feel certainty about the effects of a change in tastes (quantity demanded), rather than because the conditions for dynamic stability indicate a price rise. Smith's proposition is not inconsistent with demand and supply curves, but by itself is a qualitative dynamic proposition requiring no function at all.

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COMMENT

By Paul A. Samuelson

Professor Donald F. Gordon has listed five criticisms of the methodology of my *Foundations of Economic Analysis*. To the extent that I understand his arguments, it is primarily his fourth criticism concerned with the Correspondence Principle relating dynamic stability and comparative statics that seems to me to be in need of amplification and qualification. Here are my reactions to the specific points Gordon makes. I hope that these hasty interpretations will be regarded as tentative.

**Criticism 1.** The hypothesis that a competitive firm with an unchanging cost curve will never reduce its quantity supplied when market price rises is according to my use of terms a meaningful proposition: i.e., it is conceptually an empirically refutable proposition. I continue to aver that the hypothesis of profit maximization is sufficient to deduce this hypothesis.

Gordon's criticism seems to me to be concerned not so much with the sufficiency of my reasoning but rather with the quite different problem of how we go about deciding that a conceptually meaningful proposition is or is not a fruitful hypothesis and whether as a result of any particular observations we are to decide that it has or has not been refuted.¹ This is an important question: indeed much of the whole theory of statistical inference is concerned with little else, and in the last decades due to the work of Haavelmo, Koopmans, Frisch, Elmer Working, and other writers on the problem of “identification” a small start on a satisfactory theory has been made. I do not think that I discussed this issue anywhere in *Foundations*, and in retrospect I feel little urge to have done so. To test, refute, or “verify” a meaningful proposition is a tough empirical job and no

¹ E.g., a fall in quantity supplied by a maximizing competitor may have been due to a simultaneous factor price increase.
amount of flossy deduction can obviate this. Realizing this should not be disillusioning; and it certainly should not tempt one to belittle meaningful propositions in favor of empty ones.

Criticism 2. This does overlap with the first point. Again Gordon seems to me to be saying: "In real life, observed changes are often the result of simultaneous changes in numerous parameters; but just which parameters have changed or how great are the relative quantitative magnitudes of the changes is a most difficult question to answer. Therefore, it is wrong (or misleading? or useless?) to try to set up meaningful hypotheses about the *ceteris paribus* effects of changes in each datum taken separately." I do not think the second sentence follows from the truth of the first.

There is one special point in this second criticism that may require special comment. I do indeed think there is point in analyzing changes in "tastes." How will thriftiness affect capital formation? How will a shift from beer to tea affect markets? How will knowledge on the part of the consumer that liver is good for anemia affect the relative prices of kidneys and liver? These are perfectly legitimate questions to ask, whether or not we have an "explanation" for the change in tastes. Indeed, in a fundamental methodological sense, it is precisely the changes in the parameters or data of a system that are, within the framework of *that* system, unexplainable. I do not see that I, or Schelling, or Hicks (some of whose *Value and Capital* theorems refer to defined changes in tastes) have any reason to avoid trying to answer such questions. I only wish that we had better success in doing so.

Criticism 3. To me this says that there may be ways of forming meaningful hypotheses other than by postulating maximization or dynamic stability. Of course, there are. No one has ever doubted it. Observe market behavior over time; make statistical scatter diagrams; and if the result suggests to you the hypothesis that the marginal propensity to consume is exactly .925, or that the elasticity of demand for rye is −.70, or that the propensity to save schedule is concave from above — then well and good, for these are all meaningful, refutable hypotheses. (The last four sentences of Gordon's criticism deal with a different issue and not one that requires any comment here.)

Criticism 4. The point made here seems to me to be a very fruitful one. The Correspondence Principle is a vague line of deductive reasoning by which we can under certain circumstances deduce, from the hypothesis that a stipulated system is dynamically stable, various
implied hypotheses about its comparative statical properties appropriate to a permanent shift in a parameter to which corresponds a new stationary equilibrium position. There is much that is unsatisfactory about the exposition of this heuristic principle and about our knowledge of its deductive properties. But the point that Gordon is here concerned with is a different one. In Foundations I did not content myself with deriving these formal properties: in addition, I stepped forward as a man of the world and casual empiricist and stated my opinion that the hypothesis of dynamical stability was a "realistic" one to make. I am no longer so sure of this.

True, there is something vaguely persuasive about the doctrine of "the nonpersistence of unstable states" that Gordon quotes from my book: indeed, as my reference to L. J. Henderson will show, this is not a new thought originated by Keynes to refute criticisms made in this Journal concerning the properties of the marginal propensity to consume; instead it can be traced back to Hippocrates, to Maxwell, to Gibbs, to Darwin, and to a host of philosophers; and when the Smyth Report told us that a certain sized arrangement of Uranium 235 would explode, most of us amateurs used this heuristic reasoning to predict that such concentrations of Uranium 235 were not likely to be found in nature by geologists.

None the less, you never get something for nothing and never empirical hypotheses from empty deductive definitions. At best your observation can tell you only that the real world (or some subset of it) is not exploding; your theoretical model or system will always be an idealized representation of the real world with many variables ignored; it may be precisely the ignored variables that keep the real world stable, and it takes a significant act of inductive inference to rule this out and permit the Correspondence Principle to deduce properties of the idealized model.

Here is an example. Some critics of Keynes alleged that he did believe, and needed to believe, that the consumption function is convex. Closer reading of his book shows this not to be so. Keynes replied arguing that all that he required was a marginal propensity to consume of less than one. And even if this were not granted, he felt that this would simply throw on the doubter the need to explain why the capitalistic system is not hopelessly unstable.2

2. My discussion of the Correspondence Principle should have made clearer the possibility that the world is stable but that our model — in this case a simple income model — is too simple to portray the world correctly. Thus, the marginal propensity to consume might exceed unity, but tightness of the money supply and movements of interest rates might be what prevents explosion. This raises questions of empirical judgment.
Well, maybe the system is unstable. That is one possibility, and as Gordon cogently points out, many of the cobweb cycles and auto-relaxation trade cycle theories of such moderns as Kaldor, Goodwin, Hicks, and others are squarely based on the notion of a system that is locally unstable at its stationary levels so that it oscillates — but because of such nonlinear elements as full-employment ceilings, capacity limitations, impossibility of disinvesting faster than at certain limiting rates, the system oscillates with a preferred finite amplitude.

A priori reasoning will not settle this empirical question. During the late 1930's I felt, and so perhaps did writers like Professor Lloyd Metzler, that the observed behavior of the macroaggregates of the American economy was most compatible with the hypothesis of dynamic stability: in those days it seemed hard rather than easy for the system to generate self-aggravating cumulative movements of explosive type. This view may have been wrong, and we may have been too confident in expressing it. Or it may have been a fruitful hypothesis for that period but not one for the present decade. Or it may still be fruitful. These are important empirical questions that cannot be answered by dividing dichotomously the world's possibilities into categories of stable and unstable and inferring that our observed world by its not having exploded away is necessarily in the stable category. So I concur in Gordon's misgiving.

Criticism 5. Here the question is whether older economists used the so-called Correspondence Principle in arriving at comparative statical results. And did they know they were using it? To the latter question the answer is surely, No. They could not have known the name, obviously; and they were not so self-conscious as to analyze the logical structure of many of their intuitions. But were they, so to speak, talking prose all their lives without knowing it?

I have never seriously examined the writings of Adam Smith and other economists to see just how much of the logic involved in the Correspondence Principle can by sympathetic reasoning be imputed

3. Readers of the modern cycle literature may receive the misleading impression that economists started with linear models and then arrived at nonlinear ones. The historical patterns are the reverse: most of the literary models that antedate the work of Frisch, Tinbergen, Metzler, and myself were very definitely nonlinear. One of the things we thought worth showing was the fact that no separate theory of the turning points or of the four phases of the cycle was needed: the same linear model could be shown to be theoretically capable of producing all phases of the cycle. This does not deny that there may be some truth to Haberler's gibe that with their nonlinear models the econometricians are almost catching up to the literary economists.
to them. I therefore welcome Gordon's quoted passage from Smith and will give it brief mathematical formulation:

\[ dp/dt = k(D - S) \]

where \( k \) is a positive number that may be approximated by a constant and where \( D \) is the quantity demanded (as shown by the demand schedule) and \( S \) is the quantity supplied (as shown by the supply schedule). To parrot Gordon's words, "Very likely Adam Smith would be surprised to find that he talked in terms of differential equations as well as prose." But, of course, how we choose to describe the contents of his thought will not alter its content or be relevant to his terminology.

Granting equation (1), do we conclude that it supplants or contradicts the Correspondence Principle? Not at all. From (1) alone, Smith and Gordon cannot conclude anything about comparative statics. They can make the following comparative dynamic statement: "If price has been stationary, and if after time \( t_0 \) the demand curve shifts rightward (and remains there), then in the following time intervals we can expect price to rise from its initial level." That is all.

If now we want to make a comparative static statement, we must somehow add hypotheses. Thus, we might stipulate the empirical hypothesis that such a shift in demand will always be followed by an approach to a new stationary equilibrium. This is an hypothesis of dynamic stability: and with this hypothesis, we can infer from (1) that the comparative statical price change must be positive. But in so doing we are using — guess what? Of course, the Correspondence Principle.

In conclusion, may I repeat my lack of confidence that I have correctly apprehended Professor Gordon's friendly criticisms.

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4. An even better case is provided by Gordon's earlier example of the simple multiplier model: \( y_t = c y_{t-1} + I \), where all variables are measured in deviations from some equilibrium levels. Our primitive instinct is dynamic: a higher marginal propensity to consume \( c \) or \( I \) will raise money income. But if \( c \) is already greater than one, the comparative statical theorem tells us that a permanent rise in autonomous investment or consumption spending is compatible only with a lower level of income! Again, it is the Correspondence Principle that is involved, only this time on the pathological side.