled laboratory experiments conducted in the late 1940s and early 1950s,[156] one of them is Chamberlin's famous market experiment. Chamberlin used graduate students in his course to construct a market, partly for pedagogical purpose, partly for testing his theory of monopolistic competition. Morgenstern predicts that "experiments of this kind may advance economics more surely as a theoretical discipline than those dealing with aggregates" (p.423). Indeed,

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[156]These experiments were discussed in E. Chamberlin (1948), "An Experimental Imperfect Market;" F. Mosteller and P. Nogee (1951), "An Experimental Measurement of Utility;" and W. Edwards (1952), "Experiments in Economic Decision-Making in Gambling Situations."

laboratory experimentation in economics, as one experimentalist observes, "has been transformed from a seldom encountered curiosity to a systematic investigation" since the mid-1970s (see 4.3.1).[157]

Morgenstern did not take into account the argument against laboratory experimentation involving human subjects: it is unfair to human subjects if they *don't* know they are used as guinea pigs, but it ruins the experiment if they *do* know since their behavior can no longer be taken at face value. Experimental psychology may be subject to this objection,[158] but not experimental economics. In the economic experiments, human subjects (usually college students) are given full, clear, and neutral instructions; they know they are being used as economic agents, and they are paid according to their performance.[159] But this does not lead economists to discount evidence from their laboratory experimentations. They conceive the laboratory economies as real economies, the general principles of economics "should be expected to apply with the same force to these laboratory economies as to those economies found in the field."[160]

The fundamental role of experimental investigation in economics has been recognized as that of generating relevant economic data. Traditionally, the data that economists have used for empirical econometric research come from business

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[157]Roth (1987), Laboratory Experimentation in Economics, p. 1.

[158]Many undergraduate students believe that psychologists intentionally deceive subjects in most experiments.

[159]This is shown by an excerpt of instructions from Plott and Smith's experiment (1978, p.150): "This is an experiment in the economics of market decision making. Various foundations have provided funds for this research. The instructions are simple, and if you follow them carefully and make good decisions, you may earn money, which will be paid to you in cash, privately, immediately after the session ends today.... In this experiment, we are going to set up a market in which some of you will be buyers and some of you will be sellers in a consequence of 'trading period'."

[160]Cf. C. Plott (1991), "Will Economics Become an Experimental Science?", p. 905.

organizations and government agencies; the nature and accuracy of these data have been frequently questioned by economists, since it is not generated for the purpose of scientific research. In his On the Accuracy of Economic Observations (1950), Morgenstern discussed "the general problem of the nature of direct data in economics and the--unsolved--problem of determining the error in many important situations" (p.438). Any empirical conclusion, economic forecast, and policy recommendation that is based on questionable data must be also unreliable. Thus, as Morgenstern said, "at the bottom of everything is the question of data. Computation without due regard to their properties is meaningless" (ibid). The power of laboratory experimentation lies in its generation of empirical data that are well suitable for econometric research: the data that are generated by carefully designed procedures, well-controlled environments, and fully manipulated variables are more reliable, more accurate, and more relevant. Of course, one could question the "artificial" nature of economic experiments, i.e., the difference between the experimental world and the real world. But we must never forget the fact that in economic experiments real people use real money to deal with real problems, to find real solutions and to get real rewards. Indeed, Morgenstern's view on economic experiments and many experimentalists' work encouraged by him have fundamentally changed our perception: economics, much like the physical and biological sciences, can be and should be viewed as an experimental discipline.

Morgenstern's revolutionary contributions to the methodology of economic science cannot be fully assessed unless they are put against the background of intellectual history. Economics has been considered a non-experimental discipline by the profession for nearly two hundred years (a detailed survey will be provided in chapter 4). It seems to me that Morgenstern occupied a unique position in the history of economic science: he made a significant contribution to the breakdown of the methodological myth that experimental methods are not applicable to nor useful in

economic investigations. His article anticipated the birth of experimental economics and even stimulated the experimental work of Vernon Smith, Charles Plott, Alvin Roth, and many others.

3.2.3 Morgenstern: An Austrian Economist? The Austrian School of Economics was founded in the 1870s by Carl Menger, who independently inaugurated the marginal revolution with Stanley Jevons in England and Léon Walras in Switzerland. Its doctrine and methodology were refined and developed by Menger's two contemporaries, Friedrich von Wieser and Eugen von Böhm-Bawerk (both of whom acknowledged him as their intellectual master), by Ludwig von Mises (the anti-positivist brother of the positivist mathematician, Richard von Mises) and Joseph Schumpeter, its second generation, and by Hayek, Haberler, Machlup and others, its third. The school had exerted an extreme influence on the economics profession: Böhm-Bawerk was the President of the Austrian Academy of Sciences, Schumpeter and Machlup were elected as presidents of the American Economic Association, and Hayek was a Nobel Laureate in economics. Besides its intellectual achievements, the school had enjoyed a great social success: Menger was appointed as a tutor to the Imperial Crown Prince Rudolf in 1876-88. Wieser, Böhm-Bawerk, and Schumpeter were Cabinet ministers.[161] Although each member of the school has a different focus, taste, and even view in his own research, all of them have commonly shared a *set* of methodological principles. In what follows, I show that the positivist methodology Morgenstern strongly preached and practiced conflicts deeply with the anti-positivist methodological principles of the Austrian School of Economics.

First, Morgenstern firmly believes that the development of economic science is crucially dependent on its intimate incorporation with modern logic and mathematics.

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[161]Cf. Streissler and Weber, "The Menger Tradition," in Carl Menger and the Austrian School of Economics, ed. by J. Hicks and W. Weber (Oxford, 1973), pp. 226-27.

As a co-founder of game theory, he has applied this new mathematical technique to the analysis of market structures such as duopoly, oligopoly, and perfect competition. His significant contribution to mathematical economics has been widely known as "the KMT model," developed in collaborating with mathematicians J. Kemeny and G. Thompson. As the director of the Econometric Research Program at Princeton University, he has been interested in the quantitative analysis of economic activities. As a methodologist, he has argued that it is impossible to state "any limits to the use of mathematics" in economics.[162] All these sharply contrast with the century-old tradition of the Austrian School--all of whose members have been vehemently opposed to employing mathematical techniques in economic researches. In the Epistemological Problems of Economics (1933), Mises claimed that "the use of mathematical formulations in economics has done more harm than good."[163] He repeated in his seminal work Human Action: A Treatise on Economics (1949): "The mathematical method must be rejected not only on account of its barrenness. It is an entirely vicious method, starting from false assumptions and leading to fallacious inferences. Its syllogisms are not only sterile; they divert the mind from the study of the real problems and distort the relations between the various phenomena."[164] This complete rejection of mathematical approaches is largely due to the historical fact that the members of the Austrian school lacked training in and understanding of mathematics. In contrast to Walras and Pareto (trained as engineers), and Jevons and Neville Keynes (accomplished logicians), Carl Menger and his disciples were graduates of the old Austrian *Gymnasien*, in which "they had not received and instruction in mathematical

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[162]Morgenstern (1963), "Limits to the Use of Mathematics in Economics," p. 29.

[163]Ludwig von Mises (1933), Epistemological Problems of Economics, 1960, pp. 116-17.

[164]Mises (1949), Human Action, 1963, p. 350.

analysis."[165] Morgenstern is the only exception: he studied mathematics under the directions of Karl Menger and his protégé, Abraham Wald, in the 1930s.[166] The Austrian's handicapped knowledge of mathematics led their economic theory to be non-mathematical and their methodology aggressively anti-mathematical, and prevented them from realizing that the application of mathematics in economics has great advantages over verbal exposition (see 4.2). The Austrian's vehement opposition to applying mathematics in economics has never been taken seriously by mainstream economists. The reason is simple, "contempt for the formal can be taken seriously only in one who has mastered formalism."[167]

Second, Morgenstern declares that economics is "ultimately an empirical science."[168] As an empirical economist, Morgenstern has conducted empirical research on such various topics as international financial transactions and business circles, the predictability of stock market prices, and economic analysis of the space shuttle system. However, the Austrian economists held that economics is an *à priori* science. Mises insisted that economics "is *à priori*, not empirical. Like logic and mathematics, it is not derived from experience; it is prior to experience."[169] The same view was also expressed in Human Action: "What assigns economics its peculiar and unique position in the orbit both of pure knowledge and of the practical utilization of knowledge is the fact that its particular theorems are not open to any verification or

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[165]Karl Menger (1973), "Austrian Marginalism and Mathematical Economics," in Hicks and Weber (1973), ed., Carl Menger and the Austrian School of Economics, p. 44.

[166]Cf. K. Menger's Postscript to the English Edition of Morality, Decision, and Social Organization (1934), p. 114; Morgenstern (1976b), p. 807.

[167]K. Menger (1934), Morality, Decision, and Social Organization, 1974, p. 83.

[168]Morgenstern (1972a), "Thirteen Critical Points in Contemporary Economic Theory," p. 1164.

[169]Mises (1933), Epistemological Problems of Economics, pp. 12-13.

falsification on the ground of experience....The ultimate yardstick of an economic theorem's correctness or incorrectness is solely reason unaided by experience."[170] However, Mises did not recognize the significant difference between logic (and mathematics) and economics: the propositions of logic and mathematics say nothing about the empirical world, they are tautologies. But propositions of economics are not tautologies. Economics is primarily concerned with the study of observable behavior; it has to explain and predict how economic agents actually behave. As for Mises's exaggerated claim of the untestibility of economic theory, Samuelson remarked in the early 1960s: "fortunately, we have left that behind us."[171]

Third, Morgenstern claims that "economics is definitely an experimental science in the wider sense as physics [sic], though this is frequently denied."[172] Morgenstern believes that this new method enables us "to predict outcomes under controlled conditions and to make it possible to conclude from those to wider applications" (ibid). However, the Austrian economists had insisted that economics is a *non-experimental* discipline. This followed from their rationalist position that the science of economics is *à priori*, independent of any experience; this was also a pragmatic consequence of the Austrians' anti-*scientistic* prescription that methods employed in natural science such as laboratory experimentation should not be used in social inquiries. Mises recognized only "the mental [thought] experiment" but denied the possibility of controlled experiments in economics, "the science of human action." He held that "in the empirical sciences the controlled experiment is indispensable for a posteriori derivation of propositions whenever experience presents only complex phenomena in which the

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[170]Mises (1949), Human Action: A Treatise on Economics, 1963, p. 862.

[171]Samuelson (1972), The Collected Scientific Papers of Paul A. Samuelson, vol. 3, p. 761.

[172]Morgenstern (1972b), "Descriptive, Predictive and Normative Theory," p. 710.

effect is produced by several interlinked causes," and that "in historical experience we can *observe* only complex phenomena, and an experiment is inapplicable to such a situation."[173] Mises's statement that in natural science "experiments are performed again and again to verify the hypotheses in question" is falsified by the fact that neither astronomy nor meteorology is an experimental science; the other statement that "we lack the possibility of performing a controlled experiment in social science"[174] is refuted by the massive practices of economic experimentations.

3.3 Hutchison: Introduction of Logical Positivism to Economic Science

Terence Hutchison is the first economist who systematically introduced the basic doctrine of logical positivism to the economics profession. His Significance and Basic Postulates of Economic Theory (1938) is the most important mark of the alliance between the new philosophy and positive economics. The book was written in the mid-1930s when logical positivism had already become an *international* movement through organized conferences, extensive publishing activities, and many personal channels; the new philosophy was also becoming increasingly *dominant* in the Anglo-American countries where many members of the Vienna Circle and the Berlin Society for Empirical Philosophy immigrated; the most important writings of logical positivists were already published such as Carnap's Logical Structure of the World (1928), "Elimination of Metaphysics Through Logical Analysis of Language" (1932) and "Testability and Meaning" (1936), Popper's Logic of Scientific Discovery (1934), Schlick's Problems of Ethics (1930), and Ayer's most popular exposition of logical positivism, Language, Truth and Logic (1936).

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[173]Mises (1933), Epistemological Problems of Economics, p. 12. Italics mine.

[174]Ibid., p. 10.

Moreover, in 1938 Hitler's Nazi regime was reaching its zenith in Germany, where the growth of irrationalism and pseudo-sciences, as Hutchison describes, was "no longer confined to hole-and-corner cranks or passive popular superstitions, but *organized* in comprehensive militant and persecuting masscreeds, attempting simply to justify crude prejudice and the lust for power."[175] Thus, the rationalistic tradition and the very existence of Western civilization were seriously endangered.

Finally, Hutchison's book was written at a time when the *apriorism* had been dominant in the economics community. Leading figures in the profession such as John Stuart Mill, Ludwig von Mises, Lionel Robbins, and Frank Knight were apriorists. The dominance of apriorism in economics will be discussed in next chapter.

All these facts make it understandable that Hutchison, who taught at the University of Bonn (1935-38), attacks apriorism and various pseudo-science methods such as metaphysical speculations, introspection, and psychologism, in his book speaks in the language of logical positivism, makes extensive references to and quotations from logical positivist philosophers,[176] proposes the intersubjective empirical test as the criterion of demarcating science from pseudo-science. It is indeed the first economics book written in the language and spirit of logical positivism. In 1941 when

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[175]All page references in this section are from Hutchison (1938), The Significance and Basic Postulates of Economic Theory, p. 11; italics added.

[176]Hutchison cites the following writings of logical positivists. Ayer: Language, Truth and Logic; Carnap: Pseudo-Problems of Philosophy, Logical Syntax of Language, Philosophy and Logical Syntax; Feigl: Theories and Experience in Physics; Frank: The Causal Laws and Their Limits; Hahn: Logic, Mathematics and Knowledge of Nature; Hempel and Oppenheim: The Theory of Type in the New Logic; Joergensen: Principles of Logic; Kaufmann: Methodology of Social Sciences; Menger: Morality, Decision, and Social Organization; Neurath: Empirical Sociology and What Is Meant by a Rational Economic Theory; Popper: The Logic of Scientific Discovery; Schlick: General Theory of Knowledge and Problems of Ethics. He also cites those who shared the general outlook of logical positivism, such as L. S. Stebbing, A Modern Introduction to Logic; C. I. Lewis, Mind and the World Order; P. W. Bridgeman, The Logic of Modern Physics. Finally, the writings of logical positivists' predecessors were cited: Russell, Introduction to Mathematical Philosophy, Mysticism and Logic, and The Scientific Outlook; Wittgenstein, Tractatus Logico-Philosophicus. The writing of Ramsey (Russell's student and Wittgenstein's associate), The Foundation of Mathematics, was also cited.

asked by Knight to state his "philosophical positions," Hutchison mentions explicitly the views of the British empiricists as well as the doctrines of positivists such as Mach, Schlick, and Carnap.[177]

At the very beginning of the book, Hutchison makes a striking contrast between "science" and "philosophy." Here "science" is understood to include all empirical inquiries into nature and society, ranging from the physical and biological to social sciences, including economics; by philosophy he means all speculative systems of metaphysics in history. According to Hutchison, it is reasonable to speak of the "advance" of science owing to the possibility of taking some results as "agreed upon and settled," and of then proceeding to new problems and new solutions. One certainly can speak of "the advance of Biology from Aristotle to Mendel, of Economics from [William] Petty to [Alfred] Marshall. But one can scarcely more appropriately speak of the *advance* of philosophy from Plato to Hegel than one can of the advance of poetry from Homer to Shakespeare" (p.6). This claim is similar to Whitehead's view that the whole history of the Western philosophy is nothing but a series of interpretations of Plato's work. The idea of the sharp *contrast* between science and philosophy is not new with Hutchison but is simply borrowed from logical positivists of the Vienna Circle. It can be found in Carnap's Pseudo-problems in Philosophy (1928) and Philosophy and Logical Syntax (1935), and in Schlick's "The Turning Point in Philosophy" (1930). For instance, Schlick in his paper asked philosophers a simple but very embarrassing question: "Has philosophy in that period [from the Greek to the 20th century] made any *progress* whatever?"[178] Schlick and all other logical positivists' answer is negative. "This is shown by the fact that basically every new system starts

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[177]Hutchison (1941), "Reply", Journal of Political Economy, 49, p. 732.

[178]This and the following quotes are from Schlick (1930), "The Turning Point in Philosophy," in Ayer (1959), Logical Positivism, pp. 53-54. Italics added.

again from the beginning, that every thinker seeks his own foundation and does not wish to stand on the shoulders of his predecessors." The philosophies of Descartes, Spinoza, and Kant are fine examples. It is understandable that logical positivists made such a sharp contrast, for all of them were scientists (by training or by profession) and admirers of science. Many of them, like Mach, actually denied that they were philosophers (Schlick is an exception); for example, Carnap wrote that "we give no answer to philosophical questions, and instead reject all *philosophical questions*, whether of metaphysics, ethics or epistemology"; "*we pursue logical analysis, but no philosophy.*"[179] Of course, the philosophy here is identified with traditional metaphysical (or speculative) systems of e.g. Plato, Descartes, and Hegel. Logical positivists firmly believed that they had effected a revolution in philosophy by destroying traditional metaphysics and value theory, as Schlick said: "I am convinced that we now find ourselves at an altogether decisive *turning point* in philosophy."[180] Being revolutionized, philosophy would be, and could be, *scientific*, as Reichenbach was confident to proclaim the rise of *scientific* philosophy.

But why is there a striking contrast between science and philosophy? Hutchison explains: "the reason why scientists, unlike philosophers, can build on and advance their predecessors' work rather than each being simply 'influenced' by it and starting afresh right from the beginning at the same problems with some completely new system, is that 'scientists' have definite, *agreed*, and relatively conclusive *criteria* for the *testing* of propositions, solutions, and theories which 'philosophers' do not accept" (p.7, italics added). Hutchison believes that it is this "acceptance of the testing of

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[179]Cf. Carnap's Introduction to "The Physical Language as the Universal Language of Science" (1932): "Author's Introduction: The Viennese Circle does not Practice Philosophy" (1934), in Alston and Nikhnikian (1963), Readings in Twentieth-Century Philosophy, pp. 393-94. Italics original.

[180]Ibid., p. 54.

propositions according to definite criteria" which has led the advance of science and its "*cumulative*," impersonal, "coral-reef-like" growth; it is the acceptance of testability that has ultimately settled scientific disputes for there is no other agreed method of "finding out whether their respective propositions were 'true' or 'false'." It is simply because of the rejection of objective tests that philosophers in two thousand years have never "come to agreement" on any single philosophical issue. If "intersubjective tests" could not be satisfactorily made, as Hutchison believes, "there could be no science" (p.9). He says: "if the finished propositions of a science, as against the accessory purely logical or mathematical propositions used in many sciences, including economics, are to have any empirical content, as the finished propositions of all sciences except of Logic and Mathematics obviously must have, then these propositions must *conceivably* be capable of empirical testing *or be reducible to such propositions* by logical or mathematical deduction. They need not, that is, actually be tested or even be *practically* capable of testing under present or future technical conditions or conditions of statistical investigation, nor is there any sense in talking of some kind of 'absolute' test which will 'finally' decide whether a proposition is 'absolutely' true or false. But it must be possible to indicate intersubjectively what is the case if they are true or false: their truth or falsity, that is, must make some conceivable empirically noticeable difference, or some such difference must be directly deducible therefrom" (pp.9-10, italics original).

Hutchison's arguments quoted in the last paragraph need to be carefully analyzed. *First*, Hutchison claims that propositions of logic and mathematics are *not* subject to empirical tests, because they fall into the domain of *formal* science rather than empirical science. This idea was originated in Hume's Treatise, promoted and developed in Wittgenstein's Tractatus, and pushed furthered by the Viennese positivist philosophers. They all argued that propositions of logic and mathematics say nothing

about the empirical world; that they only deal with the conceptual connections. Contrary to what Kant and Mill had believed, they are not synthetic but analytic. "The propositions of logic [and mathematics] therefore say nothing. They are the analytical propositions."[181] Their truth or falsity is solely determined by *symbolic* rules, not subject to empirical test. As Hahn said: "logic does not in any way deal with all objects, and it does not deal with any objects at all; *it only deals with the way we talk about objects*; logic first comes into being through language. And the certainty and universal validity of a proposition of logic, or better, its irrefutability, flows precisely from this, that it says nothing about any objects." The propositions of mathematics are of exactly the same kind as the propositions of logic: "they are tautological; they say nothing about at all about the objects we want to talk about, but are only about the way we want to talk about those objects."[182]

Second, Hutchison took an "intersubjective empirical test" as the criterion of demarcating "science" from "pseudo-science" (and philosophy). Note first that the term "intersubjective" can rarely be found in to-day's philosophy literature, but was widely used by logical positivists in the 1930s. As for "empirical test," Hutchison cites Stebbing's A Modern Introduction to Logic: "A scientific theory that is incapable of experimental testing is valueless" (p.129). It is this criterion that distinguishes "propositions which may be material for science from those that are not" and this criterion is the "effective barrier for excluding expressions of ethical or political passion, poetic emotion or metaphysical speculation from being mixed in with so-called 'science' " (p.10). As for the criterion of *demarcation* between science and pseudo-science, Hutchison was influenced by Karl Popper who was loosely affiliated with the

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[181]From Tractatus, 6.11.

[182]Hans Hahn, "Logic, Mathematics, and Knowledge of Nature," in Brian McGuinness (1987), Unified Science, pp. 29 & 35. Italics original.

Vienna Circle.[183] Following Popper's Logic of Scientific Discovery, Hutchison was chiefly interested in the criterion that demarcated science from pseudo-science, rather than the criterion that demarcated meaningful from meaningless statements that was the main concern of logical positivists. As Hutchison says: "we prefer this terminology--'science' and 'non-science'--for the distinction, to that of 'sense' [meaningfulness] and 'nonsense' [meaninglessness] which used to be employed by writers of the former Vienna circle" (p.19). But it is more reasonable for Hutchison, an economic *scientist*, to speak of the former in the 1930s when science was seriously endangered by the growth of pseudo-sciences.

Third, as for the *possibility* of empirical test, Hutchison distinguishes the *practical* possibility, according to which a proposition is testable under present or future "technical conditions" (or conditions of "statistical investigation," or of crucial experiments, i.e. "absolute" test), and the *logical* possibility, according to which it is possible in principle (i.e. not "practically" or empirically, but only "logically") to indicate intersubjectively the truth or falsity of propositions. In Hutchison's view, "the Principle of Testability" deals only with the "logical" possibility, not with the "practical" possibility, of empirical test; a statement falls into the domain of pseudo-science if it is logically impossible to test its truth or falsity. But the idea of the distinction between practical possibility and logical possibility can be traced to Schlick's "Positivism and Realism" (1932), in which he gave a very clear exposition as he discussed the meaning of the verification principle. Verifiability is used by Schlick

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[183]Like Wittgenstein, Popper experienced his formative years in Vienna. He received his doctoral degree from the University of Vienna. Popper "never participated in the meetings of the Vienna Circle, but he stood in personal intellectual contact with several members (Carnap, Feigl, Kraft)...There is no unbridgable opposition between them, rather a common bias" (see Kraft (1953), p. 9 & 26). Both Popper and the Vienna Circle had the same philosophical orientation: empiricist; both held the same attitude to philosophy: it must be critical and scientific. But in his later years Popper had the tendency to exaggerating the difference between them. As Ayer says: "The affinities between him and the positivists whom he criticized appear more striking than the divergencies" (Ayer (1959), p. 6).

in the sense of "verifiable in principle," for "the meaning of a proposition is, of course, independent of whether the conditions under which we find ourselves at a specified time allow or prevent the actual verification." The example he gave for the logical possibility of verification is that "there is a mountain of a height of 3000 meters on the other side of the moon." Even though we lacked (at that time) the technical means of verifying it, "the verification remains *conceivable*; we are always able to state what data we should have to experience in order to decide the truth or falsity of the proposition; the verification is *logically* possible, whatever be the case regarding its practical feasibility, and this alone concerns us."[184]

Fourth, Hutchison also distinguishes between *direct* and *indirect* tests. "[A] scientific proposition may not itself be empirically testable *directly*, but may be reducible by direct deduction to an empirically testable proposition or propositions (cf. propositions of Physics about electrons, α and β particles, etc.)" (p.19). Here "direct deduction" means "logical or mathematical deduction" (p.9). This idea came from the logical positivists' claim that any composite proposition can be reduced to elementary or "protocol" sentences by the techniques of mathematics or logic.

Fifth, as for the criterion of empirical test, Hutchison proposes *falsification* rather than verification. "A proposition with empirical content or an empirical proposition must be conceivably falsifiable, that is, if true, *exclude* some conceivable possibility" (p.26). For example, the empirical proposition that "This table is wooden," if true, must exclude or restrict "This table is of iron" (ibid). Why does Hutchison argue for the falsification of empirical test rather than verification? As we know, logical positivists in the early 1930s held the verification principle initiated by Wittgenstein that "the meaning of a proposition is the method of its verification" and

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[184]All from Schlick (1932), "Positivism and Realism," in Ayer (1959), Logical Positivism, p. 82.

that "the question what a sentence means is identical with the question: How is this question verified?"[185] But the problem with the verification principle, as is commonly recognized, is that the principle works only for *singular* statements; sentences of *universal* form cannot be conclusively verified by any finite set of observational data. Thus, the verification principle was severely attacked in Popper's Logic of Scientific Discovery and revised by Carnap in "Testability and Meaning." This fact partly explains why Hutchison in 1938 proposed falsification, rather than verification, as the criterion of demarcation of science from pseudo-science.

Once the criterion (or principle) of empirical testability, falsification, is established, some logical consequences are derived by Hutchison. The first one is a "division" of statements with *scientific* sense in economics "by dichotomy." This, of course, can be traced to logical positivism. Logical positivists recognized two and only two kinds of *genuine*, cognitively *meaningful* propositions: analytic and synthetic. Analytic propositions are certain and necessary only because they say nothing about reality or the empirical world. Synthetic propositions, on the other hand, belong to the domain of empirical sciences; they can never be certain yet they increase the stock of our knowledge. The idea of the dichotomy between analytic and synthetic was clearly expressed in Carnap's "The Elimination of Metaphysics Through Logical Analysis of Language" and "The Task of the Logic of Science," and Ayer's Language, Truth and Logic.[186] Hutchison applies the criterion of empirical test to dichotomizing the

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[185]Schlick (1979), Philosophical Papers, vol. 2, p. 131.

[186]See Carnap (1932), "The Elimination of Metaphysics Through Logical Analysis of Language," in Ayer (1959), Logical Positivism, p. 76; and Ayer (1936), Language, Truth and Logic, p. 31. In "The Task of the Logic of Science" Carnap indicates: "We call a sentence analytic (or tautological) if it is the consequence of any sentence, and hence, if it is true unconditionally, whatever else may be the case. A sentence is called contradictory if every sentence of the language in question is a consequence of it. A sentence is synthetic if it is neither analytic nor contradictory....Synthetic sentences are what are usually called 'statements about reality'." See McGuinness (1987), pp. 50-51

propositions of economic science by using the language (e.g. analytic, synthetic) of logical positivism. As he says: "propositions used in economic science could conveniently be classified according as to whether they were or were not conceivably falsifiable by empirical observation" (p.161); "either a proposition [of economic science] which has sense is conceivably falsifiable by empirical observation or it is not" (p.26). Thus, he claims that all meaningful propositions of economic science can be classified in two mutually exclusive categories: *analytic* and *synthetic*. Propositions of *pure* economic theory are "analytical-tautological." For instance, the statement that "under perfect competition firms are of optimum size" is an analytic proposition. The other example Hutchison gives for the proposition of pure theory is the formula of crude Monetarism: "With an increase in M, and with V and T remaining the same, P rises." ($M.V=T.P$). Analytic propositions of pure theory are certain and necessary, for "purely theoretical analysis consists in the manipulation of concepts in accordance with the rules laid down in their definitions" (p.30). But the price of the unconditional necessity and certainty of the propositions of pure theory is a complete lack of empirical content: they are "independent of all facts," they "cannot tell us anything new in the sense of telling us new facts about the world" (pp.24 & 34).[187]

Although propositions of pure theory are devoid of empirical content, their roles, as Hutchison argues, cannot be denied in science. First, they "afford us a sharp clear-cut language or system of definitions with which to approach the problems which the facts of the world raise" (p.34). Second, propositions of pure theory "enable us to pass at once from one empirical synthetic proposition to another" (p.34). For example, the analytic proposition "under perfect competition firms are of optimum size" enable

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[187]In another place, Hutchison indicates that the propositions of pure theory are concerned with "language:" "'[p]ropositions 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" (p.161).

us to pass from the proposition "competition is perfect in this market" to the proposition "The firms competing in this market are of optimum size." Third, a well-defined system of concepts enables "sharp and clear answers to be obtained from empirical investigation" (p.35). In sum, Hutchison, citing logical positivists such as Ayer, Hahn, and Schlick, emphasizes the role of propositions of pure theory in our analytical transformations for "our brains are not all-powerful" (p.35).

Propositions of *applied* economic theory and "inductive inferences" are empirical-synthetical. An inductive inference does not signify any logically "necessary" relation between p and q but "a conceivably falsifiable" inductive generalization.[188] For example, the statement "if you offer a man unconditionally *either* one shilling *or* one pound he will take the pound" (or "if the clouds are grey it is going to rain") is at least logically falsifiable in the sense that circumstances can be described (without contradicting oneself) under which the statement would be false. The difference between the propositions of pure theory and those of applied theory, according to Hutchison, is that in the former type "no empirical assertion as to the truth of p or q individually is made" (p.23), their truth or consistency is "independent of the question of fact as to whether the premise is empirically true or not" (p.24); but in the latter type of propositions the premise is asserted "as true empirically." For instance, the proposition of *applied* theory that "conditions of perfect competition hold in this or that market, therefore firms are of optimum size" (i.e. "since p therefore q" or "if p then q, and p is true") consists of two elementary propositions: (1) the proposition of pure theory "if p then q", and (2) the empirical synthetic proposition "p is true" and the further assertion of the empirical synthetic proposition q. According to Hutchison, synthetic propositions (i.e. applied theory and inductive inferences) must "conceivably

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[188]Regarding "inductive inferences," Hutchison cites Schlick's General Theory of Knowledge.

be capable of empirical testing or be reducible to such propositions by logical or mathematical deduction" (p.9). Thus, Hutchison concludes that all propositions of economic theory "with *scientific* sense" are "either conceivably falsifiable by empirical observation or not, and none can be both" (p.27).

The other logical consequence of applying the criterion of intersubjective empirical test to economic theory is that metaphysical statements must be completely excluded from economic science because they fail to meet the criterion. Hutchison suggests that "the economic scientist is transgressing the frontiers of his subject whenever he resorts to, or advances as possessing some empirical content, propositions which, whatever emotional associations they may arouse, can never conceivably be brought to an intersubjective empirical test, and of which one can never conceivably say that they are confirmed or falsified, or which cannot be reduced from propositions of which that can conceivably be said" (p.10). It makes no difference to such a transgression whether the proposition is an expression of "ethical uplift or persuasion, political propaganda, poetic emotion, psychological 'association', or metaphysical 'intuition' or speculation" (ibid.), for no one of them can survive the intersubjective empirical test according to a definite objective criterion. This idea had been already expressed in the writings of logical positivist philosophers such as Carnap, Neurath, and Schlick; they argued that metaphysical, ethical, aesthetic, and even psychological statements must be strictly excluded from the discourse of sciences; these statements could not increase our stock of knowledge but simply express (poorly!) the attitude of life.

3.4 Machlup on the Problem of Verification in Economics

Fritz Machlup has been recognized not only as a prominent economic theorist, but also as an important methodologist. His contributions to economic methodology

span half a century.[189] Fully to understand Machlup's view on the methodology of economics, we must first know the intellectual background of his education. Machlup was educated in the University of Vienna, where Carl Menger, the founder of the Austrian School of Economics, led the *Methodenstreit* with the German Historical School; where the empiricist philosopher-physicist Ernst Mach and the great sociologist Max Weber taught; where the Vienna Circle of logical positivism was formed; where Wittgenstein and Popper, two of the most important analytical philosophers in the century, experienced their formative years; where Ludwig von Mises taught him *aprioristic* methodology; where his close friends Felix Kaufmann (a member of the Vienna Circle) and Alfred Schütz worked on the methodology of social sciences. He explained well in the preface to the Methodology of Economics and Other Social Sciences (1978) that "in the intellectual milieu in which I lived it would have been surprising for any student at the university to remain uninterested in methodology" (p.ix). He asked: "how could I have escaped the lure of such [methodological] discussions?" (p.x). Also this intellectual background that mixed extreme empiricism in philosophy (Mach, the Vienna Circle) with pure apriorism in the Austrian School of Economics (Carl Menger, Mises, Hayek) partly explains why Machlup, unlike Hutchison, did *not* take an *extreme* empiricist position in the discussion of economic methodology.

Second, Machlup's work on the methodology of economics is based on the logical empiricist philosophy of the 1950s rather than on that of 1930s. In the 1950s, logical positivists had radically changed their early position of extreme empiricism and shifted research from general epistemology to the philosophy of the sciences. Issues

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[189]His first article on the economic methodology, "Why Bother with Methodology?", was published in Economica in 1936. In the early 1980s, he still contributed to the subject. All quotes are from Machlup (1978), Methodology of Economics and Other Social Sciences.

such as the structure of scientific theories, the status of theoretical terms, and the nature of hypothesis testing had become the logical empiricists' main concerns and were being thoroughly investigated. Thus, without some consideration of the changes in logical positivism, Machlup's position in economic methodology cannot be fully understood.

In the 1940s and 1950s, logical positivists focused on the structure of scientific theories. This change of research direction is in some sense an attempt to solve some serious problems that they had met early. For example, any single statement of scientific law cannot pass the empirical test because it contains theoretical terms which are empirically unobservable. So by the positivist standard, it is in the danger of counting as "meaningless." By the 1940s logical positivists recognized that the meaningfulness of a statement of scientific law should not be checked directly against the empirical data but must be considered in the *system* of scientific theory as a whole. In this aspect, the classic study was first provided by Hempel and Oppenheim in "The Logic of Explanation" (1948).[190] They propose the hypothetico-deductive model of scientific explanation (also called "the covering law model of explanation"). According to them, all truly scientific explanations have a common logical structure: they are composed of two parts, an explanandum and an explanans. The explanans contains two subclasses: "a general law" and a statement of relevant initial or boundary conditions; it "must have empirical content; i.e., it must be capable, at least in principle, of test by experiment or observation" (p.321). The explanandum, "the sentence describing the phenomenon to be explained (not that phenomenon itself)," is deduced from the explanans by the aid of the rules of deductive logic. Thus, "the explanadum must be a logical consequence of the explanans." Hempel and Oppenheim also argue that

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[190]This paper was reprinted in Readings in The Philosophy of Science, ed. by Feigl and Brodbeck (1953). All quotes in this paragraph are referred to Readings.

prediction involves the same rules of logical inference as explanation, explanations and predictions are structurally symmetrical, the only difference is that explanations come after events and predictions before events. Finally, they argue for the methodological unity of science: their model for legitimate explanation applies in both the natural and social sciences: "Our characterization of scientific explanation is so far based on a study of cases taken from the physical sciences. But the general principles thus obtained apply also outside this area. Thus, various types of behavior in laboratory animals and in human subjects are explained in psychology by subsumption under laws or even general theories of learning or conditioning; and while frequently, the regularities invoked cannot be stated with same generality and precision as in physics or chemistry, it is clear, at least, that the general character of those explanations conforms to our early characterization" (p.325).

This hypothetico-deductive model was then developed by Richard Braithwaite in his Scientific Explanation (1953).[191] He claims that it is this hypothetico-deductive method applied to empirical material which is the essential feature of science (including all the natural sciences, psychology, and social sciences; but not mathematics, logic, history, and philosophy). According to him, a scientific system consists of a set of hypotheses which form a *deductive* system, this had already been indicated by Hempel and Oppenheim. But Braithwaite shows that this deductive system "is arranged in such a way that from some of the hypotheses as premises all the other hypotheses logically follow" (p.12). Thus, the deductive system has a *hierarchical* structure: "the propositions in a deductive system may be considered as being arranged in an order of levels, the hypotheses at the highest level being those which occur only as premises in the system, those at the lowest level being those which occur only as conclusions in the

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[191]All quotes from Braithwaite, Scientific Explanation.

system, and those at intermediate levels being those which occur as conclusions of deductions from higher-level hypotheses and which serve as premises for deductions to lower-level hypotheses" (p.12). Braithwaite gives an example from physics, for it is so highly developed that its laws "form a hierarchichy in which many special laws appear as logical consequences of a small number of highly general laws expressed in a very sophisticated manner" (p.1). The system has one highest-level hypothesis: "H_1: Every body near the earth freely falling towards the Earth falls with an acceleration of *32* feet per second per second." From this hypothesis another hypothesis follows by simple mathematical techniques, i.e. by principles of the integral calculus: "H_2: Every body starting from rest and freely falling towards the Earth falls $16t^2$ feet in t seconds, whatever number t may be." There follows the infinite set of hypotheses by the logical principle (i.e. the applicative principle): "H_{3a}: Every body starting from rest and freely falling for *1* second towards the Earth falls a distance of *16* feet; H_{3b}: Every body starting from rest and freely falling for *2* seconds towards the Earth falls a distance of *64* feet" (p.13). Higher-level hypotheses will often refer to theoretical entities, while lower-level hypotheses (i.e. deduced consequences of the theory) describe observable phenomena (p.21). Finally, "the empirical testing of the deductive system is affected by testing the lower-level hypotheses in the system. The confirmation or refutation of these is the criterion by which the truth of all the hypotheses in the system is tested. The establishment of a system as a set of true propositions depends upon the establishment of its lowest-level hypotheses" (p.13).

In a series articles written in the mid-1950s and early 1960s, especially in "The Problem of Verification in Economics" (1954) and "Terence Hutchison's Reluctant Ultra-Empiricism" (1956), Machlup shows his thorough understanding of modern philosophy of science at the time and presents his argument that is based on "the changed position" of logical positivism. He frequently makes references to the writings

of logical positivist philosophers such as Richard Braithwaite, Felix Kaufmann, and Ernest Nagel. He quotes heavily Braithwaite's Scientific Explanation and makes central use of the phrase "hypothetico-deductive method." The central argument that Machlup provides is that economics is formed in a hypothetico-deductive system and that we only need to test lower-level assumptions and deduced changes.

Following Braithwaite, Machlup divides all economic assumptions into three different levels: fundamental assumptions, specific assumptions, and deduced low-level assumptions. Examples of fundamental assumptions or "high-level generalizations" are the postulates of rational action and the "economic principle" of aiming at the attainment of a maximum of given ends. An example of specific assumptions is that the expenditures for table salt are a small portion of most household's annual budgets. An example of deduced lower-level hypotheses is that a reduction in the price of the table salt will not result in a proportionate increase in salt consumption.

Once hypotheses on different levels of generality have been distinguished, it is natural for Machlup to claim that hypotheses of different degrees of testability must also be distinguished. Just as Braithwaite argues that the highest-level hypothesis of Galilio's mechanics--every body starting from rest and freely falling towards the Earth falls with the acceleration of *32* feet per second per second--is not subject to direct, independent empirical test, Machlup claims that the fundamental hypotheses of economics such as rationality and maximization are not independently testable: "there is no way of subjecting fundamental assumptions to independent verification" (p.9). He indicates that the whole system of physical mechanics rests on such fundamental assumptions: "Newton's three laws of motion are postulates or procedural rules for which no experimental verification is possible or required" (ibid). This is, as Machlup says, why fundamental hypotheses are also called "heuristic principles," "useful fictions," "procedural rules," or "definitional assumptions." These different

characterizations of the fundamental hypotheses basically come from the writings of logical positivists. For example, the term "definitional assumptions" was used by Hans Hahn, and "rules of procedure" used by Schlick and Kaufmann. Kaufmann indicates that rules of procedure are neither synthetic (not subject to empirical falsification) nor analytic (not independent of experience), but only serve their explanatory functions.[192] So any independent test of the fundamental assumptions "by reference to objective sense-experience is obviously impossible" (p.11). But Machlup insists on independent verification of the assumption selected as "assumed change" and of the conclusion derived as "deduced change." As he says: "in principle we want both assumed change and deduced change to be capable of being compared with recorded data so that the correspondence between the theory and the data can be checked" (p.14).

The whole system of hypotheses can be tested by deducing logical consequences from one set of general postulates and some set of specific assumptions, and comparing these with records of observation regarded as the approximate empirical counterparts of the specific assumptions and specific consequences. This understanding of testability holds for both the natural and social sciences. As Machlup argues, "there is no need for direct test of the fundamental postulates in physics, such as the laws of conservation of energy, or of motion; there is no need for direct test of the fundamental postulates in economics, such as the laws of maximizing utility and profit" (p.17).

There was a debate between Machlup and Hutchison on the methodology of economics in the mid-1950s. In "The Problem of Verification in Economics" (1954), Machlup characterizes Hutchison as an "ultra-empiricist," who requires that every assumption of economic theory be directly testable. Then in his response "Professor Machlup on Verification in Economics" (1956), Hutchison defends his position and

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[192]F. Kaufmann, Methodology of the Social Sciences (1944), pp. 87-88.

dismisses the charge of ultra-empiricist. Finally, Machlup replies in the "Rejoinder to a Reluctant Ultra-Empiricist" (1956). The fundamental issue is whether the behavioral postulates (or the fundamental assumptions or higher-level generalizations) should reflect the observed behavior of economic agents. Hutchison argues that the development of value theory shows a trend toward more testable formulation of the theory. The debate between Hutchison and Machlup on economic methodology is a reflection of the difference between the classical position (which requires the empirical tests of all assumptions) of the Vienna Circle and the changed position (which requires only testing of lower-lever assumptions) of logical positivism.

Semantics is a science of meaning; economic semantics as a new branch of knowledge is the inquiries into the meanings of the most widely used terms in economics. No other economist than Machlup pays great attention to the problems of economic semantics. He has shown that many scientific disagreements can be ultimately resolved by semantic analysis. Of course, semantic analysis is a difficult job, it requires a thorough training in theory, logic, and wide knowledge of the history of economic thought. It is not surprising that Machlup has conducted an enormous semantic analysis because he, like most Austrian economists, had an excellent *Gymnasium* education,[193] was familiar with several European languages, and especially had affiliations with members of the Vienna Circle who founded and cultivated semantic analysis in philosophy. Machlup writes in the preface to Essays in Economic Semantics (1962) that his motivation is "to dispel semantic and conceptual fog and allow greater visibility in areas in which both the fog and the traffic have been dense" (p.xxiii). He warns against "terminological promiscuity" and the use of "weaselwords" and "jargons" to avoid a commitment to definite and clear thought. He

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[193]Cf. Mark Perlman, "Introduction to Schumpeter's History of Economic Analysis," 1993, memo, p. 7.

goes to great pains to point out where confusion in the use of terms has produced serious misunderstanding as well as fruitless controversy. Machlup believes that terms with so many meanings that we never know what their users are talking about should either be dropped from our vocabulary or "purified" of confusing connotations. Of course, semantic clarification, like empirical investigation, is necessary but it cannot be sufficient in the search for improved knowledge.

3.5 Friedman on the Methodology of Positive Economics

It is perhaps now (1995) safe to say that Friedman's "Methodology of Positive Economics" (1953) is the most influential essay of economic methodology in the twentieth century. Though Friedman has been frequently scorned by philosophers of science (Hausman, Rosenberg), economic methodologists, and some of his fellow economists, the essay "survived to become the one article on methodology that virtually every economist has read at some stage in his career."[194] It is true that Friedman's argument in the essay is so subtle that even in the 1990s "it is difficult to find two economists who will agree on precisely what it was that Friedman said,"[195] but the essay is extremely important in two senses: it is the most cited piece in the literature of economic methodology of the century; and more importantly, it has been "the dominant paradigm" of the *working* economists. Though Friedman, unlike Hutchison and Machlup, did not explicitly refer to contemporary philosophy of science, our analysis will show that his view on the methodology of positive economics was implicitly influenced by logical empiricist philosophy.

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[194]Mark Blaug, "Paradigms versus Research Programmes in the History of Economics," in Hausman (1984), The Philosophy of Economics, p. 360.

[195]Blaug, The Methodology of Economics, 1981; 2nd. ed., 1992, p. 90.

First, Friedman makes a sharp distinction between positive and normative economics. The dichotomy of positive and normative economics can be traced to the writings of some classical and neoclassical economists of the 19th century, e.g., Senior's Political Economy, J. S. Mill's Essays on some Unsettled Questions of Political Economy and Logic, Caires' Character and Logical Method of Political Economy, and Bagehot's Economic Studies. Fundamentally "they are in agreement in regarding political economy as a *science* that is in its scope as distinguished from ethical or political, and in its method abstract and deductive."[196] John Neville Keynes, the Cambridge logician, economist, and the father of Maynard Keynes, systematically draws the distinction between positive and normative economics in The Scope and Method of Political Economy (1890). According to him, positive economics is defined as "a body of systematized knowledge concerning what is;" normative economics is "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" (p.34). Keynes indicates that the confusion of political economy as a positive science and as a normative science "is common and has been the source of many mischievous errors" (p.35).

Friedman, following Neville Keynes, dichotomizes positive and normative economics. But he goes further to draw the implication from this dichotomization: "positive economics is in principle independent of any particular *ethical* position or *normative* judgments," because it deals with "what is" not with "what ought to be."[197] Thus positive economics can be an "objective" science, "in precisely the same sense as any of the physical sciences" (p.4). Almost fifteen years later, Friedman again

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[196]Cf. John Neville Keynes, The Scope and Method of Political Economy, 1890, p. 12. Italics mine.

[197]Italics added. All quotes are referred to Friedman (1953), Essays in Positive Economics, p. 4.

emphasized this point: "there are *no* value judgments in positive economics."[198] This view has become the *standard* statement of the mainstream economic methodology and the firm belief of most working economists. Of course one can occasionally hear different voice from protesters such as Myrdal that "a 'disinterested social science' never has existed, and for logical reasons cannot exist...our very concepts are value-loaded...they cannot be defined except in terms of political valuations;" and Smithies that "hardly any economic theory can be considered ideologically neutral."[199] But the majority of economists have reached the consensus that "it does not seem necessary to retread familiar ground to show that economics as a positive science is ethically--and therefore politically--neutral."[200] Joan Robinson asserts that political ideology has been the main obstacle to the progress of economic science, and thus must be excluded from positive economics.[201] In 1968, nine leading economists participated in a symposium sponsored by New York University, all of them unanimously claimed that positive economics is an "objective" science, free of value judgments. This firm belief has also spread in many introductory and advanced economics textbooks. For instance, in an introductory book Economics (1966), Richard Lipsey and Peter Steiner indicated that "positive statements concern what is and normative statements concern what ought to be;" "disagreements over positive statements are appropriately settled by an appeal to the facts;" and "disagreements over normative statements cannot be settled merely by an appeal to facts."[202] Another author, Eugene Silberberg, held the same view:

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[198]Friedman, "Value Judgments in Economics," in Human Values and Economic Policy, ed. by S. Hook, 1967, p. 85.

[199]Myrdal (1958), Value in Social Theory, p. 1; Smithies (1954), Economics and Public Policy, p. 2.

[200]Stigler, "The Politics of Political Economists," Quarterly Journal of Economics, Nov. 1959, p. 522.

[201]Robinson, Economic Philosophy (London: C. A. Watts, 1962), p. 3.

[202]Lipsey and Steiner, Economics, 1966, p. 12.

"*Positive* economics is concerned with questions of *fact*, which are in principle either true of false. What *ought* to be, as opposed to what *is*, is a normative study, based on observer's value judgments. In this text we shall be concerned only with positive economics, the determination of what *is*."[203]

This view reflected one of the fundamental tenets of logical positivism: facts and values must be strictly dichotomized. There is no room for value judgments in science for they can be neither confirmed nor falsified. As Karl Menger said: "[t]he restriction of science to value-free statements had always been part of the program of the [Vienna] Circle, so that the members found the sentences, 'In the world everything is as it is and happens as it happens. In the world there is no value' congenial, if not especially illuminating."[204] The same idea had been advanced and developed by logical empiricists (e.g. Ernest Nagel) in the 1950s and 1960s.[205]

Next, Friedman asserts that a theory of positive economics is a complex intermixture of two elements: a language and a body of substantial hypotheses. "Viewed as a language, theory has no substantive content; it is a set of tautologies" (p.7). These tautologies consist of "formal logic and mathematics." That a proposition of logic or mathematics is not about the empirical world but simply a tautology comes from the conventional view of logical positivism and Wittgenstein. These tautologies, according to Friedman, "have an extremely important place in economics and other sciences as a specialized language" or "analytical filing system" for "organizing empirical material and facilitating our understanding of it" (pp.11 & 7). Samuelson later pushes further that mathematics is not simply one among many languages;

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[203]Silberberg (1978)., The Structure of Economics (McGraw-Hill Book Company, 1978), p. 2.

[204]Menger, "Postscript" of Morality, Decision, and Social Organization (1974), p. 94.

[205]Nagel participated in the symposium (1968, NYU) and his article was included in the Human Values and Economic Policy, see Hook (1967).

"mathematics is language." This view also can be traced to logical positivists' eulogy to formal language and their distrust of ordinary language. Another role of tautologies, as Friedman states, is that they are "essential aids in checking the correctness of reasoning, discovering the implications of hypotheses, and determining whether supposedly different hypotheses may not really be equivalent or wherein the differences lie" (p.11). In other words, mathematics and logic play an important part in our analytic transformations. This was persistently held by logical positivist philosophers such as Carnap, Hahn, and Schlick, because human minds are weak and we not "omniscient."[206]

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' " (p.8). Here, Friedman takes prediction as the ultimate goal of positive economics: "the ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful (i.e., not truistic) predictions about the phenomena not yet observed" (p.7); "its [positive economics'] task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances" (p.4). Because of his failure to put equal stress on explanation, logical positivists would not entirely agree with Friedman's statement. In the "Logic of Explanation" (1948) Hempel and Oppenheim argued that the goals of science are both explanation and prediction, the two are structurally symmetrical; they differ only in that explanations come after events and predictions come before events. But carefully reading Friedman's essay, we find that he does take explanation as a goal (though not an ultimate one) of positive economics. For instance, he indicates that "theory is intended to 'explain'" economic phenomena (p.8); "a hypothesis explains

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[206]Hahn, "Logic, Mathematics and Knowledge of Nature," in Ayer (1959), Logical Positivism, p, 159.

what it sets out to explain" (p.12); "the deduced facts must be about the class of phenomena the hypothesis is designed to explain" (p.13); "a hypothesis is important if it 'explains' much by little" (p.14); "in the 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" (p.28). These quotes suggest that in Friedman's mind positive economics both explains and predicts economic phenomena, though predictions are more fundamental that explanations. This point must be emphasized because many critics have charged that he neglected explanation as a goal of positive economics.

In theory choice, however, confirmation of a hypothesis "is not by itself a sufficient criterion for choosing among alternative hypotheses" (p.9). According to Friedman, the criteria for the acceptability of competing hypotheses, besides their passing the prediction test, should include simplicity, precision, and logical completeness and consistency. "A theory is 'simpler' the less the initial knowledge needed to make a prediction within a given field of phenomena; it is more 'fruitful' the more precise the resulting prediction, the wider the area within which the theory yields predictions, and the more additional lines for further research it suggests" (p.10). Logical completeness and consistency must also be considered because they play a subsidiary role just as "checks for arithmetical accuracy do in statistical computations" (p.10). It is not surprising that Friedman lists these criteria for accepting hypotheses because they were fully discussed in the literature of logical positivism of the 1940s.

Third, since prediction is conceived as the ultimate goal of positive economics, it leads Friedman to claim that a hypothesis or theory is to be judged not by the "realistic" nature of its assumptions but only by its predictions. As he says: "the only relevant test of the *validity* of a hypothesis is comparison of its predictions with

experience. 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 has survived many opportunities for contradictions. 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" (p.9). This quotation shows that Friedman's concern is not the issue of verification but only of confirmation. The verification criterion of logical positivism was radically rejected in the mid-1930s by Popper, Lewis, Nagel, and others with positivistic learnings, for all of them pointed out "the impossibility of absolute verification." If verification is understood as a complete and definitive establishment of truth, as Carnap indicates, "the number of instances to which the law refers--e.g. the space-time-point--is infinite and therefore can never be exhausted by our observations which are always finite in number;" "we cannot verify the law, but we can test it by testing its single instances i.e. the particular sentences which we derive from the law and from the other sentences established previously. If in the continued series of such testing experiments no negative instance is found but the number of positive instances increases then our confidence in the law will grow step by step;" thus complete verification is not feasible but only "a process of gradually increasing confirmation."[207] All this shows that Friedman's view on the acceptability of hypotheses is consistent with logical positivism.

Fourth, Friedman shows that the obstacles to the progress of positive economics come from two opposite sides: "*formalism*" and "*realism*," and *both* must be refuted. The refutation of formalism comes first. Friedman indicates that the testing of

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[207]Carnap (1936), "Testability and Meaning," in Readings in the Philosophy of Science, ed. by Feigl and Brodbeck (1953), pp. 47-49.

hypotheses in economics meets some difficulties: many economists hold that "we can seldom test particular predictions in the social sciences by experiments explicitly designed to eliminate what are judged to be the most important disturbing influence," especially, we are unable to conduct so-called "crucial experiments" (p.10). It was a widespread belief among economists (including positivist Friedman himself!) in the 1950s that economics is a non-experimental discipline. The difficulty of testing economic substantive hypotheses has led some economist's "retreat into purely formal or tautological analysis."(p.11). For instance, Oscar Lange's Price Flexibility and Employment (1944) emphasized the formal structure of the theory, the logical interrelations of the parts; Lange considered it largely unnecessary to "test the validity of his theoretical structure except for conformity to the canons of formal logic."[208] But pure mathematics and logic say nothing about the empirical world. Friedman directly follows the positivist view on the formal sciences and rightly argues that if economics is to be able to predict the unobserved phenomena it "must be more than a structure of tautologies." Moreover, the power of logic and mathematics in the analytical transformation "ultimately depends on the acceptability of the substantive hypotheses" (p.12).

Realism, on the other hand, must also be refuted in economic theorizing. The other effect of the difficulty of testing economic hypotheses by their predictions, as Friedman indicates, is to "foster *misunderstanding* of the role of empirical evidence in theoretical work" (p.12). First comes Terence Hutchison's attack on abstract, deductive economics (e.g. Robbins, von Mises, Knight) in his Significance and Basic Postulates of Economic Theory (1938); he urges more empirical tests on the basic postulates of economic theory such as rationality and maximization principles as well as

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[208]Cf. Friedman's review, "Lange on Price Flexibility and Employment: A Methodological Criticism," in American Economic Review, Sept. 1946, pp. 613-31. Reprinted in Friedman (1953), p. 283.

on the predictions. (But this upsets some apriorist economists, and Knight launches counter-attack in 1941.[209]) Then in the mid-1940s there was a great debate (mainly between Machlup and Richard Lester) called "marginalism controversy," with six articles and communications in the American Economic Review. One major issue is concerning general scientific methodology: "the legitimacy and usefulness of abstract theorizing on the basis of unrealistic assumptions, or perhaps on the basis of assumptions regarded as 'reasonable' though not 'universally true'." Lester questions the maginalist assumption that the pricing policy of manufacturing firms is setting it where marginal revenues equal marginal costs, for the empirical research conducted by Hall and Hitch (1939) shows that firms adopt a conventional mark-up above costs when setting prices. But Machlup defends the traditional profit-maximizing models and argues that it does not matter if firms do not calculate marginal costs and revenues, because they act *as if* they were calculating. All these debates provide a background information to understand why Friedman pays a great attention to the (un)realistic issues of economic assumptions. Friedman *argues against* the retreat into the conformity of the "assumptions" to "reality" as the "test of the validity of the hypothesis," he claims that this widely held view is "fundamentally wrong and productive of much mischief" (p.14). Friedman, like Machlup, argues *for* employing *unrealistic* assumptions in economic theory: "truly 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" (p.14). Of course, the reverse does not hold: unrealistic assumptions do not guarantee a significant theory.

In Friedman's view, those who have criticized the unrealistic nature of

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[209]Knight, "The Significance and basic postulates of economic theory. A rejoinder." Journal of Political Economy, 49, pp. 750-53.

assumptions of hypotheses do not fully understand the functions and significance of "modeling building." A hypothesis or theory consists of two parts: an "abstract model" and "a set of rules." According to Friedman, a model is characterized as an "abstract" or "conceptual world," it is simpler than the "real world," contains the forces that "the hypothesis asserts to be important" (p.24). Models are not intended to be descriptive; "they are designed to isolate the features that are crucial for particular problems" (p.36). In other words, a model is constructed to analyze the world, "not a photographic reproduction of it." In order to yield analytical insights, a model, like a map, has to be abstract and simplified from the real world. Thus, the criticism that the neoclassical economic theory is unrealistic because the wide discrepancies between the "assumptions" and the "real world" have been perceived is, as Friedman shows, "largely beside the point" (p.31). Since the post World War II working economists have been concerned with "model building," the major method in theoretical and empirical research. This effect, along with Friedman's emphasis on the role of models, is largely due to the impact of logical positivism.

Chapter 4. In the Spirit of Logical Empiricism: What the Working Economists Have Practiced

4.1 Introduction

In the last two chapters we investigated the epistemological and methodological foundations of mainstream economics. It was argued that mainstream economics has been epistemologically founded on logical empiricism and that the methodology of mainstream economics is the direct application of logical empiricist philosophy. Economic methodologists have written various prescriptions for working economists. These methodological prescriptions have focused on two aspects of economic research: the *formal* and the *empirical*. In the formal aspect, economic methodologists have constantly emphasized the importance of applying the formal mathematical techniques to economic investigations. In the empirical aspect, they have demanded that working economists derive empirically *testable* (verifiable or falsifiable) hypotheses.

But any study in the methodology and epistemology of mainstream economics is *incomplete* if it restricts itself merely to what the economic methodologists have preached. A methodology of science might have a firm philosophical foundation (e.g. rhetorical persuasiveness and logical coherence), but it may never have been practiced by working scientists. Practice may differ to any degree whatever from the theory underlying it. Thus this chapter extends our study from epistemological foundations and methodological prescriptions to the actual practice of mainstream working economists. It exclusively concerns what working economists *actually have done* since the 1930s, or *how* working economists have *behaved*. I will try to show that the development of modern economic science has been generally in keeping with the methodological prescriptions in both formal and empirical aspects.[210] It seems to me

that the power of modern economic methodology does not merely lie in its epistemological foundations, but in the fact of its being seriously practiced by working economists. If the methodology of mainstream economics is testable, the test must come from within economics itself. "The proof of the pudding is in the eating." Contrary to the prevailing view,[211] I believe that working economists have not merely paid lip service to logical empiricism, they have actually made serious epistemological commitments to scientific philosophy.

4.2 Formal Investigation: Mathematical Formalization in Economics

The application of mathematical method to economic analysis has a very long history. It can be traced back to Aristotle's Politics, Ethics, and Rhetoric, in which he used mathematical concepts such as "mean" and "proportion" (absorbed from the mathematical teachings of Pythagoreans) to illustrate his view on justice and exchange of commodities. He even built a mathematical model of isolated exchange. Sir William Petty (1623-87) developed a national income accounting system by using some simple mathematical techniques (differential calculus had not been invented at that time). Petty described his method as that of *political arithmetic*: "instead of using only comparative and superlative words, and intellectual arguments, I have taken the course to express myself in terms of *number, weight, or measure*; to use only arguments of sense, and to consider only such causes, as have visible foundations in nature; leaving those that depend upon the mutable minds, opinions, appetites and passions of

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[210]Thomas Mayer, a distinguished economist who has done significant empirical research, writes: "to me the exemplar of good methodological practice is Milton Friedman." See his Truth versus Precision in Economics, p. 5.

[211]Cf. Ray Canterbery and Robert Burkhardt, "What do we mean by asking whether economics is a science?" in Alfread Eichner (1983, p.22), Why Economics is not yet a Science?. They claim: "In fact, economists seem to pay only *lip service* to the positivist ideas, going off and doing their own things irrespective of whatever normative criteria philosophers of science have found to be sound."

particular men, to the consideration of others."[212] But mathematical economics as a distinct discipline was not established until the publication of Augustin Cournot's Researches into the Mathematical Principles of the Theory of Wealth (1838). Trained as a professional mathematician at the École Normale at Paris and later appointed as professor of mathematics at Lyons, Cournot (1801-77) systematically applied mathematical method (differential and integral calculus) to the analysis of market structures, i.e., perfect competition, monopoly, and duopoly. Consequently, he has been recognized as the founder of mathematical economics. His great pathbreaking work in this field anticipated the coming age of mathematical analysis in economics. Cournot in the book did not merely set forth theoretical researches and apply to them "the forms and symbols of mathematical analysis," he also showed a deep understanding of the role of mathematics in economic analysis which can hardly be improved on today. According to him, many economists like David Ricardo had a wrong idea of the nature of the applications of mathematical analysis to the theory of wealth. "They imagined that the use of symbols and formulas could only lead to numerical calculations," and they had no idea that the aim of mathematical analysis was to "find the relations between magnitudes which cannot be expressed in numbers and between *functions* whose law is not capable of algebraic expression."[213] The employment of mathematical symbols and formula, in Cournot's view, is "perfectly natural when the relations between magnitudes are under discussion" and even becomes indispensable when "the imperfections of [ordinary] language" (e.g. ambiguity of words) cannot be avoided by a purely literary exposition. Mathematical analysis enables us "to facilitate the exposition of problems, to render it [sic] more concise, to

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[212]The Economic Writings of Sir William Petty, ed., by C. H. Hull (NY: A. M. Kelley), p. 244.

[213]Cournot (1838), preface to The Mathematical Principles of the Theory of Wealth, p. 2.

open the way to more extended developments, and to avoid the digressions of vague argumentation" (p.3). One might have thought these words were written in the mid-twentieth century by Nobel laureates Samuelson or Debreu, or by positivist philosophers Carnap or Suppes.

Mathematical economics had a self-sustained development only after the marginal revolution in the 1870s. This revolution was inaugurated independently by three professional economists, Stanley Jevons (1835-82) in England, Carl Menger (1840-1921) in Austria, and Léon Walras (1834-1910) in Switzerland. It is through the marginal revolution that economics was restructured: its subject and method differed sharply from the political economy of classical orthodoxy, particularly of J. S. Mill's Principles of Political Economy (1848). "Positive economics" (or "economic science," or simply "economics") thus eventually emerged from "political economy" and "moral philosophy" through this revolution.[214] Economists' mission was neither to investigate nor to teach, as Mill had erroneously believed, "the nature of wealth and the laws of its production and distribution." The subject of economics was narrowly defined as the study of efficient *allocation* of the given resources, i.e., how individual consumers can maximize utilities given their budget constraints and how business firms can maximize profits given the technological and market constraints. But these

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[214]Before the marginal revolution, most economics books used the expression "political economy" in their titles, but afterwards, it was "economics." Jane Marcet (1816), Conversation on Political Economy. David Ricardo (1817), Principles of Political Economy. Robert Malthus (1820), Principles of Political Economy. James Mill (1821), Elements of Political Economy. John Ramsey McCulloch (1825), Principles of Political Economy. W. F. Lloyd (1832), Introductory Lectures on Political Economy. William Nassau Senior (1836), An Outline of the Science of Political Economy. Frederick List (1841), National System of Political Economy. J. S. Mil (1848), Principles of Political Economy. The subtitle of Karl Marx's Capital is A Critique of Political Economy. But after the 1870s came a great change. Menger (1871), Principles of Economics; Walras (1874), Elements of Pure Economics; Marshall (1890), Principles of Economics; of course, except Jevons' (1871), Theory of Political Economy. In the 20th century, the list, though not exclusive, includes: Lionel Robbins (1932), Essays on the Nature and Significance of Economic Science. Milton Friedman (1953), Essays in Positive Economics. Tjalling Koopmans (1957), Three Essays on the State of Economic Science.

problems cannot be adequately treated without employing mathematical techniques such as differential calculus and linear algebra. Thus, the search for *optimum* positions (equilibria) of resources allocation legitimately paved the way to a mathematical mode of argumentation. In his mathematical analysis of the theory of exchange, Jevons attempted "to substitute exact inquiries, exact numerical calculations, for guess-work and groundless argument;"[215] Walras and Vilfredo Pareto (1848-1923), trained as engineers, mathematically formulated and analyzed the general competitive equilibrium system; Francis Edgeworth (1845-1927) devised a new mathematical instrument called "the indifference curve" technique to demonstrate the indeterminate outcome of a bilateral bargaining; Irving Fisher (1867-1947), a well-trained mathematician at Yale, also made significant contributions to mathematical economics in the theory of capital, interest, and money. At the turn of the century, there was a considerable number of books on mathematical economics appearing in France, Germany, Austria, England, Italy, and the United States.[216]

Indeed, the marginal revolution opened a new epoch for mathematical economics in the history of economic literature. But in fact the discipline had not developed rapidly before the 1930s. Mathematical economists formed a very small portion of the economics community; the great majority of the profession were "literary economists,"[217] who were mathematically incompetent and naturally opposed to the application of mathematic tools to economic analysis. Cournot's pathbreaking work was completely ignored during his life time; Jevons was unable to "make a strong

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[215]Jevons (1863-84), Investigations in Currency and Finance, 1964, p. xxiv.

[216]See Irving Fisher's "Bibliography of Mathematical Economics," ending in 1897, was included in Cournot's Mathematical Principles of the Theory of Wealth, pp. 147-74.

[217]The dichotomy between "the mathematical economists" and "the literary economists" was made as early as the turn of the century. It was used in Pareto's Manual of Political Economy (1906), p. 125.

impression on his companions at any period of his life," nor did he attract any student followers;[218] the lack of intellectual recognition had caused Walras's tireless correspondence (which spanned fifty-two years and five different languages) with every major economist in the world. The literary economists took the stand that mathematics could not possibly serve to elucidate economic principles; powerful phrases were repeated such as "human liberty will *never* allow itself to be cast into equations," or "mathematics ignores frictions which are *everything* in social science."[219] Moreover, mathematical economics itself was at the stage of infancy, powerful theorems had not been discovered because they inherently depended on some advanced, yet not discovered, mathematical techniques.

However, economic science in the 1930s and 1940s entered "a phase of intensive mathematization that profoundly transformed our profession."[220] In the 1930s, several Viennese economists (Karl Schlesinger, Abraham Wald, and John von Neumann), inspired by Karl Menger, made significant contributions to the general equilibrium theory by employing advanced mathematical techniques. John Hicks and Paul Samuelson provided a mathematical analysis of the competitive process in Value and Capital (1939) and Foundations of Economic Analysis (1947) respectively. In 1944 Neumann and Morgenstern published their classic, The Theory of Games and Economic Behavior, which freed mathematical economics from its dependency on differential calculus and led to its employment of set-theoretic tools such as the

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[218]Cf. M. Keynes, "William Stanley Jevons (1835-1882): A Centenary Allocation on His Life as Economist and Statistician," Journal of the Royal Statistical Society, vol. 99 (1936), p. 545. According to Keynes's calculation, only 39,000 copies of Jevons's nine works in economics and logic had been sold by 1936.

[219]Quoted in Walras's Elements of Pure Economics, preface to the 4th edition (1900), p. 47.

[220]Debreu (1991), "The Mathematization of Economic Theory," presidential address (1990) delivered at the one-hundred-third meeting of the American Economic Association, AER, vol.81, p. 1.

convexity argument, the separating hyperplane theorem, and the fixed point theorem. More importantly, the Econometric Society, with Fisher as its first president, was founded in 1930. Affiliated with the Cowles Commission (founded in 1932 by an investment consultant Alfred Cowles at Colorado Spring, Colorado) which also "seeks to foster the development of logical, mathematical, and statistical methods of analysis for application in economics and related social sciences," the Econometric Society declared as its aim "the advancement of economic theory in its relation to statistics and mathematics" and the "unification of the theoretical-quantitative and the empirical-quantitative approach." In 1933 Econometrica and the Review of Economic Studies, the principal organs of the Society, also began publications. The research program of mathematical economics became institutionalized, and was thus transformed into a well-organized movement. Since then mathematical formalization in economics has radically changed the structure of the scientific community and advanced the professionalization of economic science. This is shown by the following statistical data provided by Nobel laureate Debrau: numbers of the Fellows of the Econometric Society (ES) have increased from 46 in 1940 to 422 in 1990; of the 40 members of the economics section of the National Academy of Sciences of the United States, 34 are ES Fellows; from 1969 to 1990, 30 economists were awarded Nobel prizes and 25 of them were ES Fellows; and finally, of the 26 living past presidents of American Economic Association, 13 are ES Fellows.[221]

One crucial factor that has contributed to this profound transformation of the economics profession is the great advancement of mathematics in the century. First, mathematical logic, pioneered by Frege and comprehensively developed by Whitehead and Russell, provides an axiomatic method and a language to formalize economic

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[221]Cf. Debreu (1991), "The Mathematization of Economic Theory," AER, vol. 81, pp. 1-2.

theory; thus, large parts of microeconomics such as individual-choice theory and the general-equilibrium analysis can be (and have been) mathematically axiomatized. Second, advanced mathematical techniques such as those from the theory of convexity and combinatorial topology have been employed to solve some technically difficult problems. The fixed point theorem (from combinatorial topology), initially proved by Brouwer in 1910 and generalized by Kakutani in 1941, has become the essential tool to prove the existence of the general competitive equilibrium; Minkowski's separating hyperplane theorem (from the theory of convexity) has been used to prove the existence of a "decentralizing" price system for Pareto efficient allocations in a multigood, multiperson economic system. Third, the theory of games, a branch of modern mathematics, provides a rigorous tool to analyze the optimal behavior of participants in the strategic game (cooperative or noncoopertative) where the outcome of one player depends not only on his actions alone but also on those of other players whose interests might be opposed to his own. The theory emerged in 1928 when von Neumann proved the fundamental theorem, the existence of an equilibrium of two-person, zero-sum games; later John Nash (who was awarded the Nobel Prize in Economics in 1994 with John Harsanyi and Reinhard Selton) in 1950 used Kakutani's fixed point theorem to successfully prove that every *n-person* game has an equilibrium point. Since then game theory has been widely applied to such various fields of economics as industrial organization, labor economics, international trade, and public policy analysis. Fourth, in search of the micro-economic foundations of macroeconomic behavior, macroeconomists (Gregory Chow, Thomas Sargent) have applied the basic ideas and methods of dynamic programming, proposed by Richard Bellman in 1957, to studying dynamic optimization problems in consumption, saving, and investment since the mid-1970s. The dynamic tools, as Sargent indicates, often provide much "analytical insight" and "computational simplicity." Indeed, modern economic analysis could not

have moved even a small step without the invention of these mathematical techniques.

The other factor that contributed to this radical transformation of the economics profession is the fundamental change of the intellectual *milieu*, especially the philosophical paradigm. Before the 1930s, speculative philosophies had been dominant everywhere: Neo-Kantianism in the German-speaking countries, Neo-Hegelianism in the Anglo-American, Idealism in Italy, and Existentialism in France. But logical empiricism as a new philosophy began to appear in Vienna in the 1920s and was soon transformed into an international movement. It has since then been the dominant philosophy in Scandinavia, Great Britain, the United States, and many other countries. Logical empiricists have cultivated *formal* techniques such as mathematical logic and metamathematics and believe that the study of modern logic is the central study in philosophy. The new philosophy has provided strong epistemological and methodological arguments for the application of formal mathematical methods to empirical inquiries, including economics. As Kaplan wrote in the 1960s: "the increasing interest during the last several decades in the application to empirical materials of various logical and even mathematical systems is clearly indebted to the positivistic philosophy of science."[222] Moreover, coinciding with the logical positivists' interest in the logic of measurement and probability, the growth of such disciplines as mathematical psychology, psychometrics, and sociometrics has been favorable to the application of mathematics to economic science.

There is no doubt that mathematical formalization has played a leading role in the development of modern economic analysis since the post-World War II period. Mathematics has increasingly become an essential tool and the language of economic theorists. Most working economists have persistently argued that economics cannot be

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[222]A. Kaplan, "Positivism," in the International Encyclopedia of Social Sciences, vol. 3, p. 394.

truly scientific without a continuous cultivation of mathematical techniques; many of them are eager to await new mathematical techniques to solve some complex economic problems. Of course, one often can hear from the Austrian economists like von Mises the protest that "the use of mathematical formulations in economics has done more harm than good."[223] But the majority of the jury has already reached substantial agreement that the application of mathematics to economics has the following major benefits. First, mathematical formalization requires that the fundamental primitive *concepts* in economic theory must be clearly defined or specified. For instance, the notion of commodity was at first narrowly specified by its physical properties. But later due to its mathematical (i.e. axiomatic) treatment in the Arrow-Debreu model, commodity was clearly defined by "its physical characteristics, its location, and its event [or the time of its availability]."[224] Second, the mathematical mode of argumentation forces us to make explicit and fully specified all *assumptions* on the mathematical representations of the primitive concepts;[225] the rules of the game prevent unnoticed or hidden assumptions from slipping into the deduction, a thing that it is very difficult to avoid in literary economics. Third, mathematics makes *presentation* of economic theory more concise and more precise. Natural language is too flexible, ambiguous, and full of misunderstandings. As Wittgenstein says: "our [ordinary] language can be seen as an ancient city: a maze of little streets and squares, of old and new houses, and of houses with additions from various periods; and this surrounded by a multitude of new boroughs with regular streets and uniform houses." Mathematics as a scientific (or artificial) language can avoid the imperfections of

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[223]Ludwig von Mises (1933), Epistemological Problems of Economics, p. 117.

[224]Debreu (1959), Theory of Value, p. 99.

[225]Cf. Samuelson (1947), Foundations of Economic Analysis, p. 92.

natural language such as ambiguities, vagueness, and digressions of argumentation. Thus, mathematics has become the most effective language in communicating ideas among economists. This is why James Buchanan claims that "mathematics is a language" and mathematical economists like Samuelson declare that "mathematics is language [itself]." Of course, the increasing use of mathematical tools has brought about serious difficulties of communication between the professional economists and the lay public (so does physics!), but this is not an argument against the application of mathematics in economics. Or if it is, it is not a particularly effective one. Fourth, mathematics allows economists to deal more easily with more than three-dimensional economic problems. "The blackboard and the printed page offer only two dimensions to our gaze. A third may be added by skillful projection or by constructing a solid exhibit, but that is about the limit."[226] In fact, the interaction of *many* economic variables is the central issue of economic science, and it can be effectively treated only by mathematics. Fifth, the mathematical mode of argumentation has greater power in analytical transformations, for the human mind is too weak to recognize immediately all implications in a given set of propositions. Powerful theorems such as Arrow's Impossibility Theorem that can be mathematically derived are so surprising that we may never have been able to imagine them otherwise. By using some simple techniques of mathematical logic, Arrow shows that if a social decision mechanism satisfies the conditions of collective rationality, the Pareto principle, and the independence of irrelevant alternatives, then it must be a dictatorship.[227] Arrow's theorem is very surprising because the three quite reasonable and supposedly desirable conditions of a social decision mechanism turn out to be inconsistent with democracy;

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[226]T. C. Koopmans (1957), Three Essays on the State of Economic Science, p. 157.

[227]Arrow, "Values and Collective Decision-Making," in Philosophy and Economic Theory, ed. by Frank Hahn and Martin Hollis (1979), p. 122.

this is totally beyond our imaginations and expectations! Sixth, the mathematical treatment of economic theory can tremendously increase research *efficiency*. "The possibility for research workers to be able to use directly the results of their predecessors is a decisive factor in the rapid development of a scientific field,"[228] like economics; the formalization provides the best objective way to convince an opponent of "a conceptual claim;"[229] it is also probably the best way to reach better agreement on what the debaters disagree about. In light of all these advantages, no serious economist currently opposes the employment of mathematical techniques in economic investigations. In what follows I show how modern mathematical tools have been effectively used in general-equilibrium analysis and construction of expected utility theory.

4.2.1 General Equilibrium Theory. Much of economic analysis before Walras was primarily concerned with the problem of partial equilibrium: the price of the good in a single-market is determined by its supply and demand and all other prices are assumed to remain fixed. But in reality, the economic system is a *whole* of which all the parts (i.e. different markets) are connected and react to each other; the price fluctuation of one market will eventually spill over to all other markets. In other words, the more fundamental issue that concerns microeconomics is not the issue of partial equilibrium in a single-market but that of general equilibrium that requires all markets simultaneously clear. Cournot, after presenting the partial equilibrium analysis in his book, made a natural conjecture that "it seems, therefore, as if, for a complete and rigorous solution of the problems relative to some parts of the economic system, it were indispensable to take the *entire system* into consideration."[230] But Cournot

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[228]Debreu (1983), Mathematical Economics, p. 7.

[229]Suppes (1967), "The Desirability of Formalization in Science," p. 563.

thought that the problem of general equilibrium is beyond "the power of mathematical analysis and of our practical methods of calculation" (ibid). Nearly four decades later, it was Walras who first constructed a formal system of general equilibrium and tackled mathematically such issues as its existence, uniqueness, and stability. As Schumpeter remarked, Walras' Elements of Pure Economics (1874) is "the Magna Carta of exact economics."

In Walras' analysis, an important distinction was made between "*the products market*," where products are offered exclusively by entrepreneurs and demanded by the consumers (entrepreneurs, land owners, workers, and capitalists), and "*the services market*," where the consumers sell productive resources such as labor, land, and capital to business firms.[231] In this system, the unknown variables are the *prices* and *quantities* of the products and services, and "the *coefficients* of production"--the quantities of each of the productive services that enter into the production of one unit of each of the products (p.239). Walras showed that if there are *m* products, *n* productive services, *m* product prices, *n* prices of productive services, *mn* technical coefficients, and one of the products as "numéraire" (or "standard commodity"), the total number of *unknowns* is $2m+2n+mn-1$ (p.241). He also showed that his system has a number of independent *equations* $2m+2n+mn-1$ that is equal to the number of unknowns. Walras erroneously believed that the proof of the existence of the general equilibrium (i.e. the solution to the simultaneous equations) was nothing but the counting of equations and unknowns: to make sure that there are as many equations in the system as unknowns to be determined. However, the equality in the number of equations and unknowns is not sufficient for the existence of a general equilibrium solution: the equations might be "inconsistent" and the set of equations "overdetermined." Moreover, the solution to

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[230]Cournot (1838), Mathematical Principles, chapter XI, p. 198. Italics added.

[231]Walras (1874), Elements of Pure Economics, preface to the 4th edition, p. 41.

the general equilibrium system may involve zero prices and even negative prices. Thus, the Walrasian system must include economic goods, free goods, and nuisance goods, but these are not allowed for by Walras.

Little progress in general equilibrium theory had been made until the 1930s, partly because the Walrasian system did not attract many abler minds; even those who were attracted to it naively believed that Walras had already solved the problem of the existence of the general equilibrium. The pathbreaking advance was made in the 1930s by some participants of the Mathematical Colloquium, presided over by Karl Menger. Menger, a distinguished geometer and topologist, was a lecturer in mathematics at the University of Vienna. Members of his colloquium included Alt, Bergmann, Gödel, Morgenstern, Nöbelling, Schlesinger, von Neumann, and Wald. (Most of them also belonged to other intellectual circles of Vienna: Menger and Gödel to the Vienna Circle of Schlick, Schlesinger and Morgenstern to Mises's Private Seminar and Mayer's Economic Seminar). Mathematicians in Menger's colloquium retained strong interest in mathematical economics. Menger himself wrote two important papers and made significant contributions to meta-economics and the economics of uncertainty.[232] Alt pioneered the axiomatic study of measurable utility.[233] Schlesinger was trained as a professional economist under the direction of Böhm-Bawerk, and his Theorie der Geld- und Kreditwirtschaft (1914) "made extensive use of some simple mathematics, uncommon at that time in German economic writing."[234] Schlesinger's interest in advanced mathematics led him, through Menger's recommendation, to take instruction

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[232]Menger, "The Role of Uncertainty in Economics" (1923) and "Remarks on the Law of Diminishing Returns" (1936), in Menger (1979). The publication of the former was followed by a surge of interest in the economics of uncertainty. As Menger indicated in 1979: "Important papers in Econometrica by K. Arrow, J. Marschak, G. Tintner and others were devoted to this topic in the 1950s" (p.260).

[233]Alt (1936), "On the Measurability of Utility," in J. Chipman (1971), pp. 424-31.

[234]Morgenstern (1968), "Karl Schlesinger," in Morgenstern (1976a), p. 509.

from the mathematician Wald in the early 1930s; and his modification (not solution) of the original equations of Walras was presented to the Colloquium.[235] It was Schlesinger's work that stimulated Wald's pathbreaking study of the existence of general equilibrium in the Walrasian system. Finally, von Neumann presented his mathematical proof of the existence of the general equilibrium in Menger's colloquium in 1937.

In a series of papers,[236] Wald showed that the equations of pure exchange have at least one solution for the prices $p_2, p_3, \cdots, p_m$ $(p_1=1)$ and A_{ij}, if the following conditions are met: (1) initial stocks A_{ij} (amount of good j held by individual i) are non-negative, that is, no person holds negative stocks; (2) there are positive stocks of each good; (3) each individual has a positive endowment, and (4) the individual's marginal utility is diminishing. Wald indicated that "conditions *1* to *4*, which prove the solubility of the equations of exchange, agree substantially with the Walrasian assumptions. Thus Walras is correct in asserting the solubility of his equations of exchange. However, this can only be proven with the aid of recondite method of modern mathematics, and the method Walras uses to attempt to prove the existence of equilibrium prices is completely inadequate."[237]

Von Neumann's paper "On an Economic Equation System and a Generalization of the Brouwer Fixed Point Theorem" was presented to a Princeton mathematical seminar in 1932 and later to Menger's Colloquium in 1937, and eventually published in

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[235]Schlesinger (1935), "On the Production Equations of Economic Value Theory," in William Baumol (1968), pp. 278-80.

[236]Wald (1935), "On the Unique Non-Negative Solubility of the New Production Equations (Part 1);" Wald (1936a), "On the Production Equations of Economic Value Theory (Part 2)," and Wald (1936b, English translation 1951 in Econometrica), "On some Systems of Equations of Mathematical Economics."

[237]Wald (1951), p. 384.

Menger's Proceedings of Mathematical Colloquium. This paper, according an authority on general equilibrium, is "the single most important article in mathematical economics."[238] Von Neumann provides the first explicit formulation of a general linear model of production. His model assumes these conditions: there are constant returns to scale; the natural factors of production, including labor, can be expanded in unlimited quantities; consumption of goods takes place only through the processes of production which include necessities of life consumed by workers and employees, in other words, all income in excess of necessities of life will be invested. Under these assumptions, von Neumann proves the existence of an equilibrium rate of growth that is equal to the interest rate. The mathematical proof is made by using a generalization of Brouwer's Fixed-Point Theorem.

Wald's and von Neumann's work was improved by Arrow (a student of Wald at Columbia University) and Debreu in a series of influential papers published in the early 1950s.[239] In the Arrow-Debreu model, the existence of competitive equilibrium is ensured if some assumptions are met. But assumptions made in the Arrow-Debreu model are much *weaker* and *closer* to economic reality than in Wald's or von Neumann's. The A-D model presents an integrated system of production and consumption. Finally, the proof of the existence of competitive equilibrium is *simpler* and *shorter*.

4.2.2 Axiomatization of Economic Theory. The axiomatic method was used in Euclid's Elements, Hobbes' Leviathan, and Spinoza's Ethics. But it was not fully understood and developed until the turn of the century. As shown in chapter 2,

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[238]Weintraub (1983), Microfoundations: The Compatibility of Microeconomics and Macroeconomics. p. 13.

[239]Arrow (1951), "An Extension of the Basic Theorems of Classical Welfare Economics;" Debreu (1951), "The Coefficient of Resource Utilization;" and Arrow and Debreu (1954), "Existence of an Equilibrium for a Competitive Economy."

axiomatics has had applied in many branches of scientific inquiries. Logical positivists (e.g. Carnap, Menger) have persistently emphasized the significance of the formal, axiomatic treatment of scientific theories ever since.[240] In chapter 3, we showed that the economic methodologist Morgenstern, inspired by Menger and other logical positivists, was the first economist who saw the great possibility and significance of axiomatizing economic theory. He indicated that the axiomatic method could be applied to all sciences, whether formal or empirical, "provided they are sufficiently developed."[241] The role of the axiomatic method, according to Morgenstern, is that it provides "a superb technique" for *systematizing* our given knowledge of economic science, for *finding* "further knowledge" deductively, and for *treating* any problem of the subject rigorously. He believed that "the axiomatic method is the most powerful and demanding way of stating a theory" (p.269). It is this belief that eventually led to his collaboration with von Neumann to axiomatize the utility theory, the leading part of microeconomics, in the Theory of Games and Economic Behavior.

Historically, utility was conceived by Jeremy Bentham, the founder of Utilitarianism, as quantitatively measurable (i.e. as a number) and thus comparable between individuals. Bentham argued that individual behavior is fundamentally motivated by "the pursuit of pleasure and the avoidance of pain;" for the community, the values of pleasure and pain can be measured by several factors such as the intensity of pleasure or pain, its duration, its fecundity, etc. The measurability of utility was also never questioned by the founders of marginal utility theory, Jevons, Menger, and Walras, and by the great Marshall. To the objection that a unit of pleasure (or pain) is difficult to conceive and thus hard to measure, Jevons replied: "but it is the amount of

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[240]Cf, Suppes (1968), "The Desirability of Formalization in Science."

[241]Morgenstern (1976a), Selected Economic Writings of Morgenstern, p. 395.

these feelings which is continually prompting us to buying and selling, borrowing and lending, laboring and resting, producing and consuming; and *it is from the quantitative effects of the feelings that we must estimate their comparative amounts*. We can no more know nor measure gravity in its own nature than we can measure a feeling; but, just as we measure gravity by its effects in the motion of a pendulum, so we may estimate the equality or inequality of feelings by the decisions of the human mind."[242] Jevons and others took utility function as "additive," i.e., the utility of a commodity is a function of the quantity of that commodity only, it is independent of the quantities of other commodities consumed. But this early notion of utility as measurable, additive, and comparable was attacked by Edgeworth (1881), Fisher (1892), and Pareto (1906).[243] They provided a modern technique involving the "indifference curve" to treat utility as *ordinal* rather than *cardinal*.

It is against this intellectual background that von Neumann and Morgenstern, by means of the axiomatic method, proved the existence of an expected utility numerical up to a linear transformation.[244] In their axiomatic treatment of utility theory for choice among uncertain options, von Neumann and Morgenstern constructed several simple, transparent axioms, each of which has an immediate intuitive meaning. Now consider a system U of entities $u, v, w, \ldots$. In U a relation is given, $u > v$, and for any number α, $(0 < \alpha < 1)$, an operation $\alpha u + (1-\alpha)v = w$. These concepts satisfy the following axioms:[245]

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[242]Jevons (1871), The Theory of Political Economy, (NY: Kelley and Millman, 1957), p. 11. Italics original.

[243]Edgeworth (1881), Mathematical Psychics; Fisher (1892), Mathematical Investigations in the Theory of Value and Prices; and Pareto (1906), Manual of Political Economy.

[244]The proof of the existence of an expected utility was not published in the first edition (1944), but only in the second (1947) as the Appendix: "The Axiomatic Treatment of Utility."

Axiom 1. For any two u, v one and only one of the three following relations holds, either $u=v$, or $u>v$, or $u<v$. This axiom is the statement of the completeness of the system of individual preferences. It means consumers *can* make choice: they are able to discriminate and evaluate any two distinct consumption plans.

Axiom 2. For any three entities u, v, and w, if $u>v$, $v>w$, then $u>w$. This axiom is the statement of the transitivity of preference. It says that consumers should make *consistent* choices. Although experimental studies have shown that in many situations choices of individual preference are not always transitive, the transitivity of preference is a generally accepted property. If one's choice is not consistent, he can be exploited by a money pump.[246]

Axiom 3. If $u<v$ then $u<\alpha u+(1-\alpha)v$. This axiom states that if v is preferable to u, then even a chance ($1-\alpha$) of v is preferable, because any kind of complementarity has been excluded.

Axiom 4. $u<w<v$ implies the existence of an α with $\alpha u+(1-\alpha)v<w$. This is the statement of the continuity of individual preference. It says that if w is preferable to u, and even more preferable v is also given, then the combination of u with a chance $1-\alpha$ of v will not affect w's preferability to it if this chance is small enough.

Axiom 5. $\alpha u+(1-\alpha)v=(1-\alpha)v+\alpha u$. This states that it is irrelevant in which order the constituents u, v of a combination are named, since the constituents are alternative events.

Axiom 6. $\alpha(\beta u+(1-\beta)v)+(1-\alpha)v=\gamma u+(1-\gamma)v$ with $\gamma=\alpha\beta$. This is the statement

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[245]Von Neumann and Morgenstern (1944), The Theory of Games and Economic Behavior, 2nd., ed. 1947, pp. 25-28.

[246]Suppose a person's preferences over objects a, b, c were satisfied: $a\leq b\leq c<a$, he could be pumped dry of money in this way. Since he likes b at least as much as a, he is willing to trade a for b; and b for c. But he strictly prefers a to c. Thus he should be willing to pay a small sum of money to trade c for a. He will end up holding a but with less money. This process can be repeated until his pocket is empty. See Ken Binmore, Fun and Games (D.C. Health and Company, 1992), p. 95.

that it is irrelevant whether a combination of two constituents is obtained in two successive steps or in one operation.

From these six simple axioms, von Neumann and Morgenstern derived the existence of a numerical utility up to a linear transformation. The mathematical techniques used for the rigorous proof of the axiomatic system are set-theoretic tools, especially the fixed point theorem. Von Neumann and Morgenstern's theory of measurable utility, as Gerald Tintner says: "represents a great advance in economics and has been used extensively in modern statistics, especially in decision theory and the personal, subjective, or Bayesian approach to statistics."[247] Moreover, their work stimulated much of the later use of the axiomatic method in other fields of economics. Debreu's axiomatic treatment of economic equilibrium, Arrow's social choice theory, and Roth's axiomatic models of bargaining are outstanding examples.[248]

4.3 Empirical Investigations in Economics

Economics, as having been rightly understood, is fundamentally an empirical science: it is primarily concerned with the study of human *behavior*. It concerns neither the "psychological introspection" of human action nor "the final cause" of national wealth, but only the observable behavior of individual consumers, business firms, and government agencies. As a behavioral science, economics has to explain and predict how economic agents actually behave. Its success or failure must be ultimately judged by intersubjective empirical tests. As Maurice Allais says: "mere logical, even mathematical, deduction remains worthless in terms of an understanding

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[247]Gerald Tintner, Methodology of Mathematical Economics and Econometrics, in the Foundations of the Unity of Science, vol. 2, p. 562.

[248]Debreu (1959), Theory of Value: An Axiomatic Analysis of Economic Equilibrium. Arrow (1951a), Social Choice and Individual Values. Roth (1979), Axiomatic Models of Bargaining.

of reality if it is not closely linked to that reality. Submission to observed or experimental data is the golden rule which dominates any scientific discipline. Any theory whatever, if it is not verified by empirical evidence, has no scientific value and should be rejected."[249]

But before the 1930s many leading economists held the methodological view that economic science is a system of *a priori* truth: "a product of pure reason, a purely axiomatic discipline," "a system of pure deductions from a series of postulates, not open to any verification or refutation on the ground of experience."[250] John Stuart Mill, Max Weber, Lionel Robbins, Frank Knight, and the Austrian economists are primarily included in this list. For instance, Mill characterized political economy as "essentially an *abstract* science, and its method as the method *à priori*....Political Economy reasons from *assumed* premises--from premises which might be totally without foundation in fact, and which are not pretended to be universally in accordance with it."[251] Robbins claimed that "the propositions of economic theory, like all scientific theory, are obviously deductions from series of postulates", and they are not subject to any empirical test.[252] As for the Austrian economists, their extreme apriorist views were already shown in the last chapter. This methodological position of apriorism taken by the early masters is not surprising partly because the technical tools (e.g. econometric techniques, experimental methods) to test economic theory were not available at the time.

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[249]Allais (1990), "My Conception of Economic Science," Methodus, vol. 2, p. 5.

[250]Quoted in Machlup (1956), "The Problem of Verification in Economics," p. 6.

[251]Mill (1844), Essays on Some Unsettled Questions of Political Economy, Hausman (1984), pp. 56-57.

[252]Robbins (1935), An Essay on the Nature and Significance of Economic Science, p. 78.

However, things have changed radically since the 1930s. First, logical empiricism has fundamentally revolutionized the way of economists' thinking: it declared all metaphysical statements are cognitively meaningless, and urged social scientists to develop empirically testable hypotheses; the rise of behavioral sciences, partly due to the influence of logical empiricism, launched another attack on the metaphysical intuition or psychological introspection in psychology, sociology, and economics. As shown in the last chapter, economic methodologists (Hutchison, Machlup, Friedman) have persistently argued for the elimination of metaphysical speculation from positive economics because they conceive it as the major obstacle to the progress of economic science. Thus, apriorism lost its ground in both philosophy and social sciences. As Samuelson remarked: "it is clear that *no á priori* empirical truths can exist in any field. If a thing has a priori irrefutable truth, it must lack factual content. It must be regarded as a meaningless proposition in the technical sense of modern philosophy."[253] Silberberg, the author of a mathematical economics textbook, also made the same claim that "the paradigm of economics, in order to be useful, must consist of refutable propositions. Any other kind of statement is useless."[254]

Second, *econometrics*, the modern field of empirical economics, was established in the 1930s; it is a new discipline that incorporates mathematics, statistics, and economics; it provides the basic tools to test economic theory against empirical data. The aim of econometrics, according to its founder Ragnar Frisch, is to "subject abstract laws of theoretical political economy or pure economics to experimental and numerical verification, and thus to turn pure economics, as far as is possible, into a science in the strict sense of the word."[255] Econometric study thus has realized the dream of Johann

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[253]Quoted in Hollis and Nell (1975), Rational Economic Man, p. 10.

[254]Silberberg (1975), The Structure of Economics, p. 9.

von Thünen and Jevons: to *measure* the variations in the marginal utility of economic goods. Specifically, by employing statistical techniques, econometricians have attempted to test economic theory using historical data and to predict future economic events using economic theory and historical data. Econometrics can provide both *explanations* and *predictions* of economic behavior within the context of economic theory. It is in empirical economics that testability, the central tenet of logical empiricism, has shown its power and its success. The principle of testability was (and still is) frequently dismissed by Austrian and Marxian economists, but the development of modern econometrics has brought a wide recognition of the principle of testability among mainstream working economists.

Third, economic experiments under controlled laboratory conditions open another major avenue to the development of empirical economics. Economic theory is now subject not only to econometric test, but also to experimental test. With the establishment of experimental economics, the prophecy of Comte and logical empiricists has been completely fulfilled: methods employed in natural science such as mathematical and experimental ones can be extended to the study of social phenomena and all sciences are unified in method. Many controlled laboratory experiments have been conducted in such different fields as individual decision-making, bargaining, auction, public goods, coordination problems, and market equilibrium. It is now widely accepted that economics, like the physical and biological sciences, is an experimental discipline. Thus, the methodological myth (created by Mill and followed by Robbins, von Mises, and many others) that economists lack the possibility of performing controlled experiments was eventually exploded by laboratory experimentation in economics.

In a word, all of these three factors have significantly contributed the

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[255]R. Frisch (1926), "On a Problem in Pure Economics," in Chipman (1971), p. 386.

breakdown of the apriorism in economics. As Samuelson says: "In connection with slavery, Thomas Jefferson has said that, when he considered that there is a just god in Heaven, he trembled for his country. Well, in connection with the exaggerated claims that used to be made in economics for the power of deduction and a priori reasoning--by classical writers, by Carl Menger, by the 1932 Lionel Robbins..., by disciples of Frank Knight, by Ludwig von Mises--I tremble for the reputation of my subject. Fortunately, we have left that behind us."[256] Of course, one still can hear the different voice in the 1990s, but fortunately the majority of the profession have ignored it. Indeed, we have.

A close look at the empirical articles published in the prestigious journals in 1980 strongly confirms our statement that mainstream working economists have developed empirically testable hypotheses and tried to use econometric tools to test them.[257] The topics of these positive economics articles, as expected, are very different and various, ranging from "The Market for New Ph.D.s" to "Murder Behavior and Criminal Justice System," from "Family Size and the Distribution of Income" to "Foreign Trade and Domestic Competition."[258] But it must be noted, first, that most of these articles have a common format: start with "The Model," followed by "Results or Findings," and conclude with "Empirical Tests." Second, it is

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[256]Samuelson (1972), The Collected Scientific Papers of Paul A. Samuelson, vol 3, p. 761.

[257]The leading journals include American Economic Review, Journal of Political Economy, and Quarterly Journal of Economics. We do not include the articles on the mathematical economics, nor history of economic thought or methodology.

[258]Other topics include: Wages, earnings and hours of first, second, and third generation American males; taxing tar and nicotine; optimal order of submitting manuscripts; effects of minimum wage in presence of fringe benefits; economics of short-term leasing; federal taxes and homeownership; decline in the male labor force participation; open market operations; effects of state maximum hours laws; job queues and layoffs; relative capital formation in the US; potential gains from economic integration in Ghana; effects of the EEC's variable import levies; unemployment, the allocation of labor, and optimal government intervention. See Boland (1982), The Foundations of Economic Method, p. 118.

shown in these articles that "model-building" (strongly advocated by logical positivism) has become the essential procedure to construct a theory or hypothesis (which consists of the model and a set of rules). These empirical models are characterized by abstraction and simplification (e.g. behavioral assumptions); like maps, they have left out some details in order to emphasize the forces that the hypotheses assert to be important. These empirical models also include a set of rules that specifies the correspondence between the variables in the model and observable phenomena. Thus, the criticism that these models are unrealistic is largely beside the point. Third, once the models are built, certain implications or theorems can be logically or mathematically derived from these empirical models. These implications are called "empirical results" or "empirical findings" in these articles; the implications cannot be challenged if one does not committed logical fallacies. Fourth, authors of these articles used historical data to test the predictions of model or hypothesis. This part, "the empirical test," is probably most controversial, because "the identification problem" in the econometric test has not been solved; more importantly, there are some fundamental issues about the "quality" of economic data. Usually, the economic data are by-products or results of business and government activities, "they often measure, describe or simply record something that is not exactly the phenomenon in which the economist is interested."[259] In many situations, agents have incentives to lie in reporting or have biases in collecting economic data. Thus, the econometric test of hypotheses is not conclusive because of inaccuracy of quantitative economic data, but this problem can be solved by carefully designed experiments under laboratory conditions.

4.3.1 Laboratory Experimentation in Economics. There is no doubt that

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[259]Morgenstern (1950), On the Accuracy of Economic Observation, p. 9.

experimental methods have played a significant role in the development of modern sciences such as physics, chemistry, biology, and genetics. The history of science has clearly shown that the failure to develop science in ancient Greece, India, and China is at least partly due to their lack of controlled experimental practices. They had suffered neither a lack of ability at abstract reasoning (e.g. mathematics), nor were they short of empirical observations (e.g. massive collections of empirical data for astrology). But they never carried out controlled, systematic experiments. The development of modern science in the Western countries, however, has been largely due to the numerous and systematic experiments (Galileo's experiments in mechanics, Boyle's experiments in pneumatics). The experimental approach, as K. Lewin observed, has been the major avenue to advance in physics: "the progress of physics from Archimedes to Einstein shows consecutive steps by which this 'practical' aspect of the *experimental procedure* has modified and sometimes revolutionized the scientific concepts regarding the physical world by changing the beliefs of the scientists about what is and what is not real."[260] It has been thus widely acknowledged that experiments in the natural sciences perform such important functions as *discoveries* of new facts and *testings* of theoretical hypotheses. Henry Poincare, a great experimental physicist, has good reason to believe that the experiment is "the sole source of truth; it alone can teach us anything new."[261]

But is it possible that experimental methods employed in natural science can be used in the inquiry to social and economic phenomena? For a long time social scientists had denied such possibility and significance. Economics, along with other social sciences, has been considered as a *nonexperimental* discipline by the economics

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[260]K. Lewin (1947), "Frontiers in Group Dynamics," in Human Relations, I, p. 9. Italics added.

[261]Cf. P. Wiener (1953), Readings in the Philosophy of Science, p. 31.

profession. This view has been widespread in public even in the 1990s when experimental economics has been well-established and fully respected by professional economists. The Encyclopedia Britannica (1991) still presents this general misperception that economics is not an experimental science: "economists are sometimes confronted with the charge that their discipline is not a science. Human behavior, it is said, cannot be analyzed with the same objectivity as the behavior of atoms and molecules. Value judgments, philosophical preconceptions, and ideological biases must interfere with the attempt to derive conclusions that are independent of the particular economist espousing them. Moreover, there is *no laboratory* in which economists can test their hypotheses."[262]

The conventional view can be traced at least to the 19th-century English economists such as Nassau Senior, John Elliott, John Stuart Mill, and Walter Bagehot. They maintained that the method of inductive reasoning (including experimental methods) is inadequate for economic investigations because of the variety and complexity of economic phenomena; the only adequate method is deductive, or *a priori*. They concluded that "experiment is a resource from which the economist is debarred."[263] Perhaps Mill's view is the most typical one. It can be found in his System of Logic (1843) and Essays on some Unsettled Questions of Political Economy (1844). In these two books, he advocated applying the logical methods of natural science to "the moral sciences" (what we would call social sciences). Mill believed there are only two modes in which the laws of nature can be ascertained--"deductively and experimentally." He claimed that "the deductive method, setting out from general

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[262]Encyclopedia Britannica (1991), p. 395. Italics mine.

[263]See John Neville Keynes (1890), The Scope and Method of Political Economy, Chapter 1, "Introduction." Parts of the book were reprinted in The Philosophy of Economics, ed. by D. Hausman (1984), pp. 75-76.

laws and verifying their consequences by specific experience, is *alone* applicable" to social inquiries.[264] Thus, Mill denied the possibility of laboratory experimentation in social sciences because of the complexities of social phenomena: "there is a property common to almost all the moral sciences, and by which they are distinguished from many of the physical; that is, that it is seldom in our power to make experiments in them."[265] Compared with physical science, political economy as a moral science has a great disadvantage: "we cannot try forms of government and systems of national policy on a diminutive scale in our laboratories, shaping our experiments as we think they may most conduce to the advancement of knowledge" (ibid). For example, "how can we obtain a crucial experiment on the effect of a restrictive commercial policy upon national wealth? We must find two nations alike in every other respect or at least possessed in a degree exactly equal of everything which conduces to national opulence and adopting exactly the same policy in all their affairs, but differing in this only that one of them adopts a system of commercial restrictions and the other adopts free trade. This would be a decisive experiment, similar to those which can almost always obtain in experimental physics" (p.428). But this experimental method is not feasible in inquiry into social phenomena "owing to the immense multitude of the influencing circumstances and our very scanty means of varying the experiment" (p.427). Thus, the only method of investigation proper to political economy is the deductive method.

Mill's view represents the general perception of the economics profession: the experimental method is not necessary for economic investigation because the only method is deductive, *a priori*; nor is it feasible because there are too many social

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[264]J. S. Mill, A System of Logic (1843), in John Stuart Mill's Philosophy of Scientific Method (1950), ed. by Ernest Nagel, p. 320.

[265]J. S. Mill, Essays on Some Unsettled Questions of Political Economy (1844), in John Stuart Mill's Philosophy of Scientific Method (1950), ed. by Nagel, p. 426.

variables to be effectively controlled. But Mill's account of experimental inquiries into political economy suffered from his two misconceptions. First, he claimed that political economy, unlike experimental physics, cannot conduct the "crucial" (or "decisive"--in his own word) experiment. But modern philosophy of science has shown that there is no crucial experiment in physics either. "Physics is not a machine which lets itself be taken apart; we cannot try each piece in isolation....Physical science is a system which must be taken as a whole; it is an organism in which one part cannot be made to function except when the parts that are most remote from it are called into play, some more so than others, but all to some degree."[266] From this organic nature, Duhem shows that "an experiment in Physics can never condemn an isolated hypothesis but only a whole theoretical group" (ibid). Consequently, a crucial experiment in physics is not possible. Second, Mill's argument against experimental inquiry into political economy is inconclusive. Of course, "we cannot try the forms of government and systems of national policy on a diminutive scale in our laboratories," but it does not follow that we cannot conduct experiments in our laboratories to investigate *individuals'* choice behavior. It seems that the idea of the possibility of using laboratory experimentation in the field of microeconomics had never crossed Mill's mind. The difficulty of experimental inquiries into macroeconomic behavior such as commercial policy and international trade made him exclude any possibility of laboratory experimentation in economic science.

Mill's account of the inapplicability of experimental inquiry to political economy had persistently influenced the way of economists' thinking, whether their theoretical orientations are neo-classical, Marxian, or Austrian. The great majority of the economics profession has followed Mill's argument; they have never recognized the

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[266]Pierre Duhem, Aim and Structure of Physical Theory, trans. by P. Wiener (Princeton University Press, 1954), pp. 187-88.

significance nor the feasibility of experimental methods. In the preface to the first volume of Capital--A Critique of Political Economy (1867), Karl Marx admitted the significance of thought experiments, but denied that of laboratory experiments, in economic research: "in the analysis of economic forms, neither microscopes nor chemical reagents are of use. The force of abstraction must replace both."[267] Marshall, who founded neoclassical economics, generally followed Mill's view that natural science, especially physics, has great advantages over "all studies of man's action," because in it "the investigator is called on for exact conclusions which can be verified by subsequent observation or experiment." Yet the economist is hampered "by his inability to experiment and even more by the absence of any objective standard to which his estimate of relative proportion can be referred."[268] Robbins, a leading economist in the 20th century, also denied the usefulness of controlled experiments in economics. According to him, economics is a deductive science: "the propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates." Thus, "we do not need controlled experiments to establish their validity: they [postulates of economic theory] are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious."[269] Finally, it is not surprising if one finds the same expression (cited in the last chapter) in the writings of the Austrian Economists, for they have insisted on the fundamental distinction between natural science and social science; methods used in natural science such as mathematical techniques or laboratory experimentations, they believe, cannot be

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[267]Marx (1867), Capital. A Critique of Political Economy, vol. 1, translated by Samuel Moore and Edward Avelling (The Modern Library, 1936), p. 12.

[268]Marshall (1890), Principles of Economics (MacMillan, 1920), 8th edition, pp. 43-44.

[269]L. Robbins (1932), An Essay on the Nature and Significance of Economic Science, 2nd ed. 1935, pp. 78-79. Robbins's book has been extremely influential, it was reprinted in 1937, 1940, 1945, 1946, 1948, 1949, 1952.

employed in social science.

Until the 1940s, no economists had ever conducted a laboratory experiment, nor did any of them even think it could be done. Economics had never been an experimental science since Adam Smith, the founder of the discipline. This is indeed the methodological *myth* in economics: experimental methods are not applicable to nor useful in economic investigations. The economics profession, regardless of their orientations as we said before, had never doubted this traditional wisdom. But this belief has been shaken fundamentally since the 1940s. There are several factors that have contributed to the breakdown of this methodological orthodoxy. The first one is the rise of the scientific philosophy--logical empiricism in the first half of the century. Logical empiricism launched a severe attack on metaphysical speculations in science and insisted that metaphysical doctrines and problems must be strictly excluded from science. Any scientific theory must be ruthlessly checked by empirical observation or experimental test, "a scientific theory that is incapable of *experimental* test is valueless."[270] Logical empiricists also believed there is no fundamental cleavage between natural science and social science; all sciences are methodologically unified. Consequently, methods (including experimental ones) employed in natural sciences can and should be extended to the study of social phenomena. This idea can be traced to the father of positivism, Auguste Comte. In the Cours de Philosophie Positive (1830-42), Comte argues that the experimental method is applicable to social science. The "indirect experiment," similar to pathological analysis, "discloses the real economy of the social body in a more marked manner than simple observation could do. It is applicable to all orders of sociological researches...to all degrees of social evolution."[271] Modern positivists have completely agreed with Comte in this aspect.

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[270]L. S. Stebbing, A Modern Introduction to Logic, in Hutchison (1938), p. 129.

As Kaplan says: "the argument that we cannot experiment in behavioral science because the problems are too complex is no more than a blanket rationalization of our ignorance as to what experiments to perform, and how to go about performing them."[272] Logical positivists have indeed contributed to the emergence of "behavioral science," though Carnap and Reichenbach were sympathetic to the theory of psychoanalysis (which is in essence speculative rather than experimental).

The second factor that contributed to the "struggle of escape from the prison of conventional patterns of economic thought" (in Vernon Smith's words) is the rise of behavioral science, especially the rise of experimental psychology. Before the mid-19th century, psychology had no independent authority, it was subordinate to philosophy. Kant, for example, had never considered empirical psychology to be a possibility. Nevertheless psychology gradually broke away from its philosophical base, and the "New Psychology," experimental inquiry into the structure and function of the human mind, began to emerge in the late 19th century. In 1879, Wilhelm Wundt established his laboratory in Leibzig and psychology began to be recognized as an experimental science. American students such as Titchener, Hall, Cattell, and Baldwin studied with Wundt and came back to establish psychological laboratories. Ribot, a French psychologist and Wundt's contemporary, characterized the new experimental psychology as follows: "The new psychology differs from the old in its spirit: it is not metaphysical; in its end: it studies only phenomena; in its procedure: it borrows as much as possible from the biological sciences."[273] In the early century, laboratory experimentation was the most common technique used in psychology. Experimental

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[271] Auguste Comte and Positivism: The Essential Writings, ed. and an intro. by Gertrud Lenzer (Harper Torchbooks, 1975), p. 244.

[272] A. Kaplan, The Conduct of Inquiry (1964), p. 166.

[273] Quoted in Jill Morawski, ed., The Rise of Experimentation in American Psychology (Yale University Press, 1988), p. 60.

psychology, as a behavioral science, was strongly allied with the new philosophy--logical empiricism--from the 1930s to 1950s (Watson, Skinner, Hull).

It should be noted that laboratory experiments must not be confused with thought (or mental) experiments, which "involve imaging conditions that differ from the known conditions and then attempting to identify the proper factor to which the imagined variations could be ascribed,"[274] nor with social (or field) experiments, in which relatively few economic variables are under the control of the experimenter. Social experiments are limited by amount of money, access to economic agents, and agents' subjective expectation of experimental results. Early social experiments were conducted by social reformers, especially utopian socialists, such as Fourier's "Phalanxes" in France and Owen's organizations in Scotland in the 19th century. Most social experiments in the 20th century have been conducted by governments, for instance, "Social Credit" in Canada (1930s), "Negative Income Tax Experiment" in the U.S (1960s-70s), and "Self-Responsibility System" in China (late 1970s).

The first economic experiment under laboratory conditions was conducted by Edward Chamberlin in 1948 at Harvard University. Experimental economics developed *slowly* but steadily in the 1950s and 1960s. During this period, several types of controlled laboratory experiments were conducted by economists, game theorists, psychologists, and philosophers. The first type of experiment, focused on testing utility theory, was conducted by F. Mosteller and P. Nogee (1951), Ward Edwards (1952), Maurice Allais (1953), Andreas Papandreou (1953), Kenneth May (1954), Donald Davidson and Patrick Suppes (1957).[275] The second type of

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[274]Morgenstern (1954), "Experiment and Large Scale Computation in Economics," p. 484.

[275]Mosteller and Nogee (1951), "An Experimental Measurement of Utility," Journal of Political Economy, LIX (1951), pp. 371-404. Edwards (1952), "Experiments in Economic Decision-Making in Gambling Situations," Econometrica, vol. 21, pp. 349-50. (Abstract). Allais (1953), "Le comportment de l"homme rationnel devant le risque: critique des postulates et axiomes de l"ecole americane."

experiment, testing the hypothesis of the well-known "prisoners' dilemma," was conducted by game theorists such as G. Kalisch *et al.* (1952), A. Hoggatt (1959), H. Sauermann and R. Selton (1960), Siegel and Fouraker (1960, 1963), M. Shubik (1961), James Friedman (1963, 67, 69).[276] The third type of experiment, focusing on testing the competitive market behavior, was conducted by Vernon Smith (1962, 64, 65), John Calson (1967), and other economists.[277]

It is interesting to note that from Chamberlin's first experiment to the late

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Econometrica, vol. 21, pp. 503-46. Papandreou (1953), "An Experimental Test of an Axiom in the Theory of Choice," Econometrica, vol. 23, p. 477. (Abstract). May (1954), "Intransitivity, Utility, and the Aggregation of Preference Patterns," Econometrica, vol. 22, pp. 1-13. Davidson and Suppes (1957), Decision Making. An Experimental Approach. Stanford University Press, 1957. Trenery Dolbear (1963), "Individual Choice Under Uncertainty: An Experimental Study," Yale Economic Papers, 3 (1963), pp. 419-70.

[276]M. Deutsch (1949), "An Experimental Study of the Effects of Cooperation and Competition upon Group Process," Human Relations, 2, pp. 199-232. G. Kalisch, J. W. Milnor, J. Nash, E. D. Nering, "Some Experimental n-person Games," Rand Corporation, RM 948, 1952. M. Flood (1954), "Game-learning theory and some decision-making experiments" in R. M. Thrall, C. H. Coombs, and R. L. Davis (eds.), Decision Process. NY: Wiley, pp. 139-158. Flood (1958), "Some experimental games" Management Science, 5, pp. 5-26. Jeremy Stone (1958), "An Experiment in Bargaining Games," Econometrica, 26, pp. 286-97. A. Hoggatt (1959), An Experimental Game," Behavioral Science, 4, pp. 192-203. H. Sauermann and R. Selton (1960), "An Experiment in Oligopoly" in General System, vol. v. Sidney Siegel and Lawrence Fouraker, Bargaining and Group Decision Making. Experiments in Bilateral Monopoly, McGraw, 1960. D. H. Stern (1960), Bargaining Experiments: An Exploratory Study. Dissertation, Princeton University. M. Shubik (1961), "Some Experimental non-zero-sum Games with Lack of Information about the Rules," Cowles Foundation Discussion Paper No. 105. Siegel and D. L. Fouraker (1961), "Bargaining, Information, and the Use of Threat," Research Bulletin 21, The Penn. State University, Dept. of Psychology. A. Rapoport and C. Orwant (1962), Experimental Games: A Review," Behavioral Science, 7, pp. 1-37. Siegel and Fouraker (1963), Bargaining Behavior, McGraw, 1963. James Friedman (1963), "Individual Behavior in Oligopolistic Markets: An Experimental Study," Yale Economic Papers, 3 (1963), pp. 359-417. J. Friedman (1967), "An Experimental Study of Cooperative Duopoly," Econometrica 35 (1967), pp. 379-97. Bruno Contini (1968), "The Value of Time in Bargaining Negotiations: Some Experimental Evidence," American Economic Review, 58 (1968), pp. 374-93. J. Friedman (1969), "On Experimental Research in Oligopoly," Review of Economic Studies, vol. 36, pp. 399-415.

[277]V. Smith (1962), ""An Experimental Study of Competitive Market Behavior," Journal of Political Economy, 70 (1962), pp. 111-137; Smith (1964), "Effect of Market Organization on Competitive Equilibrium," Quarterly Journal of Economics, 77 (1964), pp. 181-201; Smith (1965), "Experimental Auction Markets and the Walrasian Hypothesis," Journal of Political Economy, 73 (1965), pp. 387-393. John Calson (1967), "The Stability of an Experimental Market with a Supply-Response Lag," Southern Economic Journal, 33, pp. 305-21. F. T. Dolbear et al. (1968), "Collusion in Oligopoly: An Experiment on the Effect of Numbers and Information," Quarterly Journal of Economics, 82, pp. 240-59.

1960s, only about thirty papers and three monographs on experimental economics were produced. Compared to the publications in any other field of economics, this is indeed a very small amount. Experimental economics in this period grew very slowly. However, if the quantity of publications was low, the quality was very high: many of them were published in the most prestigious academic journals such as Econometrica, Journal of Political Economy, and Behavioral Science. Moreover, almost all of these experiments were conducted by American scholars, which is understandable because logical empiricism and behaviorism, both strongly encouraging the use of laboratory experimentation in scientific research, were dominant in the United States. Finally, several distinguished game theorists (Nash, Shubik, Selton) who had strong analytical and mathematical minds were involved in conducting laboratory experiments; two analytic philosophers with logical positivist orientation, Davidson and Suppes, also conducted experiments on individual decision-making. This shows that the tradition of formal research does not necessarily conflict with empirical investigations.

But since the mid-1970s, laboratory experimentation in economics has been transformed "from a seldom encountered curiosity to a systematic investigation,"[278] from a slow growth to an accelerated development. The number of research papers has grown significantly from several to 100 per year; the number of experimental researchers also has increased to hundreds; more than 30 experimental laboratories and research centers have been established;[279] textbook, monograph, and handbook of experimental economics have been published;[280] specialization of research also has

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[278]Roth (1987), eds., Laboratory Experimentation in Economics, p. 1.

[279]C. Plott (1991), "Will Economics Become an Experimental Science?", p. 901.

[280]John Hey's textbook (1991), Experiments in Economics. Davis and Holt's monograph (1993), Experimental Economics. Roth and Kagel (1994), ed. Handbook of Experimental Economics.

emerged.[281] In a word, experimental practice in economics has become well-institutionalized since the 1970s.

Controlled experiments under the laboratory conditions have increasingly played significant roles in theory-testing, fact-finding, and policy recommendations. First, laboratory experimental methods have been used to test (or modify) formal economic theory. For instance, one proposition of neoclassical theory is that markets under certain institutions will eventually converge to the state of competitive equilibrium (Adam Smith's invisible hand). Marxian economists always criticize this proposition, for they argue that what we have observed in reality is market disequalibrium and fluctuations. In a series of experiments conducted by Vernon Smith, he showed that "with remarkably little learning, strict privacy, and a modest number, inexperienced traders converge rapidly to a competitive equilibrium under the double oral auction mechanism."[282] Note that Smith's experimental market works under much *weaker* conditions than the theory suggests: large numbers of economic agents, perfect information, and price-taking behavior are not necessary. Smith's experiments show how the old theory is tested and what "new stylized" facts are discovered.

Second, laboratory experimentation has also played an important role in policy recommendation when some policy issues are raised by government regulatory agencies about the effects of changes in market institutions. Hong and Plott's posted-price markets experiments (1982) were motivated by concern whether the Interstate Commerce Commission should require barge operators to post their prices and to announce price changes in advance. The railroads argued that "public information on

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[281]Researchers at the University of Arizona conduct most of market experiments, those at the University of Pittsburgh conduct game and auction experiments, and those at California Institute of Technology conduct most of resource allocation experiments.

[282]Cf. Smith (1962, 64, 65); also Smith (1991), Papers in Experimental Economics, p. 157.

prices would make prices more competitive and protect small barge owners from large barge owners, who were allegedly making secret price concessions."[283] Their experiments were conducted with the posted-price institution (in which all prices are publicly posted and cannot be changed by the seller for some fixed period) as proposed by the railroads and with a market organization (privately negotiated prices, the existing market regime). They reported: "the results of these experiments were the opposite of those that would be predicted by the railroad industry's analysis. Contrary to the railroads' claims, the posted-price institutions caused prices to go up, efficiency to go down, and the small participants to be disadvantaged" (p.736). The experimental evidence placed a burden on price posting advocates to explain why their proposed policy had such effects. Finally, the proposal was dropped.

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[283]See Plott (1986), "Laboratory Experiments in Economics: The Implications of Posted-Price Institutions," Science, vol. 232, p. 735. Also cf. Hong and Plott (1982), Rate Filing Policies for Inland Water Transportation: An Experimental Approach," The Bell Journal of Economics, 13, pp. 1-19.

Chapter 5. The Implications for Chinese Economics

5.1 The Backwardness of Chinese Economics

The year 1978 was the historical turning point in the development of modern Chinese society. It marked the end of the "dark age," the disastrous "Great Proletarian Cultural Revolution," and the beginning of the new era, the reform of China's institutions and the opening of its door to the world. Since then the economic reform, the largest social experiment in human history, has profoundly reshaped and restructured Chinese society, its political and legal forms, economic and social institutions, and even its cultural values. The gross national product has grown with double-digit speed. "The world's largest communist society could become the world's richest capitalist economy in the next century."[284]

Naturally there has been a very high expectation for Chinese economists to contribute to the country's economic reform and world economics. It is disappointing that many articles in Chinese economics journals in the late 1970s and early 1980s, as one critic observes, "were superficial and dogmatic, unworthy of the term 'economic analysis.'"[285] The primary and fundamental task of economists was no more than to provide theoretical justification for government policies in terms of Marxian political economy. Of course, one cannot deny the fact that economists' interpretations of the Marxian economic theory were purely *pragmatical*. But the purpose of interpretations was always the same: to rationalize the political regime and government policy. In 1974, Marx's political economy was used to justify the repudiation of "bourgeois rights" that were generated by market system, yet the same theory, through a different

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[284]Richard Nixon, Beyond Peace. Cf. Time, May 2, 1994, p. 36.

[285]Robert Hsu (1991), Economic Theories in China, 1979-1988, p. ix.

interpretation, was used again in 1979 to justify market economy and the open-door policy. In recent years many Chinese economists have gradually changed their research strategies from theoretical justifications of government policy to careful investigations of empirical issues. Thus, it is expected that positive economics, to some extent, can be emerged from critical analysis of empirical phenomena.

But not much progress was made thereafter. For instance, one is still puzzled by Chinese economists' frequent confusion between positive analysis and official ideology. In 1987, the theory of the "primary stage of socialism" (which argued that China is only in the primary stage of socialism, and market mechanisms have to be used to promote productivity and thus to consolidate the socialist system) was officially incorporated into the state ideology at the Thirteenth National Party Congress. Liu Guoguang, a leading reformist economist and a vice president of the Chinese Academy of Social Sciences, claims that the "primary-stage theory should be used to guide research in various areas of social sciences in order to deepen our research."[286] In other words, scientific research has to be guided by political ideology. It is obvious that Chinese economics still has a long way to go to gain the status of *science*. Here we should not merely blame Chinese economists (professionally unethical) for their providing "little more than ex-post rationalization of the facts."[287] The reason is that economists act as economic agents, who simply maximize their benefits, given the constraints such as their own analytical training and the regime's political persecution. The crucial issue is to analyze what factors have caused this backward state of Chinese economics.

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[286]Enlightenment Daily, Jan. 2, 1988; p. 1. Quoted in Hsu (1991), p. 12.

[287]Robert Dernberger (1982), "The Status of Economics in China," p. 575.

We must note first that economic research in China had been greatly affected by political propaganda and state ideology. Free scientific inquiries into social and economic phenomena almost became impossible under the communist totalitarian regime. The "double-hundred policy" ("let one hundred flowers bloom and let one hundred schools of thought contend"[288]) has never been persistently implemented by the Chinese government and communist party. Of course, the historic policy of 1956 sometimes did encourage debates and a diversity of views, and indeed provided economists with limited room of academic freedom. But the limits have been dynamically unstable, they can change very quickly in the political arena. As a matter of fact, the "double-hundred policy" was followed by a government crackdown, the "anti-rightist" campaign, in which hundreds of thousands of intellectuals were banished to labor camps. The ideological emancipation from Mao's fundamentalist orthodoxy of the late 1970s and early 1980s was interrupted by the "anti-spiritual pollution" campaign against the influences of Western culture, in which some leading economists like professors Liu Guoguang and Dong Furen felt obliged to condemn "spiritual pollution in economics."[289] The strong advocacy of radical reform in the late 1980s, supported by the Party's General-Secretary Hu Yaobang and Premier Zhao Ziyang, was consecutively followed by the "anti-bourgeois liberalization" campaign, in which Hu was forced to resign, and by the bloody Tiananmen Square massacre, in which Zhao was removed from his office and many leading intellectuals were forced into exile. All of these show how vulnerable the situation of academic freedom is for Chinese intellectuals.

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[288]Cf. Selected Writings of Mao Tze-tung, vol. 5, p. 199.

[289]Cf. Hsu, Economic Theories in China, p. 17.

The state-controlled political environment has indeed deterred economists from constructive criticism and scientific analysis of government policies. Chinese economists have learned very quickly to protect themselves, rather than to risk their professional careers, by adapting to this environment. Some of them keep silence at the critical stage of political battle, but most economists echo political leaders' pronouncements and rationalize government policies. The social conditions such as political persecution and ideological intolerance have been unfavorable to the progress of economics as a science in China.

Chinese economics has not been well institutionalized, and particularly not professionalized, as compared with Western mainstream economics. To institutionalize is "to incorporate into a system of organized and often highly formalized belief, practice, or acceptance," says Webster's Dictionary. Robert Merton, a leading sociologist of science, defines the institutionalization of an intellectual activity as "the relatively dense interaction of persons who perform that activity. The interaction has a structure: the more intense the interaction, the more its structure makes places for authority which makes decisions regarding assessment, promotion, allocation. The high degree of institutionalization of an intellectual activity entails its teaching and administered organization."[290] Contemporary philosophy of science and sociology of science have clearly shown that the development of modern science has proceeded through the process of institutionalization and its by-product, professionalization. In the Western countries, analytical training has become extremely important for the economists' education because economic analysis has been highly professionalized and technically oriented. Through formal, professional training, students are required to have a solid understanding of analytic frameworks, research tools, empirical materials,

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[290]Robert Merton and Jerry Gaston, ed., The Sociology of Science in Europe (1977), p. 7.

and even technical jargons. Thus holding a Ph.D. degree has become a major license to teach in Anglo-American universities. However, very few Chinese economists have received advanced educations. In fact, even prior professional training is not necessary for one to become an economist; "many economists are self-taught or learned their craft on the job." It is thus to be expected that "the level of competence in economic analysis is very uneven among Chinese economists. Many of them, particularly the older ones, are weak in quantitative methods."[291]

Neo-classical economic theory has been the dominant research and teaching paradigm in the West, and significant consensus on the fundamental principles has already been achieved. One is highly surprised, however, by the fact that over the last fifteen years there has been no considerable research consensus on economic theory among Chinese practitioners. Some Chinese economists still stick to the dogmatic Marxian political economy. This is particularly true for those who have taught at the People's University. Many others have committed themselves to doctrines of the Eastern European reformists such as Oscar Lange and W. Brus (Polish), Otar Sik (Czechoslovak), Janos Kornai (Hungarian). Some have adopted the analytic frameworks and concepts of mainstream Western economics for their empirical investigations, a practice consistent with the Chinese tradition--"Western studies for Chinese use." And some economists have tried to synthesize the Marxian political economy, Kornai's theory of economic reform, and neo-classical economics. Indeed, no dominant paradigm in the professional community shows that Chinese economics is still at the stage of transition from ideology to science.

Furthermore, in the West intellectual contributions to economic science have been evaluated solely by the scientific community which has rarely been affected by

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[291]Hsu (1991), p. 14.

political authority. In other words, the rules of game have been made explicitly and implemented effectively by the economics community. However, state ideology and political authority have often intervened in the internal affairs of the Chinese economics profession. Some eminent economists who echoed different voices with the Party have been criticized publicly and banished from the academic world. Ma Yinchu, a prominent economist educated at Yale and Columbia, argued in 1959 that "the population expansion promoted in the Leap [i.e. the Great Leap Forward in 1958] was economically irrational," for modernization "would be achieved not by increasing but by limiting the population."[292] But this criticism led to his removal from the position of the president of Peking University. In 1962 Sun Yefang, the director of the Economic Institute of Chinese Academy of Science, criticized Mao's campaign for "People's Commune" as a "mistake of rash and reckless advance."[293] As a result Sun had been publicly condemned for twenty years. Any economic theory, if providing new justifications for government policy and state ideology, has for that reason been regarded by the Party as a great contribution to economic science, even though it is empirically false.

The situation of academic freedom in China has been improved over the past years as the country has become more open to the world. There are also some steps towards institutionalization and professionalization in Chinese economics. For instance, it is now recognized that those who will teach in major universities must hold Ph.D. degrees. Research techniques, analytic concepts, and even theoretical frameworks of mainstream economics have been employed since the mid-1980s. All these are significant in developing Chinese economics as a strict science. However,

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[292]Merle Goldman (1981), China's Intellectuals (Harvard University Press), p. 59.

[293]Ibid.

there is something fundamental that has been entirely unrecognized by Chinese economists. The fact that the metaphysical, dogmatic Marxian philosophy and its economic methodology have had a great negative impact upon China's economics should not be underestimated. It is my deep conviction that epistemology and methodology matter a great deal especially when economics has gone in a wrong direction. Thus, in this chapter I first make several sharp methodological contrasts between mainstream Western economics and Chinese economics; it will be argued that the backwardness of Chinese economics has been largely due to its inappropriate methodological commitments. I then present my investigation of the philosophical foundations of Chinese economic methodology; it will be shown that economics as a science in China cannot be fully developed in the soil of the dogmatic, metaphysical Marxian philosophy.

5.2 The Methodology of Chinese Economics

Among mainstream economists, there has been a widespread recognition of the unity of science. Paul Samuelson claims that "there is no separate methodological problems that face the social scientist different in kind from those that face any other scientist."[294] However, Chinese economists have had a firm belief that there is a radical distinction between economics, as a social science, and natural science. They have denied any methodological unity of science. This erroneous belief has led Chinese economists for a long time to reject vehemently the application of mathematics to economic research. Before 1979 mathematics was not included in universities' curriculum of economics teaching; since then it has formed only a small, unimportant part. Econometrics, a discipline that incorporates mathematics, statistics, and

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[294]Samuelson, "Economic Theory and Mathematics--An Appraisal," in Machlup (1991), p. 356.

economics, had been regarded as a bourgeoisie pseudo-science, and it had not been introduced into China until the late 1970s. From 1978 to 1987, Economic Researches, the leading Chinese economics journal, published more than 1,000 papers, but only about 15 of them were on mathematical economics.[295] A popular slogan in China was that "you cannot put human nature in a mathematical equation." Powerful phrases that were repeated a century ago in the Western countries to oppose the employment of mathematics have also been frequently echoed in the writings of Chinese economists: "human liberty [relations] will *never* allow itself to be cast into equations," "mathematics ignores frictions which are *everything* in social science."[296]

A widely used argument against mathematics, based on a tenet of dialectical materialism, is that social and economic relations of any kind form an organic unity of quality and quantity, but qualitative analysis is much more important than the quantitative one. In this respect, Marx's Capital has left a great intellectual legacy for Chinese economists. Marx provided some simple quantitative treatment of value, surplus value, wages, profit, average profit, production price, interest, and rent, but the purpose of Capital--A Critique of Political Economy was to provide qualitative analysis of these economic phenomena. According to Marx, classical political economists Adam Smith and David Ricardo "treat the form of value as a thing of no importance, as having no connection with the inherent *nature* of commodities;" their attention is entirely absorbed "in the analysis of the *magnitude* of value."[297] Further, Smith and Ricardo analyzed the magnitudes of profit, interest, and rent, but completely ignored the fact that they are determined by surplus-value. Finally, they investigated

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[295]My calculation does not include several papers on the nature of economic statistics.

[296]These phrases were quoted in Walras' Elements of Pure Economics, 4th ed., 1900, p. 47.

[297]Marx, Capital--A Critique of Political Economy, vol. 1, p. 93, note 1.

the factors that determine the magnitude of wages, but they never found behind the magnitude the essential relation between workers and capitalists. In contrast to the classical economists, Marx investigated what value is and how value becomes exchange-value, how surplus value is transformed into profit, rent, and interest, and why the interest of workers conflicts with that of capitalists under wage forms. In short, the non-mathematical, qualitative analysis Marx provided in Capital is fundamentally different from that of classical political economy.

Before 1978, Chinese economists, following Marx, criticized the classical political economists for stressing quantitative analysis at the expense of qualitative analysis. However, they went much further than Marx, going to the other extreme of stressing qualitative analysis by neglecting, or even discarding, quantitative analysis. This extreme view has been no longer popular over the last ten years, largely due to the fact that policy makers have urged economists to provide more accurate, more quantitative analysis. Zhao Ziyang, the former Premier, said in 1982 that in the future, the State Council would not discuss documents sent in by economic departments if they presented only qualitative analysis but no quantitative information. Many Chinese economists now have realized that economic analysis must consist of both qualitative and quantitative types. Zhang Weida, professor of economics at Liaoning University, wrote in 1983: "quantitative analysis presupposes qualititative analysis. The quantitative analysis is meaningful only under the essential prerequisite of qualitative analysis. Without qualitative analysis, quantitative analysis will lose its direction. It is the weak point of bourgeois political economy that it either ignores or conceal the qualitative aspects of productive relations."[298] It is clear from this passage that Chinese economists still hold the view that qualitative analysis is primary, fundamental,

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[298]Zhang Weida, "Methods of Quantitative Analysis in *Capital* and Their Immediate Significances," Economic Researches, 1983 (4), p. 8. Translations mine.

and essential, while quantitative analysis is secondary, derivative, and inessential. The trouble with this statement is that qualitative analysis in China has often been turned into abstract, empty commentary and speculative inquiry that have fallen into the "quagmire of metaphysics." In fact, there is a great disparity between what they promised and what they have delivered. They promised to combine qualitative analysis with quantitative analysis, but papers published in the leading economics journals show that economists delivered only quanlitative analysis, many of which have fallen into the dangerous domain of metaphysics. But economics is concerned with measurable motives, money and price, wages and profits, rent and interest, inflation and deflation, and all are quantifiable magnitudes. They cannot be adequately treated without employing mathematical tools. Thus, Jevons claimed that "it is perfectly clear that Economy, if it is to be a science at all, must be a mathematical science."[299] The vehement opposition (explicit or implicit) to the mathematical approach, influenced by the Marxian methodology, has been reinforced by the fact that most Chinese economists have been mathematically incompetent. Many of them have not received any mathematical training in their formative years; many of their rejections, in fact, are based on the ignorance and misunderstandings of mathematics. As Menger puts it: "contempt for the formal can be taken seriously only in one who has mastered formalism."[300] In contrast to Chinese economists' opposition to mathematics, the mainstream economists have employed many advanced mathematical tools such as modern logic, topology, linear and dynamic programming, and game theory. As we have already argued in the last chapter, the increased respect for economics as a separate scientific discipline since World War II has been largely due to the massive

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[299]Jevons (1871), The Theory of Political Economy, 1971, p. 3.

[300]Karl Menger (1934), Morality, Decision, and Social Organization, 1974, p. 83.

application of these rigorous mathematical tools. Logic and mathematics not only make the presentation of economic theory more precisely and concisely, but also prove to be powerful tools in analytic transformations because "the human mind is too weak, to recognize immediately all implications in a given set of propositions."[301]

The belief that natural science is sharply different from social science in methodology has also led Chinese economists to reject the possibility and significance of laboratory experimentation in economics. This conviction, of course, can be traced to Marx's erroneous belief in the impossibility of laboratory experiments in the domain of political economy: "in the analysis of economic forms, neither microscopes nor chemical reagents are of use. The force of abstraction must replace both."[302] Marx's statement has been quoted in numerous books and papers by Chinese economists to justify the belief that that methods used in natural science cannot be employed in economics. He Wei, professor of economics at the Chinese People's University, argues that "making economic theory better serve economic reality necessitates first of all an evaluation of research achievements in economics. Unlike the natural sciences, economics cannot be tested or proved in laboratories through the use of the microscope or chemical reagents. Society is its sole testing ground."[303] His claim, directly following that of Marx, is false in the sense that economic theory can be tested in laboratories through the use of human subjects. Another two economists Yu Qingwen and Gong Zhuming assert that "economics lacks experimental foundation." The reason, they give, is that "conducting economic experiment not only takes long periods of time, but also needs huge amounts of money and manpower; all these are often not

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[301]Morgenstern (1936), "Logistics and the Social Sciences," in Morgenstern (1976), p. 393.

[302]Marx (1867), Capital. A Critique of Political Economy, vol. 1, p. 12.

[303]See He Wei's speech, "Evaluate Research Achievements," at the Symposium on the Current Role of Economics. Social Sciences in China, Dec. 1983, p. 15.

permissible in reality."[304] Here, economic experiments are conceived of social experiments only; it seems that controlled laboratory experimentation in economics have never occurred to them. Moreover, their normative conclusion does not follow from the premises. Social experiments such as those of "Social Credit " in Canada and "Negative Income Tax" in the U.S are extremely important in government policy recommendations, for they can "provide significant quantities of new information not available so far."[305] Thus even if conducting experiments takes a long time and requires a lot of money, it is nevertheless justifiable. In a word, the prevailing view, largely due to the dogmatic attitude towards Marxism, is that economic experiments under controlled conditions cannot be conducted.[306]

Moreover, the rejection of laboratory experimentation in economics can also be explained by the Chinese intellectual tradition. In ancient China, the civilization suffered neither a lack of ability in abstract reasoning (because mathematics was even more advanced than other civilizations), nor was it short of empirical observations, in which massive collections of empirical data led to the establishment of astrology rather than astronomy. But it never carried out systematic, controlled experiments of the kind that have played a significant role in the development of modern science. Further, it seems that Chinese intellectuals and social scientists have suspected, distrusted, and even opposed the use of *human* subjects in laboratory experimentation. This opposition has been largely due to political propaganda and ideological campaigning that the so-called "behavioral sciences" are nothing but bourgeoisie pseudo-sciences. The

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[304]Yu Qingwen and Gong Zhuming, "Preliminary Exploration into Mathematical Models for the Systematic Planning of the National Economy," Economic Researches, 1980 (2), p. 52. Trans. mine.

[305]Morgenstern (1954), "Experiment and Large Scale Computation in Economics," p. 499.

[306]Also see Yan Zhenwu, "My View on the Object and Nature of Statistics," Economic Researches, 1980, (5), pp. 74-75.

positivist movement with its wide employment of experimental methods in education, psychology, sociology, and other behavioral sciences has not been initiated in China (and other socialist countries). Thus, the rise of experimental economics has been more difficult.

Chinese economists, influenced by the Marxian economic methodology, have never made the sharp distinction between positive and normative economics, facts and values, "what is" and "what it ought to be." In Marx's view, political economy is intimately related to the conflicting interests of social classes such as workers, capitalists, and landlords; it has not been and cannot be a value-free science. In the preface to the first volume of Capital, he argued rhetorically: "in the domain of Political Economy, free scientific inquiry meets not merely the same enemies as in all other domains. The peculiar nature of the material it deals with, summons as foes into the field of battle the most violent, mean and malignant passions of the human breast, the Furies of private interest."[307] Classical political economists, from Petty to Smith and Ricardo in England, from Physicates to Sismonde in France, have investigated "the *real* relations of production in bourgeois society" and touched "the *true* relation of things."[308] Thus, classical political economy, in investigating empirical issues, had been developed as a science. However, classical economists "stick in their bourgeois skin" and are but "the ideological representatives" of capitalists in their theoretical research.[309] Here "the science of bourgeois economy" had reached the limits beyond which it could not pass. As for the "vulgar economy," it has become the "apologetic" of capitalist system and it thus marked the bankruptcy of "a bourgeois science of

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[307]Marx, Capital, vol.1, p. 15.

[308]Marx, Capital, vol.1, pp. 93 & 594.

[309]Marx, Capital, vol.1, pp. 594 & 627.

political economy" (p.20). In the Materialism and Empirico-criticism (1908), Lenin, following Marx, made an extreme statement: "*not a single* professor of [bourgeois] political economy, who may be capable of very valuable contributions in the field of factual and specialized investigations, can be trusted *one iota* when it comes to the general theory of political economy. For in modern society the latter is as much a *partisan* science as is *epistemology*. Taken as a whole, the professors of economics are nothing but learned salesmen of the capitalist class, while the professors of philosophy are learned salesmen of the theologians."[310]

Marx's effective rhetoric and Lenin's extremely exaggerated claim, quoted on numerous occasions, have profoundly influenced Chinese economists' thinking. They have held that contemporary western economics is nothing but part of capitalist ideology, thus it is "vulgar" and "unscientific." Chinese economists have completely ignored the fact that the mainstream economists have persistently drawn the distinction between positive analysis and value judgments, and have attempted to exclude any normative judgments from positive economics and to "purify" economic science from any ideological or ethical "pollution." Historically, the dichotomy of positive and normative economics can be found in the writings of the 19th-century British economists like Mill, Senior, Caires, and Bagehot,[311] who had been labeled as Vulgar economists" by Marx. But it is the contemporary economists who have drawn such a sharp distinction explicitly, persistently, and emphatically. Milton Friedman, a Nobel Laureate, has emphasized that "there is no value judgment in positive economics;" "positive economics is in principle independent of any particular ethical position or

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[310]V. I. Lenin, Materialism and Empirico-Criticism, in Collected Works of Lenin (Moscow: Progress Publishers, 1962), vol. 14, pp. 342-43.

[311]See J. N. Keynes's review in his The Scope and Method of Political Economy (1980), p. 12.

normative judgments," because it deals with "what is" not with "what ought to be."[312] George Stigler, another Nobel Laureate, claims that "economics as a positive science is ethically--and *therefore politically*--neutral."[313]

Chinese economists not only have believed that it is not possible to make a clear distinction between positive and normative economics, they also have conceived such distinction as undesirable. They have argued that Marx's political economy is the union of science and ideology. In the Poverty of Philosophy (1847), Marx claimed that "just as the [classical] *economists* are the scientific representatives of the bourgeois class, so the *Socialists* and the *Communists* are the theoreticians of the proletarian class."[314] Marx's Capital, which scientifically "lays bare the economic law of motion of modern society," was called, on the Continent, "the Bible of the working class." As Frederick Engels, Marx's lifetime collaborator and closest friend, wrote 1886: "[t]hat the conclusions arrived at in this work [Capital] are daily more and more becoming the fundamental principles of the great working class movement, not only in Germany and Switzerland, but in France, in Holland and Belgium, in America, and even in Italy and Spain; that everywhere the working class more and more recognizes, in these conclusions, the most adequate expression of its condition and of its aspirations, nobody acquainted with that movement will deny."[315] It has been the common view among Chinese economists that Marxist political economy has a strong, distinctive class nature, and that it is geared to serve the practice of the proletariat; it is a science that correctly reflects the objective laws governing social and economic movement.

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[312]Friedman (1967), "Value Judgments in Economics," in Human Value and Economic Policy, ed., by S. Hook (1968), p. 85; and Friedman (1953), Essays in Positive Economics, p. 4.

[313]Stigler (1959), "The Politics of Political Economists," p. 522.

[314]Marx (1847), The Poverty of Philosophy, p. 125. Italics original.

[315]Engels (1888), "Preface to the First English Translation of Capital," in Capital, vol.1, p. 30.

Thus economic research is for no other purpose than to provide justifications for the Communist party's policies. Fang Shen, professor of economics at the People's University, wrote in 1983: "[t]he basic task of theoretical economic research is to reveal the laws of economic development and foresee its courses, which is equivalent to probing into, expounding and summarizing the party's economic policies on a theoretical basis."[316] As Morgenstern says: "economists have been admonished time and time again to leave their political and other value judgments out of their theories and outside their classrooms, or at least to make it clear when they are speaking as scientists, and when as citizens, politicians, religious persons, etc. This advice is well taken and should be scrupulously followed, no matter how difficult."[317]

Chinese economists have persistently drawn the dichotomy of phenomena and essence; what they have committed can be best described as what Karl Popper calls "methodological essentialism." According Popper, the school was founded by Aristotle, who "taught that scientific research must penetrate to the essence of things in order to explain them. Methodological essentialists are inclined to formulate scientific questions in such terms as 'what is matter?' or 'what is force?' or 'what is justice?' and they believe that a penetrating answer to such questions, revealing the real or essential meaning of these terms and thereby the real or true nature of the essences denoted by them, is at least a necessary prerequisite of scientific research, if not its main task."[318] In Chinese economists' view, it is very important to distinguish essence (or law, roughly equivalent to Kant's "things-in-themselves") from phenomena (or appearances, "things that appear"). The essence of things is the law governing the motion of

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[316]Fang Shen, "Probing into, Expounding and Summarizing Party's Policies on a Theoretical Basis," the speech at the Symposium on the Current Role of Economics," Social Science in China, 1983 (12), p. 15.

[317]Morgenstern (1976b), The Selected Works of Oskar Morgenstern, p. 471.

[318]Popper (1957), The Poverty of Historicism (London: Routledge), pp. 28-29.

system, lying much deeper in the empirical world, while empirically observable phenomena conceal and distort the essence. The main task of economic research is to discover economic laws and reveal the essence of economic motions, and then use the essence to explain empirically observable phenomena.[319] In fact, they have argued that only those who side with the party's line and are armed with the Marxian world-view can discover economic laws and penetrate to the essence.

This methodological dogma, in fact, has been backed by Marxian political economy. According to Marx, it is extremely important for scientific researchers to make such a distinction between appearances and their essence, between "all phenomena and their *hidden* substratum." "The former appear directly and spontaneously as current modes of thought; the latter must first be discovered by science" (p.594). Thus, the purpose of scientific inquiry into economic phenomena is ultimately to penetrate to, or discover, the essence of the capitalist system rather than simply "to stick to appearances." In the empirical world, profit appears as price of capital, rent as price of land, and interest as price of money. But scientific analysis, argued Marx, shows that profit, rent, and interest are nothing but the various forms of the essential entity, surplus-value, created only by workers. The essence, the interest-conflict between capitalists, workers, and landlords, is thus revealed. "An exact analysis of the process, therefore, demands that we should, for a time, disregard all phenomena that hide the play of its *inner* mechanism" (p.619). Wages appear as "value and price of labor," but this phenomenal form conceals the essential relation manifested therein, viz., the "value and price of labor-power." The price of goods, the phenomenal form of value, appears as it is determined by both supply and demand of

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[319]Cf. Chen Jiyuan, "The Position of Economic System in the Socialist Part of Political Economy," Economic Researches, 1984, (11), PP. 53-54; also Zhu Tiezhen, "Taking a Dialectical View of the Superiority of the Socialist Economic System," Economic Researches, 1980, (9), p. 40.

goods, but Marx argued that price (or "exchange value") is only determined by the magnitude of the value of goods, and that magnitude is "the amount of labor socially necessary, or the labor-time socially necessary for its production" (p.46). Thus, it seems to Marx that a "scientific analysis" of price is not possible before we have a conception of the inner nature of price, the value created only by labor. It is not possible "just as the apparent motion of the heavenly bodies are not intelligible to any but him, who is acquainted with their real motions, motions which are not directly perceptible by the senses" (p.347). In Marx's view, the phenomenal forms such as wages, profit, and interest offer to "the vulgar economists a secure basis of operations for this shallowness, which on principle worships appearances only" (p.539). Vulgar economy (the school that Marx defined as that after Ricardo and Sismonde), which "has really learnt nothing," "sticks to appearances in opposition to the law which regulates and explains them" (p.335).

It is thus understandable why Marx claimed that the ultimate aim of Capital is "to lay bare the economic *law* of motion of modern society" (p.14). Despite Marx's bold claim, he was never successful in applying the labor theory of value (essence) to give a adequate analysis of profit, rent, and interest, the empirically observable phenomena (appearance).[320] The fundamental point here is not whether labor is the true "source" or "cause" of economic value. One could refute this theory by giving many counter examples. "For economics as a positive science, however, which has to *describe* or *explain* actual processes, it is much more important to ask how the labor theory of value works as a tool of analysis, and the real trouble with it is that it does so very badly."[321] It does not work in the case of oligopoly or monopoly. It cannot

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[320]According to the labor theory of value, price is only determined by the labor, which forms the substance of value. "Commodities, in which equal quantities of labor are embodied, or which can be produced in the same time, have the same value" (p. 46).

explain, for instance, that in 1989, van Gogh's "Portrait of Dr. Gachet" sold for $82.5 million, for there is no proportionality between the market value of the painting and the quantity of labor embodied in it. Moreover, even in the case of perfect competition it works smoothly only if "labor is the only factor of production." As Schumpeter remarked: "Reasoning on the lines of the labor theory of value is hence reasoning on a very special case without practical importance."[322] Since the theory cannot provide accurate descriptions and explanations of "actual processes" or empirically observable phenomena, it is almost "dead and buried" in the Western academic world.

From the positivistic point of view, economics is ultimately conceived as an empirical science. It is primarily and fundamentally concerned with the study of the observable human behavior; it must explain and predict how economic agents actually behave. To save phenomena is the only business of economic research. Thus, modern economic analysis has not been concerned with the question of "what" but of "*how*," they have been committed to the functionalism or/and behaviorism rather than essentialism. The theory of marginal utility, the core of mainstream economics, is superior to Marx's labor theory of value for these purposes. It has provided much more adequate explanations of *actual processes* such as those of oligopoly, monopoly, and perfect competition. Moreover, penetrating the phenomena of things to study their essence falls inevitably into the domain of speculative metaphysics. It is not surprising that Marx was accused of treating economics "metaphysically" by many of his contemporary and modern critics.[323] Modern economists have realized the danger of essentialism: one is "thereby moving on dangerous ground, surrounded by swamps of

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[321]Joseph Schumpeter (1951), Ten Great Economists. From Marx to Keynes, p. 28. Italics added.

[322]Schumpeter (1951), Ten Great Economists, pp. 28-29.

[323]Quoted in Marx, Capital, vol.1, p. 21.

pseudo-problems."[324]

5.3 Philosophical Foundations

The last section presented a striking contrast in methodology between the Western mainstream economics and Chinese economics. It was shown how significantly the different methodological orientations of the two different worlds have affected the advancement of economic science and the practices of working economists. In the last analysis, economic methodology cannot be separated from, and must be backed by, its philosophical foundations. Mainstream economics, in fact, has been methodologically and epistemologically supported by logical empiricism, while Chinese economics has been founded on Marxian economic methodology and its philosophical doctrine, dialectical and historical materialism. In this section, I attempt to conduct a further investigation of the backwardness of Chinese economics: the study will shift from economic methodology to philosophical foundation. My purpose is to show that Chinese economics cannot be fully developed as a strict science in the soil of dogmatic, metaphysical Marxian philosophy.

From an analytic point of view, it seems that Marx's philosophy can be eminently compatible with logical positivism. In the Scientific Conception of the Vienna Circle, the "manifesto" of the new philosophical movement, Marx was listed as a precursor of logical positivism. This can hardly be surprising. First, both faced and attacked the same enemy--the traditional metaphysics of Plato and Descartes, Leibniz and Hegel. Logical positivism rejected all metaphysical statements as devoid of cognitive meanings, and as having added nothing to the stock of our knowledge. Similarly, Marx launched attacks on traditional metaphysics, for it conceived whole

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[324]Menger (1973), "Austrian Marginalism and Mathematical Economics," in Carl Menger and the Austrian School of Economics, p. 55.

history as a series of ideological conflicts that are determined and created by social consciousness. In 1845, he criticized the "drunken speculation" of Hegel's Absolute Idealism: "*Hegel's* conception of history assumes an *Absolute* or *Absolute Spirit* which develops in such a way that mankind is a mere *mass* bearing it with a varying degree of consciousness or unconsciousness. Within *empiric*, exoteric history he therefore has a *speculative*, esoteric history develop. The history of mankind becomes the history of the *abstract* spirit of mankind, a *spirit beyond all man*!"[325] Contrary to Hegel's metaphysical speculation, Marx undertook massive empirical, scientific investigations of modern capitalist economic systems and ancient societies. In this aspect, he was influenced by his contemporary materialist, positivist philosopher Ludwig Feuerbach, who was also acknowledged in the manifesto as a precursor of logical empiricism.[326]

Second, logical positivists held that once all metaphysical, ethical, and theological statements are to be completely rejected, and once the context of discovery is clearly distinguished from the context of justification, what remains for philosophy is epistemology, or what Carnap called "the logic of science."[327] Epistemology is the only *legitimate* field of philosophy. Engels, a founder of the Marxian philosophy, would entirely agree with the logical positivists' view, for he claimed that what remains for philosophy is formal "logic" and "epistemology" after the rejection of all traditional metaphysics.[328]

Third, both logical positivism and Marxism held a hostile attitude towards religion. For instance, Ayer and Carnap, two leading logical positivist philosophers,

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[325]Marx and Engels, The Holy Family (London: Lawrence & Wishart, 1956), p. 115. Italics original.

[326]Marx wrote in 1845: "Feuerbach represented *materialism* in the *theoretical* domain." Ibid., p. 168.

[327]Carnap (1934), "The Logic of Science," in McGuinness (1987), p. 46.

[328]Engels (1888), Ludwig Feuerbach and the Outcome of Classical German Philosophy, p. 35.

were in fact atheists. So were Marx and Engels, for whom, religion is "the *opium* of the people."[329] In short, the affinities between logical positivism and the philosophy of Marx and (Engles) cannot be denied. As a matter of fact, Bogdanov, Lunacharsky, and other Bolshevik Party philosophers, attempted to "reconcile" Mach's positivism and Marxism at the very beginning of the century.

But Marx's philosophy appears with a double face. Its other face, the metaphysical and speculative aspects, also cannot be denied. Besides, there is a difference between the philosophy of Marx and the Marxian philosophy. In fact, the Marxian philosophy, by means of a different interpretation of the philosophy of Marx, has even become anti-positivist, dogmatic, ideological in the contemporary communist movement of Russia and China. The just-started movement of the articulation of Marxism with Mach's positivism in Russia, unfortunately, was immediately crackdown by Lenin, the Bolshevik Party's leader, who waged a war against all varieties of empiricism such as positivism and pragmatism. In Materialism and Empirio-criticism. Critical Comments on a Reactionary Philosophy, Lenin launched a severe attack on empiricist philosophers such as Richard Avenarius, Helmholtz, Poncaré, and especially Ernst Mach, the "godfather" of logical positivism (or what Lenin called "modern positivism," or "empirio-criticism"). Mach became the main target of the attack because he "characterizes Engels's laws of nature, laws of external nature, and the necessity of nature as 'metaphysics';" because "a red thread that runs through *all* the writings of *all* the Machists is the stupid claim to have risen above materialism and idealism, to have transcended this obsolete antithesis;" because he declares that "religious opinion is a private affair."[330] All these, according to Lenin, illustrate "the

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[329]Marx (1843), "Contributions to the Critique of Hegel's Philosophy of Right," in Karl Marx, Early Writings, trans. and ed. by T. Bottomore, (London: C.A. Watts & Co. Ltd., 1963), p. 44. Italics original.

actual class utilization of empirio-criticism by bourgeois reactionary." Thus, modern positivism was politically proclaimed as a form of "reactionary bourgeois philosophy."[331] The ally of positivism, American pragmatism initiated by Charles Peirce, William James, and John Dewey, was also attacked by Lenin, because it "ridicules the metaphysics both of materialism and idealism, acclaims experience and only experience, recognizes practice as the only criterion, refers to the positivist movement in general, *especially turns for support to Ostwar, Mach, Pearson, Poincaré* and *Duhem*" (italics original, p.342). Pragmatism was politically labeled as "one of the most reactionary philosophical trends of modern times, a convenient form for theoretically defending the interests of the imperialist bourgeoisie" (p.386).

Materialism and Empirio-criticism has had no impact on the western academic world. It contributed nothing to analytical philosophy nor even to speculative inquiry; it is a purely ideological propaganda. Yet it had been regarded as "an extremely important philosophical work" and become a "Philosophical Bible" for nearly a century in the communist world.[332] More than any other work of the founders of Marxism, this single book, to which students have to devote much time and by which philosophical researches have to be judged, has significantly affected the Chinese Marxian philosophy. In the 1950s, the communist regime in China launched a big campaign (which was neither discussion nor debate) against John Dewey's pragmatism and his chief Chinese protégé, Hu Shih, who in the 1920s/30s had initiated a positivist/pragmatist movement in China where metaphysical and speculative philosophies had been dominant for more than two thousand years.[333] It is thus not

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[330]Lenin (1908), Materialism and Empirio-criticism, pp. 188, 341, & 344.

[331]Ibid., p. 342., footnote, and p. 343.

[332]See the preface to volume 14 of Collected Works of Lenin, p. 11.

[333]Cf. D. Kwok (1965), Scientism in Chinese Thought (1900-1950), chapter 4.

surprising that positivism, along with other forms of empiricism, has not been able to flourish under any communist regime.

We may remind ourselves that logical positivism was historically rooted in two different intellectual traditions, the empiricism of Hume, Comte, Mill, and Mach, and the logicism of Frege and Russell. Both insisted that we can derive all knowledge from experience and both radically rejected *all* variety of speculative metaphysics. Logical positivists declared themselves to have risen above the metaphysical statements such as those of materialism or idealism and they avoided making any ontological commitments. In contrast, Marx, despite his overturning the hierarchical structure of traditional metaphysics, did not deconstruct, displace, or dislodge the speculative system; he was a critic of old traditional metaphysics, but never rejected metaphysics itself as a legitimate discipline. In the afterword to the second edition of the first volume of Capital, he, on the one hand, rejected the Hegelian mysticism, and expressed the metaphysics of materialism on the other: "to Hegel, the life-process of the human brain, i.e., the process of thinking, which, under the name of 'the Idea,' he even transforms into an independent subject, is the demiurgos of the real world, and the real world is only the external, phenomenal form of 'the Idea.' With me, on the contrary, the ideal is nothing else than the material world reflected by the human mind, and translated into forms of thought."[334] Marx claimed that Hegel's philosophy "is standing on its head," and "it must be turned right side up again." His reversal of Hegel's "absolute idealism" leads to the reconstitution of metaphysics: matter over mind, life over consciousness, and economic basis over "political and ideological superstructure." These ontological commitments of materialism were emphasized again and again by Engels, Lenin, and Chinese Marxist philosophers.[335]

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[334]Marx, Capital, vol. 1, p. 24.

The metaphysical feature of Marx's philosophy is exemplified by his historical conception of materialism. Marx argued, first, that "in a social production of their life," men enter into definite relations of production that are indispensable and independent of their will; "the sum total of these relations of production constitutes the economic structure of society, the real foundation, on which rise legal and political superstructure and to which correspond definite forms of social consciousness."[336] As he said: "The mode of production of material life conditions the social, political and intellectual life process in general. It is not the consciousness of men that determines their being, but, on the contrary, their social being that determines their consciousness."

Second, Marx claimed that economic change will lead the *dynamic* transformation of political superstructure and social consciousness. With the change of the economic foundation, the entire immense superstructure, such as the legal, political, religious, aesthetic or philosophic--in short ideological forms, is more or less rapidly transformed. As he said: "we cannot judge of such a period of transformation by its own consciousness; on the contrary, this consciousness must be explained rather from the contradictions of material life, from the existing conflict between the social productive forces and the relations of productions."

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[335]In his Ludwig Feuerbach, Engels declared that "The great basic question of all philosophy especially of modern philosophy, is that concerning the relation of thinking and being, spirit and nature." Lenin in the Materialism and Empirio-criticism also declared that "the fundamental philosophical trends are materialism and idealism. Materialism regards nature as primary and spirit as secondary; it places being first and thought second. Idealism holds the contrary view. This root distinction between the 'two great camps' into which the philosophers of the 'various schools' of idealism and materialism are divided Engels takes as the corner-stone, and he directly charges with 'confusion' those who use the terms idealism and materialism in any other way." Ai Ssu-ch'i, "China's single most important populariser of the philosophy of Marxism," claimed that "matter is primary,...spirit and consciousness are secondary." (see J. Fogel, p. 45).

[336]From Marx's preface to A Contribution to the Critique of Political Economy, in Caute (1967), p. 49.

Third, Marx emphasized that the religious world is but the reflection of the real world. People's religious belief, their "false social consciousness," is a reflection of their positions in society, no matter how distorted the reflection might be. As he said; "*Religious* suffering is at the same time an *expression* of real suffering and a *protest* against real suffering. Religion is the sigh of the oppressed creature, the sentiment of a heartless world, and the soul of soulless conditions."[337] It thus followed that man cannot emancipate himself from religion until its social conditions are fully abolished. "The religious reflex of the real world can, in any case, only then finally vanish, when the practical relations of everyday life offer to man none but perfectly intelligible and reasonable relations with regard to his fellowmen and to nature."[338]

Logical empiricism has argued that facts and values must be strictly dichotomized. It does not deny that ethical values are indispensable for individuals as well as for communities. However, value statements are significantly devoid of cognitive meanings. The lack of cognitive significance has led logical empiricists to exclude value judgments from science and philosophy; there is no room for value judgments in science and epistemology for they can be neither confirmed nor verified. In contrast, Marxian philosophy has strong a value commitment and argues that value-free philosophy is impossible. Marx believed that it was his philosophy that brought to the proletarian the consciousness of its role. He declared that "just as philosophy finds its *material* weapons in the protariat, so the proletariat finds its *intellectual* weapons in philosophy."[339] Lenin fundamentally rejected any reconciliation between materialism and idealism, any combination between Marxism and modern positivism. He argued

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[337]Cf Karl Marx's Early Writings, p. 44. Italics original.

[338]Marx, Capital, vol.1, pp. 91-92.

[339]Marx (1844), "Contribution to the Critique of Hegel's Philosophy of Right," Early Writings, p. 59.

that "behind the epistemological scholasticism of empirio-criticism one must not fail to see the struggle of parties in philosophy, a struggle which in the last analysis reflects the tendencies and ideology of the antagonistic classes in modern society. Recent philosophy is as partisan as was philosophy two thousand years ago."[340] In the western countries the professors of philosophy are nothing but "learned salesmen of the theologians," claimed Lenin. All these extreme claims have been followed by Chinese philosophers. Ai Ssu-ch'i, "the Communist Party's chief thinker" from the 1930's to 1960's, put it directly: "all knowledge is class knowledge, class determines philosophy, and all philosophy is partisan."[341]

As for its view regarding the function of philosophy, logical positivism is fundamentally different from the Marxian philosophy. Since logical positivists reject all metaphysical statements and delimit the subject of philosophy to epistemology, as shown in chapter 2, they have radically changed the traditional view of the function of philosophy. Philosophy is conceived of as a "critique of language:[342] "the task of philosophical work lies in this clarification of problems and assertions, not in the propounding of special 'philosophical' pronouncements."[343] But from the viewpoint of Marxism, this positive conception of philosophy must be completely rejected. In The German Ideology, Marx and Engels wrote: "philosophy and the study of the actual world have the same relation to one another as masturbation and sexual love."[344] What they proposed, of course, was not to dismiss or discontinue philosophy itself.

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[340]Lenin, Materialism and Empirio-criticism, p. 358.

[341]Fogel, Ai Ssu-ch'i's Contribution to the Development of Chinese Marxism, p. 45.

[342]All quotes are from Wittgenstein, Tractatus Logico-Philosophicus, 4.112, 4.11, 4.111.

[343]Neurath (1973), Empiricism and Sociology, p. 306.

[344]Marx and Engels (1845), The German Ideology, p. 103.

Instead, they criticized their contemporary speculative metaphysicians who essentially separated philosophy from reality, theory from revolutionary praxis. "The philosophers have only *interpreted* the world differently, the point is, to *change* it."[345] Marxist philosophy, as it seemed to them, must be ultimately united with the workers movement. Marx argued metaphorically: "The *emancipation of Germany* will be an *emancipation of man. Philosophy* is the *head* of this emancipation and the *proletariat* is its *heart*. Philosophy can only be realized by the abolition of the proletariat, and the proletariat can only be abolished by the realization of philosophy."[346]

In the 20th century, the philosophy of Marx has been radically transformed into official ideology and political propaganda for communist movements. Once Marxian philosophy became an official ideology, its impact on the development of science and philosophy itself is disastrous. All scientific researches (including researches in physical, biological, and economic sciences) have to be guided and judged by the Marxian philosophy under the communist regimes. It now seems that unless economics is freed from ideology, unless it is disconnected from the philosophy of Marxism, the science of economics cannot flourish in China.

5.4 Conclusions

In the last two decades, professional scholars and ordinary citizens have witnessed "a tremendous resurgence" of interest in epistemological, methodological, and sociological issues concerning economic science. This has led to the publications of numerous books such as The Methodology of Economics, Beyond Positivism: Economic Methodology in the Twentieth Century, Methodology and Economics, and

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[345]Cf. David Caute, Essential Writings of Karl Marx, p. 43. Italics original.

[346]Marx (1844), Contribution to the Critique of Hegel's Philosophy of Right. ERKM, p. 59.

Economics and the Philosophy of Science, to name only few. The present research, in fact, was stimulated by, and has benefited from, the all previous work. Looking back over the present research, I like to identify three areas in which it has made contributions to philosophy and economics.

5.4.1 Contribution to the History of Economic Methodology. The fundamental differences between the present research and the previous study on the history of economic methodology are twofold. First, all recent publications on the impact of logical positivism upon the social and behavioral sciences have completely ignored the fact that Oskar Morgenstern's writings were dynamically influenced by the logical positivist philosophy; Hutchison, Friedman, and Samuelson have been frequently mentioned but never Morgenstern. Mark Blaug's Methodology of Economics (1980), the most popular and influential book on the subject, does not even mention Morgenstern's work in this field,[347] nor do the writings of philosophers of economics like Daniel Hausman and Alexander Rosenberg.[348] The present research showed that Morgenstern was a strong advocate of the methodological principle of positivism, that methods employed in natural science such as mathematical techniques and laboratory experimentations can and should be employed in the social sciences, particularly in economics.

Second, authors of the all previous work, restricted by their trainings or professions, have not paid enough attention to the historical fact that logical positivism *dynamically* influenced the methodology of economics, i.e., the *evolution* of the positivist philosophy had been continuously reflected in the writings of economic

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[347]Blaug's book has been extremely popular: 1st ed. 1980, reprinted: 81, 82, 83, 84, 85, 86, 87, 88, 90. Bruce Caldwell does not mention Morgenstern either. See his Beyond Positivism: Economic Methodology in the Twentieth Century (1982).

[348]Hausman (1991), The Inexact and Separate Science of Economics; and Rosenberg (1992), Economics--Mathematical Politics or Science of Diminishing Returns?.

methodologists. The present work showed that Hutchison's work (1938) reflected "the *classical* position of the Vienna Circle"[349] and Machlup's work (1955) reflected the later *changed* position of logical positivism; and I emphasized that the *debate* between Hutchison and Machlup in the mid-1950s is a reflection of the *difference* between the classical and the changed positions of logical positivism itself. The present work, based on the detailed analysis of the historical texts, carefully identified where and how each author was influenced by the new philosophy.

5.4.2 Contribution to the History of Social Science. I believe the central weakness of current study of economic methodology is that researchers have not engaged in any *empirical* investigation of the history of economic science; they sit in armchairs, speculate about what methodologists have said, but never "get their hands dirty;" they never take a look at what the working economists have practiced. The speculation has led both pro- and anti-positivist methodologists to the erroneous conclusion that working economists have only paid lip services to logical positivism. Ray Canterbery and Robert Burkhardt argue that the positivist methodology is unsound, not simply because logical positivism is false, and therefore dead, but also because it has never been seriously practiced by the professionals: "economists seem to pay only lip service to the positivist ideas, going off and doing their own things irrespective of whatever normative criteria philosophers of science have found to be sound."[350] Mark Blaug, on the other hand, holds that "there is nothing much wrong with standard [positivistic] economic methodology as laid down in the first chapter of almost every textbook in economic theory; what is wrong is that economists do not

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[349]This phrase was coined by Ayer, see his Logical Positivism, p. 2.

[350]Canterbery and Burkhardt, "What do we mean by asking whether economics is a science?" in Alfred Eichner (1983), Why Economics is not yet a Science?, p. 22.

practice what preach."[351] These claims are bold, but unfortunately, without the support of any empirical evidence from the actual activities of economic scientists.

By contrast with the standard way of doing methodology, the present research undertook empirical investigation of the historical record of scientific economics. I surveyed numerous articles and books and paid great attention to the actual practice of the working economists. It has been shown that the development of modern economic science has been generally in keeping with the methodological prescriptions (epistemologically backed by logical positivism) in both formal and empirical aspects. I used three examples (the proof of the existence of general equilibrium, the construction of expected utility theory, and the development of experimental economics) to demonstrate that working economists have not merely paid lip service to logical empiricism, they have actually made serious epistemological commitments to scientific philosophy.

5.4.3 Contribution to Chinese Economics. The present research has made important contributions to China's study of economics. From perspectives of epistemology, methodology, and sociology, this study has explained why mainstream Western economics is so advanced and why economics in China is so backward. By my personal experience and critical analysis, I believe there is something fundamentally wrong with China's economics that, unless it radically changes its epistemological principles and methodological rules, little advance in scientific economics can be expected. I completely agree with many Chinese economists that philosophy makes a difference, but I argued that the Marxian philosophy has made a wrong difference--it has had a negative impact upon China's economics. Thus I have urged Chinese economic researchers to abandon the Marxian dogmatic metaphysics and to adopt the

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[351]Mark Blaug (1980), The Methodology of Economics, p. xiii.

empiricist philosophy. It is my deep conviction that Chinese economics cannot be scientific unless it is founded on the empiricist methodology. Moreover, I have urged Chinese economists to substitute positive analysis for moral preaching, exact inquiries for guesswork, and empirical/experimental investigation for metaphysical speculation.

Since the first draft of this work was finished in last May, three great intellectual events have occurred in China, all reported in People's Daily, the largest official newspaper in the country. The first economic science laboratory in China has been established at the People's University. According to the report, the laboratory, funded by the central government and financial institutions, is going to use experimental approaches to make policy recommendation as well as to test economic theory. This shows that the Chinese economics community has eventually abandoned the Marxian dogma that economics is a non-experimental discipline. It is expected that economics in China will become less speculative, more experimental, much like the physical and biological sciences. Moreover, it indicates that even the extreme-leftist university in China is moving in the positivist direction, for the People's University has been famous, as well as notorious, for the predominance of Mao's fundamentalists.

Second, The International Conference of Tscha Hung's Contribution to Philosophy was held at Beijing University, the Chinese home of intellectual excellence and liberalism. Dr. Hung, a member of the Vienna Circle and close associate with the leading positivists such as Schlick, Feigl, Hempel, and Ayer, has been chiefly responsible for the introduction of logical positivism into China. The significance of this conference lies not merely in the official recognition of Hung's personal contribution to philosophy, it testifies that the Chinese philosophical community has radically changed its extreme anti-positivist mentality that positivism is "a reactionary philosophy," defending "the interests of the imperialist bourgeoisie." Hopefully, the scientific thinking of logical positivism will flourish and provide an epistemological

foundation for the science of economics. It is expected that Beijing University, where its president Dr. Shi Hu, the Chinese protégé of John Dewey, taught pragmatism, will continuously play a leading role in promulgating positivism.

Third, last year the State Educational Commission of China issued a new regulation that all major universities faculty must hold Ph.D. degrees to practice teaching and research. The significance of this regulation, as I can perceive from the sociological perspective, is that scientific research activities in China will become more institutionalized and particularly, more professionalized. I believe that this institutional change will become a major driving force for the rapid advance of social sciences, particularly of economics, because through the professional training, prospective economists will be incarnated into the current research paradigms.

All these most recent developments convince me that scientific economics in China is taking off in the right direction. With the success of its economic reform, the collapse of communism, and the growing integration of the global economy, there is a great opportunity, as we can perceive now, for Chinese economists to undertake a revolutionary transformation in China's economics .

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