Friedman and Machlup on the Significance of Testing Economic Assumptions

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Summary: ***This article questions Milton Friedman's methodological position in a famous essay dating to 1948 where he questions the validity of tests of the assumptions of economic theory. Valid tests, he maintains, by and large, concern the empirical implications of hypotheses derived from the theory. The truth or falsehood of assumptions is "largely irrelevant." In response, this article argues that Friedman's position is questionable but for different reasons depending on the nature of the assumptions. "Auxiliary assumptions" concern the environment in which the test is supposed to take place. "Generative assumptions" concern the postulates or theorems from which the hypothesis is derived. If auxiliary assumptions are false, all test results bear less weight, whether are confirmatory or disconfirmatory. If generative assumptions are false, positive results confer less confirmation on the hypothesis. The article further questions Friedman's famous proposal to treat the assumptions of economic theory as "as if" statements, which are not really supposed true but simply taken for granted. The article goes on to question a related one by Fritz Machlup agreeing with Friedman about the irrelevance of tests of "generative assumptions." Unlike Friedman, however, Machlup stands on the authority of philosophers of science, who, he claims, maintain that theoretical postulates in science should be regarded as "rules" that must be followed.***

I. Introduction

The spectacular advance of modern science is usually attributed very largely to the development of deductively related bodies of general statements known as theories. The construction of theories in science has not only brought improvement in order and clarity and broadened the scope of empirical generalization but has also scored extraordinary predictive successes, some of which were never even envisaged when the theories began. No wonder that, as awareness of the possibility of "social sciences" grew in the nineteenth century, there emerged an enormous zeal for the erection of social theories. Of the social theories that thrived during the nineteenth century, the most viable one has proven to be an offspring of the preceding century, when this general zeal had not yet begun, that is, economic theory. Despite the venerability of economic theory at present, it still does not command nearly as much respect as some of the theories in the physical sciences. This is, of course, a rank understatement: for well over a century, economic theory has been subject to frequent attacks, including completely subversive attempts. The theory and its assumptions have been the victims not only of constant opposition but of abuse.[2]

[1] I owe an inestimable debt to Carl G. Hempel and Paul Benaccerraf for guidance and criticism in the early stages of preparation of this paper some six or seven years ago. I have also benefited from comments by Gaston V. Rimlinger.

[2] To illustrate, without attempting to be compendious: Thorstein Veblen (1898); Barbara Wootton, *Lament for Economics* (1938); and Sidney Schoeffler (1955).

In the face of such extreme antagonism, partisans have traditionally armed themselves with some sort of a reasoned reply.

During the period 1880-1920 the friends and advocates of economic theory adopted on the whole a moderate, conciliatory stance. They agreed that economic theory made false assumptions, and admitted that the value of the theory depended greatly on the degree of accord between the assumptions and the facts. Yet they insisted, first, that the assumptions did correspond broadly with events; and second, that the sacrifice of some accuracy for simplicity was justified in view of the complexities of facts. Further, they emphasized the importance of combining the use of simplifying assumptions with several protective measures : (1) the pursuit of intensive inductive study in all areas of economic, whether closely related to economic theory or not ; (2) the determination of reasonable proximity between major assumptions and facts prior to any application of economic theory to practice ; and (3) the alteration of theoretical assumptions, to whatever degree possible, in keeping with the particular case involved.[3]

With the notable advance of the purely logical branch of economic theory in the 1920's and 1930's, this conciliatory support of economic theory lost enormous appeal among theorists. A tendency arose, partly unconscious, to avoid the impression that economic theorists bore the responsibility to make their assumptions as realistic as they could, given practical limitations in knowledge, returns to additional theoretical invention, and time. Increasingly, economic theory was described in a manner suggesting no positive declarations about facts. Concomitantly, some narrow characterizations of economic theory took hold, such as "box of analytical tools", "filing cabinet", "conceptual framework", and "procedural device". A growing number of people also hinted broadly, both inside and outside the classroom, that theory is its own reward.

In 1948 Milton Friedman tried to supply a logical foundation for the developing attitude that the realism of assumptions is not a genuine, or is only a secondary concern. While granting that the assumptions of economic theory are false, he denied any significant resulting handicap. In his view, assumptions must simply work, that is, yield reliable results. Whether or not assumptions correspond with the facts is – with a few reservations – without interest. Since, according to Friedman, all previous attacks on economic theory had been founded principally on observed discrepancies between economic assumptions and facts, these attacks were mostly beside the point.[4] In 1955 Fritz Machlup joined forces with Friedman, claiming to bring with him the support of the experts in the philosophy of science and logic and of a whole tradition in economics (or "political economy"). Machlup, in fact, side with Friedman only with regard to so-called "fundamental assumptions" but still, on this count alone, must be viewed as an ally.[5]

This paper attempts to show that Friedman and Machlup offer an exaggerated defense of economic theory. Previous attempt to show this in connection with Friedman are all, in my opinion, somewhat unsatisfactory, by virtue of incompleteness if nothing else.[6] The argument presented here

[3] See John Neville Keynes (1891); Alfred Marshall, *Principles of Economics* (1961), passim, esp. pp. 24-31, 636-46; Henry Sidgwick (1901, pp. 35-52 and 1913, pp. 133-7). For highly similar views, see Vilfredo Pareto (1896-97, I, pp. 304-5, II, 1-8 and 1927, pp. 1-39) and Knut Wicksell (1934, I, pp. 9-11).
[4] Milton Friedman (1948).
[5] Fritz Machlup (1955).
[6] See Tjalling C. Koopmans (1957); Eugene Rotwein (1959); Ernest Nagel (1963; and Paul Samuelson (1963). While broadly in agreement with all of these writings, I am particularly in close accord with Koopmans and the

is that the most judicious defense of economic theory is the one that was prevalent in the 1880-1920 period. Thus, my general thesis is that unrealism of assumptions is a serious drawback, granted that inaccurate assumptions may be sufferable and indeed wise. I will try to show, further, that tests of economic assumptions – all economic assumptions, regardless of categorization – are valuable and bear implications for economics, apart from psychology, sociology, history, or any other bordering discipline. The discussion focuses first on Friedman's position, then on Machlup's, and the article conclude with a few summary remarks.

II – The Friedman Thesis

A – Thesis and qualifications

Friedman maintains that the accuracy of assumptions is not pertinent to the test of a hypothesis. In his view, the validity of any statement can be determined only by checking the truth of its implications. To contemplate the realism of assumptions in testing a hypothesis is wrong. His section on "The Significance and Role of the 'Assumptions' of a Theory" qualify this argument in several ways, and it may be best to observe these qualifications at once.

First, Friedman recognizes a difficulty in his position if a statement (A) is used to derive a hypothesis (*B*) while in a different context, *B* serve to derive *A*. A simple example, slightly adapted from the illustration in Friedman's essay, may be presented. Imperfect competition, together with several other assumptions, implies price stability, but under a different set of conditions, price stability implies imperfect competition. Thus, imperfect competition may be an assumption of a hypothesis of price stability and vice versa. In cases of this sort, Friedman admits, evidence bearing on either statement would be relevant in determining the validity of the other. To quote : "What are called the assumptions of a hypothesis can be used to obtain some indirect evidence on the acceptability of the hypothesis in so far as the assumptions can themselves be regarded as implications of the hypothesis… ."[7] However, he softens the impact of this concession by maintaining that "in so far as the assumptions can themselves be regarded as implications of the hypothesis", the realism of the assumptions "generally refer[s] to a class of phenomena different from the class which the hypothesis is designed to explain". In his view, "the weight attached to this indirect evidence depends on how closely related we judge the two classes of phenomena to be."[8]

Further, Friedman acknowledge that the confirmation of an assumption is partial support for a hypothesis:

"Suppose it can be shown it [The hypothesis] is equivalent to a set of assumptions including the assumption that man seeks his own interest. The hypothesis then gains indirect plausibility from the success for other classes of phenomena of hypotheses that can also be said to make this assumption; at least, what is being done here is not completely unprecedented or unsuccessful in all

philosopher Nagel. For some generally favorable appraisals of Friedman's position, mixed, except in the first case, with light criticism, see Campbell R. McConnell (1955); Kurt Klappholz and Joseph Agassi (1959); and George C. Archibald (1959).

[7] Friedman (1948, p. 28).

[8] Friedman (1948, p. 28).

other uses."[9]

Finally, as I understand him, Friedman concedes that, whenever a satisfactory test is absent, the realism of assumptions may contribute toward the evaluation of a hypothesis. In his words:

The decisive test is whether the hypothesis works for the phenomena it purports to explain. But a judgment may be required before any satisfactory test of this kind has been made, and, perhaps, when it cannot be made in the near future, in which case, the judgment will have to be based on the inadequate evidence available.[10]

In the light of these qualifications, the opening description of Friedman's thesis may seem to exaggerate his position. Indeed, he is careful never to say that the realism of assumptions is "irrelevant" without adding the adverb "largely".[11] Nevertheless the qualifications play a minor, rather inconspicuous role in his essay as a whole. They appear after the close of the main arguments, which are stated quite categorically. Moreover, Friedman's attitude toward past and prospective tests of economic assumptions reveals an extremely low degree of concern over his qualifications. In disputing previous tests of profit maximization, Friedman not only questions the workmanship of the experiments, but denies the logical basis for them. The unqualified version of Friedman's thesis, therefore, presents a reasonably accurate image of his position, and at the very least, this version of his thesis requires concentrated attention.

B – Argument 1 : The Inverse relation of realism and abstraction

In arguing his position, Friedman opens with an avowedly paradoxical view. "To be important", he maintains, "a hypothesis must be descriptively false in its assumptions". The main relevant paragraph may be quoted in full:

"In so far as a theory can be said to have "assumptions" at all, and in so far as their "realism" can be judged independently of the validity of predictions, the relation between the significance of a theory and the "realism" of its "assumptions" is almost the opposite of that suggested by the view under criticism. 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 (in this sense). The reason is simple. A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained."[12]

Clearly, Friedman is correct in asserting that the provision of simple and reliable hypotheses with wide application requires considerable abstraction. On what ground, however, does he claim

[9] Friedman (1948, p. 29).
[10] Friedman (1948, p. 30).
[11] Cf. Rotwein (1959, p. 555, note 6, and pp. 567-68). See also Koopmans (1957, pp. 137-39).
[12] Friedman (1948, pp. 14-15).

that abstraction involves the use of false assumptions? Evidently, Friedman considers abstraction as implying the assumption that some aspects of reality are absent. It is admittedly true that, in ordinary English usage, “assume that *x* is absent” may be substituted for “abstract from *x*” in many contexts. There is no meaningful sense, however, in which a hypothesis may be said to require the assumption that what it abstracts from is absent. No hypothesis hinge on such an assumption in any way. For instance, the statement that the marginal productivity of factors is inversely related to quantity involves abstraction from the sequence in which goods are produced. But the statement does not rest upon the assumption that good are produced without any time relation to one another. In abstracting, we leave some facts out of consideration. This commits us to the view that these facts do not matter, not to the assumption that the fact do not exist.

Further problems arise due to Friedman’s failure expressly to limit his use of the term “assumption” in the previous quotation to allegation of the absence of perceptible facts. In saying that “important assumptions must be descriptively false”, Friedman seems to argue that abstraction involves false allegation beyond the mere assertion that certain observable aspects are gone. This suggestion cannot be seriously contemplated. Abstraction is clearly consistent with the exclusion of false classes of implications from statements, and cannot require the admission of false information. In fact, as Friedman would probably agree despite his opposite suggestion, abstraction is the major instrument for achieving scope without sacrificing accuracy.

To elaborate, the exercise of everyday concepts and terms almost invariably leads to error in attempts to develop broad generalizations. Abstraction, and the subsequent introduction of technical terms, allows the elimination of ambiguities and exceptions from statements. As long as difficulties arising in the use of everyday terms can be traced to some specific classes of implications in a statement, increased abstraction enables superior formulation. Countless examples are available in economics. Thus, to present a simple case, reliance on ordinary concepts and terms poses serious obstacles in defining the general relation of buyers’ behavior to price. Yet the use of highly abstract concepts and terms make possible the compact and trustworthy statement that “the demand for all non-inferior goods is negatively sloping”.[13]

In conclusion, the relationship between abstraction and truth is not as recondite as Friedman would have everyone believe. Quite conformably with prevalent suppositions, abstraction facilitates the attainment of truth, and does not necessitate the acceptance of false assumptions, or immersion in “unrealism” of any sort.[14]

C – Argument 2 : The uninformativeness of knowledge of discrepancies between assumptions and facts

Next, Friedman set forth a more substantial argument, evolved with the aid of an intriguing illustration. Though Friedman’s presentation is intricately bound up with his illustration, a general formulation of the argument is possible. The starting-point, which remains somewhat implicit in his text, is that assumptions need only be approximated. That is, some finite departure from an assumption can be admitted in all or nearly all cases. The tolerable degree of discrepancy,

[13] For a highly illuminating and developed example see Carl G. Hempel (1958a).
[14] Cf. Nagel (1963, pp. 214-16).

theoretically speaking, can exceed any perceived deviation. Thus, while the acceptable deviation is sometimes discernible only with the use of refined instruments, at other times it may be evident to the unassisted senses. Since the acceptable deviation between assumptions and facts can be either "large" or "small", knowledge of the actual magnitude of a discrepancy, by itself, yields meager, nearly useless information. The proper way to resolve whether an assumption is adequately fulfilled is to check the observable implications of the hypothesis in question. If the implications are true, the assumption is warranted. Otherwise, the assumption must be rejected. It follows (barring the aforementioned special circumstances which Friedman discusses later) that the actual correspondence between assumptions and reality is irrelevant. To quote the central passage:

"We may start with a simple physical example, the law of falling bodies. It is an accepted hypothesis that … the distance travelled by a falling body in any specified time is given by the formula $s = ½ gt^2$, where s is the distance travelled in feet and t is time in seconds. The application of this formula to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves *as if* it were falling in a vacuum. Testing this hypothesis by its assumptions presumably means measuring the actual air pressure and deciding whether it is close enough to zero. At sea level the air pressure is about 15 pound per square inch. Is 15 sufficiently close to zero for the difference to be judged insignificant? Apparently it is, since the actual time taken by a compact ball to fall from the roof of a building to the ground is very close to the time given by the formula. Suppose, however, that a feather is dropped instead of a compact ball. The formula then gives wildly inaccurate results. Apparently, 15 pounds per square inch is significantly different from zero for a feather but not for a ball."[15]

Some aspects of Friedman's position can hardly be disputed. There is no question, for instance, that many discrepancies between assumptions and reality are quite acceptable. He is also correct in holding that mere size cannot determine whether a discrepancy is large or small. Whether 15 pounds air pressure, or any other positive amount less than infinity, is significantly greater than zero, to echo Friedman, clearly depends on the experiment on hand. One degree of seller influence over price will invalidate the assumption of perfect competition in some instances, but not others. Does this mean that the disparity between an assumption and reality has no bearing on the testing of a hypothesis?

To cope with this question, a distinction must first be introduced between two types of assumptions: (1) statements which are used in conjunction with the hypothesis in order to deduce predictions; and (2) statements which serve in deriving the hypothesis itself. For want of better names, the two types may be referred to, respectively, as "auxiliary" and "generative" assumptions. Some frequently employed assumptions usually function in one role rather than the other. In economics, for instance, "*ceteris paribus*" is typically an "auxiliary" assumption and "profit maximization" a "generative" assumption. But logically speaking, every assumption may serve in either capacity, depending on the particular test involved. To illustrate, the assumption of perfect competition may be used is deriving the hypothesis, say, that all firms in industry *Y* produce where marginal cost equals price, and may therefore function as a "generative" assumption. However, in relation to the hypothesis, say, that every perfectly competitive firm in industry *Y* produces where marginal cost equal marginal revenue, perfect competition is an "auxiliary" assumption. This

[15] Friedman (1948), pp. 16-17.

hypothesis does not presuppose perfect competition, but will only yield implications about the behavior of particular firms under the assumption that those firms are perfectly competitive.[16] In Friedman's example, the assumption of a vacuum is clearly an "auxiliary" one.[17] His argument, however, is intended to apply to "generative" as well as "auxiliary" assumptions, and thus must be examined with regard to both.[18]

In respect to "auxiliary" assumptions, consider a hypothesis, *H*, which, like any ordinary hypothesis, is expressible as saying that if certain type of conditions, $C_1, C_2, \ldots, C_n$, are realized, a certain type of event, *E*, occurs. *H* does not yield any predictions alone, but only in conjunction with a set of statements A_x, affirming that the conditions, $C_1, C_2, \ldots, C_n$, are true.[19] The members of A_x are "auxiliary" assumptions. Suppose, in accord with a fundamental implicit understanding in Friedman's argument, that the maximum tolerable discrepancy between $C_1, C_2, \ldots, C_n$, and the facts is not known. Now imagine that by using *H* and A_x together a statement about a specific spatio-temporal location, namely, an observation statement, 0, is derived.

If *0* is found to be true, then by inductive logic, both *H* and A_x acquire a degree of confirmation. That is, the outcome increases the probability that both *H* and A_x are true. However, it may be shown that any evidence contrary to A_x will also play a role in the interpretation of the test result. Given any deviation between A_x and reality, then along with the chance that experimental conditions conform adequately to the requirements, it is also possible that the facts fall outside the boundary conditions for testing *H*. If this possibility should hold, *H* would be consistent with false results. Thus, as long as the facts do not correspond fully with A_x, the truth of *0* is not necessarily favorable to *H*. Consequently, the greater the evidence opposed to A_x, the lower the degree of confirmation which the truth of *0* confers upon *H*. This reasoning applies without modification to Friedman's experiment with the compact ball. In this example, the accuracy of the predicted time of fall increases the probability of truth of the law of falling bodies and the assumption of a vacuum. However, the presence of 15 pounds of air pressure per square inch admits the possibility that the test is faulty, and thereby diminishes the significance of the result. In arriving at a different conclusion, Friedman simply ignores the chance of an invalid experiment.

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[16] I do not mean to exclude the possibility of a particular statement serving both as an "auxiliary" and "generative" assumption at once in an experiment. A simple illustration of this possibility can easily be constructed. Suppose that increasing marginal cost is among the assumptions employed in deriving the hypothesis that a fall in the output of butter will result in a rise in the price of margarine. In testing this hypothesis, we may determine that the output of butter has fallen without immediate evidence of the fall, relying mainly on the probable impact of medical reports about the cholesterol content of butter, and the blanket assumption of increasing marginal cost. Should this be done, increasing marginal cost would function both as an "auxiliary" and a "generative" assumption of the hypothesis.

[17] Cf. Nagel ((1963), p. 212.

[18] In developing the distinction between "generative" and "auxiliary" assumptions, I have chosen to avoid the conventional distinction between (1) "fundamental assumptions" and theorems and (2) assumptions "which refer to the antecedent clause of a conditional theoretical statement" (see Nagel (1963)). The latter two-part distinction divides statements on the basis of an accepted definition of a theory, and results in a fairly defined list of "fundamental assumptions" and theorems and an automatic exclusion of most assumptions from this "theoretical" group. On the other hand, my distinction is purely functional and permits any statement to belong to either the "generative" or the "auxiliary" classification, to shift from one to the other group, and even to belong to both groups on occasion. Since some economic assumptions sometimes function as "generative" and at other times as "auxiliary" assumptions, this classification has clear expository value.

[19] For further exposition, see the classic paper by Hempel and Paul Oppenheim (1953).

Suppose, next, that *O* is tried and found false. As long as the experiment deserves attention, or, in other words, is not manifestly a fraud, the result increases the probability that H is false. But the more the discrepancy between assumed conditions and reality, the greater the chance that the requisite test conditions are unfulfilled. Consequently, the greater is this discrepancy, the less reason there is to expect *H* to yield true implications, and the less disconfirmatory is the falsehood of *O*. In Friedman's example of the feather, therefore, the false prediction weakens the probability that the law of falling bodies is true, but the actual degree of environmental air pressure reduces the weight of the result. Friedman manages somehow to reach a strikingly different conclusion. He interprets the false predicted time of fall to mean that the assumption of a vacuum is false while the law of falling bodies is true. It may be readily seen that, if this interpretation could be logically sustained, all contradictory results would be attributable to the experimental environment, and no hypothesis could ever be disconfirmed. When a prediction is false, we must allow for the possibility that the hypothesis – in this case, the law of falling bodies – is wrong. The logician Carl G. Hempel advances this point in a somewhat different context, while, by sheer coincidence, employing the same example:

Let us imagine, by way of analogy, a physicist propounding the hypothesis that under ideal conditions, namely in a vacuum near the surface of the Earth, a body falling freely for *t* seconds will cover a distance of exactly $16\ t^2$ feet. Suppose now that careful experiment yields results differing from those required by the hypothesis. Then clearly the physicist cannot be content simply to infer that the requisite ideal conditions were not realized: in addition to this possibility, he has to allow for the alternative that the hypothesis under test is not correct.[20]

The unusual persuasiveness of Friedman's conflicting conclusion in his illustration can be explained. In his example, there are covering general laws enabling fairly accurate predictions of the magnitude of the error which the law of falling bodies will yield both in the instance of the feather and the compact ball. In addition, the reader knows beforehand, on the basis of general experience and education, that the law of falling bodies is liable to give a fairly accurate forecast of the time of fall of the compact ball, but will necessarily yield miserable results in the experiment of the feather. It is this prior knowledge, rather than the test results, which account for the reasonable appearance of Friedman's inferences. Supposing that all ground for anticipating the outcome of his experiment were removed, say, by the substitution of *x* and *y* for the terms "feather" and "compact ball", then the lack of justification for his conclusion would become patent.[21]

To summarize the main conclusions attained thus far in this section, given ignorance of the extent to which deviations between "auxiliary" assumptions and the facts can be tolerated, all evidence opposed to these assumptions increases the ambiguity of test results. Regardless of the test outcome, any discrepancy between "auxiliary" assumptions and reality raises some likelihood that the experiment is invalid. As a consequence, the lower the evidential support for A_x, the smaller the significance of both positive and negative results. The realism of "auxiliary" assumptions, thus, is

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[20] Hempel (1952), p. 78 (the title of the paper is not present in the book, but appears on the reprint). Cf. Andreas G. Papandreou (1963) and Archibald (1959, pp. 61-62, and 1963a).
[21] Cf. Koopmans (1957), p. 139.

plainly relevant.[22]

Friedman's argument may now be examined with regard to "generative" assumptions. Suppose H is derived from a set of statements or "generative" assumptions, A_g ; and presume, for the sake of simplicity, that no member of A_g belong to A_x. Now consider the significance of evidence opposed to A_g with regard to H.

Unlike testimony conflicting with A_x, such evidence would not interfere with the interpretation of outcomes of any tests of H. "Generative" assumptions are not logically required in deducing observation statements. Consequently, whatever the facts with respect to A_g, the results of checking observation statements derived from H and A_x can be unambiguously interpreted as either for or against H. Together with this point, it may be noted that the falsehood of A_g would not preclude the truth of H. False assumptions, like all other false statements, may bear true and non-trivial implications. The statement that all animal can fly, for instance, correctly implies that under a wide variety of circumstances, all winged animals can fly. As long as any false matter included in A_g is not also contained in H, the falsehood of A_g is perfectly compatible with the truth of H. In principle, therefore, no mere volume of evidence contradicting A_g need cause H to be abandoned. Furthermore, concurrent with a large amount of such contradictory evidence, H may enjoy great predictive success, and possess high confirmation.[23]

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22 In his article, Rotwein (*1959,* pp. 556-64) argues to the same effect, maintaining that under positive air pressure, a true test outcome does not necessarily support the law, and that "the probable inaccuracy of the prediction grow as the values of the variables of the given case depart from those under which we have been led to expect the prediction" (*ibid*., p. 563). But, unfortunately, he rest his argument on some obscure considerations, including the nature of causality and human predispositions toward chance, and does not adequately emphasize the logical issues raised by Friedman's position.

The argument in the text regarding "auxiliary assumptions" also emerges quite clearly in Papandreou (1958 and 1963). I am, however, quite skeptical regarding Papandreou's general emphasis. He seems to be largely preoccupied with the fact that, unless the truth of "auxiliary" assumptions can be ascertained, hypotheses cannot be – and the term here is quite crucial – refuted. He builds his distinction between "theories" and "models" on the issue of possible refutation, and devotes much attention to the types of logical structures which hypotheses may assume in order to be refutable if they derive from a "model" (consisting of irrefutable statements) rather than a "theory" (refutable) (see Papandreou (1958, pp. 8-9, 101-20 [esp. 116-17], and 141, and 1963)). The trouble with this perspective, in my opinion, is that although general scientific statements may be capable of refutation in principle, as a rule scientific hypotheses are rejected because of disconfirmation, or in other words, on ground of high probability of error. The main reason for this fact is that even in the most advanced sciences, as Friedman suggests, there is usually room for doubting whether the appropriate conditions for an experiment were adequately fulfilled. Consequently, as long as statements can be disconfirmed, which is true of those in Papandreou's "models", I do not see why the issue of refutability, as such, should be of particular concern. This criticism applies, a fortiori, to Geoffrey P.E. Clarkson (1963). Clarkson utterly confuses the issues of refutation and disconfirmation (see, for example, pp. 21-22, 82-83, 142). He maintains, contrary to the foregoing text, that if "auxiliary assumptions" are not known to be "true", in the strict sense, the results of experiments are neither confirmatory nor disconfirmatory (see pp. 95-96 in particular). He also bases a whole program of reform of existing consumer theory essentially on the absence of *refutability* of this theory, which he interprets as implying lack of "empirical meaning".

23 In his attempt to refute Friedman, Clarkson (1963) argues in sharp opposition to these views that "the prediction of an event can only be employed as a means of testing a theory when some parts of the theory have already been well confirmed" (p. 90), and moreover, contrary to his own position at some points (for example, p. 80), that "the derived laws [of the theories of utility and demand] could be empirically confirmed if and only if the basic postulate were empirically confirmed" (p. 103). This view rests on a confusion between "auxiliary" and "generative" assumptions, or perhaps more appropriately in this context, between "antecedent conditions" and "general theoretical statements". Proceeding on the basis of an example where the major

Yet these factors do not establish Friedman's case. While the falsehood of A_g does not thwart the testing of *H* or refute the hypothesis out of hand, it may nevertheless reduce the probability that *H* is true. If Friedman's thesis is to be meaningful, he must be maintaining, in effect, that evidence opposed to A_g would not raise the odds against *H*. Framed in this manner, his position is extremely weak.

In case A_g is true, then all of its implications, including *H*, must be true. However, if A_g is false, then some of its implications must be false, and these implications may affect the substance of *H*. Whatever the past success of *H* may be, should the hypothesis bear false content, it may yield false predictions in the future. Thus, broadly speaking, the more of evidence conforms to A_g the greater the rational basis for confidence in *H*; and vice versa, the less this evidence agree with A_g, the less this basis for confidence.[24] Furthermore, it may also be observed that *H* may derive an important measure of its existing support from the testimony favorable to A_g. If A_g did not possess sufficient factual corroboration to warrant some faith in its conclusions, this assumption would most likely not be used to develop hypotheses or hunches. There is every reason to think, therefore, that a marked reduction in the evidential basis for A_g would notably undermine *H*. In fact, in cases where hypotheses derive from a well-established general theory, the bulk of their support often stems from the theory. There are surely many instances in the physical sciences where hypotheses have been dismissed only because some covering law or theories had come under serious question.

D – Argument 3: The presence of "undesigned" classes of implications.

Friedman, however, is not satisfied to maintain, as the preceding arguments would imply, that if assumptions work they should be used, regardless of the extent of their unrealism. In addition, he holds that false information contained in assumptions may not concern classes of implications which the hypothesis is "designed to explain." Every hypothesis, he believes, harbors some implications which are completely without interest, and only findings concerning the other implications are pertinent. This argument, it may be noted, is not entirely consistent with his previous stand. If false information can be countenanced with complete equanimity, there is no need to plead that some of the implications of assumptions are "undesigned". Evidently, Friedman recognizes that false assumptions pose the danger of false predictions, and thus, that false assumptions have a certain likelihood not to "work".[25]

The notion that hypotheses contain "undesigned" classes of implications has serious merit. Every hypothesis (H) is limited to some specific classes of event by conditional clauses and other verbal restrictions. Each condition for the application of H, for instance, eliminates a whole class of

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hypothesis has the form "if *R,* then *S*", Clarkson concludes correctly that unless *R* is true, the truth of *S* does not necessarily support the hypothesis. However, he treats *R* as a general theoretical statement, whereas, of course, *R* is an "antecedent condition" (or an "auxiliary" assumption) in the example and, even if occasionally employed elsewhere in a "generative" role, can hardly be taken to be a "general theoretical statement". (In order to crosscheck this point, consider first that *R* cannot logically imply *S* or the hypothesis would simply be *R*, and next, that the economic postulates and theorems do not appear as subordinate clauses of more general economic statements). Clarkson's error underlines the importance of the related distinctions between "antecedent conditions" and "general theoretical statements", and "auxiliary" and "generative" assumptions (see pp. 88-92, and compare with pp. 19-20 where the same reasoning is correctly presented in relation to "antecedents").

[24] Cf. Nagel (1963), p. 215 ; Rotwein (1959), pp. 568-69, and also William J. Baumol (1959), p. 6.

[25] Cf. Rotwein (1959), p. 560, note 5, and the two paragraphs extending on pp. 560-61, and pp. 565-67.

implications which, in the absence of the condition, would follow from H together with its "auxiliary" assumptions (A_x). There is always a chance that these conditions are insufficiently restrictive in various ways, or that the sentence component of *H* asserting what happens under these conditions is too comprehensive. As presently stated, *H* and A_x may admit various classes of implications that concern types of events which the investigators are not interested in explaining or classes of implications which they have overlooked and would not accept upon notice. There are, in fact, some general reasons to think that most scientific statements are never perfectly consistent with the researcher's proper empirical interests and intentions. First of all, the assertion of all necessary qualifications and limitations of scope would often require intense consideration of outside problems, perhaps in different fields. In addition, this assertion would frequently cost an inordinate amount of time and resources from the perspective of current concerns.

However, the likelihood that negligible kinds of discrepancies exist between economic assumptions and facts is not a warrant for discarding any contradictory test evidence. Before any negative result can be safely dismissed, it is necessary to be able to discern the "undesigned" classes of implications. Friedman views this ability as present to an important degree in economics. According to him, every hypothesis in economic theory is attended by a set of rules defining the class of phenomena which the hypothesis is supposed to explain.[26] While these rules are inexact, and Friedman deplores the failure to devote greater attention to their elaboration, he believes that "to a considerable extent the rules can be formulated explicitly."[27]

The issue of the existence of these rules can be examined, first, from a highly common-sense point of view. If economic rules of application of the sort which Friedman describes can be formulated to any significant degree, then what are some examples? What, for instance, is a rule determining any one class of "undesigned" implications of the postulate of profit maximization? Are there any kinds of conditions under which no producer is expected to do so? Admittedly, it may not matter for most purposes whether producers try to maximize their profits to the very last cent, but given the wide range of functions which the profit-maximization postulate serves in economics can any practical deviation from the assumption be confidently termed completely inconsequential? Likewise, that are the rules for finding the "undesigned" classes of implications of the theorem of negatively sloping demand for non-inferior goods? Friedman supplies no answer to these questions. The only support for his view that can be inferred from his essay is the indication that the same firm or industry may be regarded as perfectly competitive for some purposes, but not for others. However, economic theory, in its most unlimited form, does not clearly assert the omnipresence of perfect competition. Hence the specification of rules concerning the proper application of this assumption, or the supply of guidelines determining when perfect competition can and cannot be assumed, does not plainly entail any "undesigned" classes of implications. Moreover, Friedman's one or two examples of legitimate and illegitimate applications of the assumption of perfect competition to a single industry do not provide a single tenable rule of application regarding perfect competition, let alone with respect to any other economic assertions. While these examples may be fairly convincing, it would be very difficult to infer a general rule of application from them.[28]

[26] Friedman (1948), p. 24.
[27] Friedman (1948), p. 25.
[28] Friedman (1948), pp. 36-37. Archibald (1961) has made a highly related point with regard to Friedman's major example, involving the cigarette industry (esp. pp. 3-4), and in return has received a sharp rebuke from

Aside from the practical problem of identifying rules of application in economics, it may be observed, more generally, that it any "undesigned" classes of implications could be properly identified, they could then be eliminated. In other words, if we could pinpoint precisely something that we do not wish to say, we could cease to say it. Thus, in order to substantiate his contention that certain adverse test results can be completely disregarded, Friedman should be able to show that, by introducing appropriate qualifications and conditions, he could narrow the empirical scope of economic theory without impairing the explanatory and predictive value of the theory. Until he can meet this test, there is every reason to think that all negative findings must be accepted as disconfirmatory.[29]

To elaborate on this view beyond the point reached in the preceding section, assume that there is reason to suspect, along with Friedman, that some of the testimony advanced, say, against the profit-maximization postulate does not impair the theoretical statement. Granted, however, the inability to reconcile the postulate satisfactorily with the contrary evidence, how can we be sure ?

Friedman (1963) (see also Archibald (1963) for his rejoinder). In his answer, Friedman maintains that his example is intended to show the importance of specifying the rules of application, rather than existing knowledge of those rules. But I must confess that, whatever Friedman may say elsewhere in his paper regarding the need for developing and elaborating the economic rules, like Archibald, I cannot help but read the cigarette example and the entire surrounding context as an attempt to show the current availability of such rules and the neglect of the critics of economic theory of their existence. The following three opening statements of paragraphs shortly prior to the cigarette example set the tone for the illustration :

"The confusion between descriptive accuracy and analytical relevance has led not only to criticisms of economic theory on largely irrelevant ground but also to misunderstanding of economic theory and misdirection of effort to repair supposed defects" (Friedman (1948), p. 34).

"This tendency is perhaps most clearly illustrated by the interpretation given to the concepts of 'perfect competition' and 'monopoly' and the development of the theory of 'monopolistic' or 'imperfect competition'" (*ibid.*).

"As always the hypothesis as a whole ["that, for many problems, firms could be grouped into 'industries' such that the similarities among the firms in each group were more important than the differences among them"] consists not only of this abstract model but also of a set of rules, mostly implicit and suggested by example, for identifying actual firms with one or the other ideal type and for classifying firms into industries" (*ibid*., pp 35-36).

As may be gleaned from these quotations, the general trend of Friedman's argument is that, in refusing ever to apply various assumptions to particular economic units, the critics of economic theory, Marshallian and otherwise, are blind to the existence of certain "rules".

[29] Cf. the following statement by Koopmans (1957, p. 139) : "To state a set of postulates, and then to exempt a sub-class of their implications from verification is a curiously roundabout way of specifying the content of a theory that is regarded as open to empirical refutation". See also Nagel (1963), pp. 217-18.

There is a great deal of reciprocity between the argument at this point and Samuelson's (1963) criticism of Friedman. Samuelson stresses that whatever is known to be false in an argument should simply be purged. While this view is correct, I think it bears emphasis that to remove false information from a statement requires knowledge of the false classes of implications rather than merely the false instances themselves. In other words though a statement may be known to be imperfect, there may be no clear avenue of improvement. Samuelson's failure to give adequate consideration to this point has led him, in my view, to an important error in criticizing Friedman. Samuelson supposes that a favorable outcome of a check of an implication of a general statement only supports the whole statement if no parts of the statement are known to be false. However, since there may be no obvious way to remedy the defects of the statement, the only suitable interpretation of a favorable test result may be as adding a degree of confirmation to the statement as a whole. The basis for this interpretation is that every favorable result enhances the chances that the statement, as constituted, will yield true predictions in the future. However, Samuelson's error in refusing to accept some positive results as confirmatory is far less serious than that of Friedman in supposing that some negative outcomes are non-disconfirmatory.

Even though the negative results, as such, appear trivial, the part of the theory to which the results pertain may carry latent significance. In any general theory including many theorems, the interrelations among statements and definitions may be extremely complex, and any portion of the theory may be an unsuspected cause of many false predictions. Furthermore, even if certain practical errors in the theory have not born any adverse effects in the past, they could become catastrophic in the future. Theories are supposed to provide new and untried hypotheses, and whatever may be the past record, later hypotheses may be highly influenced by on-going theoretical mistakes. It follows that any errors uncovered in assumptions may also be considered as clues regarding the possible improvement of the theory.

There is also a noteworthy and curious inconsistency in Friedman's position. While minimizing the importance of contradictory findings, he sometime displays a lively appreciation of the impact of evidence supporting any fragment of a theory on the entire theoretical edifice.[30] It is quite rational to believe that every favorable result confers some degree of confirmation on every part of a theory and all logically affiliated hypotheses. But granted this view, it is difficult to see how the converse effect of negative results can be denied.

E – Economic theory as a set of "as if" statements

In connection with the problem of "undesigned" implications, a particular semantic proposal by Friedman that is enjoying some following should be discussed; namely, the idea of regarding assumptions as "*as if*" statements. Specifically, the proposal is that assumption be construed to mean that certain events take place *as if* the assumptions were true, instead of being understood as out-right assertions. The basis for the appeal of this construal is obvious: It would mean that assumptions, as conventionally stated, are never even supposed to be true. Thus, the construal is in keeping with Friedman's view that certain fallacies in assumptions have no bearing. In addition, the construal answers the conspicuous demand of many economists for some sort of logical protection against unfriendly criticism of economic theory. A quotation will make Friedman's proposal clearer:

"Consider the problem of predicting the shots made by an expert billiard player. It seem not at all unreasonable that excellent predictions would be yielded by the hypothesis that the billiard player made his shot as if he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc, describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas… .

It is only a short step from these examples to the economic hypothesis that under a wide range of circumstances individual firms behave as if they were seeking rationally to maximize their expected return… and had full knowledge of the data needed to succeed in this attempt."[31]

The "*as if*" construal of economic assumptions, whatever its appeal, has some important drawbacks. In particular, the construal is inconsistent with the explanatory and predictive function of economic theory and scientific statements in general. With regard to the explanatory function, suppose the question arises why a given individual shoots expert pool. The answer that he shoots

[30] See, for example, n. 9 above and the associated quotation in the text.
[31] Friedman (1948), p. 21.

expert pool because he is a competent mathematician, and can make the balls travel in accord with complicated calculations, etc., can at least be accepted. But the reply that he shoots pool *as if* he were a competent mathematician, and so forth, is beside the point. This answer concerns the question "how possibly" rather than the question "why". The correct answer to "how possibly", of course, is completely independent of the actual state of affairs and the genuine reasons why. Thus, Friedman might as well have said that the billiard player shoot the balls into the pockets *as if* the balls were magnetized and the pockets were magnets ; or as if there were invisible ridge in the table shifting in accord with the player's will and leading the balls into the pockets. The question "why" calls for an assertion of fact, suitable for an introduction with the term "because". Statements of possibilities and suggested analogies do not constitute explanations. In sum, the economic postulates cannot appear in an "*as if*" form when they are used in explanation. Since the postulates frequently serve in order to explain, the "*as if*" construal would seriously misrepresent these statements.

The "*as if*" formulation is also impossible to reconcile with the predictive function of general scientific statements. Departing from the rather trivial example of the expert pool player, consider the assertion that businessmen behave as if they were trying to maximize their profits, or (if such an alternative be possible) that under certain conditions businessmen behave as if they were attempting to maximize profits. Given the requisite circumstances, if any, what can we predict, or in other words, what observable implications can we derive from this "*as if*" statement? Unfortunately, no observable implications seem to follow. If businessmen act only *as if* trying to maximize profits, then evidently they do not exactly try to maximize profits, at least not all of the time, and perhaps sometime they do not even try to maximize profits at all. As a result, no specific conclusion about businessmen's actions, however vague and tentative, can be strictly derived from the statement. In basing any prediction on the assumption of profit maximization, it may safely be concluded that there is implicit reliance on the declaration that, maybe only under some *specified* conditions, and maybe only with regard to some *defined* subgroup, businessmen really and truly try to maximize their profits.

Since "*as if*" statements can serve neither to explain nor to predict, Friedman's proposal to interpret the economic postulate as "*as if*" statements should be rejected.[32] Broadly speaking, in using the economic postulates to explain and predict, we commit ourselves to what they say about the world, and thus it would seem almost mandatory to interpret them accordingly as straightforward declarations of fact.

F – Conclusion

In conclusion, lest the point be lost amid the criticisms, I would like to emphasize the presence of an important measure of agreement with Friedman in these pages. Most notably, I

[32] A similar objection can be raised against all of the weak characterizations of economic theory mentioned in the introduction, such as "box of analytical tools", "filing cabinet", "conceptual framework", etc. Admittedly, these characterizations are correct in stressing that economic theory provides a compact, efficient way of structuring and storing masses of information, which is useful, for instance, in writing textbooks and cataloguing data ; and they are also correct in stressing that the theory helps in raising questions and suggesting possible replies. But since the theory is continually employed in the further effort to explain and predict the future, it does not seem accurate to depict economic theory as a mere analytical device (for viewing things, asking questions, compiling knowledge, etc.). In employing these weak characterizations, therefore, it may be said that economists have unduly modest pretentions. The most acceptable characterization, I submit, is that economic theory is a set of general statements about the world.

concur fully with his view that the disconfirmation of an assumption, as such, does not disqualify any hypothesis. First, false "auxiliary" assumptions impede testing but do not carry the least suggestion that the hypothesis is false.[33] Second, false "generative" assumptions may bear true and non-trivial implications. A hypothesis about price behavior founded partly on the profit-maximization postulate could be true, granted that the postulate is false. There is no doubt that, in arguing this general point of view, Friedman faces strong opposition from many quarters. Many writers, mostly in the social sciences and humanities, evidently regard assumptions as the pillars on which whole theories and even entire disciplines stand. Judging from some accounts, science is a pyramid with assumptions on the bottom and empirical knowledge sitting precariously on top. Friedman's strenuous objection to this extreme position is amply justified. Allowing that assumptions are often an important source of support for hypotheses, that they affect what is found, surely it is fundamentally the facts that support theories, rather than the other way around. If all corroborating evidence for empirical theories were lost, it would seem that theories and their assumptions would soon collapse. On the other hand, the destruction of all assumptions could not negate all the previous testimony of our senses. As Friedman ably points out, although this is by no means the only relevant consideration, the facts apprehended at any moment in time are always compatible with infinitely many assumptions.

Unfortunately, however, Friedman does not confine his remarks to the previous points. (To some extent, however, he fosters the quite contrary impression that everything that he says is logically attendant; witness the disarming title of his main section, concerning the irrelevance of the truth of assumptions: "Can a Hypothesis Be Tested by the Realism of Its Assumptions?") Friedman also states that a test of assumptions is completely pointless, unless, as he mentions only after concluding his main arguments, a few special kind of circumstances hold. This position must be unreservedly rejected. As noted, the greater the deviations between test conditions and "auxiliary" assumptions, the lower the ability to perform tests ; and the greater the extent to which "generative" assumptions are inaccurate, the lower is the probability that the derived hypotheses are true. More generally, with regard to "generative" assumptions, the more unrealistic these assumptions, the weaker the basis for relying on them in order to develop hypotheses or hunches. Hence, contrary to Friedman, tests of economic assumptions are generally useful in testing and appraising hypotheses, and in indicating possible avenues of improvement in theory and hypotheses.[34]

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[33] Cf. Nagel (1956), p. 215.

[34] Nagel (1963) fully corroborate these conclusions. He reasons that if "unrealistic" is understood in the sense of false, rather than abstract, Friedman's arguments are wrong. Nagel's article suffers, however, from a failure (or at least the strong symptoms thereof) correctly to assess Friedman's main objectives. Perhaps intending to give the benefit of a doubt, Nagel interprets Friedman's support of unrealistic assumptions strictly as an effort to defend the use of abstract theory. That is, Nagel does not acknowledge Friedman's interest in undercutting the rational basis for testing assumptions and in denying the relevance of the results of tests of assumptions. Concomitantly, Nagel focuses largely on what seem to me to be a subsidiary aspect of Friedman's position, namely, the view that abstract theoretical terms could, if so desired, be completely eliminated from statements. Nagel regard this view as an essential link in Friedman's effort to establish the acceptability of abstract theory whereas, if my reading is correct, the view is a secondary accompaniment of Friedman's attempt to show that certain aspects of assumptions are "undesigned" and that the facts relating to them are negligible. Whatever the true significance of this view, Nagel's refutation is conclusive. I may also note that Nagel does not mention Friedman's argument for "*undesigned*" classes of implications. Yet on all issue where this paper and Nagel's meet, there is, I believe, total agreement.

III – The Machlup View

A – Introduction

In his article on "The Problem of Verification in Economics",[35] Machlup adopts Friedman's view of the irrelevancy of tests of assumptions, but only with respect to one particular type of assumptions, namely, "fundamental assumptions". Other assumptions, Machlup believes, require some degree of corroboration in an experiment.[36] In support of his position regarding "fundamental assumptions", Machlup follows a special line of reasoning. To begin with, he holds that "fundamental assumptions" cannot be tested "directly" or "independently". To quote him: "There is no way of subjecting fundamental assumptions to independent verification"[37] ; and "Fundamental assumptions are not directly testable and cannot be refuted by empirical investigation".[38] In addition, Machlup also argues, particularly with respect to economics, that "fundamental assumptions" are not supposed to be tested "independently" since they can be properly understood as making no factual declarations. These two arguments are not fully consistent: If "direct" tests of "fundamental assumptions" are entirely precluded, as the first argument says, then the argument that these assumptions may be properly interpreted in a way rendering "direct" tests inappropriate is completely superfluous. Nevertheless, Machlup develops both arguments together and does not separate the two.[39]

Machlup's stand is of interest largely as an example of a certain kind of opposition to direct testing of theoretical postulates. Concern with his stand is also dictated by his taunting suggestion that his conclusions are commonly accepted in the field of the philosophy of science. "Logicians and philosophers of science", he asserts, "have long tried to make this perfectly clear".[40] No attempt will be made to deal with Machlup's position as a whole ; the aim will be only to treat a few important selected topics in connection with his stand, namely : (1) the distinction between "direct" and "indirect" tests ; (2) the argument that "fundamental assumptions" are inherently impossible to test directly ; and (3) the argument that it is illegitimate to test "fundamental assumptions" because they can be appropriately understood as rules, or more generally, as making no declarations about facts.

[35] Machlup (1955).
[36] See Machlup (1955), pp. 8-16, and the interesting paragraph on pp. 18-19. Machlup fails to recognize his disagreement with Friedman regarding "non-fundamental" assumptions. According to Machlup, the "only serious flaw in the otherwise excellent essay" by Friedman is the failure to consider the requirement of "understandability" (*ibid*., p. 17, n. 42).
[37] Machlup (1955), p. 9.
[38] Machlup (1955), p. 11.
[39] Machlup's confusion of the two arguments is particularly evident on p. 11 of the article. Later on, in his rejoinder to a reply by Terence W. Hutchison, Machlup (1956, p. 487) admits the error, saying : "Perhaps it was confusing when in addition to stating that these fundamental assumptions need not be independently verified, I also indicated that they cannot be so verified". Also in the same general passage (*ibid*., p. 488), Machlup abandons the argument that "fundamental assumptions" are inherently impossible to test directly. In his subsequent writing, however, including one lengthy methodological article, Machlup makes a number of cross-references to his 1955 essay, without indicating the least change in his position (see Machlup (1960) and 1963, pp. 48, 79). It may also be noted that his reply to Hutchison (1956) is a spirited defense on the whole (see Machlup (1956)). The original article by Machlup and the subsequent exchange with Hutchison have been translated into Italian in a book under the authorship of Machlup, Hutchison, and Emile Grunberg (1959).
[40] Machlup (1955), p. 10.

B – The distinction between direct and indirect tests

The distinction between direct and indirect tests plays a central part in the writing of Friedman and Machlup, since both authors contend that, for one reason or another, postulates can be properly tested only in a roundabout or indirect way. The reason for discussing the distinction with regard to Machlup is that he deals with it more explicitly and more satisfactorily than Friedman.

Friedman presents the distinction by contrasting testing by mere "perception" of "descriptive accuracy" with testing by checking the validity of derived implications ; and Machlup does essentially the same at one point.[41] The distinction between comparing a statement against the facts and checking its implications, however, is highly superficial. Every test involves a checking of something which follows from a statement or set of statements by implication. Thus, while Friedman may wish to regard an investigation of the propensities of individual businessmen *a, b, c, …, n*, to maximize profits as a mere test of the descriptive accuracy of the profit-maximization postulate, the fact remains that this test constitutes a check of bona fide implications. At best, the distinction between checking a statement, as such, and checking its implications can be understood as one between a check of a whole as opposed to a check of only a part of a statement. The latter distinction, however, makes sense only with regard to statement whose entire empirical content is amenable to checking with a finite set of observations (for example, "this tree is an elm"). In the instance of statements yielding an infinite set of implications requiring separate observations, which comprises the whole class of general theoretical statements, checking the entire empirical content of the statements is not feasible.

In an effort to advance beyond the crude distinction between comparing statements against the facts and checking their implications, Machlup asserts that a direct test entails the derivation of implications from a statement alone whereas an indirect test involves the derivation of implications from a statement in conjunction with other statements. This proposed definition, while a step in the right direction, is also inadequate. The definition provides no basis for a distinction between direct and indirect tests of conditional statements, which constitute, of course, the dominant portion of the entire output of scientific hypotheses. A conditional statement, by itself, yields no observable implications whatever, and can never be tested in complete isolation from other statements.[42] The statement "if *x*, then *y*" does not imply anything about the world at any particular spatio-temporal location unless it is brought together with an affirmation that *x* is true. The combined assertion of "if *x*, then *y*" and "*x* is true" implies *y* and whatever observable implications *y* may bear. Thus, according to the proposed definition of a direct test, no direct test of the main assertion "if *x*, then *y*" is possible, and all tests of conditional statements are indirect.[43]

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[41] To quote the relevant passage in Machlup (1955, p. 7): "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*" (p. 7).

[42] Though, of course, the statement "if *x*, then *y*", by itself, does bear infinitely many conditional implications. For one thing, the statement implies itself. More significantly, the statement yield implications of the form "if *z* implies *x*, then *z* implies *y*".

[43] To offer an economic example, consider the conditional assertion: "If wages are inflexible, then a fall in the aggregate demand for goods will cause unemployment". This assertion, alone, says nothing whatever about observable conditions at any specific point in space and time. But a conjunction of this assertion and the statements that "wages are inflexible" and "government spending on goods and services has declined" implies

The only tenable distinction between a direct and an indirect test, agreeing with the general intentions of Machlup and Friedman, is one between a test that employs only statements that are logically essential for the derivation of observable implications and a test that uses additional statements. In other words, an indirect test involves the checking of implications derived from a statement together with one or more others that are not required in order to draw some observable implications by means of the first. Machlup's reply in his exchange with T.W. Hutchison[44] indicates an attempt, although not an altogether successful one, to formulate the distinction in this way.[45]

C – The impossibility of testing "fundamental assumptions"

Machlup's argument that "fundamental assumptions" are impossible to test directly rest essentially on the experience in physics.[46] As frequently noted, the physical postulates cannot be directly tested. Like many other observers, Machlup is extremely impressed with this point and prone to generalize on its basis. However, the difficulty of testing the physical postulates in any relatively straightforward manner is easy to diagnose ; it arises from the fact that these postulates contain various highly abstract terms, such as atom and molecule, whose counterpart in nature are not subject to immediate sensory observation. It does not follow that there are significant barriers to direct testing of postulates in other disciplines, particularly in a field as distantly related to physics as economics.[47] Moreover, the experience of physics does not signify that "fundamental assumptions", in general, are impossible to test directly.[48]

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a present rise in unemployment. Thus, on the basis of the present definition, the only possible test of the hypothesis is indirect.

[44] See Hutchinson (1956) and Machlup (1956).

[45] Machlup (1956), pp. 484-85.

[46] See Machlup, (1955), pp. 9-10.

[47] Cf. Koopmans (1957), p. 135. In Machlup (1960), he maintains that the theoretical concepts of economics are highly abstract, and thus, by intimation, supports his earlier thesis that the economic postulates require indirect testing. However, Machlup's argument rest entirely on the supposed classification of all scientific concepts into two groups: "operational concepts" and "pure constructs". "Operational concepts" are those which can be defined as a set of operations. "Pure constructs", on the other hand, are either mathematical, or else bear only "some indirect reference to facts of experience" and are "derived through idealization, heroic abstraction, or inventions" (*ibid*., p. 577). In order to show that the economic concepts of "price" and "quantity" are "theoretical" or "pure constructs", Machlup is satisfied to point out the inadequacy of any purported operational definition of either term. Given, however, Machlup's own demonstration of the extreme weaknesses of "operationalism", or the thesis that all scientific concepts should be "operational", his bipartite division of concept is untenable. For example, as Machlup indicates quite clearly (*ibid.*, p. 558), all objects of sensory perception, such as ant or lilac, cannot be operationally defined, lest the concepts be made synonymous with the operations of the observer in identifying these objects. Yet the concepts of an "ant" and a "lilac" can hardly be regarded as "pure constructs". The need for some intermediary classification (or classifications) is clear, and the possibility that many economic concepts belong to this (these) intermediary grouping (s) is equally plain. Thus Machlup's argument for the high abstractness of the economic concepts does not stand.

[48] Machlup provides very little evidence of philosophical support for his view. His two lengthy quotations from prominent philosophers (Machlup (1955), p. 10) do not say, as he supposes, that scientific postulates are impossible to test directly, but rather that postulates which are only indirectly testable have proven to be extremely valuable in science. His most impressive citation of philosophical support is the statement by Richard B. Braithwaite (1953): "The empirical testing of the deductive system is effected by testing the lower-level hypotheses in the system". This assertion, though in seeming agreement with Machlup, actually offers him no

To elucidate the falsehood of the thesis that empirical postulates cannot be tested directly, first, the status of a statement as a postulate in a theoretical system implies nothing about the testability of that statement. Any general statement can serve as a postulate in a deductive system. Furthermore, a "lower-level" hypothesis in one deductive system can be a "fundamental assumption" in another. To carry the argument further, there is no reason, in principle, why in a certain system the theoretical postulates might not be more readily testable than the theorems and many of the "lower-level" hypotheses. In one of his writings, the noted philosopher Rudolf Carnap emphasizes this practical possibility,[49] which derives mainly from the feasibility of introducing new terms at any step in a deductive sequence. Since the terms which are imported after a deductive process has begun can be highly technical, the derived statements of a theory may be more abstract and less susceptible to direct testing than the underived one. A single economic example, intended to be correct, but not to supply the ground for any generalization about abstract relations in economics,[50] will suffice. The assertion that indifference curve are convex, everyone will probably admit, is more abstract than the statement that consumer always prefer variety to uniformity of goods. Yet, as is generally well known, the latter assertion can serve as a partial basis for deriving the convexity of indifference curve. The main reason for this condition, evidently, is that the special terms "indifference curve" and "convex" are injected at a late point in the argument.[51]

D – The illegitimacy of direct tests of empirical "rules"

Perhaps the most challenging part of Machlup's stand, requiring some detailed attention, is his view that the economic postulates *ought not*, or cannot *legitimately* be tested directly because they can be understood not to be assertions of fact. His main support for the belief, as the reply to Hutchison explicitly shows, is the following:

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; they are and remain accepted as long as they have heuristic value, but will be rejected in favor of other rules

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assistance. Braithwaite use the term "lowest-level" hypothesis to mean any general statement which is last in a deductive order, regardless of the simplicity of the derivation. Moreover, in the relevant context, the statement refers unambiguously to the possibility of a direct test. Machlup (1955, p. 6) also claims that Nassau W. Senior, John Elliot Cairnes, and John Stuart Mill denied "the independent objective verifiability of the fundamental assumptions" of economic theory. There is no doubt, however, that all three of these writers, held that at least some of the "fundamental" economic assumptions could be independently (and "objectively") verified, for example, the "Law of population". For explicit indications see Senior (1875), pp. 77, 88-89, and Mill (1844), pp. 145-46. Moreover, Mill (1844) clearly opposes the notion that the economic postulates require no direct testing. In this essay, Mill vigorously argues that the main economic assumptions should never be put to work without priorly ascertaining their general applicability to the particular case involved (see *ibid*., pp. 139-41, and 149-50).

[49] Rudolf Carnap (1939), esp. p. 64.

[50] In this connection see the interesting though rather unusual comment by Herbert Simon (1963).

[51] Cf. Hutchison (1956), pp. 481-82.

(assumptions) which seem to serve their explanatory functions more successfully.[52]

To quote a closely related passage in the "Rejoinder":

"In his comment on the nature and significance of the maximization postulate Professor Hutchison conveys the impression that he recognizes as scientifically legitimate only two kinds of statements: Propositions which by empirical tests can, at least conceivably, be proved to be false, and definitions without empirical content. If so, he rejects a third category of propositions used in most theoretical systems: the heuristic postulates and idealized assumptions in abstract models of interdependent constructs useful in the explanation and predictions of observable phenomena… .

Logicians have long recognized this intermediate category of propositions, which are neither *a priori* nor *a posteriori* in the strict sense of these terms."[53]

On the basis of the general content of Machlup's essay, his argument can be interpreted substantially as follows:

Step I. Logicians "have long recognized" the presence of statements which are neither "*synthetic*" nor "*a priori*" ;

Step II. Some special terms have been addressed to such statements;

Step III. These terms are applicable to the postulates of economic theory; and therefore

Step IV. Like all other statements which are suited to bear those labels, the economic postulates ought not to be tested independently.

In order to avoid prolonged discussion, I will confine myself to the principal issues related to economics. It is convenient to begin with Step III. With close bearing to this step, one can question Machlup's assertion (which does not emerge fully above) of the equivalence or at least equal admissibility of a variety of different portrayals of the economic postulates. Machlup is explicitly indifferent as to whether the postulates are understood as "rules of procedure", "heuristic postulates", "idealized assumptions", "useful fictions", "resolutions", or "definitional assumptions", to list only his most prominent appellations.[54] Yet some of these terms differ greatly, and the application of all of the appellations to the economic postulates does not seem appropriate.

The conception of the economic postulates as "definitional assumptions", if not illogical, is certainly misleading. The adjective "definitional" is usually understood to pertain either to sentences defining a concept or term, or to a particular concept or term. Given this construal of the term, the economic postulates are clearly not "definitional". Whatever other meaning may be assigned to "definitional", the term retains the connotation "necessarily true". Yet we know that the assertion of non-optimizing behavior, for example, involves no inherent contradiction. More generally, it would spell a great misfortune for economics if the economic postulates were necessarily true, since then all of their implications would be empirically irrefutable and no hypotheses or predictions could be

[52] Machlup (1955), p. 16.
[53] Machlup (1956), p. 486.
[54] See Machlup (1955), pp. 9, 16 and (1956), pp. 486, 487, including n. 5.

derived from them.[55]

Nor is it clear that all economic postulates can be interpreted as "idealized assumptions". On the basis of current philosophical usage, an "idealized" assumption is one which holds true only under certain limiting conditions which can never be even closely approximated in practice outside of the laboratory. Accordingly, the "assumption" of a "pure gas", for instance, is "idealized". If this special philosophical meaning is accepted, then objective factors largely determine whether a given statement is "idealized" or not, and it cannot simply be postulated at will that the "fundamental" economic assumptions are "idealized". In particular, "optimizing behavior" is not a very convincing example of an "idealized assumption".

Avowedly, though, the postulates of economic theory can be interpreted as "procedural rules", "heuristic postulates", "useful fictions", or "resolutions". Consequently, the subsequent treatment of Machlup's position will be restricted to these possible appellations. Despite some difference in meaning between the four terms, the notion of a "rule" seems to approximate all four, and therefore I will focus on the interpretation of the economic postulates as rules.

Machlup strongly suggests that there are some logically unimpeachable grounds, established by philosophers, for viewing the economic postulates as rules. Judging from his account, it has been conclusively shown that certain scientific propositions, particularly theoretical postulates, are not statements about the world, but rules or something similar. Yet his only attempt to corroborate this impression is the consideration, implicit in his reference to Felix Kaufmann, that some propositions that relate to experience (and thus are not "a priori") are retained in science despite contradictions by the facts (and therefore are not "synthetic"). According to Kaufmann, Machlup's principal authority, any proposition which is known to contain a single false implication, and is nevertheless retained, must be understood as a rule of procedure.[56] Kaufmann's position, however, is highly debatable. At most, Kaufmann is able to show that, if false propositions are interpreted as statements instead of rules, there is some difficulty in reconciling the use of these propositions with prediction. In support of the view that false scientific propositions can be rationally understood as statements rather than rules, though, it can be argued (1) that in predicting, one questions whether any of the operating premises are really false; and (2) that predictions can always be based on false statements if sufficient allowance is made for the probability that the predictions are false. In fact,

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[55] Hutchinson is responsible for considerable confusion, I believe, on the question of the logical truth-status of the economic theoretical statements. In Hutchison (1938), he argues that the postulates and theorems of economic theory are tautological and hence necessarily true. Though by this time Hutchinson may have altered his opinion, his early view continues to exert influence among economists, and therefore deserves attention. His sole supporting arguments for his 1938 position are : first, that "in formulating a system of definitions one is in one and the same process formulating a series of analytical-tautological propositions of pure theory" (*ibid.*, p. 30) ; and second, that every proposition which follows by implication within a deductive system is a tautology. With regard to the first argument, the construction of a theoretical vocabulary does not necessitate any statements at all, and granted that tautologies can usually be manufactured easily, any set of technical terms can be used to formulate non-tautological statements, given a sufficient vocabulary and adequate syntactical rules. Otherwise, of course, no technical terms could ever serve in order to obtain empirical statements. As for the view that all theoretical implications are tautological, which applies to theorems and not postulates, here again a logical mistake is involved. If statement *X* is derived from *Y*, then *X* is necessarily true if *Y* is true, but not if *Y* is false. Hutchinson's view easily leads to a *reductio ad absurdum* since every statement is theoretically derivable from infinitely many others. Incidentally, among those who have indorsed Hutchison's argument are some eminent economists.

[56] See Felix Kaufmann (1944), pp. 83-89.

the general trend in philosophy seems opposed to the interpretation of any empirical propositions as rules.[57] On the basis of widespread philosophical opinion, every empirical proposition in science can be formulated and understood simply as a factual statement. Thus, as might have been supposed apart from any philosophical considerations, there is no logical necessity to view the economic postulates as rule.

Suppose, however, that the economic postulates are nevertheless construed as rules. Then there arises the central problem connected with Machlup's argument, involving Step IV: How would the "rules" interpretation of the economic postulates affect the possibility and legitimacy of independent tests ? Machlup seems to suppose an enormous adverse effect. Bur in order to explain, he always fall back upon the idea of a contradiction in the very notion of a direct test of a rule. This contradiction, it may be shown, is illusory.

To begin with, a proposition cannot be blindly accepted in science because of its construal as a rule. Science is not an art or game of skill, where the object is to accomplish a certain end within defined constraints. The object of science is to explain and predict events, and the adequacy of any rule employed in science must be judged in the light of their contributions to this objective. Thus, even supposing that the construal of empirical postulates as rules dispose of the question of their "truth", "falsehood", "confirmation", or "disconfirmation", the issue of the "usefulness", "adequacy", "reliability", or "correctness" of the postulates will still remain.[58] This issue must be resolved on the basis of empirical findings.

Machlup, in fact, recognizes that indirect tests affect the usefulness of empirical rules. But if the construal of postulates as rules does not remove the need for testing, the construal, as such, does not justify any opposition to direct testing. Consequently, Machlup's objection to the direct testing of postulates is not implicit in the construal of postulates as rules. So far as I can see, none of the proponents of the construal of theoretical postulates as rules, including Kaufmann,[59] have ever advocated self-imposed limitations on direct testing. Moreover, Machlup's view cannot be justified. To cite the main criticism, inherent in the discussion of Friedman's thesis, the results of direct tests bear on the usefulness of empirical "rules". The most pertinent previous argument is that the

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[57] See Nagel's "Review" of Stephen Toulmin (1954), reprinted in Nagel (1956), pp. 303-15; Hempel (1958b); and also H. Gavin Alexander (1958).

[58] At one point in Machlup (1956, p. 486), he does not rely on the interpretation of "fundamental assumptions" in some non-declarative form, such as "rules" or "resolutions", etc., in defending the view that "fundamental assumptions" are neither true nor false and cannot be disconfirmed. Instead, he implicitly consents to regard the postulates as assertions, bur argues (1) that they predicate only "about ideal constructs"; and (2) that "they cannot be 'falsified' by observed facts … because auxiliary assumptions can be brought in to establish correspondence with almost any kind of facts". These remarks lack cogency. First, according to general understanding in logic, assertions are either "true" or "false" whether they predicate about ideal constructs of anything else (cf. Kaufmann (1944), p. 87). Second, to import new "auxiliary assumptions" in the face of contradictory evidence does not avert the possibility of disconfirmation, but merely revises the original assertion. Upon complete formulation, the "auxiliary assumptions" of every statement are implicit. The only reasonable ground for arguing that "fundamental assumptions" cannot be disconfirmed and are neither true nor false, in my opinion, is that they possess some non-declarative form. This is the sole inferable ground for the argument in Machlup's original article.

[59] Kaufmann base his designation of certain empirical propositions as rules exclusively on their actual treatment in the relevant disciplines. While raising the question of the best method of determining the reliability of these particular "rules", he offers no answer (see Kaufmann (1944), pp. 85-87, 233). It is also noteworthy that much of Kaufmann's emphasis is on different kinds of "rules".

correspondence between "generative" assumptions and facts alter the probability of true predictions, and thus modifies the "reliability", "usefulness", etc., of those assumptions, or in terms of the present convention, "rules".

IV – Conclusion

Having argued extensively the significance of tests of economic assumptions, it should be emphasized one last time in closing that there has been no effort in this paper to promote the idea that true assumptions are absolutely essential in scientific work. Indeed, since a single false implication suffices to render an entire statement false, it would be genuinely astounding if any moderately important true scientific hypothesis were in stock. The central argument in these pages has been that every inaccuracy in assumptions is disadvantageous. The essential basis for this conclusion is that every falsehood in assumptions either hinders testing or potentially gives rise to false hypotheses and predictions. This conclusion, which is not new in economics, as noted in the introduction, currently requires stress due to the serious challenge of Friedman and Machlup.

The argument in this paper does not imply, it is important to add, that economic theory suffers grave damage each time a negative result of a test of an economic assumption takes place. The actual disconfirmatory significance of any negative test finding always depends largely on the individual circumstances involved; in particular, the character of the experiment, the quality of the execution, and the nature of the results. Thus, the outcome of former tests of the economic postulates may be barely detrimental. But even if this were so, future tests of these assumptions might prove very upsetting. In general, the potential impact of a disconfirmatory test result corresponds closely to the intensity of current reliance on the statement in question. On this basis, and in order to highlight the significance of the preceding argument, I may venture the opinion with regard to economic that there are certain conceivable outcomes of tests of profit maximization which, if they actually took place, would prove devastating.

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