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## **Essays in Positive Economics Part I - The Methodology of Positive Economics \***

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### **Introduction**

In his admirable book on *The Scope and Method of Political Economy* John Neville Keynes distinguishes among "a positive science ... a body of systematized knowledge concerning what is; a normative or regulative science...[,] a body of systematized knowledge discussing criteria of what ought to be...[,]an art... [,] a system of rules for the attainment of a given end"; comments that "confusion between them is common and has been the source of many mischievous errors"; and urges the importance of "recognizing a distinct positive science of political economy." <sup>1</sup>

This paper is concerned primarily with certain methodological problems that arise in constructing the "distinct positive science" Keynes called for - in particular, the problem how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the "body of systematized knowledge concerning what is." But the confusion Keynes laments is still so rife and so much of a hindrance to the recognition that economics can be, and in part is, a positive science that it seems well to preface the main body of the paper with a few remarks about the relation between positive and normative economics.

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### **I. THE RELATION BETWEEN POSITIVE AND NORMATIVE ECONOMICS**

Confusion between positive and normative economics is to some extent inevitable. The subject matter of economics is regarded by almost everyone as vitally important to himself and within the range of his own experience and competence; it is

\* I have incorporated bodily in this article without special reference most of my brief "Comment" in *A Survey of Contemporary Economics*, Vol. II (B. F. Haley, ed.) (Chicago: Richard D. Irwin, Inc., 1952), pp. 455-57.

I am indebted to Dorothy S. Brady, Arthur F. Burns, and George J. Stigler for helpful comments and criticism.

1. (London: Macmillan & Co., 1891), pp. 34-35 and 46.

the source of continuous and extensive controversy and the occasion for frequent legislation. Self-proclaimed “experts” speak with many voices and can hardly all be regarded as disinterested; in any event, on questions that matter so much, “expert” opinion could hardly be accepted solely on faith even if the “experts” were nearly unanimous and clearly disinterested.<sup>2</sup> The conclusions of positive economics seem to be, and are, immediately relevant to important normative problems, to questions of what ought to be done and how any given goal can be attained. Laymen and experts alike are inevitably tempted to shape positive conclusions to fit strongly held normative preconceptions and to reject positive conclusions if their normative implications - or what are said to be their normative implications - are unpalatable.

Positive economics is in principle independent of any particular ethical position or normative judgments. As Keynes says, it deals with “what is,” not with “what ought to be.” Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields. In short, positive economics is, or can be, an “objective” science, in precisely the same sense as any of the physical sciences. Of course, the fact that economics deals with the interrelations of human beings, and that the investigator is himself part of the subject matter being investigated in a more intimate sense than in the physical sciences, raises special difficulties in achieving objectivity at the same time that it provides the social scientist with a class of data not available to the physical sci-

2. Social science or economics is by no means peculiar in this respect - witness the importance of personal beliefs and of “home” remedies in medicine wherever obviously convincing evidence for “expert” opinion is lacking. The current prestige and acceptance of the views of physical scientists in their fields of specialization - and, all too often, in other fields as well - derives, not from faith alone, but from the evidence of their works, the success of their predictions, and the dramatic achievements from applying their results. When economics seemed to provide such evidence of its worth, in Great Britain in the first half of the nineteenth century, the prestige and acceptance of “scientific economics” rivaled the current prestige of the physical sciences.

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entist. But neither the one nor the other is, in my view, a fundamental distinction between the two groups of sciences.<sup>3</sup>

Normative economics and the art of economics, on the other hand, cannot be independent of positive economics. Any policy conclusion necessarily rests on a prediction about the consequences of doing one thing rather than another, a prediction that must be based - implicitly or explicitly - on positive economics. There is not, of course, a one-to-one relation between policy conclusions and the conclusions of positive economics; if there were, there would be no separate normative science. Two individuals may agree on the consequences of a particular piece of legislation. One may regard them as desirable on

balance and so favor the legislation; the other, as undesirable and so oppose the legislation.

I venture the judgment, however, that currently in the Western world, and especially in the United States, differences about economic policy among disinterested citizens derive predominant from different predictions about the economic consequences of taking action - differences that in principle can be eliminated by the progress of positive economics - rather than from fundamental differences in basic values, differences about which men can ultimately only fight. An obvious and not unimportant example is minimum-wage legislation. Underneath the welter of arguments offered for and against such legislation there is an underlying consensus on the objective of achieving a "living wage" for all, to use the ambiguous phrase so common in such discussions. The difference of opinion is largely grounded on an implicit or explicit difference in predictions about the efficacy of this particular means in furthering the agreed-on end. Proponents believe (predict) that legal minimum wages diminish poverty by raising the wages of those receiving less than the minimum wage as well as of some receiving more than the

3. The interaction between the observer and the process observed that is so prominent a feature of the social sciences, besides its more obvious parallel in the physical sciences, has a more subtle counterpart in the indeterminacy principle arising out of the interaction between the process of measurement and the phenomena being measured. And both have a counterpart in pure logic in Gödel's theorem, asserting the impossibility of a comprehensive self-contained logic. It is an open question whether all three can be regarded as different formulations of an even more general principle.

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minimum wage without any counterbalancing increase in the number of people entirely unemployed or employed less advantageously than they otherwise would be. Opponents believe (predict) that legal minimum wages increase poverty by increasing the number of people who are unemployed or employed less advantageously and that this more than offsets any favorable effect on the wages of those who remain employed. Agreement about the economic consequences of the legislation might not produce complete agreement about its desirability, for differences might still remain about its political or social consequences but, given agreement on objectives, it would certainly go a long way toward producing consensus.

Closely related differences in positive analysis underlie divergent views about the appropriate role and place of trade-unions and the desirability of direct price and wage controls and of tariffs. Different predictions about the importance of so-called "economies of scale" account very largely for divergent views about the desirability or necessity of detailed government regulation of industry and even of socialism rather than private enterprise. And this list could be extended indefinitely.<sup>4</sup> Of course, my judgment that the major differences about economic policy in the Western world are of this kind is itself a "positive" statement to be accepted or rejected on the basis of empirical evidence.

If this judgment is valid, it means that a consensus on "correct" economic policy depends much less on the progress of normative

economics proper than on the progress of a positive economics yielding conclusions that are, and deserve to be, widely accepted. It means also that a major reason for dis-

4. One rather more complex example is stabilization policy. Superficially, divergent views on this question seem to reflect differences in objectives; but I believe that this impression is misleading and that at bottom the different views reflect primarily different judgments about the source of fluctuations in economic activity and the effect of alternative countercyclical action. For one major positive consideration that accounts for much of the divergence see "The Effects of a Full-Employment Policy on Economic Stability: A Formal Analysis," *infra*, pp. 117-32. For a summary of the present state of professional views on this question see "The Problem of Economic Instability," a report of a subcommittee of the Committee on Public Issues of the American Economic Association, *American Economic Review*, XL (September, 1950), 501-38.

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inguishing positive economics sharply from normative economics is precisely the contribution that can thereby be made to agreement about policy.

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## II . POSITIVE ECONOMICS

The ultimate goal of a positive science is the development of theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed. Such a theory is, in general, a complex intermixture of two elements. In part, it is a "language" designed to promote "systematic and organized methods of reasoning." <sup>5</sup> In part, it is a body of substantive hypotheses designed to abstract essential features of complex reality.

Viewed as a language, theory has no substantive content; it of tautologies. Its function is to serve as a filing system organizing empirical material and facilitating our understanding of it; and the criteria by which it is to be judged are appropriate to a filing system. Are the categories clearly and precisely defined? Are they exhaustive? Do we know where to file each individual item, or is there considerable ambiguity? Is the system of headings and subheadings so designed that we can quickly find an item we want, or must we hunt from place to place? Are the items we shall want to consider jointly filed? Does the filing system avoid elaborate cross-references?

The answers to these questions depend partly on logical, partly on factual, considerations. The canons of formal logic alone can show whether a particular language is complete and consistent, that is, whether propositions in the language are "right" or "wrong". Factual evidence alone can show whether the categories of the "analytical filing system" have a meaningful empirical counterpart, that is, whether they are useful in analyzing particular class of concrete problems. <sup>6</sup> The simple example of "supply" and "demand" illustrates both this point and the pre-

5. Final quoted phrase from Alfred Marshall, "The Present Position of Economics" (1885), reprinted in *Memorials of Alfred Marshall*, ed. A. C. Pigou (London: Macmillan & Co., 1925), p.

164. See also “The Marshallian Demand Curve,” *infra*, pp. 56-57, 90-91.

6. See “Lange on Price Flexibility and Employment: A Methodological Criticism,” *infra*, pp. 282-89.

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ceding list of analogical questions. Viewed as elements of the language of economic theory, these are the two major categories into which factors affecting the relative prices of products or factors of production are classified. The usefulness of the dichotomy depends on the “empirical generalization that an enumeration of the forces affecting demand in any problem and of the forces affecting supply will yield two lists that contain few items in common.”<sup>7</sup> Now this generalization is valid for markets like the final market for a consumer good. In such a market there is a clear and sharp distinction between the economic units that can be regarded as demanding the product and those that can be regarded as supplying it. There is seldom much doubt whether a particular factor should be classified as affecting supply, on the one hand, or demand, on the other; and there is seldom much necessity for considering cross-effects (cross-references) between the two categories. In these cases the simple and even obvious step of filing the relevant factors under the headings of “supply” and “demand” effects a great simplification of the problem and is an effective safeguard against fallacies that otherwise tend to occur. But the generalization is not always valid. For example, it is not valid for the day-to-day fluctuations of prices in a primarily speculative market. Is a rumor of an increased excess-profits tax, for example, to be regarded as a factor operating primarily on today’s supply of corporate equities in the stock market or on today’s demand for them? In similar fashion, almost every factor can with about as much justification be classified under the heading “supply” as under the heading “demand.” These concepts can still be used and may not be entirely pointless; they are still “right” but clearly less useful than in the first example because they have no meaningful empirical counterpart.

Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to “explain.” Only factual evidence can show whether it is “right” or “wrong” or, better, tentatively “accepted” as valid or “rejected.” As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is

7. “The Marshallian Demand Curve,” *infra*, p. 57.

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comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted (“frequently” or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never “prove” a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been “confirmed” by experience.

To avoid confusion, it should perhaps be noted explicitly that the “predictions” by which the validity of a hypothesis is tested need not be

about phenomena that have not yet occurred, that is, need not be forecasts of future events; they may be about phenomena that have occurred but observations on which have not yet been made or are not known to the person making the prediction. For example, a hypothesis may imply that such and such must have happened in 1906, given some other known circumstances. If a search of the records reveals that such and such did happen, the prediction is confirmed; if it reveals that such and such did not happen, the prediction is contradicted.

The validity of a hypothesis in this sense is not by itself a sufficient criterion for choosing among alternative hypotheses. Observed facts are necessarily finite in number; possible hypotheses, infinite. If there is one hypothesis that is consistent with available evidence, there are always an infinite number that are.<sup>8</sup> For example, suppose a specific excise tax on a particular commodity produces a rise in price equal to the amount of the tax. This is consistent with competitive conditions, a stable demand curve, and a horizontal and stable supply curve. But it is also consistent with competitive conditions and a positively or negatively sloping supply curve with the required compensating shift in the demand curve or the supply curve; with monopolistic conditions, constant marginal costs, and stable demand curve, of the particular shape required to produce this result; and so on indefinitely. Additional evidence with which the

8. The qualification is necessary because the "evidence" may be internally contradictory, so there may be no hypothesis consistent with it. See also "Lange on Price Flexibility and Employment," *infra*, pp. 2 82-83.

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hypothesis is to be consistent may rule out some of these possibilities; it can never reduce them to a single possibility alone capable of being consistent with the finite evidence. The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria "simplicity" and "fruitfulness," themselves notions that defy completely objective specification. A theory is "simpler" the less the initial knowledge needed to make a prediction within a given field of phenomena; it is more "fruitful" the more precise the resulting prediction, the wider the area within which the theory yields predictions, and the more additional lines for further research it suggests. Logical completeness and consistency are relevant but play a subsidiary role; their function is to assure that the hypothesis says what it is intended to say and does so alike for all users - they play the same role here as checks for arithmetical accuracy do in statistical computations.

Unfortunately, we can seldom test particular predictions in the social sciences by experiments explicitly designed to eliminate what are judged to be the most important disturbing influences. Generally, we must rely on evidence cast up by the "experiments" that happen to occur. The inability to conduct so-called "controlled experiments" does not, in my view, reflect a basic difference between the social and physical sciences both because it is not peculiar to the social sciences - witness astronomy - and because the distinction between a controlled experiment and uncontrolled experience is at best one of degree. No experiment can be completely controlled, and every experience is partly

controlled, in the sense that some disturbing influences are relatively constant in the course of it.

Evidence cast up by experience is abundant and frequently as conclusive as that from contrived experiments; thus the inability to conduct experiments is not a fundamental obstacle to testing hypotheses by the success of their predictions. But such evidence is far more difficult to interpret. It is frequently complex and always indirect and incomplete. Its collection is often arduous, and its interpretation generally requires subtle

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analysis and involved chains of reasoning, which seldom carry real conviction. The denial to economics of the dramatic and evidence of the “crucial” experiment does hinder the adequate testing of hypotheses; but this is much less significant than the difficulty it places in the way of achieving a reasonably prompt and wide consensus on the conclusions justified by the available evidence. It renders the weeding-out of unsuccessful hypotheses slow and difficult. They are seldom downed for good and are always cropping up again.

There is, of course, considerable variation in these respects. Occasionally, experience casts up evidence that is about as direct, dramatic, and convincing as any that could be provided by controlled experiments. Perhaps the most obviously important example is the evidence from inflations on the hypothesis that a substantial increase in the quantity of money within a relatively short period is accompanied by a substantial increase in prices. Here the evidence is dramatic, and the chain of reasoning required to interpret it is relatively short. Yet, despite numerous instances of substantial rises in prices, their essentially one-to-one correspondence with substantial rises in the stock of money, and the wide variation in other circumstances that might appear to be relevant, each new experience of inflation brings forth vigorous contentions, and not only by the lay public, that the rise in the stock of money is either an incidental effect of a rise in prices produced by other factors or a purely fortuitous and unnecessary concomitant of the price rise.

One effect of the difficulty of testing substantive economic hypotheses has been to foster a retreat into purely formal or tautological analysis.<sup>9</sup> As already noted, tautologies have an extremely important place in economics and other sciences as a specialized language or “analytical filing system.” Beyond this, formal logic and mathematics, which are both tautologies, are essential aids in checking the correctness of reasoning, discovering the implications of hypotheses, and determining whether supposedly different hypotheses may not really be equivalent or wherein the differences lie.

But economic theory must be more than a structure of tautol-

9. See “Lange on Price Flexibility and Employment,” *infra*, *passim*.

ogies if it is to be able to predict and not merely describe the consequences of action; if it is to be something different from disguised mathematics.<sup>10</sup> And the usefulness of the tautologies themselves

ultimately depends, as noted above, on the acceptability of the substantive hypotheses that suggest the particular categories into which they organize the refractory empirical phenomena.

A more serious effect of the difficulty of testing economic hypotheses by their predictions is to foster misunderstanding of the role of empirical evidence in theoretical work. Empirical evidence is vital at two different, though closely related, stages: in constructing hypotheses and in testing their validity. Full and comprehensive evidence on the phenomena to be generalized or “explained” by a hypothesis, besides its obvious value in suggesting new hypotheses, is needed to assure that a hypothesis explains what it sets out to explain - that its implications for such phenomena are not contradicted in advance by experience that has already been observed.<sup>11</sup> Given that the hypothesis is

10. See also Milton Friedman and L. J. Savage, “The Expected-Utility Hypothesis and the Measurability of Utility,” *Journal of Political Economy*, LX (December, 1952), 463-74, esp. pp. 465-67.

11. In recent years some economists, particularly a group connected with the Cowles Commission for Research in Economics at the University of Chicago, have placed great emphasis on a division of this step of selecting a hypothesis consistent with known evidence into two substeps: first, the selection of a class of admissible hypotheses from all possible hypotheses (the choice of a “model” in their terminology); second, the selection of one hypothesis from this class (the choice of a “structure”). This subdivision may be heuristically valuable in some kinds of work, particularly in promoting a systematic use of available statistical evidence and theory. From a methodological point of view, however, it is an entirely arbitrary subdivision of the process of deciding on a particular hypothesis that is on a par with many other subdivisions that may be convenient for one purpose or another or that may suit the psychological needs of particular investigators.

One consequence of this particular subdivision has been to give rise to the so-called “identification” problem. As noted above, if one hypothesis is consistent with available evidence, an infinite number are. But, while this is true for the class of hypotheses as a whole, it may not be true of the subclass obtained in the first of the above two steps - the “model.” It may be that the evidence to be used to select the final hypothesis from the subclass can be consistent with at most one hypothesis in it, in which case the “model” is said to be “identified”; otherwise it is said to be “unidentified.” As is clear from this way of describing the concept of “identification,” it is essentially a special case of the more general

[problem of selecting among the alternative hypotheses equally consistent with the evidence - a problem that must be decided by some such arbitrary principle Occam’s razor. The introduction of two substeps in selecting a hypothesis makes this problem arise at the two corresponding stages and gives it a special cast. While the class of all hypotheses is always unidentified, the subclass in a “model” need not be, so the problem arises of conditions that a “model” must satisfy to be identified. However useful the two substeps may be in some contexts, their introduction raises the danger that different

criteria will unwittingly be used in making the same kind of choice among alternative hypotheses at two different stages.

On the general methodological approach discussed in this footnote see Tryvge Haavelmo, "The Probability Approach in Econometrics," *Econometrica*, Vol. XII (1944), Supplement; Jacob Marschak, "Economic Structure, Path, Policy, and Prediction," *American Economic Review*, XXXVII, (May, 1947), 81-84, and "Statistical Inference in Economics: An Introduction," in T. C. Koopmans (ed), *Statistical Inference in Dynamic Economic Models* (New York: John Wiley & Sons, 1950); T. C. Koopmans, "Statistical Estimation of Simultaneous Economic Relations," *Journal of the American Statistical Association*, XL (December, 1945), 448-66; Gershon Cooper, "The Role of Economic Theory in Econometric Models," *Journal of Farm Economics*, XXX (February, 1948), 101-16. On the identification problem see Koopmans, "Identification Problems in Econometric Model Construction," *Econometrica*, XVII (April, 1949), 125-44; Leonid Hurwicz, "Generalization of the Concept of Identification," in Koopmans (ed.), *Statistical Inference in Dynamic Economic Models*.]

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consistent with the evidence at hand, its further testing involves deducing from it new facts capable of being observed but not previously known and checking these deduced facts against additional empirical evidence. For this test to be relevant, the deduced facts must be about the class of phenomena the hypothesis is designed to explain; and they must be well enough defined so that observation can show them to be wrong.

The two stages of constructing hypotheses and testing their validity are related in two different respects. In the first place, the particular facts that enter at each stage are partly an accident of the collection of data and the knowledge of the particular investigator. The facts that serve as a test of the implications of a hypothesis might equally well have been among the raw material used to construct it, and conversely. In the second place, the process never begins from scratch; the so-called "initial stage" itself always involves comparison of the implications of earlier set of hypotheses with observation; the contradiction these implications is the stimulus to the construction of new

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hypotheses or revision of old ones. So the two methodologically distinct stages are always proceeding jointly.

Misunderstanding about this apparently straightforward process centers on the phrase "the class of phenomena the hypothesis is designed to explain." The difficulty in the social sciences of getting new evidence for this class of phenomena and of judging its conformity with the implications of the hypothesis makes it tempting to suppose that other, more readily available, evidence is equally relevant to the validity of the hypothesis - to suppose that hypotheses have not only "implications" but also "assumptions" and that the conformity of these "assumptions" to "reality" is a test of the validity of the hypothesis

*different from* or *additional to* the test by implications. This widely held view is fundamentally wrong and productive of much mischief. Far from providing an easier means for sifting valid from invalid hypotheses, it only confuses the issue, promotes misunderstanding about the significance of empirical evidence for economic theory, produces a misdirection of much intellectual effort devoted to the development of positive economics, and impedes the attainment of consensus on tentative hypotheses in positive economics.

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).<sup>12</sup> 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

12. The converse of the proposition does not of course hold: assumptions that are unrealistic (in this sense) do not guarantee a significant theory.

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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.

To put this point less paradoxically, the relevant question to ask about the “assumptions” of a theory is not whether they are descriptively “realistic,” for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. The two supposedly independent tests thus reduce to one test.

The theory of monopolistic and imperfect competition is one example of the neglect in economic theory of these propositions. The development of this analysis was explicitly motivated, and its wide acceptance and approval largely explained, by the belief that the assumptions of “perfect competition” or “perfect monopoly” said to underlie neoclassical economic theory are a false image of reality. And this belief was itself based almost entirely on the directly perceived descriptive inaccuracy of the assumptions rather than on any recognized contradiction of predictions derived from neoclassical economic theory.

The lengthy discussion on marginal analysis in the *American Economic Review* some years ago is an even clearer, though much less important, example. The articles on both sides of the controversy largely neglect what seems to me clearly the main issue - the conformity to experience of the implications of the marginal analysis - and concentrate on the largely irrelevant question whether businessmen do or do not in fact reach their decisions by consulting schedules, or curves, or multivariable

functions showing marginal cost and marginal revenue. <sup>13</sup>

Perhaps these

13. See R. A. Lester, "Shortcomings of Marginal Analysis for Wage-Employment Problems," *American Economic Review*, XXXVI (March, 1946), 62-82; Fritz Machlup, "Marginal Analysis and Empirical Research," *American Economic Review*, XXXVI (September, 1946), 519-54; R. A. Lester, "Marginalism, Minimum Wages, and Labor Markets," *American Economic Review*, XXXVII (March, 1947), 135-48; Fritz Machlup, "Rejoinder to an Antimarginalist," *American Economic Review*, XXXVII (March, 1947), 148-54; G. J. Stigler, "Professor Lester and the Marginalists," *American Economic Review*, XXXVII (March, 1947), 154-57; H. M. Oliver, Jr., "Marginal Theory and Business Behavior," *American Economic Review*, XXXVII (June, 1947), 375-83; R. A. Gordon, ["Short-Period Price Determination in Theory and Practice," *American Economic Review*, XXXVIII (June, 1948), 265-88.

It should be noted that, along with much material purportedly bearing on the validity of the "assumptions" of marginal theory, Lester does refer to evidence on the conformity of experience with the implications of the theory, citing the reactions of employment in Germany to the Papen plan and in the United States to changes in minimum-wage legislation as examples of lack of conformity. However, Stigler's brief comment is the only one of the other papers that refers to this evidence. It should also be noted that Machlup's thorough and careful exposition of the logical structure and meaning of marginal analysis is called for by the misunderstandings on this score that mar Lester's paper and almost conceal the evidence he presents that is relevant to the key issue he raises. But, 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. The papers by Oliver and Gordon are the most extreme in the exclusive concentration on the conformity of the behavior of businessmen with the "assumptions" of the theory.

**HHC:** [bracketed] content displayed on p.16 of original.

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two examples, and the many others they readily suggest, will serve to justify a more extensive discussion of the methodological principles involved than might otherwise seem appropriate.

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