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COMMUNICATIONS

PROFESSOR MACHLUP ON VERIFICATION IN ECONOMICS

According to Professor Machlup it is significant to distinguish two schools of thought on the subject of verification in economics, which he describes as the "A Priori" and the "Ultra-Empiricist." Of the "Ultra-Empiricist" he writes: "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." (Italics added.)[1] In fact Ultra-empiricists "refuse to recognise the legitimacy of employing at any level of analysis propositions not independently verifiable" (p. 7).

Professor Machlup claims that he could give "dozens" of examples of the "Ultra-Empiricist" position. The one he chooses to cite is that outlined in my book *The Significance and Basic Postulates of Economic Theory (1938)*. He makes it clear that he is not concerned so much with extracting a single statement or two which is at fault, but with the position represented throughout the book—("I have selected Hutchison" Professor Machlup writes).

I find I wrote (*op. cit.*, p. 9): "If the finished propositions of a science, as against the accessory purely logical or mathematical propositions used in many sciences, including Economics, are to have any empirical content, as the finished propositions of all sciences except of Logic and Mathematics obviously must have, (6) then these propositions must *conceivably* be capable of empirical testing *or be reducible to such propositions* by logical or mathematical deduction. They need not, that is, actually be tested or even be *practically* capable of testing under present or future technical conditions or conditions of statistical investigation, nor is there any sense in talking of some kind of "absolute" test which will "finally" decide whether a proposition is "absolutely" true or false. But it must be possible to indicate intersubjectively what is the case if they are true or false; their truth or falsity, that is, must make some conceivable empirically noticeable difference, or some such difference must be directly deducible therefrom." (Italics as in original.) The note (6) attached to this passage ran: "This seems to us obvious. But the contrary view that Economics is, or ought to be, not an empirical science at all but a formal science just like Mathematics and Logic is (1937) held by a number of authorities led by Professor L. von Mises. Cf. *Gründprobleme der Nationalökonomie* and his lecture in *Actes du Congrès Internationale de Philosophie*, Paris, 1937. In future references we may, for reasons of brevity, omit this obvious qualification to the Principle of Testability: that a scientific proposition may not itself be empirically testable *directly*, but may be reducible by direct deduction

[1] Cf. F. Machlup, "The Problem of Verification in Economics," *Southern Economic Journal*, July 1955, p. 8.

to an empirically testable proposition or propositions (cf. propositions of Physics about electrons, α and β particles, etc.)."

This was the first and only relatively full account of the position on verification which I tried to expound in my book. This passage now seems to me rather old-fashioned, and even slightly crude and ungrammatical in the way it is formulated. But one thing it indubitably is *not*, and that is an example of what Professor Machlup calls "Ultra-empiricism." In fact it explicitly denies what he describes as "the essence of the ultra-empiricist position on verification."

Fortunately I do not have to rely on my own interpretation of my writings of eighteen years ago, if any interpreting is necessary. In his work *Economic Theory and Method* (recently published in a new English edition), Professor F. Zeuthen makes it clear that he is quoting me in diametrically the opposite sense to Professor Machlup, (and I venture to assume that Professor Zeuthen would not have chosen to quote me in that sense, or any other, if there had seemed to him to be any question of the direction my argument was taking). Professor Zeuthen writes: "If statements about reality are to have a meaning, and if they are not direct statements as to individual observations, it must be possible, by means of logical transformations to translate them at least into possible observations. There must be a possibility of verifying their reality or the reality of their consequences. In a rationalized theory, as, for instance, in micro-physics, it is still not considered necessary to be able to translate each individual statement into the language of reality, if only verification of a certain complex of statements is possible. In this connection we may also quote Paul Samuelson: 'By a meaningful theorem I mean simply a hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions.' ... Direct or indirect measurability (or the possibility of other factual testing) is a necessary condition for the avoidance of mystery, where everyone may have his own ideas as to the same words. Scientific statements about reality must be verifiable by others. As Hutchison says, they must 'conceivably be capable of empirical testing or be reducible to such propositions by logical or mathematical deductions. If there is no conceivable possibility of proving if an assertion is right, it is of a mystical character' " (*op. cit.*, pp. 8–9).

I am afraid it seems to me that—doubtless through my own fault—Professor Machlup completely failed to understand the position I was trying to outline, particularly since not a single one of the very brief passages of mine which he quoted seems to me, when taken in context, to make the point which Professor Machlup seems to imagine it makes.[2] Professor Zeuthen may be easier to follow,

[2] I don't wish to claim any particular wisdom or rectitude for my propositions, only that Professor Machlup has not interpreted accurately what he very briefly quotes. For example (a) Professor Machlup quotes me as writing "that propositions of pure theory, by themselves, have no prognostic value," and states that this proposition, "as it stands," is "unassailable." However, in his determination to assail the unassailable, Professor Machlup proceeds to interpret "propositions of pure theory, by themselves, have no prognostic value" as meaning "an attack against the use of empirically unverifiable propositions in economic theory regardless of their conjunction with other propositions." (b) Professor Machlup writes, "With regard to the 'fundamental assumption' of economic theory con-

and it might help to elucidate Professor Machlup's categories if he could explain whether Professor Zeuthen fell into the category of "Ultra-Empiricists," or that of "A Priorists"; or indeed how the other important contributors in the last decade, to the methodology of economics, such as Samuelson, Lange, Little and Friedman are to be placed in relation to these categories.[3]

While the trouble with Professor Machlup's "Ultra-Empiricist" category simply seems to be that the one example he gives falls quite obviously outside it, the trouble with his "A-Priorist" category seems to be that it is much too elastic and comprehensive to be significant, while at least one or two of the various authorities Professor Machlup describes as "A-Priorists" might well have much preferred to be called "empiricists," if they were to be called anything. Professor Machlup agrees that his term covers writers of very different epistemological views, ranging from J. S. Mill to Mises. After telling us (p. 5) that he is simply concerned with two "extreme positions" Professor Machlup proceeds, while indeed defining "Ultra-Empiricism" in extreme terms, to leave "A-Priorism" very elastic. In fact it is very hard to tell whether his two categories are meant to describe two extremes, with a large third middle ground in between; or whether "A-Priorism" is being so stretched as to include all the middle ground up to the frontier line of "Ultra-Empiricism," the former comprising all those who are prepared to recognize "indirect" methods of verification or confirmation, and the latter those who explicitly reject indirect verification and insist on "direct" in-

cerning 'subjectively rational' and maximising behaviour, Hutchison states that 'the empirical content of the assumption and all the conclusions will be the same—that is nothing.'" Here I would simply like to quote my complete sentence which was concerned with Professor Mises' apparently circular method of formulating the fundamental assumption (not with other methods): "If one thinks it worth while, one can say 'people behave as they do behave' in as many different ways as one likes, but one will not learn anything further about their behaviour; for the empirical content of the assumption and all the conclusions will be the same—that is nothing."

[3] Cf. the following passage from Prof. Zeuthen's chapter on Material and Method in Economics, *op. cit.*, pp. 14–15: "How the conception of economics as an empirical, i.e., a logical-empirical science is compatible with a considerable amount of deduction and theorizing will be apparent from the following statement by O. Lange: 'Theoretical economics puts the pattern of uniformity in a coherent system. This is done by presenting the laws of economics as a deductive set of propositions derived by the rules of logic (and of mathematics) from a few basic propositions. The basic propositions are called assumptions or postulates, the derived propositions are called theorems. Theoretical economics thus appears (like all other theoretical sciences) as a deductive science. This, however, does not make it a branch of pure mathematics or logic. Like the rest of economics, economic theory is an empirical science. *Its assumptions or postulates are approximative generalizations of empirical observations; e.g., the assumption that business enterprises act so as to maximise their money profit.* Some inaccuracy of approximation (e.g., some considerations, like safety, may keep enterprises from maximizing money profit) is accepted for the sake of greater simplicity. The theorems, in turn, are subjected to test by empirical observation. A deductive set of theorems to be subjected to empirical test is also called a theory, hypothesis, or a model. We can thus say that theoretical economics provides hypotheses or models *based on generalizations of observations and subject to empirical test.* Since the assumptions (postulates) underlying a model are only approximative, the theorems do not correspond directly to results of empirical observations.' " (Italics added.)

dependent verification or confirmation only (assuming Professor Machlup can give an example of this category).[4]

However, it seems doubtful whether any distinction which is made to turn on whether or not "indirect" verification or testing is accepted, could be at all serviceable—even if it were more lucidly defined, less questionably labelled, and less erroneously exemplified. Supposing (A) I have tested and confirmed (1) that a plot of ground forms a right-angled triangle, and (2) that the two shorter sides are 30 and 40 yards long; and supposing (B) that I have checked my calculation or deduction via the Pythagoras theorem that the longest side is 50 yards long. Professor Machlup apparently insists that there are "dozens" of economists who would *deny* that the performance of these measurements and tests, as to the two shorter sides and the right angle, entitled me to regard as to that extent tested and verified the proposition (C) that the third side was 50 yards long? These "dozens" of "Ultra-Empiricist" economists (whose existence I beg leave to doubt) would continue to regard proposition (C) as a completely unconfirmed piece of speculative guess-work until I had tested or confirmed it "directly" and "independently" by separately measuring the 50-yard side (which might conceivably for technical reasons be very difficult or practically impossible).

Anyhow, it would not seem to be committing some incredibly naive and dangerous methodological error if I *did* attempt to test (C) directly and independently by a separate measurement, provided this was technically or practically possible. Whether (C) was tested directly or indirectly would be a matter of practical convenience and of the degree of confirmation aimed at. It is not clear how any serious controversial point can arise here, or how in such a case any very interesting distinction can be made to turn on whether or not the "indirect" testing of (C) is acceptable or not.

So much for the critical-historical elements in Professor Machlup's paper. Perhaps, now I have started, I may go on to express one or two doubts about his more positive thesis. The point at issue,—and there is a point at issue,—lies rather in Professor Machlup's conception of "fundamental assumptions" or "high-level generalizations" *in economics.* The only example he gives of this special type of proposition is "the fundamental assumption" 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 with equal risk" (pp. 10–11). These are all variations on the ubiquitous assump-

[4] One function of this elastic category "A Priorist," which is first described as "extreme" but which is then stretched to include J. S. Mill, seems to be to cast an aura of respectable moderation on the certainly highly "extreme" political and methodological dogmatizing of Professor L. Mises. There have been previous examples in the last decade or so of associates or disciple of Professor Mises volunteering such explanations as that when Mises said "impossible" he really meant "possible," or when he said "a priori" he really meant "empirical." Now, according to Professor Machlup, when Professor Mises held that in economics "the fundamental postulates are *a priori* truths, necessities of thinking" (p. 6) "*all*" he "had in mind however provocative (his) contentions seemed" as an "objection . . . to verifying the basic assumptions in isolation."

tion, the central assumption in 'micro-economic' analysis, of 'maximising' or "rational" action. It might be helpful to know whether Professor Machlup can cite any other examples of a "fundamental assumption" in economics beyond this one and its variants. If so, the point at issue might well be illuminated, while if not, it would stand out in a pretty clearly defined way as turning on the status and nature of this proposition about "maximising" and/or "rational" conduct.[5] We should like to note here that Professor Machlup describes this "fundamental assumption" as "empirically meaningful," which would appear to mean "conceivably falsifiable empirically"; or, at any rate, Professor Machlup does not interpret this fundamental assumption as a more or less disguised definition, without empirical content, that is, as simply saying that people maximise what they maximise, or that economic conduct must, by definition, be rational—(as Professor Mises appears to hold).

Now the main difficulty with this fundamental assumption, throughout its history,—since, roughly speaking, Bentham,—has been that of knowing just what content, if any, it has been meant to possess, just when, where, and how far it is applicable, and therefore just what the significance may be of the conclusions about human activities which can logically be deduced from it. At one time this fundamental assumption was formulated to the effect that the consumer "maximised his satisfaction" or "utility," the firm its "profits," or even that society, in certain conditions, maximised its aggregate "social satisfaction" or "utility" or "welfare." What was necessary, in the first instance, with such formulations of this fundamental assumption, was more *clarity* rather than more confirmation or verification, that is, not any actual testing so much as a specification of what a test would amount to, or of the more precise circumstances under which the generalisation was to be regarded as "confirmed" or "disconfirmed."

Professor Machlup goes on to describe this fundamental assumption of "maximising" or "rational" action, and its variants, as "assumptions which, though empirically meaningful, require no independent empirical tests but may be significant steps in arguments reaching conclusions which are empirically testable."

[5] Professor M. Friedman (*Essays in Positive Economics*, p. 16n) commenting on Professor Machlup's presentation of the marginal productivity doctrine (*American Economic Review*, Sept. 1946, pp. 519–54) notes that "in Machlup's emphasis on the logical structure, he comes perilously close to presenting the theory as a pure tautology, though it is evident at a number of points that he is aware of this danger and anxious to avoid it." I must say that Professor Machlup's "anxiety" on this point might well have seemed more pressing both in 1946 and in 1955. Anyone who was "anxious" could easily set about relieving his anxiety by giving an outline specification of the empirical content of the maximisation-of-returns hypothesis for the case in which he was concerned, that is, by indicating the conditions by which the hypothesis could be tested in a particular individual case. Professor Mises, of course, is not in the least "anxious" on this score: quite the reverse, he repudiates all anxiety by claiming that all economic action is "rational"—by definition presumably—and Professor Machlup seems at times most anxious to defend Professor Mises' position. We would note, in addition, that the point of view we are advocating is summed up very succinctly by Professor Friedman as follows (*op. cit.*, p. 41): "It is necessary to be more specific about the content of existing economic theory and to distinguish among its different branches."

It can certainly be agreed that actual independent tests may not be "*required*." But if one claims that a proposition is "empirically meaningful," or a "significant step," one *is* "required" to indicate where that significance begins and ends, what "work," if any, the proposition can and does do, and just why it is not a superfluous fifth wheel on the car—(as any such proposition as "all economic action, being rational, maximises whatever it maximises" certainly is).[6]

Of course it does not matter in principle whether the specification of the conditions of a test of this fundamental assumption is obtained "directly" and "independently," or by working back "indirectly" from the specified tests of the conclusions to the assumption from which the conclusions are deduced. According to Professor Machlup these conclusions *are* "empirically testable," that is, reasonably specific descriptions are available of what constitutes a test of them. What he does not show is how "empirically testable" conclusions about human actions can be deduced with logical inevitability from "empirically meaningful" assumptions about human actions, while these assumptions are to be regarded by themselves as either not conceivably, or not possibly, or not practically, or only "gratuitously" and "misleadingly," testable—(which of these adverbs he he really means, Professor Machlup never quite makes clear). In fact, what exactly is the contrast that Professor Machlup seems to be implying between "empirically meaningful" and "empirically testable," with regard to propositions about economic actions? At this point Professor Machlup rides off on analogies from physical theories without demonstrating that there is any relevant analogy in economic theory. In the social sciences there are, of course, considerable difficulties all along the line in testing *any* proposition. Professor Machlup does nothing to show that it is in any respect more difficult to confirm or "disconfirm" assumptions, "fundamental" or otherwise, about human actions in economic theorising, than it is to confirm or "disconfirm" the conclusions about human actions. The comparatively simple maximising theories of human action in micro-economics cannot in this respect relevantly be compared with the theories of physics. Methodological generalisations and analogies from physics are liable to be of rather limited significance in the interpretation and elucidation of specific

[6] Cf. the chapter 'The Analysis of Consumers' Behaviour' in *Welfare Economics*, by I. M. D. Little (p. 14 ff.). On the maximisation hypothesis as applied to the consumer, Mr. Little writes (pp. 20–21): "Where the chief difficulty lies is in the interpretation of the axiom 'the individual maximises utility'. . . . In the past economists have often been attacked on the grounds that their theories only applied to selfish people; such attacks were brushed aside as absurd. But they were not absurd. It was the economists who were wrong in suggesting that positive economics had any necessary connexion with satisfactions at all. Nor could the economist argue that he had some positive objective tests which showed him to whom the theory applied, and to whom it did not apply, and that it didn't matter whether it was really a test of satisfaction or not. He could not make this reply because no such test had been suggested. . . . One economist has tried to get over this difficulty by saying that it does not matter what a man tries to maximise, so long as he tries to maximise something, say his weight or his misery. But this amounts to a determination to say that whenever the economist *can* explain a man's behaviour then that man must be maximising *something*. It gives no indication whatever as to when the theory can be applied and when not."

economic theories and propositions.[7] In short, while admitting the principle of indirect verification, we cannot agree to the kind of loose and sweeping appeal to it which Professor Machlup seems to be making. Much more particularity and precision seems to be desirable.

Let us take the example where economists have for decades tried to draw the most sweeping and consequential practical conclusions from theories built round the fundamental assumption of maximizing or rational actions, that is, the theory of welfare economics and of consumers' behaviors. When we take a conclusion such as that of Walras (and many others) that "free competition procures within certain limits the maximum of utility for society," exactly the reverse procedure to that claimed as essential by Professor Machlup seems to be "required." It hardly seems very promising when confronted with such a "conclusion" to try to test it "directly." On the contrary, exactly reversing the process insisted upon by Professor Machlup, one must work back from the "conclusion" to the assumptions, and in particular the "fundamental" assumption about the individual consumer and producer, and enquire what would constitute a test of this fundamental assumption.

Again, Professor Machlup mentions, as a variant of the fundamental assumption, that "consumers can arrange their preferences in an order." How was this formulation arrived at and how did it come to replace for most economists (including, apparently, Professor Machlup) the earlier formulations in terms of "maximizing utility"? Simply thanks to the increasingly rigorous insistence by a long line of economists—(Fisher, Pareto, Slutsky, Hicks and Allen, Samuelson, and Little)—that the fundamental assumption of the theory of consumers' behaviour be testable.

When, on the other hand, Professor Machlup formulates the fundamental assumption to the effect that "people act rationally," it is not in the least clear what would constitute a test of this assumption and whether even it is testable. Not knowing how it can be tested, one cannot tell at all precisely what can be deduced from it. Nevertheless, brandishing this generalisation that all economic

[7] Cf. Little (*op. cit.*, pp. 2–4) on welfare economics: "In contrast to the undoubted validity of the formal deduction, what are called the foundations of the theory have always been shrouded in darkness. What are the foundations of a theory? The answer is, those postulates from which the theorems are deduced." In physics "it does not really matter in the least whether one believes that such words as 'electrons' and 'molecule' stand for entities of a peculiar kind, or whether one believes that they are merely words which serve a useful practical purpose. . . . But drawing analogies between physics and other studies can result in harm. . . . The analogy with physics breaks down in two important ways, which should lead one to suspect that what holds for one may not hold for the other. First, the concepts of physics about which people are not clear, do not appear in the conclusions. The conclusions are about macroscopic or microscopic objects, not about electrons. By contrast, in welfare economics, the conclusions are about welfare. Secondly, physicists' conclusions are verified or falsified; ours are not. . . . I do suggest that the reality of the theory (of welfare economics) has been badly overestimated by economists." These arguments apply to a lesser but none the less a very important extent—to the "maximising of utility" and the theory of the consumer, and even to the "maximising of profits" and the theory of the firm.

action was (or even must be) "rational" some economists—notably Professor Mises, whom Professor Machlup seems so concerned to defend—have proceeded to claim that wholesale political conclusions were logically deducible from it, and were thus to be regarded as established conclusions of economic science.[8] It is not difficult to understand why those wishing to propagate sweeping political dogmas as the established logical conclusions of scientific economic theory, should resist the claim that some procedure for testing should be described for these conclusions, and/or for the assumptions, including the fundamental assumption, from which they were deduced. I am afraid that Professor Machlup's doctrines on verification and verifiability in economics are not merely questionable in themselves as an account of the structure of micro-economic theory, but may be used in defence of a kind of politico-intellectual obscurantism that seeks to avoid not merely the empirical testing of its dogmas, but even the specification of what would constitute tests.

London School of Economics T. W. Hutchison

REJOINDER TO A RELUCTANT ULTRA-EMPIRICIST

From the tone of Professor Hutchison's reply to my article I infer that he was hurt by my characterization of his position as one of ultra-empiricism. I am sorry that I hurt or angered him; I am glad that he rejects, at least on principle, the position which I called ultra-empiricism; and I am puzzled by many of his comments which still strike me as ultra-empiricist.

I agree fully with Professor Hutchison that his opening statement—on page 9 of his book and on page 476 of his note above—is a rejection of what I call ultra-empiricism. Whereas ultra-empiricists require direct empirical testing of propositions used as fundamental assumptions in a theoretical system, Professor Hutchison in this declaration seems satisfied with the *conceivable* testability of

[8] Cf. *Kritik des Interventionismus*, pp. 23–24, and *Liberalismus*, pp. 3, 78 and 170: "Liberalism is the application of the doctrines of science to the social life of men. . . . Liberalism and Political Economy were victorious together. No other politico-economic ideology can in any way be reconciled with the science of Catallactics. . . . One cannot understand Liberalism without Political Economy. For Liberalism is applied Political Economy, it is state and social policy on a scientific basis. . . . Liberalism starts from the pure sciences of Political Economy and Sociology which within their systems make no valuations and which say nothing about what ought to be or what is good or bad, but only ascertain what is an how it is. If this science shows that of all conceivable possible organisations of society only one, that resting on private property in the means of production, is capable of existing, because none of the others can be carried through, there is nothing in this which justifies the term optimism. . . . He who recommends a third type of social order of regulated private property, can only deny altogether the possibility of scientific knowledge in the field of Economics." Cf. also W. H. Hutt, *Economists and the Public*, p. 367: "Our plea is in short for that economic liberty which was dimly visualised by the Classical economists, and whose coincidence with the *summum bonum* has been an implication of the subsequent teachings of economic orthodoxy. We have attempted to show that expert, dispassionate and disinterested thought on these matters has been the preserve of those whose gropings in a world of divergent beliefs and arguments (beset on all sides by the lure of interests) have led them to the path of orthodox tradition."

the deduced *consequences* of these propositions. I might have quoted his statement on this point in support of my position that direct testing is not required—had he not in effect repudiated it by much of what followed it in his book. And he does it again in his note, as I shall attempt to show presently.

But do we really mean the same thing when we speak of "indirect testing"? Perhaps the crucial misunderstanding lies right here. Professor Hutchison mentions that a proposition not itself empirically testable directly must be "reducible by *direct* deduction to an empirically testable proposition or propositions." (Emphasis supplied.) This formulation suggests a requirement that the implications of any single proposition be tested independently of those of other propositions with which it is conjoined to constitute a "case." In fact, however, the *conjunction of logically independent propositions and derivation of their joint consequences* is the essence of indirect testing.

If assumption A can neither be subjected to any direct empirical test nor reduced "by direct deduction to an empirically testable proposition," its indirect verification can be accomplished by combining it with an assumption B which is directly testable; if a consequence C can be deduced from the conjunctive hypothesis A *plus* B—but not from either one alone—and if C is empirically tested, A is regarded as having passed the indirect test.

I suspect that Professor Hutchison does not accept the validity of indirect verification in this sense. Unfortunately, he makes no reference to my detailed exposition and schematic representation of the conception and operation of indirect testing. Silent on this, he professes to accept indirect testing and then proceeds to demand direct (independent) tests. I had pointed out that fundamental postulates, such as the maximization principle, are "not subject to a requirement of independent verification"; they are considered as verified, together with the whole theory of which they are a part, when the deduced consequences of their conjunction with an evident and substantive change and with assumed conditions relevant to the case are shown to correspond to observed events. Thus, if the fundamental postulate (e.g., that firms prefer more profit to less profit at equal risks[1]) is combined with assumptions about economic institutions and conditions (e.g., certain forms of competition) and with assumptions about certain substantive changes (e.g., the imposition of import quotas on certain products); and if we deduce from this conjunction of assumptions certain consequences (e.g., increases in the excess of domestic over foreign prices); and if these deduced consequences are found to be in relatively good correspondence with observed events (e.g., increases in the excess of domestic over foreign prices of bicycles) subsequent to actual changes of the kind in question (e.g., the imposition of import quotas on bicycles); then the theory is regarded as verified, and the fundamental postulate is regarded as verified with it.

Now, those who do not accept this "indirect verification" of the fundamental postulate but demand that the assumption of attempted profit maximization be

[1] On the problem of differences of risk and uncertainty in connection with differences in profits see my book *The Economics of Sellers' Competition* (Baltimore: Johns Hopkins Press, 1952), pp. 53-56.

empirically tested independently of the other propositions (about competition, import quotas, and bicycle prices) are the "ultra-empiricists" discussed in my article. If he understands this, I wonder whether Professor Hutchison will still deny membership in the society of ultra-empiricists or whether instead he will be eager to confirm it.

That Professor Hutchison misunderstands the essence of indirect verification is suggested by his example about the length of the unmeasured side of a triangular plot of land. If he had referred to the Pythogoras theorem as the general proposition in need of verification and to the lengths of the sides of his plot as the independently verifiable propositions, he might have come nearer to our problem, the validation of the use of universal propositions. What he really showed was (a) that he had confidence in the reliability of the Pythagoras theorem, (b) that he was sure his plot was reasonably close to a perfect right-angled triangle, and (c) that his measurements of the two short sides were reasonably accurate. The whole example has little to do with the question of the direct or indirect verification of fundamental assumptions employed in general theory.

That Professor Hutchison is not satisfied with the indirect verification of such universal propositions employed as fundamental postulates in general theory can be seen from several comments. For example, he contends (p. 478) that I have failed to state whether they are "conceivably falsifiable empirically" or rather definitions "without empirical content." (I had said they were "heuristic principles," "procedural rules," etc. See pp. 9 and 16). Then he demands (p. 478) "a specification of what a test would amount to, or of the more precise circumstances under which the generalization [of maximizing conduct] was to be regarded as 'confirmed' or 'disconfirmed.' " (I had stated repeatedly that the test consisted in checking the correspondence of observed events with the "assumed changes" and the "deduced changes" of the entire theoretical model. See especially p. 18.) It is quite obvious that Professor Hutchison, contrary to his initial declaration, wants more than indirect testing of the fundamental postulates of general theory.

Professor Hutchison asks whether my category of apriorism in economics is "so stretched to include all the middle ground up to the frontier line of 'ultra-Empiricism.' " (Since he also questions that I could name any "example of this category," he must believe that on my classification *all* economists are apriorists!) The answer is that I know very few "extreme apriorists" (e.g., Professor von Mises). The middle ground between the extreme positions is very large indeed; of the economists whom Professor Hutchison asked me to classify, it includes Zeuthen, Samuelson, Lange, and Friedman; none of them holds that no conceivable kind of experience could ever cause him to give up his theory, and none of them wants his fundamental assumptions empirically tested independently of the propositions with which they are combined when the theory is applied.

Professor Hutchison asks whether I can cite any other fundamental assumption in economics "beyond" that of "maximizing or rational action." It all depends on what one regards as fundamental. Perhaps the assumption that only limited outputs can be obtained from given resources should be called fundamental; it

"underlies" all economic problems, but it does not always become a relevant step in the argument. Perhaps still other (or narrower) assumptions should be proposed for inclusion, though frankly I had not intended it.

If the question referred to the possible replacement of, rather than addition to, the assumption of maximizing conduct, my answer would be that substitutes have been proposed, but not successfully. Some writers on the equilibrium of the firm (theory of output and price) have advanced "security of survival" and similar postulates in lieu of profit maximization (for the enterprise economy), but the proposed substitutes were less simple and less comprehensive. Yet, I grant the possibility that better postulates may be proposed, and therefore I have described the "Fundamental Postulates" as "Assumed Type of Action (or Motivation)" instead of limiting them to that of "maximizing conduct."[2]

In his comments 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 prediction of observable phenomena.

Such propositions are neither "true or false" nor empirically meaningless. They cannot be false because what they predicate is predicated about ideal constructs, not about things or events of reality. Yet they are not empirically "meaningless," because they are supposed to "apply" or correspond broadly to experienced events. They cannot be "falsified" by observed facts, or even be "proved inapplicable," because auxiliary assumptions can be brought in to establish correspondence with almost any kind of facts; but they can be superseded by other propositions which are in better agreement with these facts without recourse to so many auxiliary assumptions.

Logicians have long recognized this intermediate category of propositions, which are neither *a priori* nor *a posteriori* in the strict sense of these terms.[3] (One may, with Friedman, prefer to say that a theoretical system has two parts, an analytical one demonstrating valid inferences, and a synthetic one stating correct applications.[4]) I had mentioned this category of propositions in my article (p. 16), but Professor Hutchison chose to disregard my remarks on this issue.

It was necessary to bring this up again because Professor Hutchison said

[2] The assumption of maximizing conduct of the householder may, of course, be broken down into several parts—that each person has preferences, that these preferences are consistent (transitive) and can be orderly arranged, that he wishes to follow these preferences in deciding on his actions, etc.—and it is possible to call each of these a separate postulate. This, I suppose, is not questioned here.

[3] They were called "procedural rules" by Felix Kaufmann, "complex-analytic propositions" by Wm. P. Montague, "constitutive, non-epistemic" propositions by Henry Margenau.

[4] Milton Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 24–25.

(p. 478) that if I called the fundamental assumption (of maximizing behavior) "empirically meaningful" I should mean it to be "conceivably falsifiable empirically." I do not. Resolutions to analyse certain aspects of experience with the aid of a heuristic postulate, or even of a pure fiction, are not "falsifiable" but nevertheless "empirically meaningful."[5]

At another point (pp. 478–479) Professor Hutchison realizes that I did not mean that the fundamental assumptions about human actions should or could be empirically tested, and he asks me to show "how 'empirically testable' conclusions about human actions can be deduced" from those untested or untestable fundamental assumptions. I thought I had shown it with sufficient clarity; of course, the conclusions are deduced not from the fundamental assumptions in isolation but from their conjunction with other assumptions including some whose correspondence with factual observation is established.

I can easily comply with Professor Hutchison's request by pointing to the illustration I gave above, where I showed how a relative price increase for bicycles was the empirically testable consequence deduced from the partly untested or untestable assumptions. But Professor Hutchison repeats that I had done "nothing to show that it is in any respect more difficult to confirm or 'disconfirm' assumptions, 'fundamental' or otherwise, about human actions in economic theory, than it is to confirm or 'disconfirm' the conclusions about human action." Can there be any doubt that a direct empirical test of the motivations behind businessmen's actions, such as a test whether their decisions are made in an attempt to maximize profits, would be "more difficult," to say the least, than a test that higher prices are paid for bicycles?

Perhaps it was confusing when, in addition to stating that these fundamental assumptions *need not* be independently verified empirically, I also indicated that they *cannot* be so verified. Some economists who agree that no independent verification is required would none the less hold that such verification is *possible;* and others would contend that any special tests are *unnecessary* because the assumptions are *self-evident* statements of common experience. Common experience, however, tells us merely that we (that is, I and those with whom I have talked about it) *can* follow our preferences in choosing among the alternatives open to us and that we usually do it. Common experience, moreover, tells those of us who are or were in business that we *usually* attempt to make such decisions as would promise us the highest returns, but it does not tell us that *all* businessmen do so in *all* their actions, Indeed we know, also from common experience, that there are times when many businessmen refrain from following the most profitable courses of action and instead act to meet some demands of "patriotism" or to obey the moral suasion of governmental authorities. Are there any objective tests

[5] Some may wonder how one may possibly interpret the "fundamental assumptions" alternatively as rules of procedure (imperative statements), definitions (resolutions), useful fictions, and "true" empirical propositions. The answer lies in the convertibility of propositions. The following formulation may suggest how it can be done: "In analysing problems of this sort *let us proceed* by assuming that things will work *as if* businessmen were always attempting to maximize their money profits (and perhaps they actually do!)".

possible by which the assumption of profit maximization could be verified independently of the uses to which the assumption is put in economic theory?

We could *conceivably* place researchers into every business office to analyse every decision that is made and check the motivations behind it. This would not be quite reliable unless our researchers were invisible, had invisible lie detectors or perhaps mind-reading apparatus. In case we are satisfied with what is *practically possible*, we could have exceptionally competent and skillful survey researchers examine in carefully devised interviews a sample of the decisions made by a sample of businessmen. The object would be to establish the relative frequency of decisions consistent with profit maximization: In what percentage of their decisions do businessmen believe that they are acting in the best (long-term) interest of their firm (that is, of its owners)? Surely, some businessmen do so some of the time; probably, most businessmen do so most of the time. But we would certainly not find that all of the businessmen do so all of the time. Hence, the assumption of consistently profit-maximizing conduct is contrary to fact.

Of course, no proposition about empirical facts can be absolutely certain; but here we are defending an assumption of which we are certain that it does not always conform to the facts. If the deviations are insignificant we can safely neglect them. But we do not know *how* significant they might be, especially because the relative strength of non-profit objectives changes with the conditions of the time, changes probably also with the kind of decisions, and changes perhaps also with several other factors. What then should be done? Just what is being done: to accept maximizing conduct as a heuristic postulate and to bear in mind that the deduced consequences may sometimes be considerably out of line with observed data. We can, to repeat, test empirically whether the outcome of people's actions is most of the time reasonably close to what one would expect *if* people always acted as they are unrealistically assumed to act. Again, the "indirect verification" or justification of the postulate lies in the fact that it gives fairly good results in many applications of the theory.

Professor Hutchison has several questions concerning the assumption of maximizing conduct; we shall call it for short the Assumption (with capital A). He asks (a) "just what content, if any, it has been meant to possess," (b) "just when, where, and how far it is applicable," (c) "what a test [of it] would amount to," (d) under what circumstances it "was to be regarded as 'confirmed' or 'disconfirmed.' " And he finds that I am "required" to indicate (e) the range of the "significance" of the Assumption, (f) "what 'work,' if any," it can do, and (g) "just why it is not a superfluous fifth wheel on the car." I shall attempt brief answers to all seven questions.

(a) I am not sure what sort of "content" it is that is in question. Does "content" refer to specific data of experience that have gone "into" the Assumption and are now an integral part of it, as in the case of a universal proposition whose subject can be defined by complete enumeration? In this sense the Assumption has no determinate "content." Or, rather, is the question whether the Assumption is to apply to empirical data of a certain class, and whether it would matter if it did or did not apply? In this sense the "content" of the assumption of profit maximization can readily be illustrated. Suppose (1) the government announces that price reductions would be in the national interest, (2) wage rates have just been raised, (3) raw-material prices have gone up, (4) no changes in technology have occurred for many years, and (5) aggregate demand has not changed. Should we expect product prices to rise or to fall? If firms did not attempt to maximize profits, they might well act in accordance with what the government publicizes to be in the national interest, and prices would be reduced. The Assumption does make a difference.

(b) The applicability—"when, where and how far"—of the Assumption, or rather of theories based on it, can be "prescribed" in broadly formulated directives, but there will always be a wide margin for the use of good judgment. The "directions for use" may be different for explanations of past events and for predictions of future events. In general, for purposes of prediction, we should *not* apply the Assumption to particular households or to particular firms, but only to large numbers of households or firms, or rather to cases where the deduced events, such as changes in prices, outputs, consumption, exports, imports, etc., are regarded as the outcome of actions and interactions of large numbers of firms and households. We should apply it only with reservations in times when strong moral suasion is exerted to make people disregard their usual preferences or interests, such as in war time when patriotic objectives are strongly pressed.

(c) Our discussion of the "kind of test" to which the Assumption should be subjected has probably been sufficient to warrant our conclusion that the test of the pudding lies in the eating and not in its ingredients. If we find no better theory to explain or predict changes in prices, outputs, etc., etc., and if our present theory does no worse than it has been doing, we may consider our Assumption as warranted.

(d) The Assumption will of course never be considered as "confirmed" for good, but only until further notice. Under what circumstances is the Assumption to be regarded as "disconfirmed"? When a theory not using this Assumption is proposed and is shown to work equally well for a wider range of problems, or with a smaller number of variables or provisos, or more reliably or more accurately for the same range of problems and with the same number of variables or qualifications—then the Assumption will have outlived its usefulness and will be sent to the limbo of "disconfirmed propositions." (And even this need not be beyond recall.)

(e) May I take the "range of the significance" to mean the same thing as the "when, where, and how far" of the applicability of the Assumption? If so, I may refer to what I said under (b). These answers, however, are strictly confined, as was my article, to positive economics, that is, to explanations and predictions of economic changes and events. Normative or evaluative economics has been outside the scope of my discussion; hence, I am not examining the significance of the Assumption for welfare economics. To give a simple example, we have been concerned with questions like "what consequences can be expected from the removal of a tariff," not with questions like "whether these consequences would be desirable" and "whether the tariff ought to be removed."

(f) The kind of "work" the Assumption does for us was indicated under (a), where its "content" was discussed.[6] Let me add two more illustrations, (A) from the theory of the household, and (B) from the theory of the firm and industry. (A) Suppose (1) the tastes for foodstuffs are given, (2) the substitutability between vinegar and lemon in salad dressings, the complementarity between salad dressings and salads, and the income elasticities of demand for both are all given with the tastes, (3) the prices of lettuce and other salads are reduced, (4) disposable incomes rise, and (5) the price of vinegar is increased. If we trust the Assumption we can predict increased consumption or increased prices of lemons (or longer queues if lemon prices are fixed, and more bootlegging if lemons are rationed). Without the Assumption we cannot say anything, for if people do not follow their preferences, act inconsistently and haphazardly, "given" scales of preference mean nothing. (B) Assume (1) the technological conditions of production are given, (2) entry into the textile industry is open, (3) the supply of productive services required for textiles is elastic, and (4) the demand for grey goods increases. On the basis of the Assumption we can explain or predict a larger output of grey goods; without the Assumption we cannot. If businessmen like smaller profits just as well as bigger profits, or even better, why should any manufacturer increase his output when demand increases? If businessmen are not tempted by opportunities to make more profit, why should anybody take up the production of grey goods? It is hard to understand how any doubt can be entertained as to "what work" the Assumption does for us.

(g) The question whether the Assumption is not really "superfluous" is, I believe, disposed of with our description of the "work" it does for us. To be sure, the same work might possibly be done by a different assumption—and we know that many versions of the fundamental "Type of Action" have been used over the years—but I doubt whether the difference can be very great. But while the Assumption might be replaced by an alternative, it cannot be eliminated without replacement; it is not a redundant part in the theory. It is perhaps possible to put an indefinite number of "behavior functions" in the place of our Assumption, with the stipulation that all consumers will consistently stick to these functions. Such a stipulation would be neither simpler nor more realistic than the Assumption; and since the required knowledge of all behavior functions would be a heavy burden for the theory of consumer behavior, this whole approach is distinctly inferior to the traditional theory. The latter has yielded a large number of generalizations as "deduced consequences" even without knowledge of the exact preference systems of consumers, merely on the basis of some very general properties of such preference systems. As for the theory of production in an enterprise economy, the Assumption appears to be indispensable. Never could a behavioristic approach provide all the millions of "entrepreneurial behavior functions" which would be needed to do the job that is now done by the simple postulate of profit maximization.

A few minor misunderstandings remain to be cleared up. The assumption

[6] I prefer to speak of "the work it does" rather than of "the content it has"; both are metaphors, to be sure, but the latter, I think, is quite infelicitous.

that "consumers can arrange their preferences in an order" is not, as Professor Hutchison believes (p. 479), a "variant" of the fundamental assumption, "replacing" earlier formulations in terms of "maximizing utility." Nor has it been proposed "thanks to the insistence" of Hicks and Allen, Samuelson, and Little to make the theory testable. Instead, the phrase was used by Robbins[7] and can be traced back to Čuhel[8] and the earlier Austrians; and it was proposed in order to spell out the logical prerequisites of maximizing utility.

In a footnote (p. 481) Professor Hutchison approvingly quotes I. M. D. Little concerning certain differences between physics and economics in the use of fundamental assumptions. One of the differences singled out for emphasis is supposed to be that the "concepts . . . about which people are not clear"—pure constructs, idealizations, and postulates—"do not appear in the conclusions" in physics, but do so in "welfare economics." I have not discussed welfare economics and do not intend to do so. But that the controversial, "untested" assumptions "do not appear in the conclusions" holds, as I have demonstrated, for positive economics no less than for physics.

In another footnote (p. 480) Professor Hutchison believes that he has found an ally in Professor Friedman, who held that I had come "perilously close" to a tautological formulation of the theory. But by pressing his demand for an independent empirical test of the profit maximization postulate Professor Hutchison has placed himself right in the center of the target of Friedman's attack. It was the main theme of Friedman's methodological essay that fundamental assumptions do their work even if they are contrary to fact, and that it is a mistake to attempt empirical tests for them besides those of the findings derived from the theory of which they are a part.

There is, furthermore, the charge of "tautology," which is implied in some of Professor Hutchison's strictures against my work and is made explicit in the quotation from Friedman. The judgment that a certain theory is "purely tautological" may mean rather different things: that the theory is underdetermined and can yield no specificable conclusions; that some of the important variables are unknowable or changing in undetermined ways; that the *ceteris paribus* clause is used without specifying the *cetera* or their significance for the outcome; that the deduced conclusions can never be tested against data of experience; that the theory constitutes an internally consistent and closed system; that some of the assumptions are "empirically empty." I shall comment here only on the last two meanings of the charge.

A fully developed theoretical system will always be "an internally consistent set of assumptions and definitions, such that each proposition is capable of being logically deduced from the assumptions and definitions (in the manner of a theorem)."[9] This was, and probably still is, recognized by Professor Hutchison,

[7] Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* (London: Macmillan, 1932), pp. 56, 86, and elsewhere.

[8] Franz Čuhel, *Zur Lehre von den Bedürfnissen* (Wien, 1908), pp. 186–216.

[9] Arnold M. Rose, *Theory and Method in the Social Sciences* (Minneapolis: University of Minnesota, 1954), p. 263.

who once wrote that pure theory must necessarily be of a form such that "what it proves must be contained in the assumptions and cannot be obtained from any other sources." Hence, "to criticize a proposition of pure theory *as such* as tautological, or circular, or as assuming what it requires to prove, is beside the point."[10]

The assumptions that consumers act to "maximize their expected satisfaction" and entrepreneurs act to "maximize their expected profits" are sometimes considered as "empirically empty" or "tautological" because (a) we cannot know whether or not the consumers and entrepreneurs really believe that their actions are the best of the alternatives considered, (b) whatever they do can thus be interpreted as being what they consider to be "the best under the circumstances," and (c) as long as we do not know their tastes, preferences, and alternative anticipations, we cannot deduce any particular way of acting from the assumptions standing by themselves.

The point, however, is that the assumptions do not stand by themselves but are combined with other assumptions, including some about certain substantive changes which are observable by us as well as by the consumers or firms concerned. Our theory does not tell or explain what the decision makers have been doing, or have preferred to do, or have avoided to do before the changes in question occurred; it deals only with the ways in which decisions will be changed by the occurrence and its repercussions. No matter how many pounds of lemons consumers have been purchasing, they will try to purchase more; the theory tells us this from the assumptions furnished. No matter how many yards of grey goods manufacturers have been producing, they will produce more; the theory can tell us this on the basis of the assumptions supplied.[11] An assumption apparently quite "empty" or without empirical implications as long as it stands alone may become of definite empirical significance when combined in a model with other assumptions.

Finally, there is that polemical red herring dragged across the trail: veiled charges of sympathizing with controversial value judgments, "indirect" accusations based on guilt by association with others accused directly. I was first inclined to overlook it, because I thought that silence on my part would be the most eloquent response. I have been persuaded, however, that my rejoinder would be sadly incomplete without a comment on this confusion, innocent or deliberate, between positive economics and political evaluation.

Not a single passage or sentence in my article could in fairness be interpreted as dealing with political implications, value judgments, policy advice, welfare economics. Yet, in the last pages of his reply Professor Hutchison throws a heavy barrage against alleged welfare implications of my argument. Furious salvos are

[10] T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London: Macmillan, 1938), p. 36.

[11] See my reply to R. A. Gordon, who had interpreted methodological subjectivism as leaving "theory saying that businessmen do what they do because they do it." Fritz Machlup, *The Economics of Sellers' Competition* (Baltimore: Johns Hopkins Press, 2952), p. 36.

fired against the "maximum of utility for society" in connection with Walras and free competition, and against "wholesale political conclusions" in connection with Mises and liberal economic policies.

If Professor Hutchison really believes that my "doctrines on verification and verifiability" can be used (and are designed?) to "propagate sweeping political dogmas" and to defend "politico-intellectual obscurantism" he does precisely what he apparently considers objectionable in others: he confuses normative (ethical) judgments with positive propositions of economic theory. Yet, at the same time he claims to be an advocate of Professor Friedman's dictum that "It is necessary to be more specific about the content of existing economic theory and to *distinguish among its different branches*.[12] Would that Professor Hutchison practiced what he advocates.

The Johns Hopkins University FRITZ MACHLUP

A NOTE ON TESTING THE TRANSITIVITY AXIOM

Recently Pfouts[1] in discussing the testing of certain fundamental assumptions in the theory of ordinal utility has stated "that if we can show empirically that the transitivity rule is not obeyed by individual consumers then there is something wrong with the existing theory of consumer's preferences." Pfouts cites the results of May[2] as preliminary evidence indicating a tendency toward non-transitivity in human choice.

It is the purpose of this note to clarify the conditions under which the transitivity axiom would be refuted and to make clear the type of tests under which the axiom could never be refuted. The transitivity condition stated in Pfouts' terminology is that if $X_i \, R \, X_j$, and $X_j \, R \, X_k$, then it must follow that $X_i \, R \, X_k$, where R indicates the relation "preferred or indifferent to." But we must recognize that in ordinal utility theory $X_k \, R \, X_i$ and even $X_j \, R \, X_i$ are also allowable relationships provided that all the R's in the relations represent the condition of "indifference." In fact in ordinary utility theory one can use the definition that indifference exists between X_i and X_j if and only if $X_i R \, X_j$ and $X_j \, R \, X_i$.

We are now able to appreciate that any binary choice experiment, and May's in particular, in which the individual being tested *must* make a choice between X_i and X_j only demonstrates the relation R; the existence of a cyclical pattern $X_i \, R \, X_j \, R \, X_k \, R \, X_i$ in no way contradicts the axiom of transitivity. An equivalent statement of the transitivity condition to be tested which pinpoints the critical test to be made is that if $X_i \, R \, X_j$, $X_j \, R \, X_k$, then not $X_k \, P \, X_i$, where P indicates the relation "preferred to."

We may easily demonstrate by an intuitive approach that cyclical choice patterns of R are admissible. Consider an individual who is indifferent to X_i,

[12] Milton Friedman, *op. cit.*, p. 41. Emphasis supplied.

[1] R. W. Pfouts, "Prolegomena to the Testing of Utility Theory," *Southern Economic Journal*, Vol. XXII, October 1955, pp. 178–188.

[2] K. O. May, "Transitivity, Utility and Aggregation in Preference Patterns," *Econometrica*, Vol. XXII, January 1954, pp. 1–13.