John B Woosley

ELEVENTH PRESIDENT OF THE SOUTHERN ECONOMIC ASSOCIATION, 1939–1940

(Correction: The April, 1955 issue of this Journal, in which appears the photograph of Dean Robert H. Tucker, of Washington and Lee University, failed to carry the legend that he was the Tenth President of the Southern Economic Association, during 1938–1939.)

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THE PROBLEM OF VERIFICATION IN ECONOMICS*

FRITZ MACHLUP

The Johns Hopkins University

I

It will be well for us first to clear the ground lest we get lost in the rubble of past discussions. To clear the ground is, above all, to come to a decision as to what we mean by verification and what it can and cannot do for our research and analysis.

The Meaning of Verification

A good book of synonyms will have the verb "verify" associated with the more pretentious verbs "prove," "demonstrate," "establish," "ascertain," "confirm," and with the more modest verbs "check" and "test." The verbs in the former group would usually be followed by a "that"—"we shall prove that . . ."—the verbs in the latter group by a "whether"—"we shall check whether. . . ." Besides this difference between "verify that" and "verify whether," there is the difference between verification as a process and verification as an affirmative result of that process. By using "*test*" for the former and "*confirmation*" for the latter we may avoid confusion. Where the distinction is not necessary, "verification" is an appropriate weasel-word, meaning both test and confirmation.

Verification in research and analysis may refer to many things including the correctness of mathematical and logical arguments, the applicability of formulas and equations, the trustworthiness of reports, the authenticity of documents, the genuineness of artifacts or relics, the adequacy of reproductions, translations and paraphrases, the accuracy of historical and statistical accounts, the corroboration of reported events, the completeness in the enumeration of circumstances in a concrete situation, the reliability and exactness of observations, the reproducibility of experiments, the explanatory or predictive value of generalizations. For each of these pursuits, the term verification is used in various disciplines. But we intend to confine ourselves to the last one mentioned: the verification of the explanatory or predictive value of hypothetical generalizations.

Although definitions are sometimes a nuisance rather than an aid, I shall try my hand at one, and say that verification in the sense most relevant to us—the

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testing of generalizations—is *a procedure designed to find out whether a set of data of observation about a class of phenomena is obtainable and can be reconciled with a particular set of hypothetical generalizations about this class of phenomena.*

Truth and Reality

I have carefully avoided the words "truth" and "reality," although the Latin *veritas* forms the root of the term defined. I eschewed references to truth and reality in order to stay out of strictly epistemological and ontological controversies. Not that such discussions would be uninteresting or unimportant; he who never studies metaphysical questions, and even prides himself on his unconcern with metaphysics, often does not know how much in fact he talks about it. To stay away from metaphysics one has to know a good bit about it.

The function of words chosen—testing, checking, confirming—is precisely to enable us to leave the concepts of truth and reality in the background. If I should slip occasionally and say that a proposition is "true" or a phenomenon is "real," this should be taken merely as an unguarded way of speaking; for I mean to say only that there seems to be considerable "support" or "evidence" for the proposition in view of a marked *correspondence* or consistency between that proposition and statements about particular observations.

Special and General Hypotheses

My definition of verification related only to hypothetical generalizations. But the status of *special hypotheses about single events or unique situations* (and their causes, effects, and interrelations) also calls for examination, for it is with these that economic history and most of applied economics are concerned. Such special hypotheses—to establish the "facts"—are of course also subject to verification, but the rules and techniques are somewhat different from those of the verification of general hypotheses.

In a murder case we ask "who done it?" and the answer requires the weighing of several alternative special hypotheses. Such special hypotheses may be mental constructions of unobserved occurrences which could have taken place in conjunction with occurrences observed or conclusively inferred. It is an accepted rule that a special hypothesis will be rejected if it is contradicted by a single inconsistency between a firmly established observation and any of the things that follow logically from the combination of the special hypothesis and the factual assumptions of the argument.

But this weighing and testing of special hypotheses in the light of the known circumstances of the case always involves numerous *general* hypotheses. For example, the generalization that "if a man is at one place he cannot at the same time be at another place" may be of utmost importance in verifying a suspicion that Mr. X was the murderer. And whenever observations have to be interpreted and special hypotheses applied to reach a conclusion about what are the "concrete facts," the argument will presuppose the acceptance of numerous general theories or hypotheses linking two or more (observed or inferred) "facts" as possible (or probable) causes and effects. This is the reason why it has to be said over and over again that most of the facts of history are based on previously formed general hypotheses or theories. Although this has been an important theme in the discussion of the relation between theory and history, and one of the central issues in the *Methodenstreit* in economics, it is not an issue in our discussion today. At the moment we are concerned with the verification of general hypotheses and theories, not of propositions concerning individual events or conditions at a particular time and place. But this much ought to be said here: to establish or verify "historical facts," we must rely on the acceptance of numerous general hypotheses (theories); and to verify general hypotheses we must rely on the acceptance of numerous data representing "facts" observed or inferred at various times and places. We always must take something for granted, no matter how averse we are to "preconceptions."

Theories, Hypotheses, Hunches, Assumptions, Postulates

No fixed lines can be drawn between theories, hypotheses, and mere hunches, the differences being at best those of degree. There are degrees of vagueness in formulation, degrees of confidence or strength of belief in what is posed or stated, degrees of acceptance among experts, and degrees of comprehensiveness or range of applicability.[1]

A hunch is usually vague, sometimes novel, original, often incompletely formulated; perhaps more tentative than a hypothesis, although the difference may lie just in the modesty of the analyst. A hypothesis may likewise be very tentative; indeed, some hypotheses are introduced only for didactic purposes, as provisional steps in an argument, in full knowledge of their inapplicability to any concrete situation and perhaps in preparation for a preferred hypothesis. Distinctions between hypotheses and theories have been suggested in terms of the strength of belief in their applicability or of the comprehensiveness (range) of their applicability.[2] But so often are the words theory and hypothesis used interchangeably that there is not much point in laboring any distinguishing criteria.

Perhaps it should be stressed that every hypothesis may have the status of an "assumption" in a logical argument. An assumption of a rather general nature which is posited as a "principle" for an argument or for a whole system

[1] The belief that a "hunch" is something fundamentally different from a "theory" may be responsible for certain antitheoretical positions of some historians and statisticians. Those who claimed the priority and supremacy of fact-finding over "theoretical speculation" might have accepted the contention that you cannot find facts without having some hunch. But this is practically all that the theorists meant when they claimed that theory must precede fact-finding, whether historical or statistical, and that history without theory, and measurement without theory are *impossible*. There are kinds of fact-finding which presuppose full-fledged theories; some simpler kinds may start with vague hunches.

[2] "A hypothesis is an assumption . . . tentatively suggested as an explanation of a phenomenon." Morris R. Cohen and Ernest Nagel, *An Introduction to Logic and Scientific Method* (New York: Harcourt, Brace, 1938), p. 205.—"A hypothesis . . . is . . . a theory which has, at present at least, a limited range of application. It is promoted to the status of a theory if and when its range is deemed sufficiently large to justify this more commendatory appelation." Henry Margenau, "Methodology of Modern Physics," *Philosophy of Science*, Vol. II (January 1935), p. 67.

of thought, but is neither self-evident nor proved, is often called a "postulate." Just as there may be a connotation of tentativeness in the word "hypothesis," there may be a connotation of arbitrariness in the word "postulate."[3] But since no fundamental assumption in an empirical discipline is definitive, and since all are more or less arbitrary, it is useless to insist on subtle distinctions which are (for good reasons) disregarded by most participants in the discussion.[4]

Confirmation versus Non-Disconfirmation

How is a hypothesis verified? The hypothesis is *tested* by a two-step procedure: first deducing from it and the factual assumptions with which it is combined all the conclusions that can be inferred, and second, confronting these conclusions with data obtained from observation of the phenomena concerned. The hypothesis is *confirmed* if reasonable correspondence is found between the deduced and the observed, or more correctly, if no irreconcilable contradiction is found between the deduced and the observed. Absence of contradictory evidence, a finding of non-contradiction, is really a negation of a negation: indeed, one calls a hypothesis "confirmed" when it is merely *not dis*confirmed.

Thus, the procedure of verification may yield findings compelling the rejection of the tested hypothesis, but never findings that can "prove" its correctness, adequacy or applicability.[5] As in a continuing sports championship conducted by elimination rules, where the winner stays in the game as long as he is not defeated but can always be challenged for another contest, no empirical hypothesis is safe forever; it can always be challenged for another test and may be knocked out at any time. The test results, at best, in a "confirmation till next time."

Several logicians use the word "falsification" for a finding of irreconcilable contradiction; and since a hypothesis can be definitely refuted or "falsified," but not definitely confirmed or "verified," some logicians have urged that we speak only of "falsifiable," not of verifiable propositions. Because the word "falsification" has a double meaning, I prefer to speak of refutation or disconfirmation. But the dictum is surely right: testing an empirical hypothesis results either in its disconfirmation or its non-disconfirmation, never in its definitive confirmation.

[3] Cf. Wayne A. Leeman, "The Status of Facts in Economic Thought," *The Journal of Philosophy*, Vol. XLVII (June 1951), p. 408.—Leeman suggests that economists prefer the term "assumption" because it "escapes . . . the undesirable connotations" of the terms "hypothesis" and "postulate."

[4] "So far as our present argument is concerned, the things (propositions) that we take for granted may be called indiscriminately either hypotheses or axioms or postulates or assumptions or even principles, and the things (propositions) that we think we have established by admissible procedure are called theorems." Joseph A. Schumpeter, *History of Economic Analysis* (New York: Oxford University Press, 1954), p. 15.

[5] There are no rules of verification "that can be relied on in the last resort. Take the most important rules of experimental verification: reproducibility of results; agreement between determinations made by different and independent methods; fulfillment of predictions. These are powerful criteria, but I could give you examples in which they were all fulfilled and yet the statement which they seemed to confirm later turned out to be false. The most striking agreement with experiment may occasionally be revealed later to be based on mere coincidence. . . ." Michael Polanyi, *Science, Faith and Society* (London: Cumberlege, 1946), p. 13.

Even if a definitive confirmation is never possible, the number of tests which a hypothesis has survived in good shape will have a bearing on the confidence people have in its "correctness." A hypothesis confirmed and re-confirmed any number of times will have a more loyal following than one only rarely exposed to the test of experience. But the strength of belief in a hypothesis depends, even more than on any direct empirical tests that it may have survived, on the place it holds within a hierarchical system of inter-related hypotheses. But this is another matter, to be discussed a little later.

Nothing that I have said thus far would, I believe, be objected to by any modern logician, philosopher of science, or scientist. While all points mentioned were once controversial, the combat has moved on to other issues, and only a few stragglers and latecomers on the battlefield of methodology mistake the rubble left from long ago for the marks of present fighting. So we shall move on to issues on which controversy continues.

II

Which kinds of propositions can be verified, and which cannot? May unverified and unverifiable propositions be legitimately retained in a scientific system? Or should all scientific propositions be verified or at least verifiable? These are among the controversial issues—though my own views are so decided that I cannot see how intelligent people can still quarrel about them, and I have come to believe that all good men think as I do, and only a few misguided creatures think otherwise. But I shall restrain my convictions for a while.

Critizing extreme positions is a safe pastime because one may be sure of the support of a majority. But it is not for this reason but for the sake of a clear exposition that I begin with the presentation of the positions which *extreme apriorism*, on the one side, and *ultra-empiricism*, on the other side, take concerning the problem of verification in economics.

Pure, Exact, and Aprioristic Economics

Writers on the one side of this issue contend that economic science is a system of *a priori* truths, a product of pure reason,[6] an exact science reaching laws as universal as those of mathematics,[7] a purely axiomatic discipline,[8] a system of pure deductions from a series of postulates,[9] not open to any verification or refutation on the ground of experience.[10]

[6] "The ultimate yardstick of an economic theorem's correctness or incorrectness is solely reason unaided by experience." Ludwig von Mises, *Human Action: A Treatise on Economics* (New Haven: Yale University Press, 1949), p. 858.

[7] "There is a science of economics, a true and even exact science, which reaches laws as universal as those of mathematics and mechanics." Frank H. Knight, "The Limitations of Scientific Method in Economics," in R. G. Tugwell, ed., *The Trend of Economics* (New York: Crofts, 1930), p. 256.

[8] "Economic theory is an axiomatic discipline. . . ." Max Weber, *On the Methodology of the Social Sciences* (Glencoe, Ill.: Free Press, 1949), p. 43.

[9] "Economic analysis . . . consists of deductions from a series of postulates. . . ." Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* (London: Macmillan, 2nd ed., 1935), p. 99.

[10] "What assigns economics its peculiar and unique position in the orbit of pure knowl-

We must not attribute to all writers whose statements were here quoted or paraphrased the same epistemological views. While for Mises, for example, even the fundamental postulates are *a priori* truths, necessities of thinking,[11] for Robbins they are "assumptions involving in some way simple and indisputable facts of experience."[12] But most of the experience in point is not capable of being recorded from external (objective) observation; instead, it is immediate, inner experience. Hence, if verification is recognized only where the test involves objective sense-experience, the chief assumptions of economics, even if "empirical," are not independently verifiable propositions.

This methodological position, either asserting an *a priori* character of all propositions of economic theory or at least denying the independent objective verifiability of the fundamental assumptions, had been vigorously stated in the last century by Senior[13] and Cairnes,[14] but in essential respects it goes back to John Stuart Mill.

Mill, the great master and expositor of inductive logic, had this to say on the method of investigation in political economy:

> Since ... it is vain to hope that truth can be arrived at, either in Political Economy or in any other department of the social science, while we look at the facts in the concrete, clothed in all the complexity with which nature has surrounded them, and endeavor to elicit a general law by a process of induction from a comparison of details; there remains no other method than the *a priori* one, or that of 'abstract speculation.'[15]
>
> By the method *a priori* we mean ... reasoning from an assumed hypothesis; which is not a practice confined to mathematics, but is of the essence of all science which admits of general reasoning at all. To verify the hypothesis itself *a posteriori*, that is, to examine whether the facts of any actual case are in accordance with it, is no part of the business of science at all but of the *application* of science.[16]

This does not mean that Mill rejects attempts to verify the results of economic analysis; on the contrary,

> We cannot ... too carefully endeavor to verify our theory, by comparing, in the particular cases to which we have access, the results which it would have led us to predict, with the most trustworthy accounts we can obtain of those which have been actually realized.[17]

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edge and of the practical utilization of knowledge is the fact that its particular theorems are not open to any verification or falsification on the ground of experience." Ludwig von Mises, *op. cit.*, p. 858.

[11] Ludwig von Mises, *op. cit.*, p. 33.

[12] Lionel Robbins, *op. cit.*, p. 78, also pp. 99–100.

[13] Nassau William Senior, *Political Economy* (London: Griffin, 3rd ed., 1854), pp. 5, 26–29.

[14] John E. Cairnes, *The Character and Logical Method of Political Economy* (London: Macmillan, 1875), especially pp. 74–85, 99–100.

[15] John Stuart Mill, "On the Definition of Political Economy; and on the Method of Investigation Proper to It" in *Essays on Some Unsettled Questions of Political Economy* (London, 1844, reprinted London School of Economics, 1948), pp. 148–49.

[16] *Ibid.*, p. 143.

[17] *Ibid.*, p. 154.

The point to emphasize is that Mill does not propose to put the *assumptions* of economic theory to empirical tests, but only the *predicted results that are deduced from them.* And this, I submit, is what all the proponents of pure, exact, or aprioristic economic theory had in mind, however provocative their contentions sounded.[18] Their objection was to verifying the basic assumptions in isolation.

Ultra-Empirical Economics

Opposed to these tenets are the ultra-empiricists. "Empiricist" is a word of praise to some, a word of abuse to others. This is due to the fact that there are many degrees of empiricism. Some economists regard themselves as "empiricists" merely because they oppose radical apriorism and stress the dependence of theory on experience (in the widest sense of the word); others, because they demand that the results deduced with the aid of theory be compared with observational data whenever possible; others, because they are themselves chiefly concerned with the interpretation of data, with the testing of hypotheses and with the estimates of factual relationships; others, because they are themselves engaged in the collection of data or perhaps even in "field" work designed to produce "raw" data; others, because they refuse to recognize the legitimacy of employing at any level of analysis propositions not independently verifiable. It is the last group which I call the ultra-empiricists.[19] Then there are the ultra-ultra-empiricists who go even further and insist on independent verification of all assumptions by objective data obtained through sense observation.

The ultra-empiricist position is most sharply reflected in the many attacks on the "assumptions" of economic theory. These assumptions are decried as unverified, unverifiable, imaginary, unrealistic. And the hypothetico-deductive system built upon the unrealistic or unverifiable assumptions is condemned either as deceptive or as devoid of empirical content,[20] without predictive or

[18] "Aprioristic reasoning is purely conceptual and deductive. It cannot produce anything else but tautologies and analytic judgments." While this sounds like an "empiricist's" criticism of the aprioristic position, it is in fact a statement by Mises. (*Op. cit.*, p. 38.) Mises emphasizes that "the end of science is to know reality," and that "in introducing assumptions into its reasoning, it satisfies itself that the treatment of the assumptions concerned can render useful services for the comprehension of reality." (*Ibid.*, pp. 65–66.) And he stresses that the choice of assumptions is directed by experience.

[19] It is in this last meaning that empiricisms has usually been discussed and criticized in philosophy. In the words of William James, radical empiricism "must neither admit into its constructions any element that is not directly experienced, nor exclude from them any element that is directly experienced. For such a philosophy, *the relations that connect experiences must themselves be experienced relations, and any kind of relation experienced must be accounted as 'real' as anything else in the system.*" William James, *Essays in Radical Empiricism* (New York: Longmans, Green, 1912), pp. 42–43.

[20] "That 'propositions of pure theory' is a name for . . . propositions not conceivably falsifiable empirically and which do not exclude . . . any conceivable occurrence, and which are therefore devoid of empirical content. . . ." T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London: Macmillan, 1938), p. 162.

explanatory significance,[21] without application to problems or data of the real world.[22] Why deceptive? Because from wrong assumptions only wrong conclusions follow. Why without empirical significance? Because, in the words of the logician Wittgenstein, "from a tautology only tautologies follow."[23]

If the ultra-empiricists reject the basic assumptions of economic theory because they are not independently verified, and reject any theoretical system that is built on unverified or unverifiable assumptions, what is the alternative they offer? A program that begins with facts rather than assumptions.[24] What facts? Those obtained "by statistical investigations, questionnaires to consumers and entrepreneurs, the examination of family budgets and the like."[25] It is in research of this sort that the ultra-empiricists see "the only possible scientific method open" to the economist.[26]

This, again, is the essence of the ultra-empiricist position on verification: the ultra-empiricist is so distrustful of deductive systems of thought that he is not satisfied with the indirect verification of hypotheses, that is, with tests showing that the results deduced (from these hypotheses and certain factual assumptions) are in approximate correspondence with reliable observational data; instead, he insists on the independent verification of all the assumptions, hypothetical as well as factual, perhaps even of each intermediate step in the analysis. To him "testable" means "directly testable by objective data obtained by sense observation," and propositions which are in this sense "non-testable" are detestable to him.

The Testability of Fundamental Assumptions

The error in the antitheoretical empiricist position lies in the failure to see the difference between *fundamental* (heuristic) hypotheses, which are not inde-

[21] ". . . that propositions of pure theory, by themselves, have no prognostic value or 'causal significance.' " T. W. Hutchison, *op. cit.*, p. 162.—The clause "by themselves" makes Hutchison's statement unassailable, because nothing at all has causal significance by itself; only in conjunction with other things can anything have causal significance. But if Hutchison's statement means anything, it means an attack against the use of empirically unverifiable propositions in economic theory, regardless of their conjunction with other propositions. Indeed, he states that "a proposition which can never *conceivably* be shown to be true or false . . . can *never* be of any use to a scientist" (*ibid.*, pp. 152–53).

[22] With regard to the "fundamental assumption" of economic theory concerning "subjectively rational" and "maximizing" behavior, Hutchison states that "the empirical content of the assumption and all the conclusions will be the same—that is, nothing." *Ibid.*, p. 116.

[23] Ludwig Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge & Kegan Paul, 1951), p. 167.

[24] ". . . if one wants to get beyond a certain high level of abstraction one has to begin more or less from the beginning with extensive empirical investigation." T. W. Hutchison, *op. cit.*, p. 166.

[25] *Ibid.*, p. 120. This does not answer the question: "what facts?" Precisely what data should be obtained and statistically investigated? What questions asked of consumers and entrepreneurs?

[26] *Ibid.*, p. 120. I could have quoted from dozens of critics of economic theory, from adherents of the historical, institutional, quantitative schools, and these quotations might be even more aggressive. I have selected Hutchison because he is the critic best informed about logic and scientific method.

pendently testable, and *specific* (factual) assumptions, which are supposed to correspond to observed facts or conditions; or the differences between hypotheses on different levels of generality and, hence, of different degrees of testability.

The fundamental hypotheses are also called by several other names, some of which convey a better idea of their methodological status: "heuristic principles" (because they serve as useful guides in the analysis), "basic postulates" (because they are not to be challenged for the time being), "useful fictions" (because they need not conform to "facts" but only be useful in "as if" reasoning), "procedural rules" (because they are resolutions about the analytical procedure to be followed), "definitional assumptions" (because they are treated like purely analytical conventions).

A fundamental hypothesis serves to bring together under a common principle of explanation vast numbers of very diverse observations, masses of data of apparently very different sort, phenomena that would otherwise seem to have nothing in common. Problems like the explanation of the movements in wages in 13th and 14th century Europe, of the prices of spices in 16th century Venice, of the effects of the capital flows to Argentina in the 19th century, of the consequences of German reparation payments and of the devaluation of the dollar in the 1930's; problems like the prediction of effects of the new American quota on Swiss watches, of the new tax laws, of the increase in minimum wage rates, and so forth,—problems of such dissimilarity can all be tackled by the use of the same fundamental hypotheses. If these hypotheses are successful in this task and give more satisfactory results than other modes of treatment could, then we accept them and stick by them as long as there is nothing better—which may be forever.

That there is no way of subjecting fundamental assumptions to independent verification should be no cause of disturbance. It does not disturb the workers in the discipline which most social scientists so greatly respect and envy for its opportunities of verification: physical science. 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; and, as Einstein put it, "No one of the assumptions can be isolated for separate testing." For, he went on to say, "physical concepts are free creations of the human mind, and are not, however it may seem, uniquely determined by the external world."[27]

Much has been written about the meaning of "explanation." Some have said that the mere *description* of regularities in the co-existence and co-variation of observed phenomena is all we can do and will be accepted as an *explanation* when we are sufficiently used to the regularities described.[28] There is something to this view; but mere resignation to the fact that "it always has been so" will not for long pass as explanation for searching minds. The feeling of relief and satisfied curiosity—often expressed in the joyous exclamation "ah haahh!"—comes to most analysts only when the observed regularities can be deduced from

[27] Albert Einstein and Leopold Infeld, *The Evolution of Physics* (New York: Simon and Schuster, 1938), p. 33.

[28] Cf. P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927), p. 43.

general principles which are also the starting point—foundation or apex, as you like—of many other chains of causal derivation. This is why Margenau, another physicist, said that an explanation involves a "progression into the constructional domain. We explain by going 'beyond phenomena.' "[29] But this clearly implies that the explanatory general assumptions cannot be empirically verifiable in isolation.

Logicians and philosophers of science have long tried to make this perfectly clear. Although appeals to authority are ordinarily resorted to only where an expositor has failed to convince his audience, I cannot resist the temptation to quote two authorities on my subject. Here is how the American philosopher Josiah Royce put it:

> One often meets with the remark that a scientific hypothesis must be such as to be more or less completely capable of verification or of refutation by experience. The remark is sound. But equally sound it is to say that a hypothesis which, just as it is made, is, without further deductive reasoning, capable of receiving direct refutation or verification, *is not nearly as valuable to any science as is a hypothesis whose verifications, so far as they occur at all, are only possible indirectly, and through the mediation of a considerable deductive theory*, whereby the consequences of the hypothesis are first worked out, and then submitted to test.[30]

And here is the same idea in the words of the British philosopher of science, Richard B. Braithwaite:

> For science, as it advances, does not rest content with establishing simple generalizations from observable facts. It tries to explain these lowest-level generalization by deducing them from more general hypotheses at a higher level. . . . As the hierarchy of hypotheses of increasing generality rises, the concepts with which the hypotheses are concerned cease to be properties of things which are directly observable, and instead become 'theoretical' concepts—atoms, electrons, fields of force, genes, unconscious mental processes—which are connected to the observable facts by complicated logical relationships.[31]

And he states that "the empirical testing of the deductive system is effected by testing the lowest-level hypotheses in the system."[32]

Assumptions in Economics, Pure and Applied

Examples of *fundamental assumptions* or "high-level generalizations" in economic theory are that people act rationally, try to make the most of their opportunities, and are able to arrange their preferences in a consistent order; that entrepreneurs prefer more profit to less profit with equal risk.[33] These are

[29] Henry Margenau, *The Nature of Physical Reality* (New York: McGraw-Hill, 1950), p. 169.

[30] Josiah Royce, "The Principles of Logic," in *Logic, Encyclopaedia of the Philosophical Sciences*, Vol. I (London: Macmillan, 1913), pp. 88–89.

[31] Richard Bevan Braithwaite, *Scientific Explanation: A Study of the Function of Theory, Probability and Law in Science* (Cambridge: University Press, 1953), p. ix.

[32] *Ibid.*, p. 13.

[33] For most problems of an enterprise economy no exact specifications about "profit"

assumptions which, though empirically meaningful, require no independent empirical tests but may be significant steps in arguments reaching conclusions which are empirically testable.

Examples of *specific assumptions* are that the expenditures for table salt are a small portion of most households' annual budgets; that the member banks are holding very large excess reserves with the Federal Reserve Banks; that there is a quota for the importation of sugar which is fully utilized. Examples of *deduced "low-level hypotheses"* are that a reduction in the price of table salt will not result in a proportionate increase in salt consumption; that a reduction in the discount rates of the Federal Reserve Banks will at such times not result in an increase in the member banks' lending activities; that a reduction in sugar prices abroad will not result in a reduction of domestic sugar prices. All these and similar specific assumptions and low-level hypotheses are empirically testable.

Perhaps a few additional comments should be made concerning the fundamental assumptions, particularly the postulate of rational action, the "economic principle" of aiming at the attainment of a maximum of given ends. Any independent test of this assumption by reference to objective *sense*-experience is obviously impossible. Those who accept findings of introspection as sufficient evidence may contend that the fundamental assumption can be, and constantly is, verified. Those who accept findings of interrogation (that is, replies to questions put to large numbers of introspectors) as "objective" evidence may contend that the assumption of "maximizing behavior" is independently testable. But such a test would be gratuitous, if not misleading. For the fundamental assumption may be understood as an idealization with constructs so far removed from operational concepts that contradiction by testimony is ruled out; or even as a complete fiction with only one claim: that reasoning *as if* it were realized is helpful in the interpretation of observations.[34]

Economists who are still suspicious of non-verifiable assumptions, and worry about the legitimacy of using them, may be reassured by this admission: The fact that fundamental assumptions are not directly testable and cannot be refuted by empirical investigation does not mean that they are beyond the pale of the so-called "principle of permanent control," that is, beyond possible challenge, modification or rejection. These assumptions may well be rejected, but only together with the theoretical system of which they are a part, and only when a more satisfactory system is put in its place; in Conant's words, "a theory is only overthrown by a better theory, never merely by contradictory facts."[35]

(whose? for what period? how uncertain? etc.) will be needed. There are some special problems for which "specific assumptions" concerning profit are needed. Needless to say, the assumption about entrepreneurs will be irrelevant for problems of centrally directed economies.

[34] Or, again in a different formulation: the fundamental assumption is a resolution to proceed in the interpretation of all data of observation as if they were the result of the postulated type of behavior.

[35] James B. Conant, *On Understanding Science* (New Haven: Yale University Press, 1947), p. 36.

III

What I have said and quoted about assumptions and hypotheses on various "levels" of abstraction may itself be too abstract, too remote from our ordinary terms of discourse, to be meaningful to many of us. Perhaps it will be helpful to try a graphical presentation of a simple model of an analytical system combining assumptions of various types.

A Model of an Analytical Apparatus

The design for the model was suggested by the usual metaphors about an analytical "apparatus," "machine," or "engine of pure theory." Something goes into a machine and something comes out. In this case the input is an assumption concerning some "change" occurring and causing other things to happen, and the output is the "Deduced Change," the conclusion of the (mental) operation. The machine with all its parts furnishes the connection between the "assumed cause," the input, and the "deduced effect," the outcome. The main point of this model is that *the machine is a construction of our mind, while the assumed and deduced changes should correspond to observed phenomena, to data of observation, if the machine is to serve as an instrument of explanation or prediction.* In explanations the analytical machine helps select an adequate "cause" for an observed change; in predictions it helps find a probable "effect" of an observed change.[36]

The machine consists of many parts, all of which represent assumptions or hypotheses of different degrees of generality. The so-called *fundamental assumptions* are a fixed part of the machine; they make the machine what it is; they cannot be changed without changing the character of the entire machine. All other parts are exchangeable, like coils, relays, spools, wires, tapes, cylinders, records, or mats, something that can be selected and put in, and again taken out to be replaced by a different piece of the set. These exchangeable parts represent *assumptions about the conditions* under which the Assumed Change must operate. Some of the parts are exchanged all the time, some less frequently, some only seldom. Parts of type A, the Assumed Conditions as to "type of case," are most frequently exchanged. Parts of type B, the Assumed Conditions as to "type of setting," will stay in the machine for a longer time and there need be less variety in the set from which they are selected. Parts of type C, the Assumed Conditions as to "type of economy," are least exchangeable, and there will be only a small assortment of alternative pieces to choose from.

Now we shall leave the engineering analogies aside and discuss the status of all these assumptions regarding the operational and observational possibilities and the requirements of verification.

Verified Changes under Unverified Conditions

Both the Assumed Change and the Deduced Change should be empirically verifiable through correspondence with data of observation. At least one of the two has to be verifiable if the analysis is to be applied to concrete cases. Hence

[36] On the problem of prediction versus explanation see the chapter on "Economic Fact and Theory" in my book *The Political Economy of Monopoly* (Baltimore: Johns Hopkins Press, 1952), pp. 455 ff.

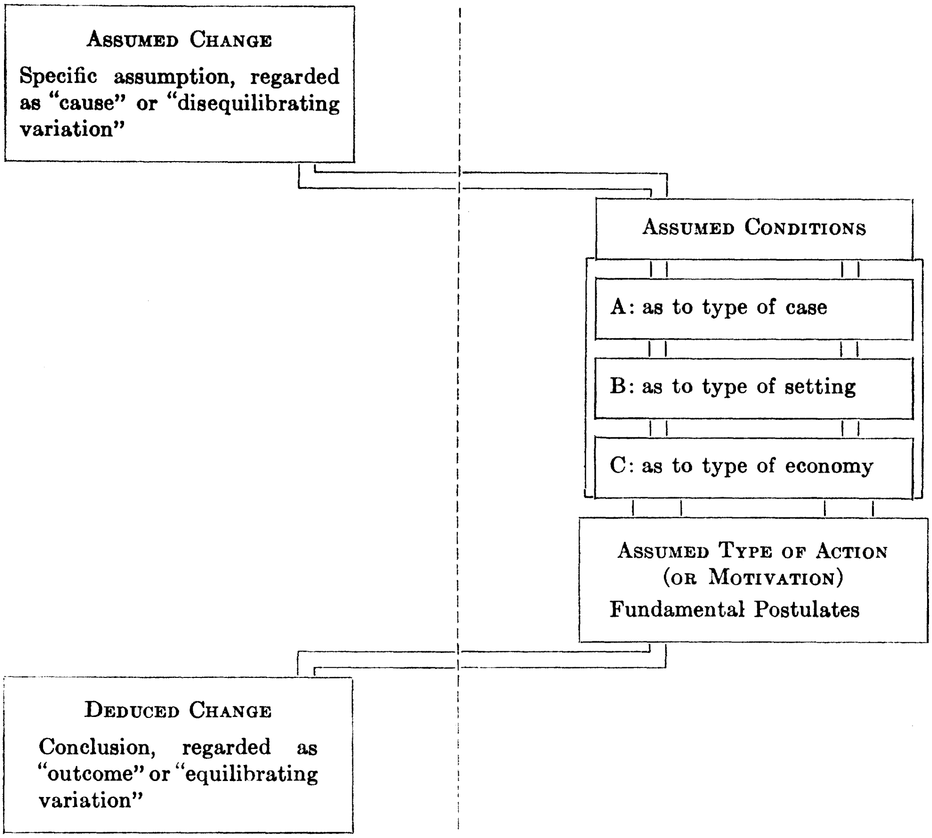

FIG. 1. A MODEL OF THE USE OF AN ANALYTICAL APPARATUS

On the right side is the "machine of pure theory," a mental *construction* for heuristic purposes; on the left side are assumptions of independent and dependent variables whose *correspondence* with data of observation may be tested.

the concepts employed to describe the changes should, if possible, be operational. This raises no difficulty in the case of most kinds of *Assumed Change* in whose effects we are interested, for example: changes in tax rates, customs duties, foreign-exchange rates, wage rates, price supports, price ceilings, discount rates, open-market policies, credit lines, government expenditures, agricultural crops—matters covered in reports and records. There are difficulties concerning some other kinds of Assumed Change, such as improvements in technology, greater optimism, changed tastes for particular goods—things for which recorded data are often unavailable. As regards the *Deduced Change* the requirement that it be operational will usually be met, because we are interested chiefly in effects upon prices, output, income, employment, etc.,—magnitudes reported in statistical series of some sort. To be sure, the figures may be unreliable and the statistical concepts may not be exact counterparts to the analytical concepts, but we cannot be too fussy and must be satisfied with what we can get.

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. The analysis would be neither wrong nor invalid, but it would not be very useful if it were never possible to identify the concrete phenomena, events, and situations, to which it is supposed to apply. Once we have confidence in the whole theoretical system, we are willing to apply it to concrete cases even where only one of the two "changes," either the "cause" or the "effect," is identifiable in practice, rather than both. For example, we are prepared to base policy decisions on explanations or predictions where one of the phenomena cannot be isolated in observation from the complex of simultaneous variations. For purposes of verification of the entire theory, however, we shall have to identify both the phenomena represented by the Assumed Change and the Deduced Change—although such verification may be practical only on rare occasions.

We need not be particularly strict concerning the verification of the *Assumed Conditions*. Regarding them, a casual, perhaps even impressionistic empiricism will do, at least for most types of problems. The Assumed Conditions refer to personal characteristics, technological or organizational circumstances, market forms, enduring institutions—things of rather varied nature. Few of the Conditions are observable, except through communication of interpretations involving a good deal of theorizing by the parties concerned. Often the Conditions are not even specified in detail, but somehow taken for granted by analysts working in a familiar milieu. All of the Conditions are hypothetical parameters, assumed to prevail at least for the duration of the process comprising all the actions, interactions and repercussions through which the Assumed Change is supposed to cause the Deduced Change.

Assumed Conditions of Type A, that is, as to "*type of case*," refer to conditions which may vary from case to case and influence the outcome significantly, but are sufficiently common to justify the construction of "types" for theoretical analysis. Here is a list of examples: type of goods involved (durable, non-durable, perishable; inferior, non-inferior; taking up substantial or negligible parts of buyer's budget; substitutable, complementary; etc.); cost conditions (marginal cost decreasing, constant, increasing; joint costs, etc.); elasticity of supply or demand (positive, negative, relatively large, unity, less than unity); market position (perfect, imperfect polypoly; collusive, uncoordinated oligopoly; perfect, imperfect monopoly); entry (perfect, imperfect pliopoly); expectations (elastic, inelastic; bullish, bearish; certain, uncertain); consumption propensity (greater, smaller than unity); elasticity of liquidity preference (infinite, less than infinite, zero).

Assumed Conditions of Type B, that is, as to "type of setting," refer to conditions which may change over brief periods of time—say, with a change of government or of the political situation, or during the business cycle—and are apt to influence the outcome in definite directions. A list of examples will indicate what is meant by conditions prevailing under the current "setting": general business outlook (boom spirit, depression pessimism); bank credit availability (banks loaned up, large excess reserves); central bank policy (ready to monetize government securities, determined to maintain easy money policy, willing to let interest rates rise); fiscal policy (expenditures fixed, adjusted to tax revenues, geared to unemployment figures; tax rates fixed, adjusted to maintain revenue, etc.); farm program (support prices fixed, flexible within limits, etc.); antitrust policy (vigorous prosecution of cartelization, etc.); foreign aid program; stabilization fund rules; trade union policies.

Assumed Conditions of Type C, that is, as to "*type of economy*," refer to conditions which may vary from country to country and over larger periods of time, but may be assumed to be "settled" for a sufficiently large number of cases to justify taking these conditions as constant. Examples include legal and social institutions; private property; freedom of contract; corporation law; patent system; transportation system; enforcement of contracts; ethics of law violations; social customs and usages; monetary system (gold standard, check system, cash holding habits).

Assumed Conditions are exchangeable because the effects of an Assumed Change may have to be analysed under a variety of conditions: for example, with different degrees or forms of competition, different credit policies, different tax structures, different trade union policies, etc. But it may also be expedient, depending on the problem at hand, to regard a variation of an Assumed Condition as an Assumed Change, and *vice versa*. For example, the problem may concern the effects of a wage rate increase under various market conditions or, instead, the effects of a change in market position under conditions of automatic wage escalation; the effects of a change in monetary policy with different tax structure, or the effects of a change in the tax structure under different monetary policies.

After listing the many examples of the various types of Assumed Conditions it will probably be agreed that a rigid verification requirement would be out of place. Usually the judgment of the analyst will suffice even if he cannot support it with more than the most circumstantial evidence or mere "impressions." Suppose he deals with a simple cost-price-output problem in a large industry, how will the analyst determine what "type of case" it is with regard to "market position?" Lacking the relevant information, he may first try to work with a model of perfect polypoly[37]—although he knows well that this cannot fit the real situation—and will note whether his deduced results will be far off the mark. He may find the results reasonably close to the observed data and may leave it at that. For to work with a more "realistic" assumption may call for so many additional assumptions for which no relevant information is available that it is preferable and unobjectionable to continue with a hypothesis contrary to fact. When a simpler hypothesis, though obviously unrealistic, gives consistently

[37] Under perfect polypoly the individual seller assumes that his own supply will not affect any other seller or the market as a whole and, thus, that he could easily sell more at the same price and terms. This condition was also called "pure competition," "perfect competition," or "perfect market" (although it has little to do with any effort of "competing" or with any property of the "market"). See Fritz Machlup, *The Economics of Sellers' Competition* (Baltimore: Johns Hopkins Press, 1952), pp. 85–91, and pp. 116 ff.

satisfactory results, one need not bother with more complicated, more realistic hypotheses.

Ideal Type of Action, Unverified but Understood

While solid empirical verification is indicated for the Assumed Change, and casual empirical judgments are indicated for the Assumed Conditions, the *Assumed Type of Action* forms the fundamental postulates of economic analysis and thus is not subject to a requirement of independent verification.

Various names have been suggested for the fundamental postulates of economic theory: "economic principle," "maximization principle," "assumption of rationality," "law of motivation," and others. And their logical nature has been characterized in various ways: they are regarded as "self-evident propositions," "axioms," "*a priori* truths," "truisms," "tautologies," "definitions," "rigid laws," "rules of procedure," "resolutions," "working hypotheses," "useful fictions," "ideal types," "heuristic mental constructs," "indisputable facts of experience," "facts of immediate experience," "data of introspective observation," "private empirical data," "typical behavior patterns," and so forth.

Some of these characterizations are equivalent to or consistent with each other, but some are not. How can a proposition be both *a priori* and empirical, both a definition and a fact of experience? While this cannot be, the distinctions in this particular instance are so fine that conflicts of interpretation seem unavoidable. Logicians have long debated the possibility of propositions being synthetic and yet *a priori*, and physicists are still not quite agreed whether the "laws" of mechanics are analytical definitions or empirical facts. The late philosopher Felix Kaufmann introduced as a middle category the so-called "rules of procedure," which are neither synthetic in the sense that they are falsifiable by contravening observations nor *a priori* in the sense that they are independent of experience;[38] they are and remain accepted as long as they have heuristic value, but will be rejected in favor of other rules (assumptions) which seem to serve their explanatory functions more successfully.

If this debate has been going on in the natural sciences, how could it be avoided in the social sciences? If issues about "self-evident," "inescapable," or "indisputable" insights arose concerning the physical world, how much more pertinent are such issues in the explanation of human action, where man is both observer and subject of observation! This, indeed, is the essential difference between the natural and the social sciences: that in the latter the facts, the data of "observation," are themselves results of interpretations of human actions by human actors.[39] And this imposes on the social sciences a requirement which does not

[38] Felix Kaufmann, *Methodology of the Social Sciences* (New York: Oxford University Press, 1944), pp. 77 ff, especially pp. 87–88.

[39] ". . . the object, the 'facts' of the social sciences are also opinions—not opinions of the student of the social phenomena, of course, but opinions of those whose actions produce his object. . . . They [the facts] differ from the facts of the physical sciences in being . . . beliefs which are as such our data . . . and which, moreover, we cannot directly observe in the minds of the people but recognize from what they do and say merely because we have

exist in the natural sciences: that all types of action that are used in the abstract models constructed for purposes of analysis be "understandable" to most of us in the sense that we could conceive of sensible men acting (sometimes at least) in the way postulated by the ideal type in question. This is the crux of Max Weber's methodology of the social sciences, and was recently given a refined and most convincing formulation by Alfred Schuetz.[40]

Schuetz promulgates three postulates guiding model construction in the social sciences: the postulates of "logical consistency," of "subjective interpretation," and of "adequacy." The second and third of these postulates are particularly relevant here:

> In order to explain human actions the scientist has to ask what model of an individual mind can be constructed and what typical contents must be attributed to it in order to explain the observed facts as the result of the activity of such a mind in an understandable relation. The compliance with this postulate warrants the possibility of referring all kinds of human action or their result to the subjective meaning such action or result of an action had for the actor.
>
> Each term in a scientific model of human action must be constructed in such a way that a human act performed within the life world by an individual actor in the way indicated by the typical construct would be understandable for the actor himself as well as for his fellowmen in terms of common-sense interpretation of everyday life. Compliance with this postulate warrants the consistency of the constructs of the social scientist with the constructs of common-sense experience of the social reality.[41]

Thus, the fundamental assumptions of economic theory are not subject to a requirement of independent empirical verification, but instead to a requirement of understandability in the sense in which man can understand the actions of fellowmen.[42]

IV

We are ready to summarize our conclusions concerning verification of the assumptions of economic theory. Then we shall briefly comment on the verification of particular economic theories applied to predict future events, and on the verification of strictly empirical hypotheses.

Verifying the Assumptions

First to summarize: We need not worry about independent verifications of the fundamental assumptions, the Assumed Type of Action; we need not be

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ourselves a mind similar to theirs." F. A. v. Hayek, "Scientism and the Study of Society," *Economica, New Series*, Vol. V (August 1942), p. 279. Reprinted F. A. v. Hayek, *The Counter-Revolution of Science* (Glencoe, Ill.: Free Press, 1952).

[40] Alfred Schuetz, "Common-Sense and Scientific Interpretation of Human Action," *Philosophy and Phenomenological Research*, Vol. XIV (September 1953), pp. 1–38. *Idem.*, "Concept and Theory Formation in the Social Sciences," *The Journal of Philosophy*, Vol. LI (April 1954), pp. 257–273.

[41] Schuetz, "Common-Sense, etc.," p. 34.

[42] Disregard of this requirement is, in my view, the only serious flaw in the otherwise excellent essay on "The Methodology of Positive Economics" by Milton Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 3–43.

very particular about the independent verifications of the other intervening assumptions, the Assumed Conditions, because judgment based on casual empiricism will suffice for them; we should insist on strict independent verifications of the assumption selected as Assumed Change and of the conclusion derived as Deduced Change; not that the theory would be wrong otherwise, but it cannot be applied unless the phenomena to which it is supposed to apply are identifiable. *Simultaneous verifications of Assumed Change and Deduced Change count as verification—in the sense of non-disconfirmation—of the theory as a whole.*

Now it is clear why some writers insisted on the *a priori* nature of the theory and at the same time on its empirical value for the area of Applied Economics; for one may, if one wishes, regard the theory, or model, as a construction *a priori*, and the directions for its use, the instructions for its applications,[43] as an empirical appendage in need of verification. Returning to the analogy of the analytical machine, one may say that the machine and its parts are always "correct," regardless of what goes on around us, whereas the *choice* of the exchangeable parts and the *identification* of the events corresponding to the Assumed and Deduced changes may be wrong.

Testing the Predictive Values of Theories

We have examined the empiricists' charges against the theorists—charges of contemptuous neglect of the requirement of verification—and have concluded that these charges must be dismissed insofar as they refer to a failure to verify all assumptions directly and in isolation from the rest of the theory. We must yet examine another count of the charge of insufficient attention to verification: an alleged failure to test the correspondence between Deduced (predicted) and Observed outcomes. These kinds of tests are obligatory.

If verification of a theory takes the form of testing whether predictions based on that theory actually come true, one might think that this can be done in economics no less than in the physical sciences. It cannot, alas, because of the non-reproducibility of the "experiments" or observed situations and courses of events in the economy. For, while certain types of events, or "changes," recur in the economy often enough, they recur rarely under the same conditions. If some significant circumstances are different whenever a phenomenon of the same class recurs, each recurrence is virtually a "single occurrence." Economic theory applied to single events, or to situations significantly different from one another, cannot be tested as conclusively as can physical theory applied to reproducible occurrences and conditions.

Not long ago I was challenged to admit that my theories, even though applied to ever-changing circumstances, could be tested provided I were prepared to make unconditional predictions which could be compared with actual outcomes. Of course, I could only dare make unconditional predictions—without hedging about probability and confidence limits—where I was absolutely certain that my diagnosis of the situation (i.e., of *all* relevant circumstances) *and* my foreknowl-

[43] Cf. Milton Friedman, *op. cit.*, pp. 24–25.

edge of government and power group actions *and* the theory on which the prediction rests were all perfectly correct. Suppose that I was so foolhardy as to be sure of all this and that I did make a number of unconditional predictions. Still, unless reliable checks were possible to verify separately every part of my diagnosis and of my anticipations regarding government and power group actions, my theory could not be tested. There could be lucky "hits" where wrong diagnoses would compensate for mistakes due to bad theories; there could be unlucky "misses" where wrong diagnoses spoiled the results of good theorizing. Despite a large number of good hits the theories in question could not be regarded as confirmed, even in the modest sense of not being disconfirmed, because a joint and inseparable test of diagnosis, anticipations, and theory says nothing about the theory itself.

Where the economist's prediction is *conditional*, that is, based upon specified conditions, but where it is not possible to check the fulfillment of all the conditions stipulated, the underlying theory cannot be disconfirmed whatever the outcome observed. Nor is it possible to disconfirm a theory where the prediction is made with a stated *probability* value of less than 100 percent; for if an event is predicted with, say, 70 percent probability, any kind of outcome is consistent with the prediction.[44] Only if the same "case" were to occur hundreds of times could we verify the stated probability by the frequency of "hits" and "misses."

This does not mean complete frustration of all attempts to verify our economic theories. But it does mean that the tests of most of our theories will be more nearly of the character of *illustrations* than of verifications of the kind possible in relation with repeatable controlled experiments or with recurring fully-identified situations. And this implies that our tests cannot be convincing enough to compel acceptance, even when a majority of reasonable men in the field should be prepared to accept them as conclusive, and to approve the theories so tested as "not disconfirmed," that is, as "O. K."

Strictly Empirical Hypotheses

All this seems to circumscribe rather narrowly the scope of empirical verification, if not empirical research, in economics. But to draw such a conclusion would be rash. For there is a large body of economics apart from its theoretical or "hypothetico-deductive" system: namely, the empirical relationships obtained through correlation of observations, but not derivable, or at least not yet derived, from higher-level generalizations. Every science has such a body of strictly empirical hypotheses, no matter how fully developed or undeveloped its theoretical system may be.

I define a strictly empirical hypothesis as a proposition predicating a regular relationship between two or more sets of data of observation that cannot be

[44] This statement, it should be noted, refers to *general* theories which are part of a hypothetico-deductive system, not to strictly empirical hypotheses obtained by statistical inference. The predictions in question can never be in precise numerical terms, because no numerical magnitudes can be deduced from the assumptions of the type used in "general theory."

deduced from the general hypotheses which control the network of interrelated inferences forming the body of theory of the discipline in question. The distinction is made in almost all disciplines; it is best known as the distinction between "empirical laws" and "theoretical laws," though several other names have been used to denote the two types of scientific propositions. The philosopher Morris Cohen spoke of "concrete laws" in contrast to "abstract laws." Felix Kaufmann, though using the terms empirical and theoretical laws, characterized the former as "strict laws," the latter as "rigid laws." The physicist Henry Margenau contrasted "epistemic" or "correlational laws" with "constitutive," "exact," or "theoretical" laws. And Carl Menger, the founder of the Austrian School and protagonist in the *Methodenstreit*, distinguished "empirical laws" from "exact laws," the latter dealing with idealized connections between pure constructs, the former with "the sequences and coexistences of real phenomena."[45]

The study of the "sequences and coexistences" of the real phenomena depicted in statistical records yields correlational and other empirical findings which have to be tested and modified whenever new data on the same class of phenomena become available. While the constructs and deductions of the theoretical systems will influence the selection, collection and organization of empirical data, the particular relationships established between these data by means of correlation analysis and other statistical techniques are not deducible from high-level assumptions and can neither confirm nor disconfirm such assumptions. But these relationships, especially the numerical estimates of parameters, coefficients, or constants, are themselves subject to verification by new observations.

Verification of Empirical Hypotheses

Every one of us has lately been so much concerned with statistical demand curves, saving and consumption functions, investment functions, import elasticities and import propensities that a description of these and similar research activities is not necessary. The trouble with the verification of the empirical hypotheses derived by means of statistical and econometric analysis is that successive estimates on the basis of new data have usually been seriously divergent. Of course, such variations over time in the numerical relationships measured are not really surprising: few of us have expected these relationships to be constant or even approximately stable. Thus when new data and new computations yield revised estimates of economic parameters, there is no way of telling whether the previous hypotheses were wrong or whether things have changed.

That the numerical relationships described by these empirical hypotheses may be subject to change—to unpredictable change—alters their character in an essential respect. Hypotheses which are strictly limited as to time and space are not "general" but "special" hypotheses, or *historical propositions*. If the relationships measured or estimated in our empirical research are not universal but historical propositions, the problem of verification is altogether different—so different that according to intentions expressed in the introduction we should

[45] Carl Menger, *Untersuchungen über die Methode der Socialwissenschaften und der Politischen Oekonomie insbesondere* (Leipzig: Duncker & Humblot, 1883), pp. 28, 36.

not be concerned with it. For we set out to discuss verification of *generalizations*, not of events or circumstances confined to particular times and places. If all propositions of economics were of this sort, the dictum of the older historical school, that economics cannot have "general laws" or a "general theory," would be fully vindicated.

If a hypothesis about the numerical relationship between two or more variables was formulated on the basis of statistical data covering a particular period, and is later compared with the data of *another period*, such a comparison would be in the nature of a verification only if the hypothesis had been asserted or expected to be a universal one, that is, if the measured or estimated relationships had been expected to be constant. In the absence of such expectations the test of a continuing "fit" (between hypothesis and new data) is just a comparison between two historical situations, an attempt to find out whether particular relationships were stable or changing. A genuine verification of a previously formulated hypothesis about a given period calls for comparisons with additional data relating to the *same period*, to check whether the previous observations and their previous numerical description had been accurate. In brief, a historical proposition can only be verified by new data about the historical situation to which it refers. This holds also for geographic propositions and comparisons between different areas.

However, although the changeable "structures"[46] estimated by statistical and econometric researchers are nothing but historical propositions, there are probably limits to their variations. For example, we may safely generalize that the marginal propensity to consume cannot in the long run be greater than unity; or that the elasticity of demand for certain types of exports of certain types of countries will not in the long run be smaller than unity. Statements about definite limits to variations of special or historical propositions are again general hypotheses; they are not strictly empirical but universal in that they are deducible from higher-level generalizations in the theoretical system of economics. The various successive estimates of changeable structures may then be regarded as verifications of general hypotheses according to which certain parameters or coefficients must fall within definite limits. Since these limits are usually rather wide, verification will of course not be the rigorous kind of thing it is in the physical sciences with its numerical constants and narrow margins of error.

But neither this nor anything else that has been said in this article should be interpreted as intending to discourage empirical testing in economics. On the contrary, awareness of the limits of verification should prevent disappointments and present challenges to the empirical worker. May he rise to these challenges and proceed with intelligence and fervor by whatever techniques he may choose.

[46] In the sense used by Tjallong Koopmans and other econometricians.